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The Logical Writings of Karl Popper

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Karl Popper at Aoraki / Mount Cook, New Zealand in May 1945.

Preface

Although Karl Popper was one of the outstanding philosophers of the twentieth century, his writings on deductive logic are little known. They deserve to be known better: not only historically as part of the work of a great philosopher of science, but also systematically as a significant contribution to the debate on the foundations of logic. Here Popper advocates a view of logic which in more modern terms would be called "inferentialist" (he himself spoke of "inferential definitions" of logical signs). He developed this view of logic during his time in New Zealand (1937–1945) and afterwards in London[1], and published his results in a series of articles in the late 1940s. This work is far more than a by-product. After the composition of *The Open Society and Its Enemies* in 1940–1942 (Popper, 1945a)[2], which upon its publication in 1945 established Popper's fame as a social and political philosopher, he spent most of his research time during the following years on formal logic. His articles of 1947–1949 were written "with much enthusiasm"[3], and one can well imagine that Popper would have continued these investigations, had they found a better reception in the logic community at the time. Popper even planned to write a textbook on

[1] The frontispiece on the left shows a photograph of Popper taken during a holiday trip to Aoraki / Mount Cook, New Zealand, in May 1945. On the trip back he received a cable from Friedrich Hayek notifying him that the appointment committee of the London School of Economics and Political Science (LSE) had decided positively on his application for a readership in logic and scientific method, which shaped Popper's future career; cf. Dahrendorf (1995, pp. 422-427) and Hacohen (2002, p. 499).

[2] Together with *The Poverty of Historicism* (Popper, 1944a,b, 1945b); cf. Popper's letter to Carnap of 15 October 1942 (this volume, § 23.1).

[3] Cf. Popper (1974b, p. 1095, this volume, § 12, p. 217). This is confirmed by a circular of his wife Hennie, of Easter 1947, to a range of New Zealand friends in which she describes "his recent occupation with the salvation of logic" (Hennie Popper, 1947a): "He returned and simply dived back into his logic, saying only sh-sh-sh-sh and do you think one could type this sign on the typewriters? And there was a meeting of some biologists in London, and he went there for the whole day and came back at night late and returned again to his papers. He is terribly excited about it and says he is finding all sorts of simplifications and it's getting simpler and simpler, and more and more interesting! And it just absorbs him completely.". Brian Boyd has kindly pointed us to this passage.

elementary logic, as a remark in a letter to John C. Eccles of 10 November 1946 shows.[4]

Popper's writings on logic were reviewed by prominent mathematical logicians, and many other logicians and philosophers knew his papers or were at least aware of their existence, as occasional references show. Nevertheless, the overall consideration of this work as a philosophically significant contribution to formal logic and to the foundations of deductive reasoning was limited – something that remains the case today. To improve this situation, our idea was to produce an edition which allows the reader easy access to Popper's logical works, which at the same time is sufficiently comprehensive to cover most aspects of what we consider to be their essence.

The central part of this collection are the six articles published in 1947–1949 on the foundations of deductive logic (Chapters 2–7). They are also the topic of our introduction to Popper's inferentialist conception of logic (Chapter 1). These articles are accompanied by a short note of 1943 (Chapter 8). This note deals with negation in a manner that shows traces of what would be fully developed in the later articles, and which is interesting from the point of view of modern discussions on paraconsistent logics, the ex falso rule and disjunctive syllogism. These articles are furthermore accompanied by three later papers written in the 1950s, which exemplify that Popper continued to be interested in logic, although not primarily from the inferentialist perspective. Chapter 9 shows Popper's abiding interest in Tarski's work, here in the definitions of satisfaction and truth in contradistinction to logical consequence, which is the basis of the inferentialist approach. Chapter 10 takes up a discussion of the liar paradox by Fitch, arguing that declaring a liar sentence to be neither true nor false does not escape paradoxicality. Chapter 11 discusses the subjunctive conditional, which plays a significant role in philosophy of science when it comes to the meaning of natural laws. Chapter 12 is reproduced from Schilpp (1974) and presents Popper's reply to Lejewski (1974), who had dealt with Popper's inferential approach to logic. This reply contains many interesting comments on the background to Popper's development of his inferentialist approach. In Chapter 13 we reprint the reviews of Popper's articles on logic. These were written by renowned logicians, and some of them were very critical of Popper's approach, both for conceptual reasons and for reasons of technical exposition. We doubt, however, that these reviews were the reason for the poor reception of Popper's logical works then and now. Even the most critical of these reviews left open the possibility to pursue certain basics of Popper's works in revised forms. We would rather claim that Popper's inferentialism as well as his extensive discussion of dualities and non-classical negations (beyond just intuitionism) was too much ahead of its time. Inferentialism, proof-theoretic semantics and corresponding theories of meaning, apart from their implicit presence

[4] Popper (1946b); communicated to us by Brian Boyd. Cf. also Popper's (1946a) draft of a letter to Alexander Carr-Saunders, the then director of the LSE, in which he writes: "I may say that I am at present preparing a textbook on formal logic, not because I like writing a textbook (it interferes, on the contrary, badly with my own research programme) but because I find it necessary for my students. The existing textbooks have aims totally different from what I consider to be the aim of a modern introductory course in Logic.". A table of contents with possible titles for such a textbook can be found in Popper's Nachlass (KPS Box 371, Folder 1).

in the work of Gentzen, became prominent only in the late 1960s in the aftermath of Prawitz's (1965) interpretation of natural deduction and the establishment of a general proof theory that made the structure of proofs and inferences a topic of philosophical interest.

The second part of this volume presents a number of manuscripts from Popper's Nachlass, which are related to the published papers. Chapter 14 is a draft of a joint paper by Bernays and Popper that did not make it into a publication. Chapter 15 discusses the relation between logical and descriptive signs and situates it in relation to Tarski's concept of logical consequence and to ideas by Carnap in his *Introduction to Semantics* (Carnap, 1942). Chapter 16 is an introduction to an intended study of classical and non-classical negations, which figures prominently in Chapters 5 and 6. Chapter 17 is a somewhat later draft from 1952 which tries to derive truth tables from inferential characterizations. Chapter 18 deals with the distinction between derivation and demonstration, a topic that according to our assessment Popper would have pursued more deeply had he continued to systematically publish in logic. Chapters 19 and 20 are lecture notes (from 1939–1941) and general considerations on the origins of modern logic, respectively. These texts put modern formal logic in a broader philosophical perspective and elucidate how Popper saw logic as a discipline. We here present only a selection of these notes, which we consider to be representative of Popper's views. There are considerably more manuscripts in his Nachlass, though many of them are variants of what is published here.

In the third part we reproduce letters to and from Popper dealing with his logical works. As with the second part, this is a selection of items based on what we consider best suited to situate Popper's logical work and its background in the context of his interactions with colleagues. For somebody who wants to study sophisticated historical issues, letters exchanged with other authors might be relevant, many of which are available in the Karl Popper Collection Klagenfurt and at the Hoover Institution Library & Archives, Stanford.[5]

We decided to omit from this collection materials that deal with one nonetheless important theme, namely Popper's work on Boolean algebra. Popper had studied Boolean algebra already before he moved on to the inferential approach to logic presented here, and studied and discussed it for the rest of his career, combining purely algebraic (lattice-theoretic) investigations with his attempts to axiomatize probability without presupposing deductive logic, and with his interpretation of quantum mechanics and its logic (cf. Miller, 2016; Del Santo, 2020). In Popper's Nachlass one finds a large number of manuscripts dealing with these topics, including bundles full of algebraic calculations, but also with proofs of theorems, some of which are both systematically and historically relevant.[6] In retrospect, in his autobiography of 1974 (Popper, 1974c), Popper even claims that problems in probability theory

[5] Popper's literary estate is located at the Hoover Institution Library & Archives in Stanford. The Karl Popper Collection in Klagenfurt contains copies of the literary estate and Popper's private library.

[6] Del Santo (2020) gives a list of materials from the Karl Popper Collection which are related to the logic of quantum mechanics. Popper himself refers to this work, for example, in the letter to Carnap of 28 May 1944 (this volume, § 23.5). His correspondence with Forder (Chapter 26) is full

led him to his logical work, pointing to the well-known formal relationship between lattice-theoretic and logical properties (Popper, 1974c, fn 188). However, even if this is historically correct and not simply a later attribution[7], we have two strong reasons to not include Popper's work on Boolean algebra in the present volume. Firstly, formal relationships between Boolean algebra and some aspects of inferentialist logic do not affect at all the philosophical rationale for inferentialism. Popper's inferentialist approach to logic is a philosophical conception in its own right and is also understood and presented by him as such. Thus the present volume is self-contained, even though some ideas and results have formal counterparts in other areas. Secondly, the quantity of the notes and papers on Boolean algebra in the Nachlass, if one combined them with an edition of his published papers on probability theory and probabilistic logic, together with Popper's correspondence on these matters, would be so large that it could easily make a volume of its own. Such a volume would be a very welcome companion to the present one, with many interesting interconnections.

Editorial notes Our editorial corrections and additions are marked by ⟨⟩. Published errata have been included tacitly. Other errata, such as those found in letters or unpublished works are marked by ⟨⟩. Obvious typographic errors have been corrected without indication; however, some orthographic errors in Popper's German letters were left in place since he explicitly mentions that he is making them. In our transcriptions of handwritten manuscripts and letters we write ⟨word?⟩ or "word⟨?⟩" where we were unsure about the correct reading of a word. In quotations we use [], as usual, to frame ellipses and our additions or conversions to facilitate the flow of reading.

Popper's expressions $a_1, a_2, \ldots a_n$ and $a_1, a_2 \ldots a_n$ have been replaced by $a_1, a_2, \ldots, a_n$ throughout. The typesetting of formulas has been unified to some extent, and we have made further minor typographic modifications. For example, section numbers or letters have been moved from the beginning of first paragraphs to centered headings, some lists have been reformatted for better readability, and we have changed the font style of author names from small capitals to normal.

Popper usually cites without providing bibliographic details or bibliographies. We provide references in the text in the format ⟨Carnap, 1942⟩ or in editorial footnotes, which are marked by letters [a, b, …]. Numbers [1, 2, …] always signify author footnotes. The references sections at the end of chapters have been added by us. The bibliography at the end of this volume comprises all references.

Page concordances for Popper's published works are provided at the end of this volume. In published works, original page breaks are indicated by the symbol | with the original page number in the left margin. Three papers, namely Popper (1947d, 1948a,c), were published both by the Royal Netherlands Academy of Sciences and in Indagationes Mathematicae using the same printing plates (with minor adjustments

of references to Boolean algebra and of discussions of Boolean algebra problems related to logic. The letter to Kleene (§ 28.1) relates to his later interest in the logic of quantum mechanics.

[7] Namely, an attribution made at a time when Popper was involved in issues of probability theory and quantum mechanics. In the logical writings themselves he always describes Tarski's notion of logical consequence as his starting point.

to the titles in Indagationes Mathematicae). We give preference to the former, but also provide page numbers of the latter in the respective concordances.

In Popper's unpublished manuscripts and in his correspondence we indicate page breaks also by the symbol | with page numbers in the left margin. Underlined text is rendered in italics. English translations of letters written in German are provided by us. In many cases, only carbon copies or drafts of letters sent were available to us. These may lack the sender's signature; we have not added it in these cases.

There is a combined person and subject index for the whole volume. As this book is published open access, the reader can also use the freely available electronic version for searches.

Popper almost never wrote abstracts. Those featured in this collection are our additions. Editorial notes for the respective works are given below these abstracts.

References like "KPS Box 12, Folder 10" refer to Popper's Nachlass in the Karl Popper Collection ("Karl Popper-Sammlung") Klagenfurt, which was our main source for Popper's unpublished work.

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Hainscho from the Karl Popper Copyright Office, to David Miller for his continuous support of this editorial project and for many detailed comments and suggestions on our manuscript, and to the Karl Popper Foundation and its president Reinhard Neck for contributing to the publication costs of this volume. We thank Katherine Pawley and Garry J. Tee at the University of Auckland for information on Popper's correspondence with Henry George Forder, as well as Brian Boyd, University of Auckland, for sharing some scans of Popper's work with us, and workers at the ETH Zürich Research Collection and the Houghton Library Harvard for their help. We would also like to thank the organizers and participants of several conferences at which we had opportunity to present and discuss our research on Karl Popper's logical works. Finally, we would like to express our gratitude to the editor of the Springer series *Trends in Logic*, Heinrich Wansing, for strongly supporting our project, and to Christopher V. Jones for many helpful comments on the final draft of this volume.

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Tübingen, September 2021

David Binder
Thomas Piecha
Peter Schroeder-Heister

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Contents

Part III Correspondence

Chapter 1
Popper’s Theory of Deductive Logic

David Binder, Thomas Piecha, and Peter Schroeder-Heister

Abstract We present Popper’s theory of deductive logic as exhibited in his articles published between 1947 and 1949. After an introduction to Popper’s inferentialist approach and his idea of “inferential definitions” of logical constants, we discuss in more formal detail Popper’s general theory of the deducibility relation (which, using Gentzen’s terminology, might be called his “structural” theory), as well as his special theory of logical constants. We put special emphasis on his inferential notion of duality, which includes his analysis of the “anti-conditional” (today called “co-implication”), his systematic study of various forms of negation including classical, intuitionistic and weaker negations, his system of bi-intuitionistic logic, and his theory of quantification and identity. We also touch on his treatment of modal operators, which is based on Carnap’s *Meaning and Necessity*.

Key words: Karl Popper, logic, inferentialism, logical constant, deducibility, negation, duality, co-implication, intuitionistic logic, bi-intuitionistic logic, quantification, identity, modal operators

Karl Popper’s published articles most relevant for his inferentialist theory of logic are listed in Table 1. These core papers are accompanied in our edition by a few other papers on deductive logic published by Popper before and after them (cf. Preface). We provide a commentary to these papers and also discuss some of the objections raised by contemporary reviewers, who were quite prominent figures in mathematical logic. Even though Popper’s articles appeared in well-accessible journals and proceedings (Mind, Aristotelian Society, Royal Netherlands Academy of Sciences, 10th International Congress of Philosophy) and were reviewed in the standard periodicals (Zentralblatt für Mathematik und ihre Grenzgebiete, Mathematical Reviews, Journal of Symbolic Logic), a wider reception did not take place. One may speculate about the reasons why the academic community ignored these papers, in spite of the fact that their author was a well-known figure, although not in logic, but in both philosophy of science (*Logik der Forschung*) and social and political philosophy (*The Open Society and Its Enemies*). Why was it that the reviewers, or at least some

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Table 1 Popper's articles published between 1947 and 1949.

Title	*Original publication(s)*	*This volume*
Logic without Assumptions	Popper (1947b)	Chapter 2
New Foundations for Logic	Popper (1947c)	Chapter 3
Functional Logic without Axioms or Primitive Rules of Inference	Popper (1947d)	Chapter 4
On the Theory of Deduction, Part I. Derivation and its Generalizations	Popper (1948a,b)	Chapter 5
On the Theory of Deduction, Part II. The Definitions of Classical and Intuitionist Negation	Popper (1948c,d)	Chapter 6
The Trivialization of Mathematical Logic	Popper (1949a)	Chapter 7

of them, who had read his papers in detail, did not see the promising features of Popper's approach, which should have been visible in spite of certain shortcomings (often based on misunderstandings) that they rightly or wrongly criticized? Was it that Popper's bold claims – "new foundations", "trivialization" etc. – made them sceptical? Was it Popper's proof-theoretic framework, which he considered sufficient to lay the foundations for logic, independent of any additional sort of semantics? There were exceptions. The proof-theorist Bernays saw the merits of such an approach, which from today's perspective we would call "inferentialist",[1] as did Brouwer, Kneale and Quine (cf. § 2).

From a modern standpoint, where inferentialism and proof-theoretic semantics have become respected foundational approaches, Popper's views look quite advanced. Perhaps Popper was, with his inferentialist perspective, too much ahead of his time, and did not come back to his logical investigations, when in the spirit of a re-appraisal of Gentzen's work foundational proof-theoretic approaches started to flourish. Formal logic is not what one normally associates with Popper, and there were so many important debates in philosophy of science in the late 1960s in which Popper was involved, such that contributing to the debate on the foundational role of proof theory (Dummett, 1975; Kreisel, 1971; Prawitz, 1971) was way beyond his interests at that time.

A particular role seems to have been played by Tarski, whom Popper admired, but whose semantical approach was incompatible with the inferentialism Popper put forward in his logical papers. Popper wrote in a letter of 9 July 1982 (this volume, § 32.2): "I was discouraged at the time by the fact that Alfred Tarski, whom I admire very much, did not want to have a look at these works. I had no one else".[2]

[1] He indeed planned to write a foundational paper on logic together with Popper, which unfortunately did not materialize beyond initial stages (this volume, Chapter 14).

[2] Tarski received reprints from Popper in December 1947, and it is most likely that these were reprints of Popper (1947b,c,d). Apparently, Tarski was very busy at the time and just asked Woodger in a letter of 16 December 1947 to pass to Popper his greetings: "I got a few reprints from Popper. Would you please give him my best greetings when you see him?" (Mancosu, 2021, p. 94).

With his inferentialist approach, Popper entered uncharted logical terrain. It can well be imagined that in spite of the fact that his logical papers were written "with much enthusiasm" (Popper, 1974b, p. 1095, this volume, Chapter 12, p. 217), he felt somewhat insecure and preferred to leave a field in which he did not enjoy the authority and reputation he had acquired in the philosophy of science.

In any case, Popper's theory of deductive logic is worth reading and discussing, in particular from our modern point of view. This theory brings with it a lot of conceptual and technical insights which later became fundamental aspects of the foundational debate, such as his anticipation of Prior's operator *tonk*, the collapsing of intuitionistic into classical negation in a combined system, the inferential notion of duality and the notion of co-implication, the idea of dual-intuitionistic and bi-intuitionistic logic, the formal theory of substitution, and many more.

This chapter is structured as follows. A general introduction (§ 1) presents the fundamental ideas and concepts behind Popper's approach, in particular his theory of metalinguistic "inferential definitions" of logical constants. It proposes a reading that relates these definitions to the introduction of function symbols in first-order languages and thereby tries to justify Popper's view of inferential definitions as "explicit definitions". This reading characterizes Popper's theory as a variant of inferentialism which, however, puts no emphasis on the specific form of basic inference rules as done in modern proof-theoretic semantics. § 2 discusses the reception of Popper's logical work, in particular the above mentioned reviews, and points to some critical misunderstandings of Popper's views. In § 3 we discuss Popper's overall framework, pointing to his insistence that the whole matter is a metalinguistic approach, where, for the definition of logical constants, only restricted means – essentially positive logic with propositional quantification – are permitted. § 4 presents Popper's general theory of deduction, which is the theory of "structural" rules (to use Gentzen's terminology) without taking into account the internal deductive form or power of sentences. Popper's two approaches to characterizing this structural foundation, called "Basis I" and "Basis II" are discussed in detail. It becomes obvious that Popper's ignorance of the works of Hertz and Gentzen (Hertz, 1929a, Gentzen, 1935a,b; cf. also Schroeder-Heister, 2002) makes it more complicated than it otherwise could have been. In § 5 we discuss Popper's special theory of deduction, that is, his theory of logical constants and their inferential definability. Here we take up issues from § 1 and argue that this theory can be reconstructed in a consistent way. We discuss in particular Popper's inferential account of duality, which leads to his discussion of co-implication (called "anti-conditional" by Popper). § 6 gives a systematic account of Popper's theory of negation; more precisely, of various forms of negation including the classical and intuitionistic ones and their duals, as well as his development of a system of dual-intuitionistic logic. His discovery that certain distinct operators collapse into a single operator when combined within one system, the typical example being classical and intuitionistic negation, is particularly emphasized. § 7 is a sketch of Popper's treatment of modal logic, which is in the spirit of Carnap's *Meaning and Necessity* (Carnap, 1947). Though it is interesting to see how it fits into Popper's inferential framework, it is a bit outdated given that modal logic has made such impressive progress since Carnap's pioneering book. § 8 points to Popper's anticipation of what today is called bi-intuitionistic logic and gives a full proof of the result, only sketched by Popper, that intuitionistic negation and dual-intuitionistic negation can be combined in one system without their collapsing into one single negation. Finally, § 9 reconstructs Popper's account of quantifier logic based on a basic notion of substitution which is characterized by a list of axioms and thus represents a kind of theory of explicit substitution.

We heavily rely on our previous work on Popper's logic which resulted in four articles (Schroeder-Heister, 1984, 2006; Binder and Piecha, 2017, 2021), but deviate in several respects from the interpretation given there. The reader may well skip this chapter and study Popper's contributions directly, the most general and readable being "Logic without Assumptions" (Popper, 1947b), followed by "New Foundations for Logic" (Popper, 1947c).

1 General introduction

1.1 Deducibility: From Tarskianism to inferentialism

In his papers of 1947–1949, which are the core of the present collection, Popper claims to lay "new foundations for logic" (title of Popper, 1947c), which, at the same time, represent a "trivialization of mathematical logic" (title of Popper, 1949a). By "logic" he understands the theory of deducibility. His central notion is the deducibility of a statement a from statements $a_1, \ldots, a_n$, written as $a_1, \ldots, a_n/a$. By deducibility he does not mean the derivability in a formal system, but the semantical notion of logical consequence. For Popper, "a is a consequence of $a_1, \ldots, a_n$", "a follows from $a_1, \ldots, a_n$", "a is deducible from $a_1, \ldots, a_n$" and even "a is derivable from $a_1, \ldots, a_n$" are metalinguistic expressions which are synonymous. Providing new foundations for logic thus means developing a theory of deducibility or consequence in a novel way.

There is already the Bolzano-Tarski approach to logical consequence. Popper was well-acquainted with Tarski's version of this approach. Tarski's seminal paper on the notion of logical consequence had appeared in 1936 (Tarski, 1936b), so it was still "fresh" when Popper started working on logic.[3] Moreover, Popper was an admirer of Tarski throughout his lifetime, and it was Tarski's theory of truth that led him to give up his hesitation to use the concept of truth still prevailing in his *Logik der Forschung* (Popper, 1935, § 84; cf. Popper, 1959b, § 84, fn *1 and Popper, 1974c, § 20).

Tarski's notion of logical consequence is based on the idea of truth transmission: a follows logically from $a_1, \ldots, a_n$ if every interpretation which makes $a_1, \ldots, a_n$ true, makes a true as well. As Tarski had pointed out, this definition hinges on the definition of what a "logical constant" or "logical sign" is. An interpretation that makes $a_1, \ldots, a_n$ true and carries this truth over to a, can give all non-logical

[3] According to Popper's letter to Forder of 7 May 1943 (this volume, § 26.4) Popper even assisted Tarski in preparing the German version of this paper.

expressions in $a_1, \ldots, a_n, a$ an arbitrary meaning, but the meaning of the logical signs must be kept fixed throughout. That, for example, a follows from a-or-b and not-b, only depends on the meaning of "or" and "not", which is fixed, and not on the interpretation of "a" or "b", which is variable.

Therefore, so one can argue, by providing a satisfying definition of what a logical constant is, we obtain a satisfying theory of logical consequence and thus of deducibility, given that the notion of truth itself is not problematic and is sufficiently clarified by Tarski's theory of truth. This situation is the systematic starting point of Popper's investigations (in particular of Popper, 1947c).

There are nevertheless certain consequence laws which are independent of logical constants. These are the laws constituting a finite *consequence relation* in Tarski's sense. In a proof-theoretic setting, Gentzen (1935a,b) called them *structural rules* ("Struktur-Schlussfiguren"), where "structural" means independent of the "logical" form of the statements involved. Popper calls them "absolutely valid" since for their validity we need only be able to distinguish sentences from non-sentences, disregarding the internal structure of sentences. These rules and the various forms they take in Popper's theory will be discussed in further detail in § 4, especially in § 4.6. For our initial discussion it suffices to know that there is a core set of rules which essentially comprises reflexivity, monotonicity and transitivity, as well as permutation and contraction laws depending on whether the left side of a consequence claim is considered a list, multiset or set:

1. a/a.
2. If $a_1, \ldots, a_n/a$, then $b, a_1, \ldots, a_n/a$.
3. If $a_1, \ldots, a_n/a$ and $a, b_1, \ldots, b_m/b$, then $a_1, \ldots, a_n, b_1, \ldots, b_m/b$.

As the validity of these rules is unproblematic, we can assume that, whenever we are dealing with deducibility, it is given as a consequence relation.[4]

In what follows, "sentence" and "statement" are used synonymously. "Statement" is the term preferred by Popper. We do not use the term "proposition" since this is often used in a semantic sense as denoting the meaning of a sentence. Popper deals with syntactic entities throughout, although he emphasizes that the distinction between syntactic statements and semantic propositions does not affect his theorizing (cf. Popper, 1947b, fn 4, p. 261, this volume). His syntactic stance is quite natural, as he is throughout working in a proof-theoretic setting.

In spite of this Tarskian motivation and starting point, the idea to define the logicality of operations and thus logical consequence for complex statements leads Popper to develop a conception entirely different from Tarski's. This conception has at least three central characteristics:

1. It is not assumed that a specified formal object language is given, in which logical operations are represented by functional expressions ("sentential functions") which combine one or more sentences to a compound sentence (or, in the quantifier case, operate on open sentences). A sentence c may be a conjunction of a and b without having a special syntactic form such as $a \wedge b$ or Kab (depending on one's

[4] By that we always mean a *finite* consequence relation. Tarski's (1936b) consideration of consequences from an infinite number of assumptions goes beyond Popper's finite proof-theoretic framework. Insofar it is much more related to Gentzen (1935a,b) and Hertz (1929b).

logical notation). Tarski's approach of truth transmission under preservation of logical structure assumes that such a specification is given.

2. Logical operations are *relationally* and not *functionally* characterized. For example, that c is a conjunction of a and b is a three-place metalinguistic relation which is formally defined in the following way:

$$c \text{ is a conjunction of } a \text{ and } b \text{ if and only if } \mathcal{R}_{\text{conj}}(c, a, b)$$

where $\mathcal{R}_{\text{conj}}(c, a, b)$ is a certain metalinguistic condition. This relational view can leave open whether a logical operation, in our example conjunction, (1) always exists and (2) is uniquely determined. Both features would have to be presupposed if one preferred a functional characterization of logical operations as implicit in standard logical notation. Tarski, in considering a structure based on logical constants, sticks to a functional view from the very beginning.

3. The relational characterization of logical operations proceeds in terms of deducibility. For Popper, results of logical operations (conjunctions, disjunctions, implications, ...) are characterized by their deductive or inferential behaviour. In the above definition of conjunction, $\mathcal{R}_{\text{conj}}(c, a, b)$ describes characteristic inferences involving conjunction in terms of $/$. There are various options for choosing $\mathcal{R}_{\text{conj}}(c, a, b)$ (cf. § 5.5 below), the most straightforward of which leads to the following *relational definition of conjunction*:

(RelDef-conj) c is a conjunction of a and b if and only if

$$a, b/c \text{ and } c/a \text{ and } c/b$$

which means that $\mathcal{R}_{\text{conj}}(c, a, b)$ is "$a, b/c$ and c/a and c/b". Thus $\mathcal{R}_{\text{conj}}(c, a, b)$ formulates the standard introduction and elimination rules for conjunction in the form of consequence statements. According to this definition, we call something a conjunction if it obeys these introduction and elimination rules. For Tarski, the deductive behaviour of conjunction would not be its *definiens*, but a *consequence* of its non-deductive (e.g., truth-theoretic) characterization.

Note that we speak of the logical "operation" in contradistinction to the logical "operator" of conjunction. The operator of conjunction is the logical connective that has the form of a two-place sentential function like $\wedge$, whereas the logical operation of conjunction denotes the relation between a, b and one of its conjunctions c in the sense of (RelDef-conj), independent of what c looks like. This terminology is not perfect, as talking of an operation of "conjunction" might suggest that we are talking of a function. However, in our sense, an operation is not necessarily deterministic, (though in the case of conjunction it is[5]).

The third point turns the project of justifying deducibility upside down as compared to Tarski. Since we rely on a notion of consequence in the relational definition of logical operations, we can no longer use this notion of logical operation to define the

[5] At least with respect to logical equivalence. In a language with a conjunction operator $\wedge$, the sentences $a \wedge b$ and $(a \wedge b) \wedge (a \wedge b)$ are two syntactically different conjunctions of a and b in the sense of (RelDef-conj), but they are logically equivalent and thus have the same deductive power.

validity of consequences along Tarskian lines. In fact, such a definition of validity in terms of truth transmission becomes obsolete, as deducibility must already be available at the level of logical operations. Thus the notions of truth and truth transmission do not play a role any more in the justification of logical inference. They are discarded in favour of deducibility as a primitive notion. Logical rules are set up and explained in an inferentialist framework without any recourse to truth. All this makes Popper a logical inferentialist in the genuine sense of the term, which was only much later coined by Brandom (1994, 2000). Actually, although Popper does not use the term "inferentialism" to denote his orientation, he speaks of the "inferential characterization" of expressions and, in the case of logical constants, of their "inferential definitions". So the term "inferential" is present, and even abundant, in Popper's logical writings.

At first glance this might be surprising, given that Popper is an outspoken realist according to whom science is attempting to reach truth, and given that inferentialism is normally associated with anti-realist and sometimes even instrumentalist approaches to validity, something that Popper strongly opposes (cf. Popper, 2004). However, on a closer, second look, the inferentialist approach fits very well with Popper's conjectural approach to scientific reasoning. Logic is not only a tool used in the testing of theories, in particular in the attempt to falsify them, but is itself something that is open to criticism. The great advantage of the inferentialist framework Popper proposes is that it allows for the development of many alternative logics with a variety of logical operations. In modern terminology, he is giving a *logical framework*, that is, a framework for presenting logics. Within such a framework we can compare and challenge systems of logic, also taking into account their applicability in the empirical sciences. Viewed from this perspective, the inferentialist theory fits even better into Popper's conjecturalist approach than a monolithic theory of truth. Popper's inferentialist foundation of logic is not an approach justifying a certain logical system as the right logic. For Popper, logical laws are always relative to given definitions, and whether to pose a certain definition is not a matter of principle but a matter of whether it serves one's purposes. Thus Popper's inferentialist approach allows one to check in a perspicuous way what it means to base one's reasoning on one or other system of logic. His *new foundations* of logic is definitely not a foundationist approach in the sense strongly criticized by him at various places (e.g., Popper, 2004, Introduction).

1.2 Logical relations

As relational characterizations of logical constants, Popper gives metalinguistic definitions of the following form (which the already mentioned definition (RelDef-conj) of conjunction belongs to):

> c is a conjunction of a and b if and only if ...
>
> c is a disjunction of a and b if and only if ...
>
> c is an implication from a to b if and only if ...

c is a negation of a if and only if ...

c is a universal quantification of a with respect to x if and only if ...

c is an existential quantification of a with respect to x if and only if ...

Besides such relational definitions for the standard operations, many more for various classical and non-classical logical operations are given (details are discussed below in §§ 5.5–5.7, § 6, § 7 and § 8). The relations defined we here call *logical relations*. The defining conditions, above represented by dots (with $\mathcal{R}_{\text{conj}}(c, a, b)$ being our example for conjunction), contain as their central notion deducibility /. Here we call them "inferential conditions". It is inferential properties described in terms of deducibility that make an operation a *logical* operation. An operation is logical if it can be relationally defined by an inferential condition. By means of these logical relations, Popper is implicitly giving a criterion of logicality (cf. § 5.3 below). A relation is logical if the definiens (the dots above) is a deducibility condition of a certain form.

Now what does such an inferential condition look like syntactically? We are dealing with a metalanguage which is not fully formalized. However, from the many examples of relational definitions Popper discusses in his papers, it is fairly clear what is intended (cf. § 3.3). The only "material" sign of the metalanguage occurring in the defining condition of a logical relation is the deducibility operator /. For the treatment of quantification, we need also a substitution operation. As metalinguistic logical constants we use the constants of positive logic with propositional universal quantification, where disjunction is employed only for the relational definition of modal operations. Thus negation and existential quantification are not used, and disjunction only in a special case. For example, if c is a disjunction of a and b, an inferential condition $\mathcal{R}_{\text{disj}}(c, a, b)$ characterizing disjunction would be "for all d: c/d if and only if a/d and b/d", which at the metalinguistic level uses conjunction, implication and propositional quantification. Therefore a relational definition of disjunction would be the following:

(RelDef-disj) c is a disjunction of a and b if and only if
for all d: c/d if and only if a/d and b/d.

The operators used in the metalanguage are essentially those needed to describe inference rules, namely universal propositional quantification to express the generality of rules; conjunction to express that there may be more than one rule and that a rule may have more than one premise; and implication to express the inference lines in rules leading from consequence statements to consequence statements.

This does not mean that the metalanguage does not contain further means of expression, such as negation or existential quantification. It only means that those metalinguistic means which may occur in the *defining condition* of a logical operation – the right side of its relational definition – are restricted. Popper does not give a clear specification of these restrictions, but it is clear that what he has in mind are restrictions in the spirit of inferentialism, and this is essentially a fragment of positive logic. What this exactly means can only be concluded from the inferential characterizations Popper is giving in his writings. There is never a negation occurring in inferential conditions – their positivity is essential. Further means that do not seem to be problematic at all, such as quantification over finite sets of sentences rather than only sentences, are not considered by Popper. This led to considerable difficulties in formulating some of his structural frameworks (cf. § 4.6 below).

As mentioned in the previous section, it is not presupposed at all that operators of conjunction, disjunction etc. are available in the object language considered. This means that sentential functions like $\wedge$ or $\vee$ do not need to exist. In fact, as also mentioned there, a conjunction or disjunction of a and b need not be available at all, whether expressed by a sentential function or not. It is easy to construct consequence relations without there being conjunctions or disjunctions of given sentences (cf. Koslow, 1992, p. 108f.). By the relational definitions of conjunction or disjunction, (RelDef-conj) and (RelDef-disj), we are only *characterizing* a sentence c, *whatever it may look like*, as a conjunction or disjunction of a and b, provided it exists at all. We are not expecting that we have a formal language with fully specified operators, which, by its formation rules, would make sure that any a and b can be conjunctively and disjunctively composed, and, by its inference rules, would make sure that introduction and elimination rules for these connectives are assumed to hold.[6]

This makes it possible to consider logical relations that cannot exist in consistent languages. Popper's prominent example is the logical relation "c is an opponent of a" (*opp*), which is defined in such a way that the existence of opponents for arbitrary sentences makes the underlying language inconsistent; that is, it allows for the derivation of any consequence statement. He thus antedates the discussion about Prior's (1960) operator *tonk*. However, for Popper *opp* would be a logical operator, if it existed (cf. § 5.2 below).

The relational view of logic – the definition of "c is a conjunction of a and b", "c is a negation of a" etc. rather than the consideration of logical operators $a \wedge b$, $\neg a$ from the very beginning – allows for a very general view on logical phenomena. We are not confined to formal languages of a specific format, but can just study arbitrary consequence relations and the availability of logical relations within these consequence relations. If we further restrict the inferential conditions allowed in the definiens of logical relations, we can single out logical relations of a specific kind. We might, for example, consider inferential conditions which describe introduction and elimination rules, or rules expressing that operators generate strongest or weakest statements obeying certain rules. The latter approach is followed by Koslow (1992) in his structural approach to logic, where arbitrary consequence relations and maximality and minimality principles in the definientia of logical relations are studied. A concrete logic with certain logical operators in an object language is then a kind of model of this relational approach. In this theory we are considering logical relations in a very general way, comparable to model theory dealing with the general structural features

[6] We do not always thoroughly distinguish between a language and an (axiomatized or non-axiomatized) theory within that language. That is, we often assume that with a language certain inference rules are given as well. It will always be clear from the context, in which sense the term "language" is being used.

of concrete theories. Schroeder-Heister (2006) proposed to interpret Popper's theory in this structuralist sense, reading it as a descriptive theory of consequence relations structured by logical relations. However, even though this reading is a viable option, it does not do full justice to Popper's introduction of logical operators, which is a crucial step beyond the relational definition of logical operations. While the latter can be understood purely descriptively, the former cannot.

1.3 Logical operators and inferential definitions

For a foundation of logic, we need more than just a theory of consequence relations and the definition of logical relations within such a theory. We want to introduce logical operators as specific formal signs and to formulate valid rules governing these signs.

It may well be, of course, that the language under consideration already contains such operators, but this is not a formal requirement. Popper's foundations for logic apply to any language with the only restriction that its deducibility notion is a consequence relation. The main idea of Popper's "new foundations" is that a consequence relation is given, and that a logician, as the investigator of this consequence relation, extracts a logical structure out of it by identifying sentences that might be considered results of logical operations applied to other sentences. Once such operations have been defined by the logical relations "is a conjunction of", "is a disjunction of" etc., we may, in a next step, introduce syntactical operators representing these operations. This introduction of a syntactical operator is called an "inferential definition" by Popper. This means that we must distinguish between a relation such as "is a conjunction of" and an operator such as "$\wedge$" syntactically representing the relationally defined operation of conjunction.[7]

While the relational definition of "c is a conjunction of a and b" leaves it open whether, given a and b, there is a c at all, whether there is a single c or whether there are multiple c's, a logical operator ("logical constant") must be unique. *The conjunction of a and b* is the result of a sentential function $\wedge$ generating from a and b the compound expression $a \wedge b$. As deducibility is our basic concept, by uniqueness we mean uniqueness up to interdeducibility, written by Popper as "//". That we have uniqueness in the case of conjunction is clear from its relational definition (RelDef-conj). It can easily be seen that if c and d are both conjunctions of a and b, then c and d are interdeducible: $c /\!/ d$. The fact that for an operation to be logical we do have uniqueness, must always follow from the relational definition of the operation in question. There are operations which are not unique but nevertheless play an important logical role, even though, strictly speaking, they cannot be represented by (functional) operators. A prominent example is Johansson's negation (cf. § 5.3 and § 6.4). There can be two non-equivalent Johansson-negations of the same sentence.

The second requirement to be able to talk of *the conjunction of a and b* is existence:

[7] In these introductory remarks we only consider propositional operators; for the whole picture including quantification cf. § 9 below.

there must be a c in the language considered which is a conjunction of a and b in the sense of the relational definition. We can, of course, postulate its existence by means of formation rules which say that for every a and b the expression $a \wedge b$ belongs to our language and by introduction and elimination rules for conjunction assumed as (metalinguistic) axioms. We might call this way of proceeding the generation of logical constants *by postulation*. This is certainly legitimate and also done by Popper when constructing a logical language (cf. Popper, 1947c, beginning of § 4). However, we must make sure that in this way we do not generate undesired results, and in particular no inconsistency. This would actually happen in the case of the opponent $opp(a)$ of a. If we postulated the availability of $opp(a)$ for every a with the rules characteristic of *opp*, we would obtain a language which is inconsistent. This would not be conceptually faulty in any sense. But it would, of course, mean that the language constructed is of no use.

Popper is fully aware of this situation. According to his approach, we either consider a language given by a consequence relation, in which uniquely determined operations exist which can be relationally defined, or we construct such a language. Whether the language is consistent is a highly important but different question, which does not impair its character as a logical language.[8,9]

Now suppose we have a relational definition of a logical operation where existence and uniqueness are met. In the example of conjunction, we suppose we have (RelDef-conj) with conjunctions existing and their uniqueness following from the right side of (RelDef-conj). Defining an operator of conjunction means to define $a \wedge b$ in such a way that the relation of being a conjunction is the graph of the function $\wedge$. Due to uniqueness, $a \wedge b$ can be viewed as a special term representing any conjunction (mathematically, a representative of the equivalence class of all conjunctions). It is introduced by Popper by postulating

(InfDef-conj) $\qquad c /\!/ a \wedge b$ if and only if $a, b/c$ and c/a and c/b.

Metalinguistic statements of this kind are called "inferential definitions" by Popper, as they uniquely define the deductive power of certain operators.

For a better understanding we consider the following parallel situation in first-order logic. Here an analogue to inferential definitions is an axiom that would be used to introduce a function sign into a first-order theory with identity, for example

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[8] Here it is important to remember that on the right side of a relational definition only positive metalinguistic operators are allowed to occur. Otherwise we could construct conditions which are metalinguistically inconsistent, as Lejewski (1974) has shown (misleadingly using them as a counterargument against the Popperian approach); cf. § 5.2 below.

[9] Although not discussed by Popper, one could discard the existence requirement by considering a logical operator to be a *partial* function, which allows that for certain arguments the result of the operation is not defined. For example, there might be an opponent of certain, but not of all sentences. This would certainly result in an elegant theory, comparable to recursive function theory in which not every partial recursive function need be total. The advantage of this partial approach would be that we do not have to make sure that an operation must deliver a result for any argument. The totality of this operation would then be something to be proved subsequently and not something to be assumed in advance.

Peano arithmetic. Suppose there is a formula $\varphi(y, x_1, \ldots, x_n)$ such that we can prove existence of uniqueness for the leftmost argument:

$$\forall x_1, \ldots, x_n \exists y \forall z (z = y \leftrightarrow \varphi(z, x_1, \ldots, x_n)).$$

Then we can conservatively introduce a function symbol f by means of the axiom

$$\text{(Ax-f)} \qquad \forall x_1, \ldots, x_n, z (z = f(x_1, \ldots, x_n) \leftrightarrow \varphi(z, x_1, \ldots, x_n)).$$

Conversely, if the theory in question contains already a function constant f for which (Ax-f) is derivable, we may call f "explicitly definable" in the theory. It is therefore not absurd to call the axiom (Ax-f) a "definition" of f, as it fixes the meaning of the newly introduced constant f, even though it is not a *fully* explicit definition, which would define f through a term τ:

$$f(x_1, \ldots, x_n) = \tau(x_1, \ldots, x_n).$$

Correspondingly, it is perfectly legitimate that Popper speaks of expressions of the form (InfDef-conj) as (inferential) *definitions* and even calls them "explicit definitions", even though they are not fully explicit definitions in the sense in which, for example, conjunction $a \wedge b$ can be defined through the Sheffer stroke[10]:

$$a \wedge b \leftrightarrow (a \mid b) \mid (a \mid b).$$

As in the example from first-order logic, the operator introduced by an inferential definition is eliminable from any context, if the inferential definition is assumed as an axiom.[11]. Thus, formally, Popper's inferential definitions are well-formed entities, which makes them a precise conceptual tool. For a further discussion cf. § 5.2 below.

1.4 Logical laws and the trivialization of logic

Given inferential definitions of certain logical constants, we immediately obtain from them basic laws for these constants, namely the right hand side of the inferential definitions with the logically composed statement inserted for the leftmost variable. From the inferential definition of conjunction we obtain $\mathcal{R}_{\text{conj}}(a \wedge b, a, b)$, from the one of disjunction $\mathcal{R}_{\text{disj}}(a \vee b, a, b)$ etc. And since these conditions uniquely (up to interdeducibility) describe what is meant by conjunction, disjunction etc., they are exhaustive in the characterization of these connectives. This means that we

[10] For Popper's discussion of the Sheffer stroke under the term "alternative denial" cf. the handwritten note Popper (n.d.[a]). Cf. also § 5.8.

[11] This cannot be spelled out in detail here. A proof would proceed along the lines of the first-order case, which is very closely related to the elimination of definite descriptions. For a semantical proof cf. Mendelson (1997, § 2.9), for syntactic proofs Hilbert and Bernays (1968, § 8) and Kleene (1952, § 74). Another analogy is the handling of set terms in set-theoretic languages (which normally do not contain explicit set terms as primitives).

obtain all logical laws involving these connectives by taking $\mathcal{R}_{\text{conj}}(a \wedge b, a, b)$ and $\mathcal{R}_{\text{disj}}(a \vee b, a, b)$ etc. as axioms or rules for the deducibility relation.

This makes sense only if $\mathcal{R}_{\text{conj}}(a \wedge b, a, b)$ and $\mathcal{R}_{\text{disj}}(a \vee b, a, b)$ etc. are formulated in such a way that they can be read as axioms or rules governing deducibility. As already mentioned above, this is always guaranteed for the inferential definitions Popper presents, even though the expressive means of the metalanguage are not formally specified. Thus a relational definition of a logical operation could always be formulated as: "... if and only if the following rules hold" rather than metalinguistically circumscribing these rules. In the case of conjunction, rather than (InfDef-conj), we would write:

(InfDef-conj′) $c /\!/ a \wedge b$ if and only if c, a, b obey the following rules:

$$\frac{a \quad b}{c} \qquad \frac{c}{a} \qquad \frac{c}{b}$$

Substituting $a \wedge b$ for c, we immediately obtain that the following rules hold:

$$\frac{a \quad b}{a \wedge b} \qquad \frac{a \wedge b}{a} \qquad \frac{a \wedge b}{b}$$

which are inference rules fully characterizing conjunction.

More generally we can say that the universal form of an inferential definition of an n-place propositional operator $S(a_1, \dots, a_n)$ is

(InfDef-S) $c /\!/ S(a_1, \dots, a_n)$ if and only if

$c, a_1, \dots, a_n$ obey the rules $\mathcal{R}_S(c, a_1, \dots, a_n)$.

By substituting $S(a_1, \dots, a_n)$ for c we obtain the inference rules

$$\mathcal{R}_S(S(a_1, \dots, a_n), a_1, \dots, a_n)$$

governing S. As this instantiation is immediate and yields inference rules in a trivial way, Popper may speak of the trivialization of logic, namely the metalinguistic deduction of valid inference rules immediately from a definition. One may consider this terminology misleading and argue that the main conceptual work lies in the formulation of the rules $\mathcal{R}_S(c, a_1, \dots, a_n)$ in the first place, and thus in the formulation of the inferential definition of S. But this is a different matter. *Given* the inferential definitions of logical constants in the form (InfDef-S) (for conjunction in the form (InfDef-conj′)), obtaining their governing rules is *trivial*.

1.5 Popper and proof-theoretic semantics

We do not find any particular reflection in Popper what the basic rules for an operator – the $\mathcal{R}_S(c, a_1, \dots, a_n)$ in (InfDef-S) – should look like. When reading Popper's discussions, one gets the impression that there is no preference whatsoever. The only criterion seems to be that for a logical constant an inferential definition of whatever form can be given. There does not seem to be any criterion to distinguish between better and worse inferential conditions, provided the criteria of existence and uniqueness are met. From this point of view Popper's approach looks as if any kind of inferential characterization of logical operators is possible, in particular a characterization by any finite list of inference rules. This makes his inferentialism differ from proof-theoretic semantics (Schroeder-Heister, 2018; Francez, 2015; Wansing, 2000; Piecha and Schroeder-Heister, 2016), where one tries to directly justify *specific* inference rules as meaning conveying, and as appropriate to provide epistemologically convincing argument steps. To be sure, one finds all the rules considered in proof-theoretic semantics also in Popper, such as introduction and elimination rules, sequent-style right and left introduction rules etc. But Popper does not specifically argue in favour of any of these presentations.

The fact that Popper does not consider consistency or conservativeness to be an admissibility criterion when introducing logical operators confirms this view. Whereas in proof-theoretic semantics great emphasis is put on conservativity and on the fact that the introduction of new operators is non-creative, this is not an issue in Popper. His consideration of *opp* as an example of a logical operator (and not as a counterexample comparable to the discussion of *tonk* in modern proof-theoretic semantics), or his observation that different negations collapse into classical negation, if the latter is present, show that consistency and conservativeness are no big issues for Popper. This also corresponds to the fact that Popper finds the distinction between classical and intuitionistic logical systems very interesting without considering it to be his task to argue for one of these systems against the other, or at least to provide criteria along which to differentiate between alternative logical systems.

From the standpoint of proof-theoretic semantics, this looks more like delineating logics than providing a semantics of logical signs. Schroeder-Heister (1984) actually proposed to read Popper's approach as a theory of logicality rather than a semantics of logical constants, considering an inferential definition to be a logicality condition for the operator in question rather than a semantic definition of its meaning. It is certainly correct that Popper wants logicality determined that way (cf. § 5.2 below). And, as just mentioned, for a genuine proof-theoretic semantics of logical operators a more specific discussion of semantical rules should be required. However, reducing Popper's approach to a definition of logicality gives too little justice to the fact that inferential definitions extract inferential content from consequence relations and generate logical rules, beyond the pure fact that the operators involved are logical.[12,13]

Thus Popper definitely wants to give a semantics for the logical signs and not only a criterion of their logicality. But this sort of semantics is only very rudimentarily

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[12] Cf. also the letter by Popper to Schroeder-Heister of 10 July 1982 (this volume, § 32.3): "[. . .] my first goal was the characterization of formative signs. But then it seemed to me that, if one succeeds with it, one can immediately justify [German: 'begründen'] propositional logic; and somehow this is possible, although it is a separate step.".

[13] This holds, by the way, also for other proof-theoretic definitions of logicality, which can be read as semantical conditions, for example for Došen's (1989) definition, from which inference rules both in natural deduction-style and sequent-style format can be deduced.

and implicitly present in his writings. The most important point in this respect is that after Gentzen he is the first to consider the format of the sequent calculus (with "/" interpreted as the sequent arrow) for foundational considerations. With that comes the aspect that his characterization of logical constants uses a strict separativity criterion, which is inherent in the sequent calculus: namely, that for every constant there is a separate set of rules which does not involve any other constant. This means that Popper is not an inferentialist in the holistic sense that all inference rules taken together somehow simultaneously determine the meaning of all operators involved in these rules. Instead he puts great emphasis on the individual definability of logical operators and on the investigation of how operators interact when they are available in the same system, though defined independently.[14] This is a great achievement two decades before the philosophical considerations on the format of reasoning in the aftermath of Gentzen's systems took off the ground (e.g., in Prawitz, 1971). Furthermore, various forms of criteria discussed in proof-theoretic semantics can be modelled in the Popperian framework, such as Lorenzen's (1955) and Prawitz's (1965) inversion principle (cf. Schroeder-Heister, 2007; de Campos Sanz and Piecha, 2009), Dummett's (1991) and Prawitz's (2006) harmony requirements (cf. Schroeder-Heister, 2015, 2016; Tranchini, 2021), Sambin et al.'s (2000) "Basic Logic" (cf. Schroeder-Heister, 2013), Došen's (1989) approach of double-line rules, Schroeder-Heister's (1984) idea of the common content of rule systems, Tennant's (1978) maximality and minimality principles and so on. Taking this together, one might say that Popper is a full-fledged inferentialist and a rudimentary proof-theoretic semanticist.

In not insisting on a specific form of inferential conditions, that is, of the right hand side of his inferential definitions, Popper is much more liberal in admitting semantical rules than modern proof-theoretic semanticists. This re-emphasizes the view that, in accordance with Popper's general standpoint, the distinction of the proper logic is not a matter of foundationist, but rather of consequential reasoning, which would take into account all ends to be achieved and side conditions to be observed.

To summarize the overall philosophical structure of Popper's way of proceeding: We assume that a *consequence relation* is given, or we construct one by postulation. By means of *relational definitions* we characterize logical operations which may be available in this consequence relation. By means of *inferential definitions* we introduce logical operators as names for these operations. Using logically structured sentences thus introduced, we obtain the *inference laws* for these sentences immediately from the inferential definitions.

2 The reception of Popper's logical writings

Popper's articles on logic were reviewed by Ackermann (1948, 1949a,b), Beth (1948), Curry (1948a,b,c,d, 1949), Hasenjaeger (1949), Kleene (1948, 1949) and McKinsey (1948). Kemeny (1957) and Nagel (1943) are reviews of the papers Popper (1955a)

[14] Cf. § 6.3 below.

and Popper (1943), respectively, which accompany the key papers of our selection. All reviews are reprinted in Chapter 13 of this volume.

Ackermann gives a summary of Popper's views, which is in line with our interpretation. Beth stresses the relationship to the works of Gentzen (1935a,b), Jaśkowski (1934) and Ketonen (1945), without any further criticism. Curry in his five reviews emphasizes the similarity to Hertz's (1929b) and Gentzen's (1935a,b) approaches and points to the deficiencies of what Popper calls his "Basis II" of structural rules, which we discuss in detail in § 4.6. He acknowledges Popper's extensive treatment of negations (discussed in § 6) including the collapsing result when classical negation is present (cf. § 6.3). The final sentence of his last review (Curry, 1948d) summarizes his critique: "The whole program is very obscure, and has not been without serious error [. . .]; likewise it was anticipated, in many respects, by the work of Gentzen and others."

Hasenjaeger criticizes hidden existence assumptions in inferential definitions, something that is also the main point of Kleene's reviews. However, as shown above, this view is misleading and in any case cannot be taken as a critique of Popper. There are certainly existence assumptions: we cannot inferentially define an operator if the corresponding relationally defined operation yields no result for the arguments considered. But this is something of which Popper is well aware. Inferential definitions are always made under the supposition that results of logical operations exist, to which the inferential definition gives a name. This becomes absolutely clear from our analogy drawn between inferential definitions and the introduction of function symbols in a language with equality, which is a standard way of proceeding in mathematical logic (cf. § 1.4).

McKinsey criticizes that the consequence sign "/", which is introduced to denote absolutely valid inferences, is extended to cover inferences involving logical constants. As is also the case with Kleene, he misses the fact that Popper's metalinguistic theory applies to the established deductive practice in an object language which is not normally specified formally and employs from the very beginning conjunctions, disjunctions etc. in the sense of his relational definitions. This does not exclude, as a limiting case, that the object language is formally structured. However, when we define such a formal language, we do a creative job and must be prepared for unwanted results that may show up, including inconsistencies; Popper does not ignore these issues. A discussion of McKinsey's criticism by Popper himself can be found in an unpublished manuscript (cf. this volume, Chapter 17, Typescript 1, § 3, footnote 8).

An important point in several reviews is the emphasis that Gentzen's work (and also the related work of Hertz, 1929b, Jaśkowski, 1934 and Ketonen, 1945), in particular his sequent calculus, is highly relevant to Popper's undertakings. As pointed out in § 4.6, it can be asked whether or not Popper was aware of Hertz's and Gentzen's work when he developed his theory.

Bernays, Brouwer, Kneale and Quine responded quite positively, as their correspondence with Popper shows (cf. Chapters 21, 22, 29 and 30). Bernays planned to write a foundational paper on logic together with Popper, which unfortunately did not materialize beyond initial stages (this volume, Chapter 14). Brouwer presented several of Popper's papers to the Royal Netherlands Academy of Sciences.[15] For Kneale, Popper's formulation of logical rules became a key ingredient of his theory of logicality (cf. Kneale, 1956 and also Kneale and Kneale, 1962, § IX), which itself provides a background to later proof-theoretic approaches to logical constants (cf., e.g., Hacking, 1979 and Došen, 1989). Detailed investigations of Popper's theory are given in the articles of Lejewski (1974) in the Schilpp volume on Popper (cf. Schilpp, 1974)[16]; Schroeder-Heister (1984, 2006) and Binder and Piecha (2017, 2021), which were already mentioned above; Bar-Am (2009), which discusses a distinction between the notions of sound inference and proof; and Moriconi (2019), which provides a detailed overview and elucidates in particular Popper's treatment of negation and implication by making use of a sequent calculus setting. There are some unpublished theses dealing with Popper's logic, of which we know Cohen (1953b), Brooke-Wavell (1958) and Dunn (1963). Here we just sketch Cohen's thesis, as for systematic reasons it is the most interesting one.

Cohen's (1953b) Oxford B.Litt. thesis is highly relevant to matters discussed below in § 5.4 and § 6.2. Its second part concerns the development of a system of dual-intuitionistic logic.[17] Cohen starts from Gentzen's observation that intuitionistic logic can be obtained from the sequent calculus for classical logic by restricting the number of formulas occurring in the succedent of sequents to at most one. He then formulates a sequent calculus where the number of formulas occurring in the antecedent of sequents is restricted to at most one (without restricting succedents), which he calls the "dualintuitionistic restricted predicate calculus GL2". The idea of developing this system was suggested to him by Popper.[18] The inception of dual-intuitionistic logic can thus be attributed to Popper, and Cohen was the first to develop and investigate a system of dual-intuitionistic logic. Cohen's sequent calculus GL2 is, however, not exactly dual to intuitionistic logic, since it contains rules for both the anti-conditional and the conditional.[19]

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[15] These are Popper (1947d), communicated at the meeting of 25 October 1947, and Popper (1948a,c), both communicated at the meeting of 29 November 1947.

[16] Popper's (1974b) reply to Lejewski is reprinted in Chapter 12 of this volume.

[17] Popper (2004, fn 8, p. 431f.) cites this system as an answer to his question "whether we can construct a system of logic in which contradictory statements do not entail every statement." However, "[i]n [Popper's] opinion, such a system [which lacks e.g. modus ponens] is of no use for drawing inferences."

[18] As remarked by Cohen (1953b, p. 188). Popper (2004, fn 8, p. 431f.) first refers to his (1948a; 1948c), and then mentions that "[. . .] Cohen has developed the system [of dual-intuitionistic logic] in some detail." In fact, Cohen also gave a full analysis of this system, including a proof of cut elimination. Popper (ibid.) has "a simple interpretation of this calculus. All the statements may be taken to be modal statements asserting possibility." This interpretation can already be found in a letter from Popper to Cohen of 28 July 1953 (this volume, § 25.7), where he interprets every statement a as "a is possible" or, equivalently, as "a is satisfiable". In this letter Popper says that he "consider[s] publishing these results", without, however, doing so. Cohen (1953b, p. 208f.) names Popper's (1948a, p. 181) interpretation of sequents in terms of relative demonstrability (cf. § 4.4) as the most suitable one.

[19] Cohen (1953b, part II, § 3) first formulates the system GL1, which includes the rules FS and FA for the conditional. This system is then extended to his dualintuitionistic restricted predicate calculus

3 Popper's structural framework

Popper considers an object language $\mathcal{L}$ together with a deducibility relation on $\mathcal{L}$, which today we would call a finite consequence relation in Tarski's sense. As already mentioned, Popper uses the terms "deducible", "derivable", "follows from" and "is a consequence of" synonymously. Popper knew Tarski's work very well. Whether he knew any details of Hertz's and Gentzen's work, in particular of their structural framework, or whether he was aware of it at all, is questionable, as mentioned above and further discussed in § 4.6. The set of axioms characterizing a deducibility relation is called a *basis*. Popper mainly uses two alternative bases, called Basis I and Basis II, which also occur in different versions. We use a version of Basis I as the foundation for the rest of this introduction.[20]

3.1 Object languages

In contradistinction to modern approaches that proceed by giving an alphabet of the object language under consideration and either a grammar or inductive definition of those expressions that are to be counted as terms and formulas, Popper's approach does not presuppose any knowledge about the form or syntactic structure of the object language under consideration. Popper explicitly points to this difference; he gives a sketch of the classical approach at the beginning of Popper (1949a). The idea that logical analysis is not only applicable to formally specified languages is still present later, in Popper (2004, addendum 5 from 1963), where he stresses this fact in the context of discussing Tarski's theory of truth:

> It has often been said that Tarski's theory of truth is applicable only to formalized language systems. I do not believe that this is correct. Admittedly it needs a language—an object-language—with a certain degree of artificiality; and it needs a distinction between an object-language and a meta-language [. . .] [N]ot every language which is subject to some stated rules, or based on more or less clearly formulated rules [. . .] need be a fully formalized language. The recognition of the existence of a whole range of more or less artificial though not formalized languages seems to me a point of considerable importance, and specially important for the philosophical evaluation of the theory of truth.

His approach is indeed intended to be not only applicable to formally defined languages but also to natural language. For example, the conjunction of two statements a and b of the object language need not have any particular syntactic form like "$a \wedge b$" or "a and b".

GL2 by adding the rules GS and GA for the anti-conditional; cf. Cohen (1953b, part II, § 4). Cf. Kapsner, Miller, and Dyckhoff (2014) and Kapsner (2014, p. 128, fn 6).

[20] This version of Basis I can be found in Popper (1947c).

> [Forming the conjunction of a and b] is done, in English, by linking them together with the help of the word "and". But we need not suppose that any such word exists: the link may be effected in very different ways; moreover, the new statement need not even contain the old ones as recognizable separate parts (or "components"). (Popper, 1947c, p. 205)

To make this point clearer, consider as an example the propositional language containing only signs for negation ($\neg$) and disjunction ($\vee$) together with a calculus in which all classically valid formulas of this restricted language are derivable. This language nevertheless contains for any two formulas φ and ψ a conjunction of φ and ψ. One such conjunction may be $\neg(\neg\varphi \vee \neg\psi)$; but other variants are possible, and there is in general no way to specify *the* canonical form of conjunction in that language.

This does not preclude that an object language is formally specified in the common way by inductive definitions. Popper himself considers such languages. However, it is not required at all, and Popper considers any sort of language, formal or non-formal, provided we know what it means that a sentence of the language follows from other sentences.

We will write $\mathcal{L}$ for an object language and, following Popper, use small Latin letters $a, b, c, \ldots$ (also with indices) as variables ranging over $\mathcal{L}$. The members of an object language are assumed to be *statements* (the Popperian term; we also speak of "sentences"), so that it makes sense to say that some of them are deducible from others. Popper (1947c, p. 204) calls them "expressions of which we might reasonably say that they are true or that they are false".[21] We furthermore assume that any object language considered is nonempty.

3.2 The concept of deducibility

Popper's approach is based on the concept of deducibility (or "derivability"). It is the only undefined notion as far as propositional logic (including modal logic) is concerned. An operation of substitution is added for the treatment of first-order logic (discussed below in § 9.1). Deducibility is a relation, written with the solidus /, that ranges over the object language and holds between finitely many premises (including the case of no premises) $a_1, \ldots, a_n$ and exactly one conclusion b. In /-notation, Popper writes

$$a_1, \ldots, a_n / b$$

to express that the statement b can be deduced from the statements $a_1, \ldots, a_n$. The case $n = 0$, in which no premises occur, was not yet considered in Popper (1947c). It was added later in Popper (1948a), where the so-called *D-notation* is introduced. In

[21] He states explicitly (ibid.): "Nothing is presupposed of our $a, b, c, \ldots$ except that they are statements, and our theory shows, thereby, that there exists a rudimentary theory of inference for any language that contains statements, whatever their logical structure or lack of structure may be."

this notation, $D(a_1, a_2, \ldots, a_n)$ stands for $a_2, \ldots, a_n/a_1$, and the special case $D(a_1)$ corresponds to $/a_1$, meaning that a_1 is deducible without premises.

The $/$-notation is introduced as a horizontal variant of the notation

$$\begin{array}{c} a_1 \\ \vdots \\ a_n \\ \hline b \end{array}$$

that is often used to state rules of inference like, for example,

$$\begin{array}{c} \text{If } A\text{, then } B \\ A \\ \hline B \end{array}$$

which says that from the premise "If A, then B" together with the premise A the conclusion B can be deduced (cf. Popper, 1947c, p. 194). The $/$-notation is also used to express rules that lead from deducibility statements to deducibility statements, for example, the rule called *Thinning* by Gentzen:

$$\text{If } a_1, \ldots, a_n/b \text{ then } a_1, \ldots, a_{n+1}/b \quad \text{(ibid. p. 196).}$$

In the context of deducibility neither the order of premises nor the multiplicity of identical premises is relevant, since deducibility enjoys the following structural properties, which we state as a lemma:

Lemma 3.1

1. *Exchange of premises:*

$$\textit{If } a_1, \ldots, a_i, a_{i+1}, \ldots, a_n/b, \textit{ then } a_1, \ldots, a_{i+1}, a_i, \ldots, a_n/b.$$

2. *Contraction of premises:*

$$\textit{If } a_1, \ldots, a_i, a_i, \ldots, a_n/b, \textit{ then } a_1, \ldots, a_i, \ldots, a_n/b.$$

These properties are consequences of Popper's characterization of deducibility by his Basis I.[22] Premises $a_1, \ldots, a_n$ can thus be understood as a set $\{a_1, \ldots, a_n\}$.

3.3 The metalanguage

When Popper defines logical operations inferentially, this is carried out in a metalanguage, whose basic relation is deducibility $/$, and not in a syntactically specified object language. If we consider, for example, a two-place logical operation $\circ$, which we call "connection" and which can stand for any operation such as conjunction or implication, or mutatis mutandis for operations of other arities such as negation, Popper relies on relational definitions of the form

[22] Cf. also Popper (1947c).

$$c \text{ is a connection of } a \text{ and } b \text{ if and only if } \mathcal{R}(c, a, b)$$

and on inferential definitions of the form

$$c /\!/ a \circ b \text{ if and only if } \mathcal{R}(c, a, b).$$

As already mentioned above (cf. § 2), in most cases the defining condition $\mathcal{R}(c, a, b)$ has the form of a rule, which is described in terms of deducibility /, metalinguistic conjunction, metalinguistic implication and metalinguistic universal quantification. Popper does not specify exactly the means of expression allowed in a defining condition, but from all contexts it is clear that positive logic is sufficient.

To improve readability of metalinguistic expressions, we use the following symbolic notation for it, which is similar to Popper's:

Symbol	$\rightarrow$	$\leftrightarrow$	&	$\bigvee$	(a)
Meaning	if-then	if and only if	and	or	for all a

The universal quantifier (a) ranges over statements a of the object language. Popper notes that as long as one does not want to introduce modal connectives, one can do without disjunction.[23] Therefore, given the special status of disjunction (discussed below in § 7) and the definability of equivalence in terms of conjunction and implication, the fundamental logical operations used in the metalanguage to define arbitrary logical operations of the object language are conjunction, implication and universal quantification (over sentences). We have therefore omitted negation and existential quantification from our list of symbols (though we use, of course, these operations in our metalanguage, but in the usual informal way, not within the defining condition of a logical operation).[24]

Due to the fact that only positive logic is permitted in the defining condition of an operation, the existence of this operation can never imply that the metalanguage is inconsistent. The trivial deducibility relation, which holds for all arguments, validates any defining condition $\mathcal{R}$ and therefore falsifies the negation of it, which means that not every metalinguistic statement is true. Trivialization of the object language does not imply trivialization of the metalanguage. We state this as a lemma:

Lemma 3.2 *In any nonempty object language with a trivial deducibility relation (i.e., $a_1, \ldots, a_n / b$ holds for any $a_1, \ldots, a_n, b$), every defining condition $\mathcal{R}$ for a logical operation is satisfied.*

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[23] Cf. Popper (1947d, p. 1216): "[. . .] it may be mentioned that the only logical rules needed in the metalanguage (except where we treat modalities) are those of the positive part of the Hilbert–Bernays calculus of propositions as far as they pertain to 'if-then', 'if, and only if', and to 'and' [. . .], and the rules for identity. The rules for negation need not be assumed [. . .]; but we need rules for universal quantification, especially the rule of specification [. . .]."

[24] In his letter of 10 July 1982 to Schroeder-Heister (cf. this volume, § 32.3), Popper compares this restriction of the metalinguistic means of expression to Hilbert's programme: "[. . .] you will find that the metalinguistic use of ∀ and → needed for their full object-linguistic use is *greatly* restricted. [. . .] Here is indeed something similar to Hilbert's programme of finding a finite justification for a non-finite calculus.".

This will become relevant in the discussion of the logical constant *opp* later on (cf. § 5.2).[25]

For certain expressions of the metalanguage Popper uses a special vocabulary. Statements of the form

$$a_1, \ldots, a_n/b$$

are also called *absolute rules of derivation*, and statements of the form

$$a_1, \ldots, a_n/b \rightarrow c_1, \ldots, c_m/d$$

and iterated versions thereof are also called *conditional rules of derivation* or just *rules of derivation*. Note that rules of derivation are not rules in a calculus understood as a proof system. Popper does not develop a calculus in this sense, and what he calls rules of derivation are metalinguistically formulated statements about the deducibility relation.[26] However, due to their specific form they can be read as descriptions of rules, as they tell us that from certain deducibility statements we may pass over to another deducibility statement. Thus we follow Popper in using the term *rule* to speak about such metalinguistic expressions. This means in particular that we will often speak of $\mathcal{R}(c, a, b)$ as "defining rules" instead of "defining conditions", even if they cannot be translated immediately into rules of some object language.

3.4 The characterization of deducibility by a basis

So far, the deducibility relation / has only been defined by saying that it ranges over an object language $\mathcal{L}$. The next step consists in providing what Popper calls a *basis* for this relation.[27]

A basis is a complete and independent set of rules, formulated in the metalanguage, that axiomatizes the deducibility relation /. Completeness is here defined with respect to Popper's notion of *absolute validity*, which is similar to the notion of validity obtained by allowing only structural rules of inference.[28] Popper's idea seems to be

[25] Another simple example is the metalinguistic statement "there exists a, such that $\vdash a \mathbin{\&} \urcorner a$", where $\vdash a$ stands for the demonstrability of a and $\urcorner a$ for the refutability of a (cf. § 4.2 and § 4.3). This statement is also only true for object languages with a trivial deducibility relation, although it is a satisfiable expression of the metalanguage.

[26] Popper (1948c, p. 327) once uses the term "metalinguistic calculus".

[27] Popper (1947d) is an exception to this approach: the requirement to specify a basis for the deducibility relation is dropped, and the burden of making sure that the logic has certain structural properties is shifted to the definitions of the logical constants. For this reason he uses an extended definition of conjunction, called basic definition (DB2), to ensure reflexivity and transitivity of / as well as exchangeability of premises. Unfortunately, (DB2) corresponds to the defective Basis II of Popper (1947c), and is therefore just as problematic (cf. § 4.6).

[28] Popper gives his analysis of absolute validity in terms of so-called *statement preserving interpretations* in Popper (1947b), where he writes about absolutely valid inferences (ibid, p. 274): "There are inferences which are valid according to all our definitions, in spite of the fact that the logical form of the statements involved is irrelevant."

that even if we abstract away from any concrete logical system (containing a specific set of logical constants) under consideration, we still have a rudimentary residuum of deduction consisting of structural[29] inferences, like the inference from a statement a to the statement a.

Popper's Basis I is given by a *generalized reflexivity principle*, called (Rg), together with a *generalized transitivity principle*, called (Tg):[30]

$$\text{(Rg)} \qquad a_1, \ldots, a_n / a_i \qquad (1 \le i \le n)$$

$$\text{(Tg)} \qquad \left\{\begin{matrix} & a_1, \ldots, a_n/b_1 \\ \& & a_1, \ldots, a_n/b_2 \\ \vdots & \vdots \\ \& & a_1, \ldots, a_n/b_m \end{matrix}\right\} \rightarrow (b_1, \ldots, b_m/c \rightarrow a_1, \ldots, a_n/c)$$

The principle (Tg) is not a rule in the strict sense, but rather a schematic rule. This means that (Tg) has, depending on the value of m, many rules as instances. There is therefore no way to directly express the content of (Tg) by means of a single formula of the metalanguage. This fact was very unsatisfactory for Popper. He searched for a replacement of (Tg) that could be expressed directly in his metalanguage. We discuss his proposals and their problems in § 4.6.

The fact that (Tg) is parametrized by a natural number actually applies not only to the multiplicity of premises by m, but also to the multiplicity n of sentences on the left side of / within both (Rg) and (Tg). This could have been overcome easily by adopting a notation for finite sets of statements corresponding to contexts Γ, Δ etc. in Gentzen sequents, where the multiplicity m of premises can be modelled by conjunctively understood sets of sentences on the right hand side of /, as in the multiple premises the left hand side of / is always the same. Obviously, Popper did not want to enlarge the formalized part of his metalanguage by such additional means of expression, insisting on the fact that deducibility / is the only primitive concept of the metalanguage (for propositional logic). Instead of talking metalinguistically about finite sets of formulas, Popper preferred, under the heading "Basis II", to incorporate objectlinguistic conjunction into his structural rules. Finite conjunctions yield, of course, a substitute for talking about finite sets, and technically this has the effect of getting rid of the parametrization of structural rules with natural numbers. Conceptually, this is not unproblematic, as it blurs the distinction between the metalinguistic association of sentences by means of a comma and their objectlinguistic association by means of a conjunction operator.

[29] Although Popper does not use the term "structural".

[30] Cf. Popper (1947c).

4 The general theory of derivation

Popper's general theory of derivation does not refer to any logical signs of the object language. It studies properties of statements and relations on statements that can be defined using only the deducibility relation. Examples of such properties are being a theorem in the deducibility structure, that is, being a demonstrable statement, or being a statement that is refutable.

We discuss the following properties and relations: mutual deducibility, complementarity, demonstrability, contradictoriness, refutability and relative demonstrability. Our focus is on mutual deducibility and relative demonstrability. Relative demonstrability in particular contains the notions of complementarity, demonstrability, contradictoriness and refutability as special cases.

4.1 Mutual deducibility

The relation of mutual deducibility // is explicitly defined as follows:

(D//) $$a/\!/b \leftrightarrow (a/b \;\&\; b/a)$$

We also speak of "interdeducibility". It is an equivalence relation, as one can see by checking the rules of Basis I. Two mutually deducible statements a and b are said to have the same *logical force*.[31] The equivalence classes induced by // are thus logical forces.

Popper (1947c, p. 203) also calls two mutually deducible statements *logically equivalent*, and then calls (D//) the *substitutivity principle for logical equivalence*. The following substitution lemma holds:

Lemma 4.1 *If a and b are mutually deducible, then we may substitute b for a in every deducibility relation, that is, the following two statements are true:*
1. $a/\!/b \rightarrow (a_1, \dots, a_n/a \rightarrow a_1, \dots, a_n/b)$.
2. $a/\!/b \rightarrow (a_1, \dots, a_n, a, a_{n+1}, \dots, a_m/c \rightarrow a_1, \dots, a_n, b, a_{n+1}, \dots, a_m/c)$.

Proof (1) follows directly from (Tg), and (2) follows from (Tg) and Lemma 3.1. □

Popper also considers the following definition of //:[32]

(D//′) $$a/\!/b \leftrightarrow (c)(a/c \leftrightarrow b/c)$$

For this alternative definition we can prove the following lemma:

Lemma 4.2 *In the presence of* (Tg) *and* (Rg), (D//) *is equivalent to* (D//′).

Proof We have to show that $(a/b \;\&\; b/a) \leftrightarrow (c)(a/c \leftrightarrow b/c)$ is true. The proof from left to right uses (Tg), and the proof from right to left uses (Rg). □

[31] Cf. Popper (1947b, p. 282).

[32] Cf. Popper (1947d, def. (DB1)) or Popper (1947b, def. (7.1)).

According to (D//), $a/\!/b$ means that a and b are interdeducible; according to (D//′) it means that a and b have the same deductive content, that is, that their respective sets of consequences are the same.

4.2 Complementarity and demonstrability

The notion of complementarity for statements $a_1, \ldots, a_n$, written $\vdash a_1, \ldots, a_n$, is defined as follows:

$$(\mathrm{D}{\vdash}1) \qquad \vdash a_1, \ldots, a_n \;\leftrightarrow\; (b)(c)((a_1/c \;\&\; \ldots \;\&\; a_n/c) \rightarrow b/c)$$

For $n = 1$, we obtain the definition of a self-complementary or demonstrable statement, written $\vdash a$:

$$(\mathrm{D}{\vdash}1') \qquad \vdash a \;\leftrightarrow\; (b)(c)(a/c \rightarrow b/c)$$

Intuitively, what is expressed by the complementarity of the statements $a_1, \ldots, a_n$ is that at least one of them has to be true, that is, that taken together they exhaust all possible states of affairs. This idea is captured by saying that if the statements $a_1, \ldots, a_n$ are complementary, then any statement c that follows from each of the statements $a_1, \ldots, a_n$ individually follows from any statement b. From (D⊢1′) one can thus obtain

$$\vdash a \;\leftrightarrow\; (b)(b/a)$$

by instantiating c by a and by using basic rules. A demonstrable statement is thus a statement that follows from any statement whatsoever.[33]

4.3 Contradictoriness and refutability

The notion of contradictoriness for statements $a_1, \ldots, a_n$, written $\not\vdash a_1, \ldots, a_n$, is defined as follows:

$$(\mathrm{D}{\not\vdash}) \qquad \not\vdash a_1, \ldots, a_n \;\leftrightarrow\; (b)(c)((b/a_1 \;\&\; \ldots \;\&\; b/a_n) \rightarrow b/c)$$

For $n = 1$, we get the notion of refutability of a statement a, written $\not\vdash a$:

$$(\mathrm{D}{\not\vdash}') \qquad \not\vdash a \;\leftrightarrow\; (b)(c)(b/a \rightarrow b/c)$$

The intuition lying behind these notions is the following: a statement is refutable if it is false no matter the state of affairs, and a sequence of statements $a_1, \ldots, a_n$ is contradictory if its members cannot be true together. Contradictoriness of the

[33] For the definition of demonstrability cf. Popper (1947c, def. (D8.2+)) or Popper (1947d, def. (D11)). For the definition of complementarity cf. Popper (1948a, def. (D3.1)).

statements $a_1, \ldots, a_n$ is therefore defined by saying that any statement b that implies each of the statements $a_1, \ldots, a_n$ individually implies any statement c.[34] From (D$\urcorner'$) one can thus obtain

$$\urcorner a \leftrightarrow (c)(a/c)$$

by substituting a for b and by using basic rules. This is the definition of a self-contradictory statement, that is, of a refutable statement. From such a statement any other statement follows.

4.4 Relative demonstrability

Complementarity and contradictoriness can be combined in one relation that holds between statements $a_1, \ldots, a_n$ and $b_1, \ldots, b_m$. This relation is called *relative demonstrability* (or *relative refutability*), written $a_1, \ldots, a_n \vdash b_1, \ldots, b_m$. One of the definitions given by Popper is:[35]

$$\text{(D}\vdash\text{2)} \quad a_1, \ldots, a_n \vdash b_1, \ldots, b_m \leftrightarrow (c)((b_1/c \;\&\; \ldots \;\&\; b_m/c) \rightarrow a_1, \ldots, a_n/c)$$

For reasons explained in § 4.5, we will use the following slightly modified definition of relative demonstrability in the remainder of this paper:

Definition 4.3 *Relative demonstrability* $a_1, \ldots, a_n \vdash b_1, \ldots, b_m$ is defined by

$$\begin{aligned}\text{(D}\vdash\text{3)} \quad & a_1, \ldots, a_n \vdash b_1, \ldots, b_m \leftrightarrow \\ & (c)(d_1) \ldots (d_k)((b_1, d_1, \ldots, d_k/c \;\&\; \ldots \;\&\; b_m, d_1, \ldots, d_k/c) \rightarrow \\ & \qquad\qquad\qquad a_1, \ldots, a_n, d_1, \ldots, d_k/c)\end{aligned}$$

Note that in contradistinction to Popper's (D$\vdash$2) the definiens in (D$\vdash$3) is now formulated with context statements $d_1, \ldots, d_k$ for $0 \leq k$, which occur as additional premises in each of the deducibility relations. Definition (D$\vdash$3) is thus more general than (D$\vdash$2).

Lemma 4.4 *The concept of relative demonstrability contains, as special cases, the concepts of complementarity, demonstrability, contradictoriness and refutability.*

Proof Let $k = 0$. For complementarity, let $n = 0$. For demonstrability, let $n = 0$ and $m = 1$. For contradictoriness, let $m = 0$. For refutability, let $n = 1$ and $m = 0$. □

Lemma 4.5 *For all* $a_1, \ldots, a_n, b$*:* $a_1, \ldots, a_n/b \leftrightarrow a_1, \ldots, a_n \vdash b$.

Proof For $m = 1$ in (D$\vdash$3) we have to show that $(c)(d_1) \ldots (d_k)(b_1, d_1, \ldots, d_k/c \rightarrow a_1, \ldots, a_n, d_1, \ldots, d_k/c)$ is equivalent to $a_1, \ldots, a_n/b_1$. Let $k = 0$. Instantiating c

[34] For the definition of refutability cf. Popper (1947c, def. (D8.3)) or Popper (1947d, def. (D12)). For the definition of contradictoriness cf. Popper (1948a, def. (D3.2) and (D3.2′)).

[35] Cf. Popper (1948a, def. (D3.3′)).

by b_1 yields $b_1/b_1 \rightarrow a_1, \ldots, a_n/b_1$, and by (Rg) we get $a_1, \ldots, a_n/b_1$. Likewise for the other direction. □

This lemma allows us to replace / by $\vdash$ in all formulas of the metalanguage. This will be necessary further on to bring some of Popper's definitions into a form that makes them dualizable. Note that, conversely, $\vdash$ can only be replaced by / if $\vdash$ has exactly one succedent b. For mutual deducibility we have $a//b \leftrightarrow (a \vdash b \;\&\; b \vdash a)$.

Lemma 4.6 *The following structural rules hold for* $a_1, \ldots, a_n \vdash b_1, \ldots, b_m$*:*
1. Weakening on the left (LW) *and weakening on the right* (RW)*:*

(LW) $\quad a_1, \ldots, a_n \vdash b_1, \ldots, b_m \rightarrow a_1, \ldots, a_n, a_{n+1} \vdash b_1, \ldots, b_m$

(RW) $\quad a_1, \ldots, a_n \vdash b_1, \ldots, b_m \rightarrow a_1, \ldots, a_n \vdash b_1, \ldots, b_m, b_{m+1}$

2. Exchange on the left (LE) *and exchange on the right* (RE)*:*

(LE)
$$a_1, \ldots, a_i, a_{i+1}, \ldots, a_n \vdash b_1, \ldots, b_m \rightarrow a_1, \ldots, a_{i+1}, a_i, \ldots, a_n \vdash b_1, \ldots, b_m$$

(RE)
$$a_1, \ldots, a_n \vdash b_1, \ldots, b_j, b_{j+1}, \ldots, b_m \rightarrow a_1, \ldots, a_n \vdash b_1, \ldots, b_{j+1}, b_j, \ldots, b_m$$

3. Contraction on the left (LC) *and contraction on the right* (RC)*:*

(LC) $\quad a_1, \ldots, a_i, a_i, \ldots, a_n \vdash b_1, \ldots, b_m \rightarrow a_1, \ldots, a_i, \ldots, a_n \vdash b_1, \ldots, b_m$

(RC) $\quad a_1, \ldots, a_n \vdash b_1, \ldots, b_j, b_j, \ldots, b_m \rightarrow a_1, \ldots, a_n \vdash b_1, \ldots, b_j, \ldots, b_m$

4. If there are i, j *(for* $1 \leq i \leq n$ *and* $1 \leq j \leq m$*) such that* $a_i = b_j$*, then* $a_1, \ldots, a_n \vdash b_1, \ldots, b_m$*.*

Proof Consider the definiens of $a_1, \ldots, a_n \vdash b_1, \ldots, b_m$:

$$(c)(d_1)\ldots(d_k)((b_1, d_1, \ldots, d_k/c \;\&\; \ldots \;\&\; b_m, d_1, \ldots, d_k/c) \rightarrow a_1, \ldots, a_n, d_1, \ldots, d_k/c).$$

1. We can always strengthen the antecedent of the implication or weaken its succedent. Left weakening of / follows from Basis I. We thus get (LW) by weakening the succedent $a_1, \ldots, a_n, d_1, \ldots, d_k/c$ to $a_1, \ldots, a_n, a_{n+1}, d_1, \ldots, d_k/c$. The antecedent can be strengthened by adding the conjunct $b_{m+1}, d_1, \ldots, d_k/c$, which gives us (RW).

2. Exchange on the left (LE) is due to Lemma 3.1(1). Exchange on the right (RE) follows from the commutativity of metalinguistic conjunction.

3. Contraction on the left (LC) is due to Lemma 3.1(2). Consider $a, a/b$; by (Rg) we have a/a, and (Tg) yields a/b. By replacing / by $\vdash$ and subsequent applications of weakening and exchange we obtain (LC). Contraction on the right (RC) follows from the idempotence of metalinguistic conjunction.

4. For $a_i = b_j$ we have a_i/b_j by (Rg). Lemma 4.5 gives $a_i \vdash b_j$. By applications of (LW), (RW), (LE) and (RE) we get $a_1, \ldots, a_n \vdash b_1, \ldots, b_m$. □

Relative demonstrability $a_1, \ldots, a_n \vdash b_1, \ldots, b_m$ can be interpreted as derivability of the disjunction of $b_1, \ldots, b_m$ from the conjunction of $a_1, \ldots, a_n$. This interpretation is justified by the fact that for object languages containing conjunction $\wedge$ and disjunction $\vee$ one can show the following:

$$a_1, \ldots, a_n \vdash b_1, \ldots, b_m \leftrightarrow a_1 \wedge \ldots \wedge a_n \vdash b_1 \vee \ldots \vee b_m.$$

This is a consequence of Lemma 5.5, proved below. The concept of relative demonstrability gives thus an interpretation of Gentzen's (1935a; 1935b) sequents.[36]

4.5 Relative demonstrability and cut

Popper's definition (D$\vdash$2) of relative demonstrability

(D$\vdash$2) $a_1, \ldots, a_n \vdash b_1, \ldots, b_m \leftrightarrow (c)((b_1/c \;\&\; \ldots \;\&\; b_m/c) \to a_1, \ldots, a_n/c)$

is not wholly satisfactory, since it does not allow to show that

(Cut) $a_1, \ldots, a_n \vdash b_1, \ldots, b_m, c \to$
$(c, a_1, \ldots, a_n \vdash b_1, \ldots, b_m \to a_1, \ldots, a_n \vdash b_1, \ldots, b_m)$

holds for any object language.

Theorem 4.7 *The rule* (Cut) *does not follow from* (D$\vdash$2) *and Basis I.*

Proof We show this by means of a counterexample for the instance

$$(a \vdash b, c \;\&\; c, a \vdash b) \to a \vdash b.$$

The negation of this claim is metalinguistically equivalent to

$$(d)((b/d \;\&\; c/d) \to a/d) \;\&\; c, a/b \;\&\; \neg(a/b).$$

Now consider an object language that contains only the three statements a, b and c. Then this is equivalent (also using the rules (Rg) and (Tg) of the Basis I) to the statement

$$(c/b \to a/b) \;\&\; (b/c \to a/c) \;\&\; c, a/b \;\&\; \neg(a/b).$$

The model under which the deducibility relation is only true for $c, a/b$ (and the instances required by (Rg)) satisfies (Tg) and is the desired countermodel. □

However, if one presupposes that the object language contains conjunction, disjunction and implication, then (Cut) can be shown to hold.

[36] Cf. Popper (1948a, p. 181); cf. Cohen (1953b, p. 69f. and p. 208f.).

Theorem 4.8 *In the presence of conjunction, disjunction and implication,* (Cut) *does follow from* (D⊢2) *and Basis I.*

Proof In the presence of conjunction and disjunction, and in view of Lemma 5.5, it is sufficient to show that $(a \vdash b, c \;\&\; c, a \vdash b) \rightarrow a \vdash b$ holds, which is done as follows:

1. From the instance $b, a/b$ of (Rg) and (C>) we get $b \vdash a > b$.
2. From the assumption $c, a \vdash b$ and (C>) we get $c \vdash a > b$.
3. From the assumption $a \vdash b, c$ and (D⊢2) we then get $a \vdash a > b$.
4. From (C>) and (LC) we get $a \vdash b$. □

Since we do not want to presuppose the existence of any specific logical constants in object languages, we use (D⊢3) as a more general definition of relative demonstrability, which is formulated with additional context statements $d_1, \ldots, d_k$ (for $0 \leq k$) occurring as additional premises in each of the deducibility relations in the definiens (cf. § 4.4), which we reproduce here for easier reference:

$$\text{(D}\vdash\text{3)}\quad a_1, \ldots, a_n \vdash b_1, \ldots, b_m \leftrightarrow (c)(d_1)\ldots(d_k)((b_1, d_1, \ldots, d_k/c \;\&\; \ldots \;\&\; b_m, d_1, \ldots, d_k/c) \rightarrow a_1, \ldots, a_n, d_1, \ldots, d_k/c)$$

Given this definition, (Cut) follows from the rules of Basis I alone, without presupposing the existence of any logical constants.

Theorem 4.9 *If relative demonstrability is defined by* (D⊢3) *instead of* (D⊢2)*, then* (Cut) *follows from the rules of Basis I.*

Proof We have to show

$$\underbrace{a_1, \ldots, a_n \vdash b_1, \ldots, b_m, e}_{A} \rightarrow (\underbrace{e, a_1, \ldots, a_n \vdash b_1, \ldots, b_m}_{B} \rightarrow \underbrace{a_1, \ldots, a_n \vdash b_1, \ldots, b_m}_{C})$$

(where e is the cut formula). We assume A and B, and have to show C. In order to show C, we further assume $b_1, d_1, \ldots, d_k/c \;\&\; \ldots \;\&\; b_m, d_1, \ldots, d_k/c$, and have to show $a_1, \ldots, a_n, d_1, \ldots, d_k/c$.

1. From B we know that $a_1, \ldots, a_n, d_1, \ldots, d_k, e/c$ holds.
2. For each i with $1 \leq i \leq m$ we get from $b_i, d_1, \ldots, d_k/c$ by weakening on the left the corresponding relation $a_1, \ldots, a_n, b_i, d_1, \ldots, d_k/c$.
3. Using (D⊢3), the following is an instance of A:

$$(b_1, a_1, \ldots, a_n, d_1, \ldots, d_k/c \;\&\; \ldots \;\&\; b_m, a_1, \ldots, a_n, d_1, \ldots, d_k/c \;\&\; e, a_1, \ldots, a_n, d_1, \ldots, d_k/c) \rightarrow a_1, \ldots, a_n, a_1, \ldots, a_n, d_1, \ldots, d_k/c.$$

(1), (2) and (3) together imply $a_1, \ldots, a_n, a_1, \ldots, a_n, d_1, \ldots, d_k / c$, and by contracting the premises $a_1, \ldots, a_n, a_1, \ldots, a_n$ we get $a_1, \ldots, a_n, d_1, \ldots, d_k / c$. $\square$

A disadvantage of definition (D$\vdash$3) is that it is not an explicit definition. As in Popper's framework no mechanism is available to handle contexts consisting of finite sets or of finite lists of statements, $a_1, \ldots, a_n \vdash b_1, \ldots, b_m$ is defined through an unspecified number k of context statements $d_1, \ldots, d_k$. The relation $\vdash$ is thus not eliminable in general. Nonetheless, it is always eliminable in a given logical argument, because the number k can then be specified.

4.6 The development of Popper's formulation of a basis

Popper presented several formulations to capture what he understands by a basis. We trace this development following the order of his publications. Note, however, that the exact order of Popper (1947b) and Popper (1947c) is not completely clear; both reference the other as forthcoming and were probably written at around the same time. We restrict ourselves again to propositional logic, and do not comment on additional constraints regarding substitution that have to be made for the treatment of quantification (cf. § 9).

There are two decisive points in this development. The first is the impasse that Popper found himself in while trying to get rid of the generalized transitivity principle (Tg), which led him to develop Basis II. But Curry (1948a, this volume, § 13.5) showed that Basis II is not, contrary to what Popper claims, equivalent to Basis I. We will point out Popper's error and will show how it may be corrected. The second decisive point was when Popper rediscovered a solution of how to replace (Tg) by a simpler rule, something which had been done similarly before by Gentzen for a system of Hertz.

In Popper (1947b) the notion of *absolute validity* is developed, which justifies the rules of a basis:

> There are inferences [...] which can be shown, on our definition of validity, to be valid *whatever the logical form of the statements involved*. [...] We shall say of these inferences that they are *absolutely valid*. (Ibid., p. 274)

The basis that he considers consists of the generalized reflexivity principle (Rg) and the rule (Tg),[37] which he claims, without giving a proof, to be complete with respect to his notion of absolute validity:

> It can be shown that all absolutely valid rules of inference [...] can be reduced to two [namely (Rg) and (Tg)]. By "reduced", I mean here: every inference which is an observance of some of the rules in question can be shown to be an observance of these two rules [...]. (Ibid., p. 277)

This claim is plausible, especially in view of Lejewski's (1974) proof of the equivalence of Popper's Basis I and the systems developed in Tarski (1930a,b, 1935b, 1936a).

[37] Cf. § 3.4. In Popper (1947b) these rules are labeled (6.1g) and (6.2g), respectively.

In Popper (1947c) two different approaches to axiomatizing the deducibility relation are developed, which are summed up ibid., p. 211. Approach I (developed ibid., § 2) consists of one of several possible variants of rules equivalent to (Rg) and (Tg). Approach II (developed ibid., § 3) takes a simpler transitivity principle than (Tg) and postulates the availability of conjunction in the object language. These two approaches are exemplified by Basis I and Basis II, respectively.

Popper considers each of the combinations (Tg) + (Rg), (Tg) + (2.1) + (2.2) + (2.3) and (Tg) + (2.41) + (2.7) as a possible basis. The components are:

$$\text{(2.1)} \qquad a/a$$

$$\text{(2.2)} \qquad a_1, \ldots, a_n/b \rightarrow a_1, \ldots, a_n, a_{n+1}/b$$

$$\text{(2.3)} \qquad a_1, \ldots, a_n/b \rightarrow a_n, \ldots, a_1/b$$

$$\text{(Rg)} = \text{(2.4)} \qquad a_1, \ldots, a_n/a_i \quad (\text{for } 1 \leq i \leq n)$$

$$\text{(2.41)} \qquad a_1, \ldots, a_n/a_1$$

$$\text{(2.7)} \qquad a_1, \ldots, a_{n+m}/b \rightarrow a_n, \ldots, a_1, a_{n+1}, \ldots, a_{n+m}/b$$

$$\text{(Tg)} = \text{(2.5g)} \qquad \left\{\begin{array}{l} \quad a_1, \ldots, a_n/b_1 \\ \& \; a_1, \ldots, a_n/b_2 \\ \vdots \qquad\quad \vdots \\ \& \; a_1, \ldots, a_n/b_m \end{array}\right\} \rightarrow (b_1, \ldots, b_m/c \rightarrow a_1, \ldots, a_n/c)$$

The combination (Tg) + (Rg) is called Basis I by Popper.

All these combinations have the downside that they have to include the complicated transitivity principle (Tg) instead of a simpler one. Popper considers the rule (Tg) to be problematic and wants to get rid of it. He explains the exact problem later, in Popper (1948a, p. 177, fn 6):

> The objection against [(Tg)] is that it makes use of an unspecified number of conjunctive components in its antecedent; this may be considered as introducing a new metalinguistic concept – something like an infinite product.

This leads him to Approach II, which contains Basis II (cf. Popper, 1947c, § 3). It is supposed to axiomatize the deducibility relation by using the simpler transitivity principle (Te)

$$\text{(Te)} = \text{(2.5e)} \qquad a_1, \ldots, a_n/b \rightarrow (b/c \rightarrow a_1, \ldots, a_n/c)$$

and by additionally requiring that for any two statements a and b of the object language $\mathcal{L}$ their conjunction $a \wedge b$ is also contained in $\mathcal{L}$. Popper (1947c, p. 206) expresses the motivating idea behind Basis II as follows:

> But the main point of using conjunction is that it should permit us to *link up any number of statements into one*. If it does this, then we can always replace the m conclusions of m different inferences from the same premises by one inference.

One problem with this approach is that in the absence of (Tg), the rules for conjunction have to be sufficiently strengthened in order to work. Popper also noticed this and proposed the following slightly tweaked rules for conjunction:[38]

$$(\mathrm{Cg}_1) \qquad a_1, \ldots, a_n / b \wedge c \rightarrow (a_1, \ldots, a_n / b \;\&\; a_1, \ldots, a_n / c)$$

$$(\mathrm{Cg}_2) \qquad (a_1, \ldots, a_n / b \;\&\; a_1, \ldots, a_n / c) \rightarrow a_1, \ldots, a_n / b \wedge c$$

Basis II thus consists of (Rg), (Te), (Cg_1), (Cg_2) and the existence postulate for conjunctions.

Popper uses these additional rules for conjunction to prove that (Tg) can be obtained from Basis II. That his proof contains an error was pointed out by Curry (1948a), who provided the following arithmetical counterexample: Let the object language consist of the natural numbers. Let $a \wedge b$ be the minimum of a and b. Let $a_1, \ldots, a_n / b$ hold if, and only if, $\min(a_1, \ldots, a_n) \leq b + n - 1$. Under this interpretation (Te), (Rg) and (Cg) of Basis II are satisfied but the rule (Tg) of Basis I is not. To see this, let, in (Tg), $n = 1$, $m = 2$, $a = b_1 = b_2 = 1$ and $c = 0$. A different but analogous arithmetical countermodel is given by Bernays in a letter to Popper of 12 May 1948 (cf. this volume, § 21.7) by interpreting $a \wedge b$ as the maximum of a and b, and $a_1, \ldots, a_n / b$ as $a_1 + \ldots + a_n \geq b$.

The crucial step in the proof that Basis I and Basis II are equivalent is to show that (Tg) follows from the rules of Basis II. Popper's attempted proof (ibid, p. 210f.) proceeds as follows. He assumes $a_1, \ldots, a_n / b_i$, for $1 \leq i \leq m$, as well as $b_1, \ldots, b_m / c$, and has to show that $a_1, \ldots, a_n / c$ follows. He attempts to do this in the following way:

1. From $a_1, \ldots, a_n / b_i$ (for all $1 \leq i \leq m$) obtain $a_1, \ldots, a_n / b_1 \wedge \ldots \wedge b_m$.
2. From $b_1, \ldots, b_m / c$ obtain $b_1 \wedge \ldots \wedge b_m / c$.
3. From $a_1, \ldots, a_n / b_1 \wedge \ldots \wedge b_m$ and $b_1 \wedge \ldots \wedge b_m / c$ obtain $a_1, \ldots, a_n / c$.

Step (1) can be proved by induction on m and iterated applications of (Cg_2); Popper gives this part of the proof explicitly. Step (3) is just an instance of his transitivity principle (Te). It is therefore step (2) which must be responsible for the failure of Popper's attempted proof, and it is indeed this inference that fails to be satisfied by Curry's counterexample: if $m = 3$, $b_1 = b_2 = b_3 = 1$ and $c = 0$, then $b_1 \wedge \ldots \wedge b_m / c$ does not follow from $b_1, \ldots, b_m / c$; an analogous counterexample can be given for Bernays's arithmetical interpretation. This derivation would be possible if Popper's rule (3.4g), that is,

$$(3.4\mathrm{g}) \qquad a_1, \ldots, a_n, b \wedge c / d \;\leftrightarrow\; a_1, \ldots, a_n, b, c / d$$

were secondary to (i.e., derivable from) Basis II.[39] However, both Curry's and Bernays's interpretations invalidate (3.4g)[40]. Popper (1947c, p. 210) erroneously thinks he has already proven (3.4g):

[38] In Popper's formulation, (Cg_1) and (Cg_2) appear as one rule (Cg), which is here split up into two parts.

[39] Cf. footnote 61 for Popper's use of the term "secondary".

[40] Bernays explicitly mentions this point in his letter, and Popper explicitly acknowledges Bernays's critique of Basis II in his reply of 13 June 1948 (cf. § 21.8).

> But this situation changes completely if we drop the generalised transitivity principle (Tg) and replace it by the simpler form (Te). In this case, all the rules 3.1 to 3.5 and 3.1g to 3.4g still follow from 3.5g [= (Cg)]; but the opposite is not the case.

If rule (3.4g) were indeed secondary to Basis II, then step (2) could be proved by a simple induction on m. Basis II might therefore be salvageable if we added to the rules (Cg_1) and (Cg_2) the rule (3.4g).[41]

Popper (1947d) starts with a characterization of "[t]he customary systems of modern lower functional logic, such as *Principia Mathematica*, or the systems of Hilbert–Ackermann, Hilbert–Bernays, or Heyting, etc." (ibid., p. 1214). The fourth point of this characterization is:

> (d) Some further very general primitive rules of inference (such as some principles stating that the inference relation is transitive and reflexive) which do not refer to formative signs are assumed, either explicitly or, more often, tacitly.

This corresponds to what Popper uses his basis for. But in this article he proposes to also get rid of the basis:

> The inferential definitions of the conjunction [...] can be reformulated in such a way as to incorporate *all* the rules of inference mentioned. In this way, we can get rid of even the few trivial primitive rules (d) which were left in the previous approach; in other words, we obtain the whole formal structure of logic from metalinguistic inferential definitions alone. (Ibid., p. 1215)

Popper provides the following inferential definition (DB2) for conjunction, which contains (Cg), (Te), (2.1) and (2.2):

$$\begin{aligned} a//b \wedge c \;\leftrightarrow\; (a_1)\dots(a_n)((a_1,\dots,a_n/a \;\leftrightarrow\; &(a_1,\dots,a_n/b \;\&\; a_1,\dots,a_n/c)) \\ \text{(DB2)} \qquad &\& \; (b/c \rightarrow (a_n,\dots,a_1/b \rightarrow a_1,\dots,a_n/c)) \\ &\& \; (a_1,\dots,a_n/c \rightarrow a_1,\dots,a_n,b/c) \;\&\; a_1/a_1)) \end{aligned}$$

As this approach is based on the rules of Basis II, it suffers from the same problems. Hence (Tg) does in general not hold for the deducibility relation.

In Popper (1948a) a new notation for the deducibility relation is introduced: $D(a_1, a_2, \dots, a_n)$ stands for $a_2, \dots, a_n/a_1$, and Popper from now on explicitly allows the number of premises to be 0, that is, he allows $D(a)$, which stands for $/a$. The new basis that Popper considers consists of the following two rules (translated from D-notation into $/$-notation):

$$\text{(BI. 1)} \qquad a_2/a_1 \;\leftrightarrow\; a_2, a_2/a_1$$

$$\begin{aligned}\text{(BI. 2)} \quad a_2,\dots,a_n/a_1 \;\leftrightarrow\; (a_{n+1})\dots(a_{n+r})(a_1,\dots,a_n/a_{n+r} \;\rightarrow \\ a_{n+r-1},\dots,a_{n+1},a_n,\dots,a_2/a_{n+r})\end{aligned}$$

[41] What is actually only needed of (3.4g) is the implication from right to left.

From these two rules Popper obtains (Rg) and the rules

$$(1.44') \qquad a_1, \ldots, a_n/b \rightarrow a_n, \ldots, a_1/b$$

$$(1.45') \qquad a_1, \ldots, a_n/b \rightarrow a_1, \ldots, a_{n+r}/b$$

$$(1.46') \qquad a_1, \ldots, a_n/b \rightarrow (b, a_1, \ldots, a_n/c \rightarrow a_1, \ldots, a_n/c)$$

Popper then shows how to derive (Tg) from (1.44′), (1.45′) and (1.46′) by an induction on the number m in (Tg). He thus achieves the replacement of (Tg) by a rule which can, in contrast to (Tg), be stated in the metalanguage. He comments:

> The problem of avoiding [(Tg)] was discussed, but not solved, in [Popper (1947c)]. The lack of a solution led me there to construct Basis II, the need for which, as it were, has now disappeared. (Popper, 1948a, p. 177, fn 6)

This result also allows him to modify the implementation of the programme of Popper (1947d). Instead of using his definition (DB2), he can use this new formulation of Basis I. He mentions this possibility in Popper (1948a, p. 177, fn 6).

Already Curry (1948c) pointed out that this part of the proof is similar to what Gentzen (1932, esp. § 3) had shown, namely that the rule of "Syllogismus" of the system of Hertz (1929b) can be replaced by his cut rule, while Popper showed that his rule (Tg) can be replaced by the abovementioned rule (1.46′). The "Syllogismus" rule is practically identical to Popper's rule (Tg), and Gentzen's cut rule is identical to (1.46′). Gentzen's and Popper's proofs proceed in a very similar fashion. The question is how much Popper knew of the systems of Hertz and Gentzen. Popper (1947c, p. 204) remarks in a footnote that Bernays pointed him to the articles of Hertz (1923, 1931), and he mentions Gentzen (1935a,b) in order to compare his concept of relative demonstrability with Gentzen's sequent arrow. There are no further references to either of these authors in Popper's articles. In his autobiography, Popper (1974c, § 25) writes about his logic work in New Zealand: "I invented for myself something now called 'natural deduction'". Obviously, "natural deduction" is here understood not in the specific sense of the calculus of natural deduction, but in a generic sense covering Gentzen-style systems, in particular the sequent calculus.[42] This remark suggests that he had not been aware of Gentzen's systems and their foundational significance until the basic framework of his inferential logic was conceived.[43]

[42] Popper's (n.d.[b]) unpublished and undated handwritten notes entitled "Natural Deduction" and "Natural Deduction with Sheffer Strokes" confirm that Popper used the term "natural deduction" in this wider sense, which includes sequents formulated with relative demonstrability $\vdash$.

[43] In Popper (1948a, fn 7) we find a discussion of Gentzen sequents in relation to his own turnstile operation (which denotes relative demonstrability; cf. § 4.4). This footnote reads as a later comparison of his own conception with others that had come to his attention after their completion. In a letter to Paul Bernays of 22 December 1946 (this volume, § 21.1) he lists major papers of Hertz and the first paper of Gentzen, which is a direct discussion of Hertz's work, as items borrowed from Bernays, which indicates that Popper received knowledge of the development from Hertz to Gentzen not before 1947. However, Popper had always been aware of Gentzen's consistency proofs for arithmetic, as remarks in his correspondence with Forder show, but without any detailed knowledge of Gentzen's proofs, wrongly claiming that Tarski anticipated Gentzen's proof (cf., e.g., this volume, § 26.2 and § 26.4). Popper's unpublished notes on derivation and demonstration (cf. this volume, Chapter 18)

At the very end of Popper (1948a) a new Basis III is introduced. Popper takes two-place deducibility (or "two-termed derivability", as he says), that is, a/b, characterized by transitivity and reflexivity as a starting point, together with one of the two rules (the second is here translated into $/$-notation)

$$\text{[BIII]} \qquad b/a \leftrightarrow (c)(c/b \rightarrow c/a)$$

$$(2.1) \qquad b/a \leftrightarrow (c)(a/c \rightarrow b/c)$$

and the following definition of relative demonstrability:

$$\text{(D3.3)} \quad a_1, \ldots, a_n \vdash b_1, \ldots, b_m \leftrightarrow (c)(d)((b_1/d \;\&\; \ldots \;\&\; b_m/d) \rightarrow ((c/a_1 \;\&\; \ldots \;\&\; c/a_n) \rightarrow c/d))$$

The resulting basis is equivalent to Basis I.

Popper (1949a), the last of Popper's core articles on logic, does not contain a lot of technical development but gives an overview of his general intentions instead. Popper now seems to have adopted the approach with Basis III. He writes:

> As the sole definiens of our definitions, the idea of deducibility or derivability will be used. We can restrict ourselves to using deducibility from *one* premise. We write "$D(a, b)$" for "a is deducible from b". [...] We do not need, for the derivation of mathematical logic, to assume more about deducibility than that it is transitive and reflexive. [...] With the help of "$D(a, b)$" it is easy to define deducibility from n premises. (Ibid., p. 723)

He (ibid., p. 723) also considers to define a generalized deducibility relation $a_1, \ldots, a_n/b_1, \ldots, b_m$ in terms of $D(a, b)$ alone, with the intended meaning that if each of the premises $a_1, \ldots, a_n$ is true, then at least one of the statements $b_1, \ldots, b_m$ must be true. This corresponds to relative demonstrability.

While most of the technical development in Popper's articles is done using Basis I, a tendency can be observed towards his using two-place deducibility a/b as the underlying concept, together with derivative n-place notions like relative demonstrability that are defined in terms of two-place deducibility (which, incidentally, is the approach followed by standard categorial logic as based on two-place arrows $a \rightarrow b$; cf. Lambek and Scott, 1986).

Approach II seems ill-chosen to us, even if the suggested corrections were to be carried out. It requires conjunction in the object language in its formulation of the basis, and it confounds structural properties of the deducibility relation with the definition of a logical constant.

Our choice of (a version of) Basis I, on the other hand, allows us to stay as close as possible to Popper's technical development, which proceeds mostly by using Basis I with $n + 1$-place deducibility $a_1, \ldots, a_n/b$. This choice also makes it possible to

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refer to Gentzen's system of natural deduction without discussing it in detail. Popper (1947c, § 8), which partially overlaps with Chapter 18 of this volume, does not contain any reference to Gentzen. In his preface to Stegmüller and von Kibéd (1984, p. VII), Stegmüller mentions that he first learned of the existence of Gentzen's theory of natural deduction through Popper in 1949.

avoid a certain awkwardness in the formulation of proofs, which is already present in several proofs involving relative demonstrability; in order to show that some statement involving relative demonstrability holds, one often has to unfold the definition of relative demonstrability first, then work with the definiens, and finally reintroduce relative demonstrability. Besides its conceptual superiority, Basis I has thus also practical advantages.

5 The special theory of derivation

The general theory of derivation was purely structural (in Gentzen's terminology) and based solely on the deducibility of statements without regard for their individual form and their individual deductive power. The subject of Popper's special theory of derivation are relations between statements, which are logically complex or have a specific deductive power, and their components[44], and deals with the logical laws emerging therefrom. It is based on the relational definitions of logical operations and the inferential definitions of logical operators.

5.1 Definitions of logical constants

As discussed in § 1.3, logical constants are characterized in terms of the role they play with respect to the deducibility relation $/$. Such characterizations proceed by what Popper calls *inferential definitions*. A sign of an object language $\mathcal{L}$ is a *logical constant* if, and only if, it can be defined by an inferential definition.[45] In Popper's terminology, logical constants are called *formative signs*, in contradistinction to what he calls *descriptive signs*, such as "mountain" or "elderly disgruntled newspaper reader"; cf. Popper (1947b, p. 257). According to Popper (1947b, p. 286), "*inferential definitions* [. . .] are characterized by the fact that they define a formative sign by its logical force which is defined, in turn, by a *definition in terms of inference* (i.e., of '$/$')." Popper (1947c, p. 220) summarizes his inferential approach as follows:

> The upshot of all these considerations is this: If we have an artificial model language with signs for conjunction, the conditional . . . etc. (we have called them "formative signs" of the language in question) then the meaning of these formative signs can be exhaustively determined by the rules of inference in which the signs occur; this fact is established by defining our definitions of these formative signs explicitly in terms of rules of inference.

[44] Note that "components" are not necessarily subsentences, but sentences that are deductively related to the original sentence in a certain way, such as sentences a and b which are deductively related to a conjunction c of a and b, even if c does not syntactically contain a and b. Cf. § 1.2 above.

[45] Cf., e.g., Popper (1947b, p. 286).

As before (cf. § 3.3) we use $\circ$ as representing an arbitrary logical operation called "connection" with $\mathcal{R}$ as its defining condition ("rule"), which is used in the relational definition

$$c \text{ is a connection of } a \text{ and } b \text{ if and only if } \mathcal{R}(c, a, b)$$

and the inferential definition

$$\text{(D}\circ\text{)} \qquad c /\!/ a \circ b \;\leftrightarrow\; \mathcal{R}(c, a, b).$$

As already mentioned, $\mathcal{R}(c, a, b)$ is an expression of the metalanguage containing as relations the deducibility relation $/$ as well as the defined relations $\vdash$ and $\not\mathrel{/}$. In the course of a logical argument, definitions of logical constants containing $\vdash$ and $\not\mathrel{/}$ can always be replaced by definitions that contain only $/$.

The relational definition makes no special assumption about the object language – it just singles out connections c of a and b if they are available in the object language (otherwise there simply is no connection of a and b). The inferential definition, however, requires existence and uniqueness, which means that it is based on the presupposition that there is *exactly one* connection c of a and b in the object language considered. Here "uniqueness requirement" and "exactly one" is understood *modulo interdeducibility*. That is, there is a connection c of a and b and all other connections c' of a and b are interdeducible with c: $c /\!/ c'$. Note that in any case, every c' interdeducible with a connection c of a and b is itself a connection of a and b. This follows from the fact that according to the general theory of derivation, we have substitutivity of interdeducibles (Lemma 4.1), which means that all our inferential concepts and results are invariant with respect to interdeducibility. Popper is fully aware of these existence and uniqueness requirements, which have not been appreciated by some of his reviewers (cf. § 2)

As explained in § 1.3, (D$\circ$) can be viewed as an *explicit definition* of a connective $\circ$, and Popper is fully entitled to call it that way. (D$\circ$) conservatively introduces into a language a sign for a new operator $\circ$, which is eliminable following the standard procedures used for the introduction and elimination of function symbols or definite descriptions (cf. footnote 11 in § 1.3). It should be noted, however, that (D$\circ$) is a definition of a *metalinguistic* function which associates with any a and b their connection $a \circ b$, which is unique up to interdeducibility, so $a \circ b$ essentially denotes an equivalence class of objectlinguistic sentences, none of which must have a special form. However, once we have reached that stage, we can, of course, introduce into our object language sentences of the form "$a \circ b$", where $\circ$ is now an objectlinguistic operator in the usual sense, and take it to be the objectlinguistic representative of $a \circ b$ (understood metalinguistically). Viewed that way, (D$\circ$) can be used to introduce a logical operator into a suitable object language, provided the connection operation as a relation between (not further specified) sentences is available.

In fact, we can even devise a formal object language in the usual way and lay down the rules $\mathcal{R}(a \circ b, a, b)$ for all a and b. In that case a connection $a \circ b$ of a and b with certain inferential properties always exists by stipulation. However, when proceeding in that manner, we must be aware (and Popper is) that such a stipulation may have undesired consequences up to the generation of inconsistencies. In any case, for (D$\circ$)

to hold, we still must make sure that uniqueness is satisfied. If this is the case, we can proceed with the rule

$$\text{(C}\circ\text{)} \qquad \mathcal{R}(a \circ b, a, b)$$

as the *characterizing rule* (C∘), which corresponds to the definition (D∘). If uniqueness is satisfied, (D∘) and (C∘) are obviously equivalent. So we will often work with (C∘) rather than with (D∘), as it is easier to handle and as it incorporates the rules of inference in which one is interested. Philosophically, however, it is the relationship between (D∘) and (C∘), more precisely the derivation of (C∘) from (D∘), what constitutes Popper's "trivialization" of logic, that is the derivation of logical laws from inferential definitions (cf. § 1.4).

5.2 Popper's definitional criterion of logicality

The question we are facing now is what form characterizing rules like $\mathcal{R}(a \circ b, a, b)$ might be allowed to take. Should certain rules be disallowed because their use in a definition of form (D∘) does not in fact define a logical constant, or does any rule $\mathcal{R}$ give rise to a definition of a logical constant?

For Popper any characterizing rule which is equivalent to an inferential definition characterizes a logical constant. He calls such rules *fully characterizing*. We state this as a definition:

Definition 5.1 A rule $\mathcal{R}$ characterizing an operation $\circ$ is *fully characterizing* if, and only if, it is equivalent to an inferential definition of $\circ$.[46]

In case that $\mathcal{R}$ is given in the form $\mathcal{R}(a \circ b, a, b)$, this means uniqueness of $\mathcal{R}$ for its first argument. For Popper uniqueness is essential for any definition of a logical constant.[47] We state this as an immediate corollary of our definition.

Corollary 5.2 *A rule of the form $\mathcal{R}(c, a_1, \ldots, a_n)$ is fully characterizing if, and only if*

$$(\mathcal{R}(a, a_1, \ldots, a_n) \;\&\; \mathcal{R}(b, a_1, \ldots, a_n)) \rightarrow a /\!/ b.$$

In other words, if, and only if, a rule $\mathcal{R}$ characterizes a statement c up to mutual deducibility, then $\mathcal{R}$ is fully characterizing c.

We distinguish between the definition and the corollary because not every characterizing rule has the form $\mathcal{R}(c, a_1, \ldots, a_n)$ and can thus be used in a relational definition. Only logical constants, which are *fully* characterized, are always definable by rules of the form $\mathcal{R}(c, a_1, \ldots, a_n)$.

What about the existence requirement? The inferential definition (D∘) is a proper definition only if, in addition to uniqueness, there exists a connection of a and b

[46] Cf., e.g., Popper (1948c, § VI); this volume, Chapter 6, p. 187f.

[47] Cf. Popper (1948c, p. 324).

in the object language considered, for which we define a name. We may, of course, stipulate that there be a connection of a and b and even denote it by $a \circ b$, but this is nothing an inferential definition can do by itself without becoming creative. Unique connections must be there before we can single them out by means of an inferential definition. This is a point Popper's reviewers have strongly emphasized (cf. § 2), but also a point of which Popper is aware.

Forcing an operation to exist in an object language can make the object language inconsistent. However, if it is uniquely characterized, it is a logical constant. Popper thus allows, for example, the following definition for "opponent" (*opp*), with its characterizing rule:

(D*opp*) $$a//opp(b) \;\leftrightarrow\; (c)(b/a \;\&\; a/c)$$

(C*opp*) $$(c)(b/opp(b) \;\&\; opp(b)/c)$$

This obviously trivializes any system, since it implies $(c)(b/c)$ for any b. But this does not lead Popper to reject (D*opp*) as a definition. In a system, where *opp* exists (thus an inconsistent system), it is a logical constant because it is unique for trivial reasons.[48] Historically, it is interesting to note that the connective *opp* is quite similar to the connective *tonk*, which was later introduced into the philosophical discussion by Prior[49], there with the intention to discredit inferentialism.

Lejewski (1974, p. 644) considers an even stronger version of *opp*, called *opp**. This supposed logical constant not only turns the object language inconsistent but also the metalanguage. Its definition and characterizing rule is supposed to be:

(D*opp**) $$a//opp^*(b) \;\leftrightarrow\; (c)(b/a \;\&\; \overline{b/c})$$

(C*opp**) $$(c)(b/opp^*(b) \;\&\; \overline{b/c})$$

The definition (D*opp**) is formulated using metalinguistic negation ($\overline{\quad}$). This contrasts with all definitions considered by Popper, who never uses negation in his metalinguistic definitions of logical constants. So while it is true that the definition of *opp** would turn any metalanguage inconsistent in which it is stated and where *opp** is forced to exist, this criticism cannot be applied to the system of Popper. Relying on positive logic within defining conditions, which is mandatory (cf. § 3.3), in view of Lemma 3.2 there can be no such characterization.[50]

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[48] Cf. Popper (1947b, p. 284): "We need not make sure, in any other way, that our system of definitions is consistent. For example, we may define (introducing an arbitrarily chosen name 'opponent'): [see (D*opp*)]. This definition has the consequence that every language which has a sign for 'opponent of *b*' [. . .] will be inconsistent [. . .]. But this need not lead us to abandon [(D*opp*)]; it only means that no consistent language will have a sign for 'opponent of *b*'."

[49] Prior (1960) cites Popper (1947b), but only for giving an example of what he understands by an analytically valid inference. He does not mention the fact that Popper (1947b) discusses the *tonk*-like connective *opp*. Neither Belnap (1962) nor Stevenson (1961) noticed this in their responses to Prior's article, even though they both also explicitly mention Popper's article. It is noted, however, by Merrill (1962).

[50] For an extensive discussion of Popper's criterion of logicality cf. also Schroeder-Heister (1984, 2006).

Another question concerning the form of definitions of logical constants is the following: For two given alternative rules $\mathcal{R}$ and $\mathcal{R}'$, which are equivalent, is one preferable over the other in defining a logical constant? Popper often considers more than one possible definition of a logical constant, showing (or in some cases only indicating) that these alternative definitions are equivalent. There seems to be no logical or philosophical criterion that makes Popper prefer one definition rather than another, equivalent definition. Often, it seems, he just chooses the brevity of some definition, or the ease with which he is able to explain it.

This contrasts with more modern approaches concerning definitions of logical constants. Especially in proof-theoretic semantics as initiated by Gentzen, Prawitz and Dummett there is a strong sense of preferring one special kind of rules (either introduction rules or elimination rules) as being constitutive of the meaning of logical constants. Such considerations play no part in Popper's definitions. For example, Popper's (1947c, p. 228) definition of the universal quantifier resembles its elimination rule, while his definition of the existential quantifier resembles its introduction rule. For a further discussion cf. § 1.5.

However, the fact that Popper does not explicitly give a philosophical criterion for preferring one form of definition over another does not preclude the possibility that he has a logical one. In the more mature version of his theory presented in Popper (1948a,c), he sometimes states definitions of logical constants in a form that highlights the duality of certain of his definitions. We will follow this lead and give the definitions in a way that allows for the formulation of a duality function that transforms any definition of a logical constant into a definition of a dual logical constant.

5.3 Not-strictly-logical operations and the understanding of logical notation

It is the existence of fully characterizing rules that distinguishes logical constants from non-logical constants, and it is this criterion of logicality that leads Popper to reject, for example, Johansson's negation as a logical constant (cf. § 6.4).[51] As characterizing rule $\mathcal{R}_j$ for this negation Popper gives the following:

$$a, b \vdash \neg_j c \rightarrow a, c \vdash \neg_j b.$$

This actually characterizes a negation weaker than Johansson's. For example, the rule

$$a, c \vdash \neg_j c \rightarrow a \vdash \neg_j c$$

[51] In contradistinction to more modern terminology, for Popper "Johansson's negation" and "minimal negation" are different items, the latter denoting dual-intuitionistic negation. We will follow this terminology and refrain from designating Johansson's negation as "minimal".

which is valid in Johansson's system, is not derivable from it. Therefore, we here speak of "j-negation" rather than "Johansson's negation" (though our argumentation holds for Johansson's negation as well). It can be shown that the characterizing rule $\mathcal{R}_j$ is not fully characterizing (cf. Theorem 6.12), that is, it is not equivalent to an inferential definition.

Even though it cannot be inferentially defined, we nevertheless want to talk about j-negation and its laws in a symbolic way. We want, for example, to say that $a, \neg_j a/\neg_j c$ holds for any c and that $a, \neg_j a/c$ does not hold for all c. By that we mean that from the characterizing rule $\mathcal{R}_j$ we can infer these claims. However, how can the notation "$\neg_j a$" be understood if there cannot be an inferential definition of $\neg_j$?

Consider an object language, in which for every sentence a there is at least one j-negation of a. Consider the law

$$a, \neg_j a/\neg_j c.$$

It can plausibly be read as follows: Suppose we have a fixed selection function (in short: "selection") which chooses, for every sentence a, one of the j-negations of a. Let $\neg_j$ denote this selected j-negation of a. Then this law holds for the given selection of j-negations. In fact, this law not only holds for a specific selection, but for any selection.

Generalizing this idea means that laws for not-fully-characterizable operations are interpreted with respect to certain selections, and that such a law is valid, if it holds for any selection. This interpretation can be used even for unique (fully characterized) operations such as classical negation $\neg_k$, with the only difference that there is only one possible selection (up to interdeducibility), which means that, when we (metalinguistically) define the validity of an inference, we need not quantify over selections. For example

$$a, \neg_k a/\neg_k c$$

is valid as it holds for *the* unique selection of classical negations, namely for *the* classical negation of a and *the* classical negation of c, whereas

$$a, \neg_j a/\neg_j c$$

is valid as it holds for every selection of j-negations of a and of c.

Elaborating on this idea, something that cannot be done here, we propose to read a connection $a \circ b$, be it unique or not, as an ϵ-term as used in the epsilon calculus of Hilbert and Bernays (1939), that is, $a \circ b$ is understood as $\epsilon c\, \mathcal{R}(c, a, b)$. This means that $a \circ b$ just selects one element from the connections of a and b. We do not have the problem of non-denoting terms, as with logical operations we always assume that they generate a result, even if this is by stipulation. As the availability of a connection for any two statements a and b is guaranteed, the use of $a \circ b$ and the instantiation of universally quantified formulas of the metalanguage with terms of the form $a \circ b$ is no longer problematic. Reading $a \circ b$ as an ϵ-term also makes the existential presupposition explicit in cases where $a \circ b$ is the result of an instantiation of a universal quantifier of the metalanguage. If a connection is unique, that is, if $\circ$ is a logical constant, then we have a definite description as a limiting case of the epsilon operator.[52]

Our discussion serves as a philosophical explication of Popper's idea that we can define a unique logical operation $\circ$ by an inferential definition and a corresponding full characterization $\mathcal{R}(a \circ b, a, b)$, and non-unique logical operations $\circ$ or $\neg_j$ by characterizations $\mathcal{R}(a \circ b, a, b)$ or $\mathcal{R}_j$ without there being an inferential definition. For the following more technical discussions and results this philosophical distinction does not matter very much: we can just start from the rule $\mathcal{R}(a \circ b, a, b)$ as a characterization of $\circ$, or from $\mathcal{R}_j$ as a characterization of $\neg_j$, be it unique (*fully* characterizing) or not. Uniqueness and many other metalogical features are then possible results of our investigations.

This interpretation gives a clear understanding of what is meant when throughout his articles Popper uses notations like $a \wedge b$, $a \vee b$ and $a > b$ to speak about conjunctions, disjunctions and implications, respectively, as well as various negation signs to speak about certain uniquely-characterized and certain not-fully-characterizable negations of some object language $\mathcal{L}$. It can also be used to rectify certain objections by reviewers of Popper's papers, notably Kleene (1948), and by later commentators such as Lejewski (1974, §§ 5–6) and Schroeder-Heister (2006, § 3.1); cf. § 2.

5.4 Popper's notion of duality

Although Popper makes frequent use of duality,[53] he nowhere gives an explanation of his notion of duality. We think such an explanation is needed, since Popper does not only discuss duality in the context of classical logic, where the well-known duality based on truth functions can be applied, but also in the context of non-classical logics such as intuitionistic logic, where a different notion of duality is called for.

We propose to understand his notion of duality as being based on his concept of relative demonstrability ($\vdash$). More precisely, an inferential definition is said to be dual to another inferential definition, if it results from exchanging all statements on the left side of $\vdash$ with the statements on its right side, that is, by transforming $a_1, \ldots, a_n \vdash b_1, \ldots, b_m$ into $b_1, \ldots, b_m \vdash a_1, \ldots, a_n$.[54] Note that it is in general not possible to invert the direction of the sign of derivability ($/$), for it allows multiple statements on the left but only a single statement on the right (for alternative versions cf. § 4.6). As already mentioned in § 4.1, the replacement of $/$ by $\vdash$ is allowed. One can therefore formulate definitions of logical constants in terms of relative demonstrability instead of deducibility. This allows us to make the duality of logical

[52] This is more complicated in a case like j-negation, where $\mathcal{R}_j$ does not have the form $\mathcal{R}_j(\neg_j a, a)$, that is, where we do not have a relational definition in the strict sense. One would have to say that $\neg_j a$ selects one of the j-negation-functions satisfying $\mathcal{R}_j$ and applies it to a.

[53] He speaks of "the dual of", "dual rules", "dual definitions" etc.

[54] Cf. Kapsner (2014, p. 76), who also mentions this possibility. There is, however, a difference between the semantical concept $\models$ that Kapsner considers and Popper's defined concept $\vdash$.

constants obvious.[55] In the case of binary connectives we have to swap the arguments to produce its dual.[56] We make this understanding of the notion of duality precise by using a duality function, defined as follows:

Definition 5.3 Let $\star$ be a unary connective and $\circ$ a binary connective. The *duality function* δ is defined by the following clauses, where Γ and Π are lists of statements, and Γ^δ (resp. Π^δ) is the application of δ to each member of Γ (resp. Π):

$$\begin{aligned} a^\delta &=_{\text{df}} a, \text{ for non-compound statements } a; \\ (\star a)^\delta &=_{\text{df}} \star^\delta a^\delta; \\ (a \circ b)^\delta &=_{\text{df}} b^\delta \circ^\delta a^\delta; \\ (\Gamma \vdash \Pi)^\delta &=_{\text{df}} \Pi^\delta \vdash \Gamma^\delta. \end{aligned}$$

There are no clauses for / and //. This does not restrict the range of applicability of δ, since / can always be replaced by $\vdash$ (cf. Lemma 4.5). In the following we show that the function δ maps definitions of logical constants to definitions of what Popper considers to be their duals. We do this for conjunction and disjunction, conditional and anti-conditional, and for modal connectives.

5.5 Conjunction and disjunction

As was already mentioned, Popper gives several characterizing rules for conjunction ($\wedge$). We choose the following definition:

(D$\wedge$) $$a /\!/ b \wedge c \;\leftrightarrow\; (d)(a \vdash d \;\leftrightarrow\; b, c \vdash d)$$

(C$\wedge$) $$b \wedge c \vdash d \;\leftrightarrow\; b, c \vdash d$$

If we apply our duality function δ to the characterizing rule (C$\wedge$), we obtain:

(C$\wedge$)$^\delta$ $$d \vdash c \wedge^\delta b \;\leftrightarrow\; d \vdash b, c$$

which is equivalent to Popper's definition of disjunction ($\vee$):[57]

[55] To our knowledge, Popper mentions this duality based on relative demonstrability only once, in Popper (1948a, p. 181): "For certain purposes – especially if we wish to emphasize the duality or symmetry between '$\vdash$' and '$\not\nearrow$' – the use of '$(\ldots) \vdash$' turns out to be preferable to that of '$\not\nearrow(\ldots)$'."

[56] Our definition of duality resembles Cohen's (1953b, p. 82): "We define the *dual* of a postulate of any Gentzen-type system as the result of writing each sequent $p_1, \ldots, p_m \Vdash q_1, \ldots, q_n$ as $q_n, q_{n-1}, \ldots, q_1 \Vdash p_m, p_{m-1}, \ldots, p_1$, of interchanging $K\,p\,q$ with $A\,q\,p$ (hence also interchanging $A\,p\,q$ with $K\,q\,p$), of interchanging $F\,p\,q$ with $G\,q\,p$ (hence also interchanging $G\,p\,q$ with $F\,q\,p$) [...], and of reversing the order of premises whenever the postulate is a rule of inference having two premises." Here K stands for conjunction, A for disjunction ("alternative"), F for the conditional, and G for the anti-conditional.

[57] Our definitions (D$\wedge$) and (D$\vee$) correspond to Popper's (1948a) rules (3.71) and (3.72), respectively.

(D$\vee$) $$a /\!/ b \vee c \leftrightarrow (d)(d \vdash a \leftrightarrow d \vdash b, c)$$

(C$\vee$) $$d \vdash b \vee c \leftrightarrow d \vdash b, c$$

We immediately obtain the following introduction and elimination rules for conjunction and disjunction:

Lemma 5.4 *The following rules for conjunction and disjunction hold:*

(1) $a \wedge b / a$	*(4)* $a / a \vee b$
(2) $a \wedge b / b$	*(5)* $b / a \vee b$
(3) $a, b / a \wedge b$	*(6)* $(c)((a/c \;\&\; b/c) \rightarrow a \vee b/c)$

Proof For (1) consider the substitution instance $a \wedge b \vdash a \leftrightarrow a, b \vdash a$ of (C$\wedge$); $a, b \vdash a$ holds by (Rg). Thus $a \wedge b \vdash a$. Rules (2)–(5) are shown analogously. For (6) consider the substitution instance $a \vee b \vdash a \vee b \leftrightarrow a \vee b \vdash a, b$ of (C$\vee$); $a \vee b \vdash a \vee b$ holds by (Rg). Thus $a \vee b \vdash a, b$. From definition (D$\vdash$3) we get $(c)((a/c \;\&\; b/c) \rightarrow a \vee b/c)$. □

Lemma 5.5 *Conjunction and disjunction can be introduced and eliminated within contexts, that is, the following equivalences hold:*

$$a_1, \ldots, a_n, b, c \vdash d \leftrightarrow a_1, \ldots, a_n, b \wedge c \vdash d;$$
$$a_1, \ldots, a_n \vdash b_1, \ldots, b_m, c, d \leftrightarrow a_1, \ldots, a_n \vdash b_1, \ldots, b_m, c \vee d.$$

Proof For the first equivalence consider the following two instances of (Tg):

$$\left\{\begin{array}{l} \quad a_1, \ldots, a_n, b \wedge c \vdash a_1 \\ \& \; a_1, \ldots, a_n, b \wedge c \vdash a_2 \\ \vdots \qquad\quad \vdots \\ \& \; a_1, \ldots, a_n, b \wedge c \vdash a_n \\ \& \; a_1, \ldots, a_n, b \wedge c \vdash b \\ \& \; a_1, \ldots, a_n, b \wedge c \vdash c \end{array}\right\} \rightarrow (a_1, \ldots, a_n, b, c \vdash d \rightarrow a_1, \ldots, a_n, b \wedge c \vdash d);$$

$$\left\{\begin{array}{l} \quad a_1, \ldots, a_n, b, c \vdash a_1 \\ \& \; a_1, \ldots, a_n, b, c \vdash a_2 \\ \vdots \qquad\quad \vdots \\ \& \; a_1, \ldots, a_n, b, c \vdash a_n \\ \& \; a_1, \ldots, a_n, b, c \vdash b \wedge c \end{array}\right\} \rightarrow (a_1, \ldots, a_n, b \wedge c \vdash d \rightarrow a_1, \ldots, a_n, b, c \vdash d).$$

For the second equivalence we first show the direction from left to right:

1. From $a_1, \ldots, a_n \vdash b_1, \ldots, b_m, c, d$ and two applications of (Cut) with $c/c \vee d$ and $d/c \vee d$ we obtain $a_1, \ldots, a_n \vdash b_1, \ldots, b_m, c \vee d, c \vee d$.
2. By (RC) we obtain $a_1, \ldots, a_n \vdash b_1, \ldots, b_m, c \vee d$.

For the direction from right to left:

1. Assume $a_1, \ldots, a_n \vdash b_1, \ldots, b_m, c \vee d$.
2. This is equivalent to $(e)((b_1/e \;\&\; \ldots \;\&\; b_m/e \;\&\; c \vee d/e) \rightarrow a_1, \ldots, a_n/e)$.
3. Assume b_1/e to b_m/e, c/e and d/e.

4. From c/e and d/e follows $c \vee d/e$.
5. From b_1/e to b_m/e and $c \vee d/e$ we get $a_1, \ldots, a_n/e$ from (2).

Thus $a_1, \ldots, a_n \vdash b_1, \ldots, b_m, c, d \;\leftrightarrow\; a_1, \ldots, a_n \vdash b_1, \ldots, b_m, c \vee d$. □

Lemma 5.6 *Conjunction and disjunction are logical constants, that is, their rules are fully characterizing.*

Proof For conjunction we have to show that

$$((d)(a_1 \vdash d \;\leftrightarrow\; b, c \vdash d) \;\&\; (d)(a_2 \vdash d \;\leftrightarrow\; b, c \vdash d)) \rightarrow a_1 /\!/ a_2$$

is true. Assuming the antecedent, we substitute a_2 for d in both its conjuncts to obtain $a_1 \vdash a_2 \;\leftrightarrow\; b, c \vdash a_2$ and $a_2 \vdash a_2 \;\leftrightarrow\; b, c \vdash a_2$. By (Rg) we obtain $b, c \vdash a_2$ from the second conjunct, and with $b, c \vdash a_2$ we obtain $a_1 \vdash a_2$ from the first conjunct. Likewise for $a_2 \vdash a_1$. The proof for disjunction is similar. □

5.6 Conditional and anti-conditional

Again, we choose those formulations of the characterizing rules that highlight the duality of the definitions. For the conditional (>) this definition is:

(D>) $$a /\!/ b > c \;\leftrightarrow\; (d)(d \vdash a \;\leftrightarrow\; d, b \vdash c)$$

(C>) $$d \vdash b > c \;\leftrightarrow\; d, b \vdash c$$

If we dualize (C>), we obtain:

$(\mathrm{C}>)^{\delta}$ $$c >^{\delta} b \vdash d \;\leftrightarrow\; c \vdash d, b$$

which is equivalent to Popper's definition of the anti-conditional ($\not>$):[58]

(D$\not>$) $$a /\!/ b \not> c \;\leftrightarrow\; (d)(a \vdash d \;\leftrightarrow\; c \vdash d, b)$$

(C$\not>$) $$c \not> b \vdash d \;\leftrightarrow\; c \vdash d, b$$

As an informal observation we may state that the conditional is characterized by the deduction theorem and the anti-conditional by the dual of the deduction theorem.

On the basis of these definitions we can show that modus ponens holds for the conditional, and that a dual to modus ponens holds for the anti-conditional.

Lemma 5.7 *The following rules hold:*

1. $b, b > c \vdash c$ *(modus ponens),*
2. $c \vdash c \not> b, b$ *(a dual rule to modus ponens).*

Proof (1) In (C>) let d be $b > c$ to obtain $b > c \vdash b > c \;\leftrightarrow\; b > c, b \vdash c$. By (Rg), $b > c, b \vdash c$ follows.

[58] Our definitions (D>) and (D$\not>$) correspond to Popper's (1948a) rules (3.81) and (3.82), respectively.

(2) In (C$\not>$) let d be $c \not> b$ to obtain $c \not> b \vdash c \not> b \;\leftrightarrow\; c \vdash c \not> b, b$. By (Rg), $c \vdash c \not> b, b$ follows. □

Lemma 5.8 *Conditional and anti-conditional are logical constants, that is, their rules are fully characterizing.*

Proof For the conditional we have to show that

$$((d)(d \vdash a_1 \;\leftrightarrow\; d, b \vdash c) \;\&\; (d)(d \vdash a_2 \;\leftrightarrow\; d, b \vdash c)) \rightarrow a_1 /\!/ a_2$$

is true. Assuming the antecedent, we instantiate d by a_1 in both its left and right conjunct. From the left one gets $a_1 \vdash a_1 \;\leftrightarrow\; a_1, b \vdash c$, and thus $a_1, b \vdash c$ by (Rg). From the right one gets $a_1 \vdash a_2 \;\leftrightarrow\; a_1, b \vdash c$, and thus $a_1 \vdash a_2$. Likewise for $a_2 \vdash a_1$. The proof for the anti-conditional is similar. □

Following Popper, we will use the terms “implication” and “conditional” interchangeably to refer either to the connective $>$ or to statements of the form $a > b$. In the modern discussion, the logical constant introduced and discussed by Popper as “anti-conditional” figures under the heading “co-implication” (cf. Wansing, 2008).

5.7 Characterizations of implication and Peirce’s rule

As is well known, if implication (i.e., the conditional $>$) is defined solely by the usual implication introduction and elimination rules of natural deduction, then the addition of classical negation by its natural deduction rules is a non-conservative extension of the logic. For example, Peirce’s law $((a > b) > a) > a$ is not derivable using only the introduction and elimination rules for implication, but becomes derivable if the rules of classical negation are added.[59]

Popper (1947c) already made this observation in the context of his characterizing rules. He calls *positive logic* that part of propositional logic obtained by the definitions of the logical constants excluding negation, and remarks in Popper (1947c, p. 215): “Positive logic as defined by these rules does not yet contain all valid rules of inferences in which no use is made of negation: there is a further region which we may call the ‘extended positive logic’ [. . .].” Kleene’s (1948) review (cf. this volume, § 13.12) of Popper (1947d) cites Popper’s observation and turns it into a critique of Popper’s claim to give explicit definitions of the logical constants: “If his definitions were ‘explicit,’ as he claims, it should make no difference whatsoever, in the case of a formula containing only ‘>’, whether or not the definition of classical negation has been stated.” But it is clearly not enough to just “state” the definition. It is the existence postulate which tells us that for every statement there exists a classical negation of it in the object language which permits the proof of the demonstrability of, for example, Peirce’s law to go through.

In order to make statements such as Peirce’s law deducible without invoking negation, Popper considers the use of the following additional rule for the conditional:

[59] Cf. Dummett (1991, p. 291f.); cf. also de Campos Sanz, Piecha, and Schroeder-Heister (2014).

$$a, b > c/b \leftrightarrow a/b \tag{4.2e}$$

Reading (4.2e) from left to right amounts to a characterization of implication by Peirce's rule[60], that is, a rule version of Peirce's law $((b > c) > b) > b$. Popper also mentions that in the presence of (4.2e), the rules for classical negation can be obtained from the definitions of intuitionistic negation.[61]

Popper thus had the following insights, which were far from trivial at the time: The addition of classical negation to a logic containing implication defined by its usual introduction and elimination rules forms a non-conservative extension. Alternatively, implication can be characterized by a stronger set of rules containing a version of Peirce's rule. If this stronger set of rules for implication is used, then intuitionistic and classical negation coincide. Whether Popper was the first to consider Peirce's rule is not completely clear. Seldin (2008, § 3) "think[s ...] that Popper and Curry thought of this rule independently."

Further remarks on implication

In later unpublished typescripts, entitled "A Note on the Classical Conditional" (this volume, Chapter 17), which were written at the beginning of 1952, Popper wants to show how to derive the truth table of classical implication from the generally accepted inference rules of implication (e.g., modus ponens), together with the definition of a valid inference as an inference which transmits truth from its premises to its conclusion. One can thus say that the inference rules are considered to be primary, and the truth tables are to be justified in terms of the generally accepted rules of inference, and are thus secondary. Popper furthermore discusses the difference between classical, strict and intuitionist implication, and makes the following remark on intuitionism (this volume, Chapter 17, Typescript 2, § 8):

> While contending that the truth table of the classical conditional does not, upon closer inspection, conflict with the usages of an "ordinary language", the tendencies and the consistently developed usages of an ordinary language, I am very ready to admit with the Intuitionists (Brouwer, Heyting) that "ordinary language" usages involve us into difficulties when problems of infinities are involved, and that we may have to sacrifice classical negation, with its characteristic truth table, and especially $\langle 6.5 \rangle$ [b is false if and only if $\neg b$ is true] (and the law of the excluded middle).

[60] Popper did not call his rule "Peirce's rule".

[61] Cf. Popper (1947c, p. 216): "Also we do not need, in the presence of rule 4.2e, the whole force of our rule of negation 4.6 [i.e., classical negation], but can obtain this rule as a secondary rule from some weaker rules [...] (from the rules of the so-called intuitionist logic)." Popper (1947c, p. 200) calls a rule *secondary* if it is derivable from a set of given so-called primary rules, that is, if "every inference which is asserted as valid by the secondary rule could be drawn merely by force of the primary rules alone."

5.8 Conjoint denial, alternative denial and further connectives

Popper (1947c, p. 219) defines some connectives in terms of connectives that have already been defined by characterizing rules, noting that these definitions are unnecessary in the sense that one can always use the characterizing rules directly instead. We mention the conjoint denial

(D↓) $$a/\!/a \downarrow b \;\leftrightarrow\; a/\!/\neg(a \vee b)$$

and the alternative denial

(D⅄) $$a/\!/a \curlywedge b \;\leftrightarrow\; a/\!/\neg(a \wedge b)$$

In a letter to Alonzo Church of 2 February 1948 (this volume, § 24.3) Popper provides characterizing rules for these two connectives as follows and notes their duality:

(C↓) $$a \downarrow b \vdash c \;\leftrightarrow\; \vdash a, b, c$$

(C⅄) $$a \vdash b \curlywedge c \;\leftrightarrow\; a, b, c \vdash$$

In a handwritten note Popper (n.d.[a]) defines negation, conjunction, disjunction, conditional, anti-conditional and bi-conditional both in terms of alternative denial and in terms of joint denial.

After introducing the bi-conditional

(D$\widehat{\equiv}$) $$a/\!/b \mathbin{\widehat{\equiv}} c \;\leftrightarrow\; (\text{for any } a_1\text{: } a_1/a \;\leftrightarrow\; a_1, c/b \;\&\; a_1, b/c)$$

Popper defines the exclusive disjunction as follows:

(D≠) $$a/\!/a \neq b \;\leftrightarrow\; a/\!/\neg(a \mathbin{\widehat{\equiv}} b)$$

Further connectives defined by using already defined logical constants are the tautology

(Dt) $$a/\!/t(b) \;\leftrightarrow\; a/\!/b \vee \neg b$$

and the contradiction

(Df) $$a/\!/f(b) \;\leftrightarrow\; a/\!/b \wedge \neg b$$

Since, for any a and b, $t(a)/\!/t(b)$ and $f(a)/\!/f(b)$ hold for these unary connectives, one can simply write t and f; that is, these connectives correspond to the nullary constants verum and falsum (cf. also the end of § 6.4).

6 Negations

Popper considered several different kinds of negation. We discuss their definitions and investigate how they relate to each other. These definitions are taken from Popper (1948c) unless indicated otherwise.[62] Some of the following results were only stated without proof by Popper, and for some he gave only proof sketches. We will provide more details and fill in the missing proofs.

6.1 Classical negation

For classical negation ($\neg_k$) Popper considers several definitions, among them the following two:[63]

(D$\neg_k$1) $$a /\!/ \neg_k b \leftrightarrow (a, b \vdash \ \& \ \vdash a, b)$$

(D$\neg_k$2) $$a /\!/ \neg_k b \leftrightarrow (c)(d)(d, a \vdash c \leftrightarrow d \vdash b, c)$$

with the following two characterizing rules:

(C$\neg_k$1) $$\neg_k b, b \vdash \ \& \ \vdash \neg_k b, b$$

(C$\neg_k$2) $$(c)(d)(d, \neg_k b \vdash c \leftrightarrow d \vdash b, c)$$

These two definitions reflect two different ways of characterizing classical negation. Definition (D$\neg_k$1) is based on the idea that the classical negation of a statement b is a statement which is at the same time complementary and contradictory to b, whereas (D$\neg_k$2) is very similar to the rules for negation used in classical sequent calculus.

Lemma 6.1 *The definitions* (D$\neg_k$1) *and* (D$\neg_k$2) *are equivalent.*

Proof We consider the characterizing rules, and show first that (C$\neg_k$1) follows from (C$\neg_k$2):

In $(c)(d)(d, \neg_k b \vdash c \leftrightarrow d \vdash b, c)$ let d be b to obtain $(c)(b, \neg_k b \vdash c \leftrightarrow b \vdash b, c)$. By (Rg) one obtains $(c)(b, \neg_k b \vdash c)$, that is, $b, \neg_k b \vdash$; hence $\neg_k b, b \vdash$ by (LE). In $(c)(d)(d, \neg_k b \vdash c \leftrightarrow d \vdash b, c)$ let c be $\neg_k b$ to obtain $(d)(d, \neg_k b \vdash \neg_k b \leftrightarrow d \vdash b, \neg_k b)$. By (Rg) one obtains $(d)(d \vdash b, \neg_k b)$, that is, $\vdash b, \neg_k b$; hence $\vdash \neg_k b, b$ by (RE).

To show that (C$\neg_k$2) follows from (C$\neg_k$1) it is sufficient to show that the two implications $\vdash \neg_k b, b \rightarrow (d, \neg_k b \vdash c \rightarrow d \vdash b, c)$ and $\neg_k b, b \vdash \rightarrow (d \vdash b, c \rightarrow d, \neg_k b \vdash c)$ hold. This can be done by using (Cut). To show the first implication we assume $\vdash \neg_k b, b$, from which we obtain $d \vdash \neg_k b, b$ by (LW). Assuming $d, \neg_k b \vdash c$ we get $d, d, b \vdash c$ by (Cut), from which we get $d, b \vdash c$ by (LC). The second implication is a direct instance of (Cut), where the cut formula is the statement b. □

[62] Popper's notation for negations varies; we write $\neg_k a$, for example, where Popper would, e.g., have written a^k.

[63] In Popper (1948c), (D$\neg_k$1) is (D4.3), and (D$\neg_k$2) is (4.31).

The use of (Cut) can be justified by using our definition (D$\vdash$3)[64] for relative demonstrability ($\vdash$) instead of Popper's definition (D$\vdash$2), or by assuming that the object language contains conjunction ($\wedge$), disjunction ($\vee$) and the conditional ($>$); cf. § 4.5.[65] We take the first option here, and presuppose our definition (D$\vdash$3).

Lemma 6.2 *Classical negation $\neg_k$ is self-dual, that is, $(\neg_k a)^\delta = \neg_k a$ for the duality function δ.*

Proof By applying the duality function δ to (D$\neg_k$1). □

6.2 Intuitionistic negation and dual-intuitionistic negation

The study of formalized intuitionistic logic started with Heyting's set of axioms.[66] Originally, Heyting's formalization was written in response to a prize question proposed by Mannoury in 1927, which also asked "to investigate whether from the system to be constructed [for intuitionistic logic] a dual system may be obtained by (formally) interchanging the *principium tertii exclusi* and the *principium contradictionis*."[67] Thus the idea of dualizing intuitionistic logic was present from the very beginning of its development.[68] The principium tertii exclusi is in Popper's theory mirrored by the property of a negation of b to be complementary to b, and the principium contradictionis is mirrored by the contradictoriness of b and the negation of b. Popper uses the term "minimum definable negation" when he writes about what we call dual-intuitionistic negation ($\neg_m$), but still mentions the fact that it forms some kind of dual to intuitionistic negation.[69] For intuitionistic negation ($\neg_i$), Popper gives the following definition and characterizing rule:[70]

(D$\neg_i$) $$a /\!/ \neg_i\, b \;\leftrightarrow\; (c)(c \vdash a \;\leftrightarrow\; c, b \vdash)$$

(C$\neg_i$) $$c \vdash \neg_i\, b \;\leftrightarrow\; c, b \vdash$$

If we dualize (D$\neg_i$), we get

(D$\neg_i$)$^\delta$ $$a /\!/ \neg_i{}^\delta\, b \;\leftrightarrow\; (c)(a \vdash c \;\leftrightarrow\; \vdash c, b)$$

[64] Cf. the discussion in § 4.5.

[65] Popper states that (D$\neg_k$1) and (D$\neg_k$2) are equivalent if the existence of either conjunction or disjunction is guaranteed for arbitrary statements.

[66] Cf. Heyting (1930).

[67] Quoted from Troelstra (1990, p. 4).

[68] Cf. Kapsner (2014, p. 128).

[69] Cf. Popper (1948c, p. 323). Cohen (1953b, p. 188) remarks that "[t]he negation functor in the dualintuitionistic restricted predicate calculus GL2 [developed by Cohen] has the same properties as Popper's 'minimum definable (non-modal) negation [$\neg_m$].'" To avoid misunderstandings, it should be emphasized that Popper's usage of the term "minimum negation" has nothing to do with Johansson's (1937) "minimal calculus" and its negation.

[70] Cf. Popper (1948c, def. (D4.1)).

which is identical to Popper's definition for dual-intuitionistic negation ($\neg_m$):[71]

(D$\neg_m$) $$a /\!/ \neg_m b \leftrightarrow (c)(a \vdash c \leftrightarrow \ \vdash c, b)$$

(C$\neg_m$) $$\neg_m b \vdash c \leftrightarrow \ \vdash c, b$$

Lemma 6.3 *For intuitionistic negation $b, \neg_i b \vdash$ holds, and for dual-intuitionistic negation $\vdash b, \neg_m b$ holds.*

Proof In (C$\neg_i$) let c be $\neg_i b$; by (Rg) and (LE) we get $b, \neg_i b \vdash$. In (C$\neg_m$) let c be $\neg_m b$; by (Rg) and (RE) we get $\vdash b, \neg_m b$. □

Lemma 6.4 *Intuitionistic negation $\neg_i$ and dual-intuitionistic negation $\neg_m$ are logical constants, that is, their rules are fully characterizing.*

Proof For intuitionistic negation we have to show that

$$((c)(c \vdash a_1 \leftrightarrow c, b \vdash) \ \& \ (c)(c \vdash a_2 \leftrightarrow c, b \vdash)) \rightarrow a_1 /\!/ a_2$$

is true. In both conjuncts let c be a_1 to obtain

$$a_1 \vdash a_1 \leftrightarrow a_1, b \vdash \quad \text{and} \quad a_1 \vdash a_2 \leftrightarrow a_1, b \vdash.$$

From the first conjunct we get $a_1, b \vdash$ by (Rg), and from $a_1, b \vdash$ and the second conjunct we obtain $a_1 \vdash a_2$. The proof of $a_2 \vdash a_1$ is similar.

For dual-intuitionistic negation we have to show that

$$((c)(a_1 \vdash c \leftrightarrow \ \vdash c, b) \ \& \ (c)(a_2 \vdash c \leftrightarrow \ \vdash c, b)) \rightarrow a_1 /\!/ a_2$$

is true. In both conjuncts let c be a_1 to obtain

$$a_1 \vdash a_1 \leftrightarrow \ \vdash a_1, b \quad \text{and} \quad a_2 \vdash a_1 \leftrightarrow \ \vdash a_1, b.$$

From the first conjunct we obtain $\vdash a_1, b$ by (Rg), and from $\vdash a_1, b$ and the second conjunct we get $a_2 \vdash a_1$. The proof of $a_1 \vdash a_2$ is similar. □

6.3 Non-conservative language extensions

Popper (1948c, § V) considers non-conservative language extensions, although without using these terms. Popper (1947b, p. 282, fn 20) observes that "if, in one language, a classical as well as an intuitionistic negation exists of every statement, then the latter becomes equivalent to the former, or in other words, classical negation then absorbs or assimilates its weaker kin."[72] Moreover, he observes that this does not happen if classical negation is put together with Johansson's negation (or with yet

[71] Cf. Popper (1948c, def. (D4.2)).

[72] Cf. also Popper's remark at the end of § 3 in Chapter 16 of this volume.

another negation, “the impossibility of b”, proposed by him ibid., p. 283). However, Johansson’s negation $\neg_j$ is not a logical constant in Popper’s sense (cf. Theorem 6.12).

An example of a non-conservative language extension is the addition of classical negation $\neg_k$ to a language containing both intuitionistic negation $\neg_i$ and dual-intuitionistic negation $\neg_m$ since this addition makes classical laws hold for the two weaker negations $\neg_i$ and $\neg_m$.

Theorem 6.5 *In the presence of $\neg_k$ we have $\neg_k a /\!/ \neg_i a$, $\neg_k a /\!/ \neg_m a$ and $\neg_i a /\!/ \neg_m a$. In other words, the three negations $\neg_k$, $\neg_i$ and $\neg_m$ collapse into a single one (i.e., they become synonymous).*

Proof Classical negation $\neg_k$ satisfies the rules for $\neg_i$ and $\neg_m$, that is, we have $a \vdash \neg_k b \;\leftrightarrow\; a, b \vdash$ and $\neg_k a \vdash b \;\leftrightarrow\; \vdash a, b$, respectively. Both equivalences are direct consequences of (C$\neg_k$1) and (C$\neg_k$2), by using (Cut). Since the rules for $\neg_i$ and $\neg_m$ are fully characterizing, we have $\neg_k a /\!/ \neg_i a$ and $\neg_k a /\!/ \neg_m a$. Hence also $\neg_i a /\!/ \neg_m a$, for any object language containing $\neg_k$, $\neg_i$ and $\neg_m$. □

Popper (1948c, p. 324) also considers the more general situation where two logical functions S_1 and S_2 have been introduced by sets of primitive rules R_1 and R_2, respectively, such that $R_2 \subset R_1$. If both S_1 and S_2 are definable, and S_1 is given, then one can show that S_1 and S_2 are equivalent.

This can be generalized further, since R_2 need not be a subset of R_1; it is sufficient that R_1 implies R_2. Consider the following setting with two fully characterizing rules $\mathcal{R}_1$ and $\mathcal{R}_2$ for two n-ary constants such that $\mathcal{R}_1$ implies $\mathcal{R}_2$:

$$\mathcal{R}_1(a, a_1, \ldots, a_n) \tag{1.1}$$

$$\mathcal{R}_2(a, a_1, \ldots, a_n) \tag{1.2}$$

$$\mathcal{R}_1(a, a_1, \ldots, a_n) \rightarrow \mathcal{R}_2(a, a_1, \ldots, a_n) \tag{1.3}$$

Now assume that $\mathcal{R}_2(b, a_1, \ldots, a_n)$ holds. We get $\mathcal{R}_2(a, a_1, \ldots, a_n)$ from (1.1) and (1.3), which implies $a /\!/ b$, since we have fully characterizing rules. Hence $\mathcal{R}_1(b, a_1, \ldots, a_n)$. The two characterized constants become thus synonymous; in other words, adding $\mathcal{R}_1$ yields a non-conservative extension of systems containing $\mathcal{R}_2$.

Popper’s treatment of conservativeness also throws some light on his logical approach in general. Based on the fact that Popper does not use conservativeness as a criterion for accepting characterizing rules, one could argue that Popper is not aiming at a semantic justification of logical theories, since from a semantic theory we expect that the introduction of a new constant is always a conservative extension; cf. § 1.5.

6.4 Six further kinds of negation

Popper (1948c, § VI) considers three further kinds of negation explicitly, namely $\neg_j$, $\neg_l$ and $\neg_n$. He mentions their duals in a footnote[73] but does not study them. The negation $\neg_n$ coincides with what is nowadays called *subminimal negation*[74].

Definition 6.6 The six negations $\neg_j$, $\neg_{dj}$, $\neg_l$, $\neg_{dl}$, $\neg_n$ and $\neg_{dn}$ are given by the following characterizing rules. We also indicate the respective duals. (The characterizing rules for $\neg_k$, $\neg_i$ and $\neg_m$ are repeated below for comparison.)

Negation	*Characterizing rule*	*Rule name*	*Dual negation*	*Rule in Popper (1948c)*
$\neg_j$	$a, b \vdash \neg_j c \rightarrow a, c \vdash \neg_j b$	$(\mathrm{C}\neg_j)$	$\neg_{dj}$	(6.1)
$\neg_{dj}$	$\neg_{dj} c \vdash a, b \rightarrow \neg_{dj} b \vdash a, c$	$(\mathrm{C}\neg_{dj})$	$\neg_j$	–
$\neg_l$	$a, \neg_l b \vdash c \rightarrow a, \neg_l c \vdash b$	$(\mathrm{C}\neg_l)$	$\neg_{dl}$	(6.2)
$\neg_{dl}$	$c \vdash a, \neg_{dl} b \rightarrow b \vdash a, \neg_{dl} c$	$(\mathrm{C}\neg_{dl})$	$\neg_l$	–
$\neg_n$	$a, b \vdash c \rightarrow a, \neg_n c \vdash \neg_n b$	$(\mathrm{C}\neg_n)$	$\neg_{dn}$	(6.3)
$\neg_{dn}$	$c \vdash a, b \rightarrow \neg_{dn} b \vdash a, \neg_{dn} c$	$(\mathrm{C}\neg_{dn})$	$\neg_n$	–
$\neg_k$	$a, \neg_k b \vdash c \leftrightarrow a \vdash b, c$	$(\mathrm{C}\neg_k 2)$	$\neg_k$	(4.32)
$\neg_i$	$a \vdash \neg_i b \leftrightarrow a, b \vdash$	$(\mathrm{C}\neg_i)$	$\neg_m$	(4.1)
$\neg_m$	$\neg_m a \vdash b \leftrightarrow \vdash a, b$	$(\mathrm{C}\neg_m)$	$\neg_i$	(4.2)

Popper (1948c, p. 328) explains his names for these negations as follows: "In view of 6.2 [cf. Definition 6.6], we may call [$\neg_l a$] the 'left-hand side negation of a' (in contradistinction to Johansson's [$\neg_j a$] which, in view of 6.1 [cf. Definition 6.6], is a 'right hand side negation'). [$\neg_n a$] may be called the 'neutral negation'; it is neutral with respect to right-sidedness and left-sidedness [. . .]." As mentioned above (cf. § 5.3), Popper is wrong in calling the j-negation $\neg_j$ "Johansson's negation".

The two rules $(\mathrm{C}\neg_i)$ and $(\mathrm{C}\neg_m)$ differ slightly from the other rules in that they have only two instead of three statements occurring in each relation of relative demonstrability. However, they can also be given the same form with three such statements, as the following lemma shows.

Lemma 6.7 *The rule* $(\mathrm{C}\neg_i)$ *is equivalent to*

$(\mathrm{C}\neg_i{}')$ $$a, c \vdash \neg_i b \leftrightarrow a, b, c \vdash$$

[73] Popper (1948c, p. 328, fn 16) writes: "There are, of course, dual rules of 6.1 [$\neg_j$], 6.2 [$\neg_l$] and 6.3 [$\neg_n$], two of which are satisfied by [$\neg_m$], just as 6.1 [$\neg_j$] and 6.3 [$\neg_n$] are satisfied by [$\neg_i$]." (In Curry (1949, this volume, § 13.9), $\neg_l$ is mistakenly considered to be the dual of $\neg_j$, which it is not in the sense used by Popper.)

[74] Cf., e.g., Dunn (1999) and the references therein.

and the rule (C$\neg_m$) *is equivalent to*

(C$\neg_m{}'$) $$\neg_m a \vdash b, c \;\leftrightarrow\; \vdash a, b, c.$$

Proof Rule (C$\neg_i$) follows from (C$\neg_i{}'$) by substituting a for c and applications of (LC). Rule (C$\neg_m$) follows from (C$\neg_m{}'$) by substituting b for c and applications of (RC).

Next we show that (C$\neg_m{}'$) can be obtained from (C$\neg_m$). For the direction from right to left, assume $\neg_m a \vdash b, c$ and use (Cut) with $\vdash a, \neg_m a$, which holds by Lemma 6.3.

For the direction from left to right, we first show that $\neg_m a \vdash a, b \rightarrow \neg_m a \vdash b$ holds:

1. Assume $\neg_m a \vdash a, b$.
2. Use (Cut) with $\vdash a, \neg_m a$ (Lemma 6.3) to obtain $\vdash a, a, b$, from which $\vdash a, b$ follows.
3. Applying (C $\neg_m$), $\neg_m a \vdash b$ follows.

We now show that $\vdash a, b, c$ implies $\neg_m a \vdash b, c$.

1. Assume $\vdash a, b, c$.
2. By using definition (D$\vdash$3) and by applying universal instantiation twice, we obtain $(a/d \;\&\; b/d \;\&\; c/d) \rightarrow \neg_m a/d$.
3. Assume c/d to obtain $(a/d \;\&\; b/d) \rightarrow \neg_m a/d$.
4. Using an instance of $\neg_m a \vdash a, b \rightarrow \neg_m a \vdash b$, we obtain $b/d \rightarrow \neg_m a/d$.
5. Reintroduce c/d to obtain $(c/d \;\&\; b/d) \rightarrow \neg_m a/d$.
6. By universal quantification and application of (D$\vdash$3) we get $\neg_m a \vdash b, c$.

To show that (C$\neg_i{}'$) can be obtained from (C$\neg_i$), we first apply the duality function δ to (C$\neg_i$), which yields (C$\neg_m$). An application of δ to the equivalent (C$\neg_m{}'$) yields (C$\neg_i{}'$). □

In the following, we show how all these negations relate to each other.

Theorem 6.8 *The following statements hold for the characterizing rules given in Definition 6.6:*

(1) $\neg_k$ satisfies the rule for $\neg_i$.
(2) $\neg_i$ satisfies the rule for $\neg_j$.
(3) $\neg_j$ satisfies the rule for $\neg_n$.
(4) $\neg_k$ satisfies the rule for $\neg_l$.
(5) $\neg_l$ satisfies the rule for $\neg_n$.
(6) $\neg_k$ satisfies the rule for $\neg_m$.
(7) $\neg_m$ satisfies the rule for $\neg_{dj}$.
(8) $\neg_{dj}$ satisfies the rule for $\neg_{dn}$.
(9) $\neg_k$ satisfies the rule for $\neg_{dl}$.
(10) $\neg_{dl}$ satisfies the rule for $\neg_{dn}$.

Proof We show (7), (8), (9) and (10). The structural rules (LE) and (RE) will be used tacitly.

(7) We have to show $\neg_m c \vdash a, b \;\rightarrow\; \neg_m b \vdash a, c$, presupposing (C$\neg_m$). By Lemma 6.7 we can use (C$\neg_m{}'$) equivalently.

a. Assume $\neg_m c \vdash a, b$.
b. Therefore $\vdash c, a, b$, by (C$\neg_m{}'$).
c. Therefore $\neg_m b \vdash a, c$, again by (C$\neg_m{}'$).

(8) We have to show $c \vdash a, b \rightarrow \neg_{dj} b \vdash a, \neg_{dj} c$, presupposing (C$\neg_{dj}$).

a. Assume $c \vdash a, b$.
b. We have $\neg_{dj} c \vdash a, \neg_{dj} c \rightarrow \neg_{dj} \neg_{dj} c \vdash a, c$, as an instance of (C$\neg_{dj}$).
c. We have $\neg_{dj} c \vdash a, \neg_{dj} c$, by (Rg) and (RW).
d. Therefore $\neg_{dj} \neg_{dj} c \vdash a, c$, from (b) and (c) by modus ponens.
e. Therefore $\neg_{dj} \neg_{dj} c \vdash a, b$, from (a) and (d) by (Cut) and (RC).
f. We have $\neg_{dj} \neg_{dj} c \vdash a, b \rightarrow \neg_{dj} b \vdash a, \neg_{dj} c$, as an instance of (C$\neg_{dj}$).
g. Therefore $\neg_{dj} b \vdash a, \neg_{dj} c$, from (e) and (f) by modus ponens.

(9) We have to show $c \vdash a, \neg_k b \rightarrow b \vdash a \neg_k c$, presupposing (C$\neg_k$).

a. Assume $c \vdash a, \neg_k b$.
b. We have $\neg_k b, b \vdash$ and $\vdash c, \neg_k c$, by (C$\neg_k$).
c. Therefore $b, c \vdash a$, from (a) and (b) by (Cut).
d. Therefore $\vdash c, \neg_k c$, from (b) and (c) by (Cut).

(10) We have to show $c \vdash a, b \rightarrow \neg_{dl} b \vdash a, \neg_{dl} c$, presupposing (C$\neg_{dl}$).

a. Assume $c \vdash a, b$.
b. We have $\neg_{dl} b \vdash a, \neg_{dl} b \rightarrow b \vdash a, \neg_{dl} \neg_{dl} b$, as an instance of (C$\neg_{dl}$).
c. We have $\neg_{dl} b \vdash a, \neg_{dl} b$, by (Rg) and (RW).
d. Therefore $b \vdash a, \neg_{dl} \neg_{dl} b$, from (b) and (c) by modus ponens.
e. Therefore $c \vdash a, \neg_{dl} \neg_{dl} b$, from (a) and (d) by (Cut) and (RC).
f. We have $c \vdash a, \neg_{dl} \neg_{dl} b \rightarrow \neg_{dl} b \vdash a, \neg_{dl} c$, as an instance of (C$\neg_{dl}$).
g. Therefore $\neg_{dl} b \vdash a, \neg_{dl} c$, from (e) and (f) by modus ponens.

Statements (2)–(5) can be shown analogously, and were already presented by Popper (1948c, p. 328) without proof. For statements (1) and (6) cf. Theorem 6.5. □

Corollary 6.9 *By transitivity we have in addition that $\neg_k$ satisfies the rules for $\neg_j$, $\neg_n$, $\neg_{dn}$ and $\neg_{dj}$; $\neg_i$ satisfies the rule for $\neg_n$, and $\neg_m$ satisfies the rule for $\neg_{dn}$.*

Theorem 6.10 *We have the following dualities: $(\neg_k, \neg_k)$, $(\neg_i, \neg_m)$, $(\neg_j, \neg_{dj})$, $(\neg_l, \neg_{dl})$ and $(\neg_n, \neg_{dn})$.*

Proof The self-duality of $\neg_k$ is given by Lemma 6.2. For the duality between $\neg_i$ and $\neg_m$ cf. § 6.2. The dualities $(\neg_j, \neg_{dj})$, $(\neg_l, \neg_{dl})$ and $(\neg_n, \neg_{dn})$ can be shown by applications of the duality function δ. □

Our results on negations are summarized in Figure 6.1.[75]

In contradistinction to classical, intuitionistic and dual-intuitionistic negation, the six negations $\neg_j$, $\neg_{dj}$, $\neg_l$, $\neg_{dl}$, $\neg_n$ and $\neg_{dn}$ are not given by fully characterizing rules. Hence, these negations cannot be considered as logical constants.[76] This is especially

[75] That $\neg_m$ does not satisfy $\neg_n$ was already observed by Popper (1948c, p. 328).

[76] The idea that certain negations could be too weak to be still considered as logical constants is already present in Popper (1943, p. 50): "Systems containing the operation of *negation* may be so much weakened that contradictoriness [i.e., the derivability of any two formulas such that one is the negation of the other] only implies n-embracingness [*i.e.* we can prove that any *negation of any formula whatsoever* can be derived (ibid., p. 49)]. It appears, however, that we cannot weaken them further without depriving *negation* of the character of a logical operation."

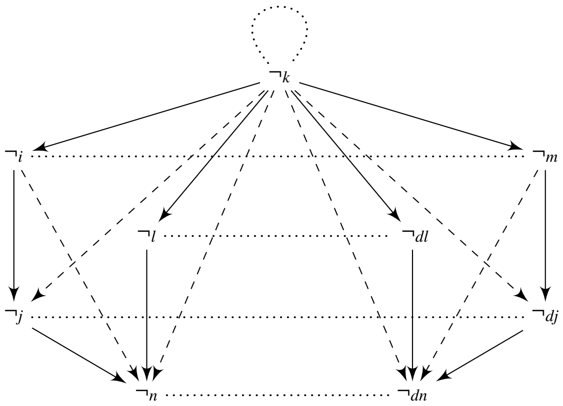

Fig. 6.1 Results on negations. The dotted lines connect those negations that are dual in the sense of Popper, with $\neg_k$ being self-dual, and the arrows show which negations satisfy the rules of which other negations; the solid arrows are due to Theorem 6.8, and the dashed arrows are due to Corollary 6.9. The diagram is complete; no further (non-trivial) relations hold.

interesting in the case of Johansson's negation $\neg_j$. In order to show this, Popper first introduces the two logical connectives t and f:

(D t) $$a /\!/ t(b) \;\leftrightarrow\; (c)(b/a \;\leftrightarrow\; c/a)$$

(C t) $$(c)(b/t(b) \;\leftrightarrow\; c/t(b))$$

(D f) $$a /\!/ f(b) \;\leftrightarrow\; (c)(a/b \;\leftrightarrow\; a/c)$$

(C f) $$(c)(f(b)/b \;\leftrightarrow\; f(b)/c)$$

For t and f, the following lemma holds.

Lemma 6.11 *For all statements b:* $\vdash t(b)$ *and* $f(b) \vdash$.

Proof In (C t) let c be $t(b)$ in order to obtain $(b/t(b) \;\leftrightarrow\; t(b)/t(b))$, from which one can obtain $b \vdash t(b)$. From $b/t(b)$ and (C t) one immediately obtains $(c)(c/t(b))$, and thus $\vdash t(b)$. Similarly for $f(b) \vdash$. □

This shows that t is a unary verum and that f is a unary falsum.

Theorem 6.12 *None of the rules for* $\neg_j$, $\neg_{dj}$, $\neg_l$, $\neg_{dl}$, $\neg_n$ *and* $\neg_{dn}$ *are fully characterizing.*

Proof The verum t satisfies the rules for $\neg_j$, $\neg_n$, $\neg_{dl}$ and $\neg_{dn}$. The falsum f satisfies the rules for $\neg_l$, $\neg_n$, $\neg_{dj}$ and $\neg_{dn}$. Classical negation $\neg_k$ satisfies the rules for $\neg_j$, $\neg_{dj}$, $\neg_l$, $\neg_{dl}$, $\neg_n$ and $\neg_{dn}$. If the rules for the six latter negations were each fully characterizing, then we would have for all a that $\neg_k a /\!/ t(a)$ or $\neg_k a /\!/ f(a)$, depending on the negation considered. But both $\neg_k a /\!/ t(a)$ and $\neg_k a /\!/ f(a)$ can only hold for contradictory object languages. □

7 Modal logic

Modal logic is introduced in Popper (1947d, § IX). Popper uses modal connectives in his proof of the compatibility of intuitionistic and dual-intuitionistic logic, which will be dealt with in § 8.

Popper considers the following six modal connectives: *necessary*, *impossible*, *logical*, *contingent*, *possible* and *uncertain*. They are taken from Carnap (1947, p. 175), and the definitions given for them by Popper are strongly influenced by Carnap's treatment of modality. Popper's definitions and characterizing rules for the modal connectives all have the following form:

(DM) $$a /\!/ M\,b \;\leftrightarrow\; (\vdash a \bigvee \urcorner a)\ \&\ \mathcal{R}(a, b)$$

(CM) $$(\vdash M\,b \bigvee \urcorner M\,b)\ \&\ \mathcal{R}(M\,b, b)$$

where M stands for any of the six modal connectives, and $\mathcal{R}(a, b)$ varies depending on the modal connective to be defined.

Table 1.3 combines the table given by Carnap (1947, p. 175)[77] and the list of definitions given by Popper (1947d, p. 1223). Carnap's half of the table contains

Table 1.3 The modal connectives from Carnap and Popper.

Carnap			*Popper*		
Name	$\Box$, $\Diamond$-*Definition*	*Semantic property*	*Sign*	*Name*	$\mathcal{R}(M\,b, b)$
Necessary	$\Box p$ $\neg\Diamond\neg p$	L-true	N	Necessary	$\vdash N\,b \leftrightarrow\ \vdash b$
Impossible	$\Box\neg p$ $\neg\Diamond p$	L-false	I	Impossible	$\vdash I\,b \leftrightarrow \urcorner b$
Contingent	$\neg\Box p \wedge \neg\Box\neg p$ $\Diamond\neg p \wedge \Diamond p$	factual	C	Contingent	$\urcorner C\,b \leftrightarrow (\vdash b \bigvee \urcorner b)$
Non-necessary	$\neg\Box p$ $\Diamond\neg p$	not L-true	U	Uncertain	$\urcorner U\,b \leftrightarrow\ \vdash b$
Possible	$\neg\Box\neg p$ $\Diamond p$	not L-false	P	Possible	$\urcorner P\,b \leftrightarrow \urcorner b$
Noncontingent	$\Box p \vee \Box\neg p$ $\neg\Diamond\neg p \vee \neg\Diamond p$	L-determinate	L	Logical	$\vdash L\,b \leftrightarrow (\vdash b \bigvee \urcorner b)$

his names for the modal connectives, their definition in terms of necessity $\Box$ and possibility $\Diamond$ and the semantic property to which they correspond. In Carnap's system every statement (or sentence, to use Carnap's terminology) falls into one of three disjoint categories. It can either be L-true, L-false or factual. A statement is called

[77] We have replaced N by $\Box$ here.

L-determinate, if it is either L-true or L-false. If a statement is not L-true, then it is either L-false or factual and so on. Popper's half of the table contains his names for the modal connectives, the corresponding symbol and the part $\mathcal{R}(M\,b, b)$ of the definition that varies with the modal connectives.

If L-truth is matched with demonstrability and L-falsity with refutability, then the close correspondence between the semantic properties given by Carnap's and Popper's definitions becomes obvious. For example, in the case of a noncontingent (Carnap's terminology) or logical (Popper's terminology) statement, the semantic property of being L-determinate corresponds to the property of being either demonstrable or refutable according to Popper's definition.

We observe that the part $\vdash M\,b\ \bigvee\ \urcorner\!/\, M\,b$ in Popper's definitions (C*M*) of the modal connectives *M* corresponds to the fact that in Carnap's system all modal statements (i.e., statements whose outermost logical constant is a modal constant) are L-determinate.[78] To express this determinateness is the only situation where Popper uses (metalinguistic) disjunction in the defining conditions of logical operators. And even here it plays only the role of a necessary side condition rather than something that enters the content of the rule $\mathcal{R}(M\,b, b)$. A more modern treatment of modal logic within the Popperian framework but not tied to Carnap's exposition might well do without disjunction in defining conditions in line with other logical operators.

Carnap's modal logic is S5, which can be axiomatized by the axioms K, T and 5. The same is the case for Popper's modal logic, as the following theorem shows.

Theorem 7.1 *From the definitions* (D*N*) *and* (D>) *we can prove the S5 axioms K, T and 5.*

Proof We consider axiom 5, that is, $\vdash N\,b > N\,N\,b$, which is shown as follows (K and T can be shown similarly):

1. $\vdash N\,N\,b \ \leftrightarrow\ \vdash N\,b$, from (D*N*).
2. $\vdash N\,b\ \bigvee\ \urcorner\!/\, N\,b$, from (D*N*).
3. If $\vdash N\,b$, then $\vdash N\,N\,b$, from (1).
4. If $\vdash N\,N\,b$, then $N\,b \vdash N\,N\,b$, by (LW).
5. If $\urcorner\!/\, N\,b$, then $(c)\,N\,b \vdash c$, hence $N\,b \vdash N\,N\,b$, by instantiating c by $N\,N\,b$.
6. Therefore $N\,b \vdash N\,N\,b$, from (2), (3) and (5).
7. Therefore $\vdash N\,b > N\,N\,b$, from (6) and (D>).

The proof can be generalized for all modal connectives *M* defined by Popper, that is, $\vdash M\,b > N\,M\,b$ holds for any *M*. □

The following result will be important in § 8, where it is used to show that the interpretation of intuitionistic negation by *I* and of dual-intuitionistic negation by *U* works.

Theorem 7.2 *Uncertainty U and impossibility I are dual modal notions.*

Proof By applications of the duality function δ (Definition 5.3). □

Each of the considered modal connectives is a logical constant in Popper's sense. We show this for *N* as an example:

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[78] Carnap (1947, p. 174) mentions this explicitly for necessity.

Lemma 7.3 *N is a logical constant, that is, its rule is fully characterizing.*

Proof We have to show that the following is true:

$$((\vdash a_1 \bigvee \nearrow a_1) \;\&\; (\vdash a_1 \;\leftrightarrow\; \vdash b) \;\&\; (\vdash a_2 \bigvee \nearrow a_2) \;\&\; (\vdash a_2 \;\leftrightarrow\; \vdash b)) \rightarrow a_1/\!/a_2.$$

Assume $\vdash a_1$. From $\vdash a_1 \leftrightarrow \vdash b$ we get $\vdash b$. From $\vdash b$ and $\vdash a_2 \leftrightarrow \vdash b$ we get $\vdash a_2$. From $\vdash a_2$ we get $a_1 \vdash a_2$. Assume $\nearrow a_1$. Hence $(c)(a_1/c)$, and by instantiating c by a_2 we get a_1/a_2. Therefore $a_1 \vdash a_2$. The proof of a_2/a_1 is similar. □

8 Bi-intuitionistic logic

The logical constants of intuitionistic and dual-intuitionistic negation are compatible in the sense that there is a logic containing both, without them collapsing into classical negation. A proof of this result was sketched by Popper (1948c, § V). We give a full proof in what follows. It consists in the exposition of the logic $\mathcal{L}_1$, for which we show that it has the following, desired properties:

1. It satisfies the rules of Basis I.
2. It contains for any two statements a and b also $a \wedge b$, $a \vee b$, $a > b$, $a \ngtr b$, $I\,a$ and $U\,a$.
3. It contains for every statement a its intuitionistic negation $\neg_i\, a$ and its dual-intuitionistic negation $\neg_m\, a$.
4. It satisfies all the inferential definitions of the logical connectives it contains.
5. Both $\neg_i\, a/\!/I\,a$ and $\neg_m\, a/\!/U\,a$ holds.
6. The duals $I\,a$ and $U\,a$ (cf. Theorem 7.2) do not collapse into each other.

Definition 8.1 The *logic* $\mathcal{L}_1$ is three-valued with truth-values d, c and r.[79] It contains a statement s with constant truth-value c. The truth-values of the compound statements of $\mathcal{L}_1$ and of s are given by the truth-tables in Table 1.4; for completeness, we add the truth-tables for N, P, L and C, which are not included in Popper (1948c).

A deducibility relation $a_1, \ldots, a_n/b$ is $\mathcal{L}_1$*-valid*, if the following conditions hold:

1. If $a_1 \wedge \ldots \wedge a_n$ has truth-value d, then b must have truth-value d as well.
2. If $a_1 \wedge \ldots \wedge a_n$ has truth-value c, then b must have either truth-value c or truth-value d.
3. If $a_1 \wedge \ldots \wedge a_n$ has truth-value r, then b can have any truth-value.

The truth-values d, c and r of $\mathcal{L}_1$ reflect Carnap's tripartition into demonstrable, factual (contingent), and refutable statements, respectively. That is, if d is read as L-true, c as factual in the sense of Carnap, and r as L-false, then the given truth-tables for $\mathcal{L}_1$ are an adequate semantics for this reading.

Lemma 8.2 $\mathcal{L}_1$ *satisfies the rules of Basis I.*

Proof That $\mathcal{L}_1$ satisfies (Rg) and (Tg) is a direct consequence of the definition of $\mathcal{L}_1$-validity. □

[79] Popper (1948c) uses the truth-values 1, 2 and 3, which are here replaced by d, c and r, respectively.

Table 1.4 The truth-tables for the logic $\mathcal{L}_1$ and for N, P, L and C.

a	b	$a \wedge b$	$a \vee b$	$a > b$	$a \not> b$
d	d	d	d	d	r
d	c	c	d	c	d
d	r	r	d	r	d
c	d	c	d	d	r
c	c	c	c	d	r
c	r	r	c	r	c
r	d	r	d	d	r
r	c	r	c	d	r
r	r	r	r	d	r

s
c

a	$I\,a$	$U\,a$
d	r	r
c	r	d
r	d	d

a	$N\,a$	$P\,a$	$L\,a$	$C\,a$
d	d	d	d	r
c	r	d	r	d
r	r	r	d	r

Lemma 8.3 *The following propositions are true:*

1. *A statement of $\mathcal{L}_1$ is demonstrable if, and only if, it has the value* d *for all valuations.*
2. *A demonstrable statement exists in $\mathcal{L}_1$.*
3. *A statement is refutable if, and only if, it has the value* r *for all valuations.*
4. *A refutable statement exists in $\mathcal{L}_1$.*
5. *It is $a /\!/ b$ if, and only if, the value of a is identical with the value of b.*

Proof (1), (3) and (5) follow directly from the definitions of demonstrability and refutability (cf. § 4.2 and § 4.3), together with the interpretation of deducibility in $\mathcal{L}_1$. A witness for (2) is the demonstrable statement $a > a$. A witness for (4) is the refutable statement $a \not> a$. □

Lemma 8.4 *The logical constants in $\mathcal{L}_1$ satisfy their respective inferential definitions* (D$\wedge$), (D$\vee$), (D$>$), (D$\not>$), (DI) *and* (DU).

Proof This is only shown for U, the other cases are analogous. The characterizing rule for U is:

$$(\vdash U\,b \bigvee \,\urcorner\, U\,b) \;\&\; (\urcorner\, U\,b \;\leftrightarrow\; \vdash b)$$

That the left conjunct is satisfied can be seen by looking at the truth-table for U, which contains only either d or r. If $U\,a$ has the truth-value d, then it is demonstrable, and if it has the truth-value r, then it is refutable.

For the right conjunct, we consider first the part $\urcorner\, U\,b \;\rightarrow\; \vdash b$, which is also satisfied: If $U\,b$ is refutable, then it has the value r, and if it has the value r, then b must have the value d, thus being demonstrable. The remaining part $\vdash b \rightarrow \urcorner\, U\,b$ is also satisfied: If b is demonstrable, then it has the value d; hence $U\,b$ has the value r, thus being refutable. □

Lemma 8.5 *$I\,a$ and $U\,a$ do not collapse in $\mathcal{L}_1$.*

Proof This is guaranteed by the existence of the statement s with truth-value c. The value of $U\,s$ is d, and the value of $I\,s$ is r. Therefore it cannot be the case that $U\,s /\!/ I\,s$, by Lemma 8.3. □

Lemma 8.6 *From the definitions* (D$\neg_i$), (D$\neg_m$), (D$>$), (D$\not>$), (DI) *and* (DU) *we can show that* $\neg_i\, a /\!/ a > I\, a$ *and* $\neg_m\, a /\!/ U\, a \not> a$.

Proof For the proof of $\neg_i\, a /\!/ a > I\, a$ we have to show that $(c)(c \vdash b > I\, b \rightarrow c, b \vdash)$ and $(c)(c, b \vdash \rightarrow c \vdash b > I\, b)$, which is just an instance of (D$\neg_i$). The latter is shown as follows:

1. Assume $c, b \vdash$.
2. Therefore $c, b \vdash I\, b$, by (RW).
3. Therefore $c \vdash b > I\, b$, by (C$>$).
4. Therefore $(c)(c, b \vdash \; \rightarrow c \vdash b > I\, b)$.

The proof of $(c)(c \vdash b > I\, b \rightarrow c, b \vdash)$ is:

1. Assume $c \vdash b > I\, b$.
2. Therefore $c, b \vdash I\, b$, by (C$>$).
3. $\vdash I\, b \bigvee I\, b \vdash$, by (C$I$). We argue by cases.
4. Assume $I\, b \vdash$.
5. Therefore $c, b \vdash$, by (2), (4) and (Tg).
6. Assume $\vdash I\, b$.
7. Therefore $b \vdash$, by (CI).
8. Therefore $c, b \vdash$, by (LW).
9. Therefore $c, b \vdash$, by (3), (5) and (8).
10. Therefore $(c)(c \vdash b > I\, b \rightarrow c, b \vdash)$.

Analogously for the proof of $\neg_m\, a /\!/ U\, a \not> a$. □

Lemma 8.7 *In* $\mathcal{L}_1$ *there is for every statement* a *an intuitionistic as well as a dual-intuitionistic negation of* a. *For* $\neg_i\, a$ *we have* $\neg_i\, a /\!/ I\, a$, *and for* $\neg_m\, a$ *we have* $\neg_m\, a /\!/ U\, a$.

Proof We have $a > I\, a /\!/ I\, a$ and $U\, a \not> a /\!/ U\, a$ in $\mathcal{L}_1$. This can be checked by constructing the respective truth-tables. Using Lemma 8.6, we obtain $I\, a /\!/ \neg_i\, a$ and $U\, a /\!/ \neg_m\, a$. The modal statements $I\, a$ and $U\, a$ exist for any statement in $\mathcal{L}_1$. Therefore intuitionistic and dual-intuitionistic negations exist for any statement in $\mathcal{L}_1$. □

Theorem 8.8 *If a logic contains for any statement* a *also its intuitionistic negation* $\neg_i\, a$ *and its dual-intuitionistic negation* $\neg_m\, a$, *then these two negations do not (necessarily) collapse, that is, we do not have* $\neg_i\, a /\!/ \neg_m\, a$.

Proof The logic $\mathcal{L}_1$ is such a logic. □

Popper (1948c) thus showed that there exists a bi-intuitionistic logic. The logic $\mathcal{L}_1$ might not be very interesting in itself. Nevertheless, it is at least interesting from a historical point of view, since it is perhaps the first example of a bi-intuitionistic logic to be found in the literature after Moisil (1942), which Popper almost certainly did not have access to.[80] Moreover, it shows that already Popper had the idea of combining different logics, a topic that today is receiving considerable attention (cf. Carnielli and Coniglio, 2020).

[80] The modern discussion of bi-intuitionistic logic was initiated by Rauszer (1974).

9 The theory of quantification

Popper extended his framework of inferential definitions to a theory of quantification at the beginning of 1947. In a letter to Paul Bernays of 19 October 1947 (this volume, § 21.6), he wrote:

> The first important result which I had finished about one week after I saw you, was the extension of the method of $a/b \wedge c \leftrightarrow a/b \;\&\; a/c$ to quantification.

The meeting that Popper refers to probably took place in Zürich on 11 or 12 April 1947[81], where Popper met Bernays in order to discuss the possibility of publishing a joint article on logic. The manuscript (this volume, Chapter 14) for this unpublished article does not have a title; in a letter to Bernays of 3 March 1947 (this volume, § 21.3), Popper suggests the title "On Systems of Rules of Inference", noting that "[t]he title is not very good, but so far I could not think of a better one".

Although they did not publish this manuscript, Popper's results found their way into several of his published articles. The most extensive discussion of these results can be found in Popper (1947c, § 7 and § 8). Additionally, there is an important footnote in Popper (1948c), an alternative axiomatization in Popper (1947d), and a very short but clear summary of his treatment of quantification in Popper (1949a). We follow the presentation of Popper (1947c) but refer to some modifications which can be found in his other articles. Some modifications of his view on quantification were only discussed in hitherto unpublished correspondence, which we will discuss in § 9.4.

Popper's theory of quantification underwent significant modifications over the course of his published articles, subsequent corrections to those articles, and in unpublished correspondence with other logicians. We present what we consider to be his most mature view on these matters, taking unpublished material into account.

Popper uses the terms "theory of quantification" or "quantification theory" instead of "first-order logic". At first, he extends his concept of object language to include open statements and his deducibility relation to range over open statements. He then adds a substitution operation which replaces free variables by other free variables, and gives rules and postulates which characterize this substitution operation. We discuss his definitions of the auxiliary concepts of identity and non-free-occurrence of a variable in a statement and, finally, his definitions of the quantifiers.

9.1 Formulas, name-variables and substitution

As explained in § 3, for propositional logic Popper considers pairs

[81] Bernays writes to Popper: "[. . .] nothing stands, as far as I can see, in the way of us seeing each other on the 11th of April in Zürich; I will certainly also be available in the midmorning of the 12th. I am looking forward to the receipt of the concept you promised me, – also with regards to the possible joint publication." (This volume, § 21.4.)

$$(\mathcal{L}; a_1, \ldots, a_n/b)$$

of an object language $\mathcal{L}$ and a deducibility relation $/$, axiomatized by a basis consisting of the rules (Rg) and (Tg). Each element of the object language $\mathcal{L}$ is presumed to be a statement, that is, something which has a truth value.

The first modification Popper makes in order to treat quantification is to consider quadruples

$$(\mathcal{L}; \mathcal{P}; a_1, \ldots, a_n/b; a\tbinom{x}{y})$$

consisting of a set $\mathcal{L}$ of *formulas*, a set $\mathcal{P}$ of *name-variables* (or pronouns), a *deducibility relation* on $\mathcal{L}$ and a *substitution operation*

$$a\tbinom{x}{y}$$

which substitutes the name-variable y for the name-variable x in the formula a. Variables $a, b, \ldots$ now range over *formulas* in $\mathcal{L}$, and variables $x, y, \ldots$ range over name-variables in $\mathcal{P}$.

Formulas can either be *open statements* (also called *statement-functions*) or *closed statements* (also called *statements*):

$$\begin{tikzcd}[column sep=small] & \text{formula} \arrow[dl] \arrow[dr] & \\ \begin{array}{c}\text{open statement,}\\ \text{statement-function}\end{array} & & \begin{array}{c}\text{closed statement,}\\ \text{statement}\end{array} \end{tikzcd}$$

An example of an open statement given by Popper is "He is a charming fellow", which can be turned into a closed statement by replacing the name-variable "He" with the name "Ernest's best friend". Popper explicitly remarks that open statements do not have a truth value on their own; an open statement cannot be considered to be true or false.

The deducibility relation is axiomatized by the same rules (Rg) and (Tg) as in the case of propositional logic, but it now ranges over arbitrary formulas, not just closed statements. For example, Popper says that one can validly deduce the open statement "He is an excellent physician" from the open statement "He is not only a charming fellow but an excellent physician".

The new substitution operation is characterized by the four postulates (PF1) to (PF4) and the six primitive rules of derivation (6.1) to (6.6), which we present in a slightly simplified form as follows.

(PF1) $$\mathcal{L} \cap \mathcal{P} = \emptyset$$

(PF2) If $a \in \mathcal{L}$ and $x, y \in \mathcal{P}$, then $a\tbinom{x}{y} \in \mathcal{L}$

(PF3) For all $a \in \mathcal{L}$ there exists an $x \in \mathcal{P}$ such that for all $y \in \mathcal{P}$: $a\tbinom{x}{y} /\!/ a$

(PF4) There exist $a \in \mathcal{L}$ and $x, y \in \mathcal{P}$: $a/a\tbinom{x}{y} \to t/f$

Note that two kinds of metalinguistic quantifiers are used: there are universal and existential quantifiers ranging over statements $a \in \mathcal{L}$ and universal and existential quantifiers ranging over name-variables $x \in \mathcal{P}$. We only use symbols for the respective metalinguistic universal quantifiers in the following; (a) means "for all statements a" and (x) means "for all name-variables x".

The postulates (PF1) and (PF2) are, in a way, only about the correct grammatical use of formulas and name-variables. The postulate (PF3) says that for every formula there is some name-variable not occurring in it. This is obvious if the set of name-variables is considered to be infinite, and if each formula is a finite object which can only mention a finite number of name-variables. The postulate (PF4), which Popper considers to be optional, excludes degenerate systems in which only one object exists. Take, for example, the open statement a to be "x likes the current weather". The deducibility of "y likes the current weather" from "x likes the current weather" only leads to a contradiction if there are at least two persons to whom x and y can refer. Postulate (PF4) was also discussed in correspondence between Popper and Carnap (cf. this volume, § 23.10 and § 23.11).

The six primitive rules of inference are given below. We will not discuss them in detail, but the reader may check that they are valid for a concrete formalized object language and a substitution operation for that language.

$$\text{If, for every } z,\ a /\!/ a\binom{y}{z} \text{ and } b /\!/ b\binom{y}{z}, \text{ then } a /\!/ b \rightarrow a\binom{x}{y} /\!/ b\binom{x}{y} \tag{6.1}$$

$$a /\!/ a\binom{x}{x} \tag{6.2}$$

$$\text{If } x \neq y, \text{ then } \left(a\binom{x}{y}\right)\binom{x}{z} /\!/ a\binom{x}{y} \tag{6.3}$$

$$\left(a\binom{x}{y}\right)\binom{y}{z} /\!/ \left(a\binom{x}{z}\right)\binom{y}{z} \tag{6.4}$$

$$\left(a\binom{x}{y}\right)\binom{z}{y} /\!/ \left(a\binom{z}{y}\right)\binom{x}{y} \tag{6.5}$$

$$\text{If } w \neq x,\ x \neq u \text{ and } u \neq y, \text{ then } \left(a\binom{x}{y}\right)\binom{u}{w} /\!/ \left(a\binom{u}{w}\right)\binom{x}{y} \tag{6.6}$$

The rules (6.1) to (6.6) characterize substitution as a structural operation; this is similar to how the basis characterizes commas in sequences of statements. It is remarkable that Popper here presents an algebraic treatment of substitution, which can be compared to the theory of explicit substitution developed much later (cf., e.g., Abadi et al., 1991).

As an intriguing sidenote, Popper compares the definition of substitution by the rules (6.1) to (6.6) to the definition of conjunction via the inferential definition (D$\wedge$). He writes:

> These six primitive rules determine the meaning of the symbol "$a\binom{x}{y}$" in a way precisely analogous to the way in which, say, [rule (D$\wedge$) determines] the meaning of conjunction [. . .] with the help of the concept of derivability "/". (Popper, 1947c, p. 226)

However, Popper's rules for substitution cannot be brought into the form of an inferential definition of an operator of the object language. Hence, substitution cannot have the status of a logical constant according to Popper's criterion for logicality; his rules for substitution do not have the form of characterizing rules (and, consequently, no fully characterizing rules can be given either). Indeed, Popper also explains substitution as follows:

> The notation
>
> $$\text{“}a\binom{x}{y}\text{”}$$
>
> will be used as a (variable) metalinguistic name of the statement which is the result of substituting, in the statement a (open or closed), the variable y for the variable x, wherever it occurs. $a\binom{x}{y}$ is identical with a if x does not occur in a. (Popper, 1947d, p. 1216)

Popper's rules for substitution may thus be viewed as an implicit characterization of a metalinguistic operation, and not as an inferential definition of a logical constant for object languages.

Next we discuss some auxiliary concepts defined with the help of both the deducibility relation and the substitution operation.

9.2 Non-dependence, identity and difference

If we work with some inductively defined formal object language, then we can easily specify the set of free variables of a formula by recursion on the structure of that formula. This possibility is excluded in Popper's approach, which is not restricted to formal languages. Popper therefore introduces the expression

$$a_{\hat{x}}$$

which can be read as "x does not occur among the free variables in a". Popper himself expresses this as "a does not depend on x", "*a-without-x*" and "x does not occur relevantly in a". The formula a does not depend on x if, and only if, substitution of some name-variable y for x does not change the logical strength of a. That is:

(D $a_{\hat{x}}$) $$a /\!/ a_{\hat{x}} \;\leftrightarrow\; \text{for every } y\text{: } a /\!/ a\binom{x}{y}$$

The second concept Popper defines with the help of deducibility and substitution is *identity*. As Popper (1947c, p. 227f., fn 24) notes, one first has to extend the object language $\mathcal{L}$ to incorporate formulas of the form $Idt(x, y)$; this is achieved by the postulate

(P *Idt*) If x and y are name variables, then $Idt(x, y)$ is a formula.

In addition, the characterizing rules for substitution have to be extended by rules of the form

(A) $$(Idt(x, y))\binom{x}{z} /\!/ Idt(z, y)$$

(B) $$(Idt(x, y))\binom{y}{z} /\!/ Idt(x, z)$$

(C) $$\text{If } x \neq u \neq y\text{, then } Idt(x, y)\binom{u}{z} /\!/ Idt(x, y)$$

With these preliminaries, Popper defines identity using the following idea:

> The identity statement "$Idt(x, y)$" can be defined as the weakest statement strong enough to satisfy the [...] formula [...]
>
> $$\text{“}Idt(x, y), a(x)/a(y)\text{”}$$
>
> that is to say, the formula corresponding to what Hilbert-Bernays call the second identity axiom. (Hilbert-Bernays's first axiom follows from the demand that the identity statement must be the *weakest* statement satisfying this formula.) (Popper, 1949a, p. 725f.)

Popper here refers to the identity axioms J_1 and J_2 of Hilbert and Bernays (1934, p. 164):

$$(J_1) \qquad a = a$$

$$(J_2) \qquad a = b \rightarrow (A(a) \rightarrow A(b))$$

This justifies the following definition of *identity* $Idt(x, y)$:

$$(\mathrm{D}\,Idt) \quad a /\!/ Idt(x, y) \leftrightarrow \text{(for every } b \text{ and } z\text{: } ((b /\!/ b_{\grave{x}} \;\&\; b /\!/ b_{\grave{y}}) \rightarrow a, b\tbinom{z}{x} / b\tbinom{z}{y}) \;\&$$
$$((\text{for every } c \text{ and } u\text{: } ((c /\!/ c_{\grave{x}} \;\&\; c /\!/ c_{\grave{y}}) \rightarrow b, c\tbinom{u}{x} / c\tbinom{u}{y}))) \rightarrow b/a))$$

Popper (1948c, p. 323f., fn 11) expands on the definition of identity $Idt(x, y)$ in order to illustrate his method of obtaining a relatively simple characterizing rule from an explicit definition that is the weakest (or strongest) statement satisfying a certain condition or axiom. He first introduces the following abbreviating notation:

$$a /\!/ a_{\grave{x}\grave{y}} \leftrightarrow (w)(a /\!/ a\tbinom{x}{w} \;\&\; a /\!/ a\tbinom{y}{w}).$$

Using this abbreviation, he defines $Idt(x, y)$ as the weakest statement strong enough to imply the axiom J_2:

$$(\mathrm{D}\,Idt^{\dagger}) \quad a /\!/ Idt(x, y) \leftrightarrow$$
$$(b)(z)((b /\!/ b_{\grave{x}\grave{y}} \rightarrow a, b\tbinom{z}{x} / b\tbinom{z}{y}) \;\&\; (((c)(u)(c /\!/ c_{\grave{x}\grave{y}} \rightarrow b, c\tbinom{u}{x} / c\tbinom{u}{y}))) \rightarrow b/a))$$

This explicit definition, which is an abbreviated version of (D Idt), can be replaced by a definition that corresponds to the following characterizing rule:

$$(\mathrm{C}\,Idt^{\ddagger}) \qquad a / Idt(x, y) \leftrightarrow (b)(z)(b /\!/ b_{\grave{x}\grave{y}} \rightarrow a, b\tbinom{z}{x} / b\tbinom{z}{y})$$

This can be seen by instantiating a in (D $Idt^{\dagger}$) with $Idt(x, y)$ in order to obtain

$$(b)(z)((b /\!/ b_{\grave{x}\grave{y}} \rightarrow Idt(x, y), b\tbinom{z}{x} / b\tbinom{z}{y}) \;\&$$
$$(((c)(u)(c /\!/ c_{\grave{x}\grave{y}} \rightarrow b, c\tbinom{u}{x} / c\tbinom{u}{y}))) \rightarrow b / Idt(x, y))).$$

The left conjunct gives the direction from left to right in (C *Idt*‡), and the right conjunct gives the direction from right to left.

Finally, *difference Dff* (x, y) is simply defined as the classical negation of identity:

$$\text{(D } \mathit{Dff}\text{)} \qquad a /\!/ \mathit{Dff}(x, y) \leftrightarrow a /\!/ \neg_k \mathit{Idt}(x, y)$$

It is interesting to see that Popper chose to treat occurrence of free variables and identity as defined notions, rather than to class them with substitution and deducibility among the primitive notions characterized by the basis. We will see in § 9.4 that Popper probably revised this position later.

9.3 Quantification

Inferential definitions of universal and existential quantification are introduced in Popper (1947c), to which he later published a list of corrections and additions (cf. Popper, 1948e), which we take into account here. Popper's aim is not to develop and analyze the theory of quantification, that is, first-order logic, but to show that his approach to quantification is at least on a par with other proposed treatments of quantification. He therefore restricts himself to stating his definitions of the quantifiers and to deriving some simple conclusions, but he does not formally develop a meta-theory of quantification. He does not, for example, discuss the completeness of his rules, the difference between classical and constructive interpretations of the existential quantifier, or the relation to models of his system.

Later, Popper (1949a) gives the clearest explanation of what intuition his inferential definition of *universal quantification* is supposed to capture. He writes:

> The result of universal quantification of a statement *a* can be defined as the weakest statement strong enough to satisfy the law of specification, that is to say, the law "what is valid for all instances is valid for every single one". Popper (1949a, p. 725)

Presupposing his rules of substitution, and writing Ax for the universal quantifier, Popper's inferential definition and the characterizing rule for universal quantification are the following:

$$\text{(D7.1)} \qquad a_{\hat{y}} /\!/ Axb_{\hat{y}} \leftrightarrow (\text{for every } c_{\hat{y}}\text{: } c_{\hat{y}}/a_{\hat{y}} \leftrightarrow c_{\hat{y}}/b_{\hat{y}}\tbinom{x}{y})$$

$$\text{(C7.1)} \qquad \text{For every } c_{\hat{y}}\text{: } c_{\hat{y}}/Axb_{\hat{y}} \leftrightarrow c_{\hat{y}}/b_{\hat{y}}\tbinom{x}{y}$$

In order to see how more ordinary presentations of the rules for universal quantification follow from these inferential definitions, we can compare them to the more familiar rules of the (intuitionistic) sequent calculus (writing $\varphi[x/y]$ for the result of substituting y for x in the formula φ):

$$\frac{\Gamma, \varphi[x/t] \vdash \psi}{\Gamma, \forall x\varphi \vdash \psi}\,(\forall\vdash) \qquad\qquad \frac{\Gamma \vdash \varphi[x/y]}{\Gamma \vdash \forall x\varphi}\,(\vdash\forall)$$

$$\frac{\Gamma, \varphi[x/y] \vdash \psi}{\Gamma, \exists x\varphi \vdash \psi}\ (\exists \vdash) \qquad\qquad \frac{\Gamma \vdash \varphi[x/t]}{\Gamma \vdash \exists x\varphi}\ (\vdash \exists)$$

with the variable condition that y does not occur free in the conclusion of $(\vdash \forall)$ and $(\exists \vdash)$.

For example, by instantiating (C7.1) with $Axb_{\hat{y}}$ and by using the rules (Tg) and (Rg) from the basis, we obtain the following rule

$$a, b_{\hat{y}}\binom{x}{y}/c \rightarrow a, Axb_{\hat{y}}/c$$

which can easily be seen to be a variant of the rule $(\forall \vdash)$ where the name-variable y takes the role of the term t. Similarly, by instantiating (C7.1) with $c_{\hat{y}}$ and reading the bi-implication from right to left we obtain the following rule, which corresponds to the rule $(\vdash \forall)$ with the variable condition that y does not occur relevantly in c:

$$c_{\hat{y}}/b_{\hat{y}}\binom{x}{y} \rightarrow c_{\hat{y}}/Axb_{\hat{y}}.$$

As was the case for universal quantification, Popper gives the clearest explanation of the inferential definition of *existential quantification* not in Popper (1947c), but in Popper (1949a, p. 725):

> The result of existential quantification of the statement a can be defined as the strongest statement weak enough to follow from every instance of a.

The inferential definition and the characterizing rule for the *existential quantifier Ex* are

(D7.2) $\qquad a_{\hat{y}}//Exb_{\hat{y}} \;\leftrightarrow\;$ (for every $c_{\hat{y}}$: $a_{\hat{y}}/c_{\hat{y}} \;\leftrightarrow\; b_{\hat{y}}\binom{x}{y}/c_{\hat{y}}$)

(C7.2) $\qquad$ For every $c_{\hat{y}}$: $Exb_{\hat{y}}/c_{\hat{y}} \;\leftrightarrow\; b_{\hat{y}}\binom{x}{y}/c_{\hat{y}}$

To elucidate what they mean, we derive some more familiar rules for the existential quantifier from its characterizing rule. Instantiating (C7.2) with $Exb_{\hat{y}}$ and using the rules of the basis we can obtain the rule

$$a/b_{\hat{y}}\binom{x}{y} \rightarrow a/Exb_{\hat{y}}$$

which corresponds to the sequent calculus rule $(\vdash \exists)$; and by instantiating (C7.2) with $c_{\hat{y}}$ and reading the bi-implication from right to left, we obtain the following rule, which corresponds to $(\exists \vdash)$:

$$b_{\hat{y}}\binom{x}{y}/c_{\hat{y}} \rightarrow Ex_{\hat{y}}/c_{\hat{y}}.$$

Popper does not consider the explicit definitions (D7.1) and (D7.2) to be improvements compared to the characterizing rules. They are given to show that universal and existential quantification can be defined using only his basis and the rules (6.1) to (6.6). He notices that these rules are not as simple as the rules of his basis, for example. But he points out that the concept of "$a_{\hat{x}}$" can be avoided in these definitions (cf. Popper, 1947c, p. 230, fn 26)[82]. Assuming $x \neq y$, one can use instead:

(7.1*) $a\binom{y}{x}/Ax(b\binom{y}{x}) \leftrightarrow a\binom{y}{x}/b\binom{x}{y}$

(7.2*) $Ex(a\binom{y}{x})/b\binom{y}{x} \leftrightarrow a\binom{x}{y}/b\binom{y}{x}$

(D7.1*) $a\binom{y}{x}/\!/Ax(b\binom{y}{x}) \leftrightarrow$ (for every c: $c\binom{y}{x}/a\binom{y}{x} \leftrightarrow c\binom{y}{x}/b\binom{x}{y}$)

(D7.2*) $a\binom{y}{x}/\!/Ex(b\binom{y}{x}) \leftrightarrow$ (for every c: $a\binom{y}{x}/c\binom{y}{x} \leftrightarrow b\binom{x}{y}/c\binom{y}{x}$)

He conceives his rules of quantification to be less complicated than those given by Hilbert and Ackermann (1928) or those given by Quine (1940, § 15), and he emphasizes that his rules in the end make use of only one logical concept, namely that of deducibility / as characterized by his basis.

9.4 An unfortunate misunderstanding

Popper (1947c, § 8) introduces a distinction between rules of derivation and rules of demonstration or proof, something considered very important by him. If he had not stopped publishing in logic, he would very likely have developed these ideas in more detail. For example, among his unpublished manuscripts there are two which are entitled "Derivation and Demonstration in Propositional and Functional Logic" (this volume, § 18.1) and "The Propositional and Functional Logic of Derivation and of Demonstration" (this volume, § 18.3), as well as another untitled manuscript (this volume, § 18.2), which also deals with this distinction. In a letter to John C. Eccles of 10 November 1946 Popper (1946b) explicitly mentions that he is "writing a paper on 'Derivation & Demonstration'".

In order to illustrate this distinction between derivation and demonstration we have to make use of the concept of relative demonstrability $\vdash$ (cf. § 4.4). If we specialize this concept to no formula on the left hand side and exactly one formula on the right hand side, we obtain the definition of a *provable formula* a: $\vdash a$. Consider now the following two formulas of the metalanguage:

(8.4) $a/b \rightarrow (\vdash a \rightarrow \vdash b)$

(8.4′) $(\vdash a \rightarrow \vdash b) \rightarrow a/b.$

Popper correctly remarks that while the first formula is valid, the second is not. This can be seen, as Popper claims, by instantiating a by a non-tautological consistent formula and b by a contradictory one. Using a more modern terminology, we may say that $\vdash a \rightarrow \vdash b$ expresses that the rule leading from a to b is *admissible*, whereas a/b expresses that this rule is *derivable*. The first formula (8.4) then expresses that the derivability of a rule implies its admissibility, and the second formula (8.4′) that the admissibility of a rule implies its derivability. The counterexample to the second formula says that admissibility does not in general imply derivability. However,

[82] Added in the corrections and additions (cf. Popper, 1948e).

it should be noted that the value of this counterexample is limited, as it does not apply to rules closed under substitution (that is, to rules identifiable with the set of their substitution instances), which is normally considered a requirement for rules to constitute a logic. In classical propositional logic, for rules closed under substitution, admissibility does imply derivability (cf. Belnap, Leblanc, and Thomason, 1963, Mints, 1976, and Humberstone, 2011).[83]

Rules of the form $\vdash a \rightarrow \ \vdash b$ are called *rules of proof* or *rules of demonstration*, in contrast to *rules of derivation* expressed by a/b (there can be more than one premise). Now Popper correctly observes that the rules of a system like Principia Mathematica (Whitehead and Russell, 1925–1927) are rules of proof and not rules of derivation. For example, the rule of modus ponens takes the form

$$\vdash a \rightarrow (\vdash a > b \rightarrow \ \vdash b)$$

rather than the form

$$a, a > b/b.$$

What Popper intends to formulate here, and in particular in his definition of a purely derivational system of primitive rules (cf. Popper, 1947c, definition (D8.1)), is, in our opinion, a criterion that allows to distinguish between formulations of logic based on axioms and rules of proof, such as those of Frege (1879), Hilbert and Bernays (1934, 1939) and Whitehead and Russell (1925–1927) on the one hand, and formulations of logic based on derivation alone, such as those of Gentzen (1935a,b) and his own, on the other hand.

Unfortunately, he applied this analysis of rules of derivation and rules of proof to the systems of Carnap as well as of Hilbert and Bernays in a way that does not take account of an important difference between his system and theirs. Popper (1947c, p. 232) warns that there are rules of proof such as

$$\vdash a_{\hat{y}} \ \leftrightarrow \ \vdash a_{\hat{y}}\binom{x}{y} \tag{8.5}$$

which are valid, whereas the corresponding rule of derivation

$$a_{\hat{y}}/a_{\hat{y}}\binom{x}{y}$$

is invalid. He continues:

> Now all the mistakes here warned against do actually vitiate some otherwise very excellent books on modern logic – an indication that the distinction between (conditional) rules of proof or rules of demonstration on the one side and rules of derivation on the other cannot be neglected without involving oneself in contradictions. (Popper, 1947c, p. 233)

Both Carnap and Bernays responded to Popper's criticism of their respective system in

[83] Formal systems not closed under substitution have many unexpected properties – in intuitionistic logic, for example, that the law of double negation elimination can be validated; cf. de Campos Sanz, Piecha, and Schroeder-Heister (2014) and Piecha, de Campos Sanz, and Schroeder-Heister (2015).

correspondence (cf. this volume, § 23.10 and § 23.11). Bernays (this volume, § 21.7) writes:

> Now I have to comment upon your critique of the formulation of the all-schema, as it is given in the "Grundlagen der Math." [Hilbert and Bernays, 1934]. I think of the passage p. 232–233 of your New Foundations. [. . .] The contradiction that you derive, starting with the schema $a_{\hat{x}} > b/a_{\hat{x}} > Axb$ which you criticize, does not arise in the formalism of the "Grundl. der Math.", because the implication plays another role here than the "hypothetical" in your formalism.

We note that Popper's letter to Carnap of 24 December 1947 (this volume, § 23.11) is also interesting for the fact that it contains an expansion of his theory of quantification by presenting several logical laws of classical first-order logic.

Popper later revised his understanding of the interaction of substitution and deducibility. While his definitions are formulated using the weaker notion of interdeducibility, he then considered it necessary to use the stronger notion of identity of statements (Popper, 1974c, p. 171, endnote 198):

> The mistake was connected with the rules of substitution or replacement of expressions: I had mistakenly thought that it was sufficient to formulate these rules in terms of interdeducibility, while in fact what was needed was identity (of expressions). To explain this remark: I postulated, for example, that if in a statement *a*, two (disjoint) subexpressions *x* and *y* are both, wherever they occur, replaced by an expression *z*, then the resulting expression (provided it is a statement) is interdeducible with the result of replacing first *x* wherever it occurs by *y* and then *y* wherever it occurs by *z*. What I should have postulated was that the first result is identical with the second result. I realized that this was stronger, but I mistakenly thought that the weaker rule would suffice. The interesting (and so far unpublished) conclusion to which I was led later by repairing this mistake was that there was an essential difference between propositional and functional logic: while propositional logic can be constructed as a theory of sets of statements, whose elements are partially ordered by the relation of deducibility, functional logic needs in addition a specifically morphological approach since it must refer to the subexpression of an expression, using a concept like identity (with respect to expressions). But no more is needed than the ideas of identity and subexpression; no further description especially of the shape of the expressions.[84]

Concerning possible future work on logic, Popper states in his reply of 13 June 1948 (this volume, § 21.8) to Bernays's letter of 12 May 1948 (this volume, § 21.7):

> I have also a number of new results – but I do not believe that I will ever dare again to publish something (except, maybe, an infinite sequence of corrections to my old publications)!

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[84] Cf. the remarks in a letter to Schroeder-Heister of 19 August 1982 (this volume, § 32.5).

However, this modest remark should not be taken too literally in light of Popper's later contributions to logic reprinted in this volume (Chapters 9–11), and also in light of his work on Boolean algebra and probability theory not presented here (cf. Preface). Even Popper's (1949a) logical contribution to the Tenth International Congress of Philosophy shortly afterwards in Amsterdam[85] carries the boldest and most provocative of his titles: "The Trivialization of Mathematical Logic". Although he may have been embarassed by technical deficiencies as well as unfortunate and misleading formulations in his exposition[86], which naturally surfaced in discussions with outstanding mathematical logicians such as Bernays, Popper was certainly aware that he was making an original conceptual contribution towards the foundations of deductive logic, something confirmed by the later development of inferentialism and proof-theoretic semantics.

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[85] From 11 to 18 August 1948. Besides, Popper was also invited to give a plenary talk on social science, entitled "Prediction and Prophecy and their Significance for Social Theory"; cf. Popper (1949d). Bernays and Popper had long planned to meet there in person; cf. this volume, § 21.9 and § 21.11.

[86] Cf. Popper's letter to Schroeder-Heister of 19 August 1982 (this volume, § 32.3).

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Part I
Published Articles

Portrait of Karl Popper from 1939, submitted as part of his application for employment at Canterbury University College (cf. Popper, 1939).

Chapter 2
Logic without Assumptions (1947)

Karl R. Popper

Abstract This article is a corrected reprint of K. R. Popper (1947b). Logic without Assumptions. In: *Proceedings of the Aristotelian Society* 47, pp. 251–292.

Editorial notes: In the original publication it says "*Meeting of the Aristotelian Society at 21, Bedford Square, W.C.1, on May 5th, 1947 at 8 p.m.*" below the title. We omitted this here. Popper sent lists of corrections to Bernays and Quine. These have not been worked into this reprint. They are reproduced in § 21.8.1 and § 30.5.1 of this volume. Further remarks by Popper can be found in footnote 8 of the unpublished typescript "A Note on the Classical Conditional" (this volume, Chapter 17, p. 286). In this footnote Popper also criticizes the review by McKinsey (1948, this volume, § 13.14). The manuscript "A General Theory of Inference" (this volume, Chapter 15) seems to be an early version of this article. The term "minimum calculus" refers to Johansson's (1937) "Minimalkalkül".

251 In this paper I shall try to explain, with a minimum of technicalities, some results of investigations into the field of deductive logic. The main problem to be discussed is *the problem of deduction itself* – more precisely, the problem of giving a satisfactory definition of "valid deductive inference".

Our method will be as follows: after having introduced, in section (1), a few auxiliary technical terms, we shall propose a definition, criticize it, and replace it by a better one, and repeat this procedure. After a few steps we shall reach, in this way, our first result – a definition which is a generalization of one due to A. Tarski[1]; and the rest of our investigation will be directed towards improving this result with the aim of avoiding some crucial objections originally urged by Tarski against his own definition.

The improved definition reached in this way suffices for establishing the validity of propositional logic and of the lower functional logic, without any further assumption.

[1] Cp. A. Tarski's lecture (delivered in 1935) "Ueber den Begriff der logischen Folgerung", ⟨Tarski, 1936b⟩.

D. Binder et al. (eds.), *The Logical Writings of Karl Popper*,
Trends in Logic 58, https://doi.org/10.1007/978-3-030-94926-6_2

(1)

Three of the auxiliary technical terms which will be introduced and explained in this section, "interpretation", "statement-preserving interpretation", and "form-preserving interpretation", are, like some other very general concepts, somewhat tedious to deal with. Their generality, and even triviality, presents an obstacle to their intuitive understanding. There is, as it were, so little in them that those who try to grasp them
252 are left with the unpleasant | feeling that there is nothing to grasp. Those who are not satisfied with concepts in which there is so little, or who cannot believe that there is not more in them, may be assured that these three auxiliary concepts are the only technical terms of such an unsatisfying generality which they will be expected to handle. The other terms, "logical form" and "logical skeleton", introduced at the end of this section, speak for themselves.

We begin by considering a number of languages, Latin, Dutch, German, Russian, etc., and by considering *translations* from one of these languages into the others – say, from Latin into English. There will be good translations and bad translations. Let us suppose that we know all the languages under consideration well enough to be unfailing judges of the various translations offered; that is to say, we can say whether a translation renders the full meaning of the statements to be translated or not; also, whether perhaps a certain kind of expression (such as the nouns or the verbs) are properly translated, while other kinds of expressions are rendered without regard to their correct meaning, etc.

Now by an *interpretation of one language in another* we shall understand a kind of translation, good or bad. Our intention is to use the term "interpretation" in such a wide sense that even extremely bad translations can still be classed as interpretations. But obviously, it will be necessary to introduce some kind of limitation to the badness of a translation if we are not prepared to accept the claim, for example, that the first twelve words of "Pride and Prejudice" constitute an interpretation of, say, the whole original text of the "Golden Ass". For certain purposes it might indeed be of some advantage to use the term "interpretation" in such a wide sense that even the example just given would be covered. But for our purposes, it turns out that a slightly narrower use of the term is useful – especially in view of the fact that we shall not be interested in such things as exhortations or exclamations, but only in *statements*.

253 | Imagine that an extremely bad translator knows just enough about a certain language – Latin, say – as to recognize what is, and what is not, a complete statement in that language. (He may, for example, know nothing beyond the fact that statements are separated by full stops.) He may then proceed by "translating" every Latin statement into an English one. We who know both, Latin and English, will, of course, realize that his mock-translation is shockingly bad, i.e., that most of the English statements do not render in any way the meaning of those Latin statements to which they correspond, and which they should translate. However, the mock-translation in question has perhaps *one* advantage. Since to each statement of the Latin original there corresponds exactly one statement of the alleged translation, it will be possible, if necessary, to check the claims of the translator step by step; in other words, there will be at least a definite claim to be checked, and a fairly definite method of checking it.

Now it turns out that it is convenient to confine our investigation to translations, including mock translations, which satisfy the minimum requirement that every complete statement of the original text is, however badly or arbitrarily, translated by – or, more precisely, co-ordinated with – one complete and meaningful statement of the translation. And a translation or mock-translation which satisfies this minimum requirement will be called here, from now on, an "*interpretation*".

It is important to realize that the concept thus defined is an extremely wide one. We may, for example, choose to co-ordinate the first, second, third . . . statement of the "Golden Ass" with the first, second, third . . . statement of "Pride and Prejudice": the result will be an interpretation in the sense here defined. But we may also choose to co-ordinate the first, third, fifth . . . statement of the "Golden Ass" with one statement, say the first of "Pride and Prejudice", and the second, fourth, sixth . . . statement of the "Golden Ass" with the statement "In Italy it rains more often than in Egypt"; and
254 the result of this utterly | arbitrary co-ordination will still be an interpretation of the "Golden Ass" in the English language, in the sense here defined.

It is perhaps difficult to imagine that such a wide concept as this concept of interpretation is of any use. However, it will help us to define a few slightly narrower concepts which turn out to be exceedingly useful; more particularly, the concept of a *statement-preserving interpretation* and, furthermore, that of a *form-preserving interpretation*.

It is possible that a certain statement of some text may, word for word, recur in some other places of that text. Let us assume, as before, that the translator or interpreter of the Latin text knows so little about this language that he only recognizes the places where full statements end. But let us also assume that he has such a splendid memory that he recognizes each Latin statement whenever it re-occurs completely, and that he decides to translate it, in all places of its complete re-occurrence, by the same statement of the English language by which he translated it when it occurred first. If he follows this method consistently, we shall say that his interpretation preserves recurrences of complete statements, or more briefly, that it is a *statement-preserving interpretation*.

The concept of a statement-preserving interpretation is still a very wide one, and one might be tempted, at first, to think that it is still too wide to be useful. It is so wide that it allows us, for example, to translate every statement of the Latin language by *one* and the same English statement – say, the statement "In Italy it rains more often than in Egypt." For in this case, our condition (viz., that every statement of the Latin text must be translated, whenever it recurs, by the same English statement by which it was translated when it first occurred) is clearly satisfied. In other words, if we wish to give a statement-preserving interpretation of some Latin text in the English language, it will be sufficient if we have at least *one* English statement at our disposal into which we may "translate" all the statements of the Latin text; although we may, of course,
255 use more | than one English statement – indeed, any number of different English statements up to the extreme case in which their number is equal to the number of the different Latin statements contained in the text to be interpreted. It is clear that, in general, it will be impossible to re-translate a statement-preserving interpretation, i.e., to re-construct the original text from the translation together with the rules of

co-ordination (e.g., a kind of statement dictionary), except in the extreme case where with every different statement of the original a different statement of the interpretation is co-ordinated. (In this extreme case we speak of a *strictly* statement-preserving interpretation.)

The conception of a statement-preserving interpretation has one outstanding advantage over the conception of an interpretation as it was first introduced, viz., that it can be easily extended so as to cover not only an interpretation of a given *text* in some language or other, but also the interpretation of a whole *language* L_1 (say, the Latin language) in another language L_2 (say, the English language). For this purpose all that is necessary is to assume that we have co-ordinated, by some method or other, to every statement of L_1 one statement of L_2. This might be done, for example, by giving a proper translation of every statement of L_1 in L_2. In this extreme case (in which the interpretation is strict) we have to use as many different statements of L_2 as there are in L_1. Or again, to use another extreme case as an example, we might co-ordinate with every statement of L_1 one and the same statement of L_2. Or we might choose, in intermediate cases, a group of 2 or 20 or 200 statements of L_2, and co-ordinate the various statements of L_1 with these, by some method or other (say, on the basis of some alphabetic similarity of the first letters). Once we have co-ordinated with every statement of L_1 a statement of L_2, we have, of course, laid down a method of giving for every text in L_1 a statement-preserving interpretation in L_2.

It may be remarked that the languages L_1 and L_2 may well coincide. We can
256 construct, for example, various | statement-preserving interpretations of the English language (L_1) in the English language (L_2), by selecting one or more – possibly a very great number – of English statements (L_2) into which all other English statements (L_1) are to be translated, in accordance with some dictionary or code.

I hope that the conception of a statement-preserving interpretation will be reasonably clear by now. That it can be a useful conception will still appear doubtful, and has to be shown later. But it will be realized that this conception comprises a great deal, not only interpretations which do not take any notice of the meaning of the statements of the language L_1 – the language to be interpreted – but also interpretations which preserve the meaning of these statements, i.e., *proper translations*. Of course, we cannot assert that the conception of a statement-preserving interpretation comprises all proper translations; there may be an excellent translation from Latin into English which is not statement-preserving but which, at times, may split up a Latin statement into more than one English statement, or which uses one English statement to render several Latin ones. But most proper translations which avoid such cases will fall into the class of statement-preserving interpretations.

It should be noted that every statement-preserving interpretation automatically preserves recurrences of groups or sequences of statements. If, for example, the group of the statements $a, b, c, \ldots$ of language L_1 occurs in a text more than once, it will be translated into L_2, by every statement-preserving interpretation, in the same manner whenever the group recurs. This, of course, is not necessarily the case with groups of *expressions shorter than statements*. Such expressions – for example, single words, or groups of words – may, by a statement-preserving interpretation, be rendered differently every time they recur in a certain text; provided, of course, that they do not recur as parts of a recurring complete statement.

Within the wide class of statement-preserving interpretations we have, of course, many sub-classes, and among them not only proper translations, but also interpretations
257 | which preserve the recurrence of certain groups of words. Among these, one class of interpretations is especially important for our purposes; we shall call this class the "*form-preserving interpretations*".

The intuitive idea of a form-preserving interpretation of a language L_1 in a language L_2 is that of an interpretation which is not only statement-preserving but which also preserves what is usually called the "logical form" of the statements which are to be interpreted. A definition which is adequate to this idea can be easily given if we assume that we can distinguish between *two kinds of signs* of the languages which we are considering, viz., the *formative signs* and the *descriptive signs*.

Formative signs (they have sometimes been called "logical signs"[2]) are such signs as, for example, the full stop, or, in the English language, such words as "and", "or", "if ... then ... ", "neither ... nor ... ", "all", "some", "there exists at least one", etc. All signs, and groups of signs, which are not classed as formative will be called "*descriptive signs*". Examples are words such as "kitten", "mountain", "strategist", "aluminium", or groups of words such as "Greek orators", "elephant bones", "elderly disgruntled newspaper reader", etc.; also – but only if "of" is not considered as formative – "Orators of Greece", "bones of an ancient ancestor of the elephants", etc. Other descriptive signs are adjectives such as "grey" or "soft", and proper names (names of individual persons or of other physical things). We shall assume, for the time being, that the distinction between formative and descriptive signs can be applied with ease and without ambiguity to all the languages in which we are interested, and more especially, to the language L_1 which is to be interpreted in some other language L_2. (This assumption will be challenged later, in section (4).)

258 | We can now define a *form-preserving interpretation* as an interpretation which (a) preserves the meaning of all the formative signs, i.e., gives a proper translation of all the formative signs, and which (b) preserves recurrences of those groups of non-formative (descriptive) expressions which, in a proper translation, would fill the spaces between the translated formative signs.

According to this definition, a proper translation (if it is statement-preserving) will in general be a form-preserving interpretation; but so will be a translation of "All men are mortal" into "All kittens are green", provided we decide to preserve, in case the descriptive sign "man" recurs in L_1, its rendering by "kitten", and, in the same way, the rendering of "mortal" by "green".

According to our definition, it is only necessary to preserve *recurrences* of descriptive signs; two or more different descriptive signs of L_1 may be rendered by the same descriptive signs of L_2, subject, of course, to the proviso that every statement

[2] The term "logical sign", introduced by Carnap together with the term "descriptive sign", has been used also by others, for example by Tarski. I prefer "formative sign" in order not to suggest that logical signs are something like logical technical terms, such as the terms "deducible from", "compatible with", "negation", etc.

is rendered by *a statement* – a true one or a false one, but in any case a meaningful statement.

Among the form-preserving interpretations, there will be some which not only preserve recurrences of descriptive signs but also differences between descriptive signs. These might be called “strictly form-preserving interpretations”.

In general – that is, except if they are strict – form-preserving interpretations cannot be re-translated or decoded. Even a knowledge of all the translation rules enables us to translate only in *one* direction – say from L_1 to L_2 – but does not enable us to re-translate our interpretation.

A necessary and sufficient condition for the possibility of decoding or retranslating a form-preserving interpretation L_2 of L_1 back into L_1 is that every statement a_1 of L_1 which is interpreted by a_2 of L_2 is, in turn, a form-preserving interpretation of a_2. This is, at the same time, a necessary and sufficient condition for a form-preserving interpretation to be strict. The situation with statement-preserving interpretations
259 is analogous. This enables us to *define the* | *strictness* of form-preserving and statement-preserving interpretations in the following way:

A form-preserving (or a statement-preserving) interpretation of L_1 in L_2 is strict if, and only if, there exists such a form-preserving (or statement-preserving) interpretation of L_2 in L_1 that each statement of L_2 is interpreted, in its turn, by the same statement of L_1 which it interprets.

So much about the ideas of a statement-preserving and of a form-preserving interpretation.[3]

With the help of the latter idea, it is now very easy to define the idea of the *logical form of a statement and of a sequence of statements* (e.g., of an argument):

Two statements a_1 and a_2, not necessarily belonging to the same language, have the same logical form if, and only if, there exist two form-preserving interpretations such that a_1 interprets a_2, and *vice versa*. (Instead of demanding two interpretations and saying “*vice versa*”, we may also insert the word “strictly” before “form-preserving”.)

Similarly, if A_1 and A_2 are groups or series of statements. (We assume that statements belonging to the same series also belong to the same language.)

Two series of statements, A_1 and A_2, not necessarily belonging to the same

[3] Apart from the two kinds of interpretations which have been explained – statement-preserving and form-preserving – there are others of some interest. I may mention the truth-preserving interpretations, i.e. interpretations which do not necessarily preserve the meaning of the statements involved but which render every true statement by a true statement. This may be achieved, in a trivial way, by translating *all* statements of L_1, whether true or false, into true statements (say, into arithmetical truisms) of L_2. If we wish to exclude such a trivial method, we can demand that the interpretation should be not only truth preserving but truth-value preserving, i.e. that it should preserve not only the truth but also the falsity of the statements of L_1. In this case, we may still render all statements of L_1 by merely two statements of L_2, viz. by one which is true and one which is false. But this interpretation will be, nevertheless, far less trivial, since we have to consider the truth or falsity of every statement of L_1 before we correlate it with one of the two statements of L_2. (In order to avoid misunderstandings, I may mention that my concept of interpretation does not coincide with Carnap’s concept, as used in his *Introduction to Semantics*, ⟨Carnap, 1942⟩, esp. p. 203 and pp. 212f. This can be seen from the fact that one of Carnap’s demands, expressed in our terminology, is that L_2 contains at least as many different statements as L_1. Also, what Carnap calls a “true interpretation” does not coincide with our “truth preserving interpretation”.)

260 language, have the same logical | form if, and only if, there exist two form-preserving interpretations such that the first (the second, etc.) statement of A_1 interprets the first (second, etc.) statement of A_2, and *vice versa*. (Again, we can simplify the definition by referring to strictness.)

The term "logical form" usually occurs in the context "the same logical form" or "different logical form", and for such contexts our definition suffices. If a separate definition is desired, we can define the *logical form of the statement* a_1 as the class of all statements (of any number of languages) which have *the same logical form as* a_1.

This is a very abstract idea; but a more concrete idea is available in case our two statements a_1 and a_2 belong to the same language. We then can say that they have not only the same logical form but, more concretely, the same "logical skeleton"; for example, the statements of the English language "All men are mortal" and "All kittens are green" have both the logical skeleton:

"All . . . . are ——"

The logical skeleton of a statement or a group of statements is obtained simply by eliminating all descriptive signs, indicating, at the same time, recurrences of descriptive signs, by some method or other. It is clear that all statements or arguments of a certain language with the same logical skeleton have the same logical form, and *vice versa*; and it is clear that the concept of logical skeleton is not only more concrete but also simpler, since it is possible to define it directly, with the help of the distinction between formative and descriptive signs, without introducing the idea of an interpretation. On the other hand, our idea of a logical form is more general, and gives us the means of constructing logic as a theory of language – or of languages – *without tying us down to any particular language*.

And this, indeed, is one of the main points of our use of interpretations: we operate with the ideas of a statement-preserving and of a form-preserving interpretation partly because this method allows us, as will be seen, to combine the modern view of logic
261 as a theory of language with the | old intuitive idea that *the validity of an inference does not depend on the language in which it is formulated*; or more precisely with the idea that, if an inference is valid in one language, then it remains valid in every proper (and form-preserving) translation.[4]

[4] I believe that, in this way, we can preserve whatever is tenable in those objections to the modern view which emphasize that logic does not deal with mere "sentences" but rather with "propositions" or perhaps with "objective thoughts" (or "thought contents") or "thoughts" or "judgements", etc. (I do not use the term "sentence" but "statement" in order to indicate that, if I speak of a statement, I do not abstract from the meaning expressed by it – whatever this may be. A further analysis of the idea of a "meaningful statement" is very desirable; but it is not, as far as I can see – cp. the end of section (6) – necessary for the understanding of logic and the problem of its foundations; nor do I believe that either psychology, behaviourism, operationalism, verificationism, phenomenalism, or perceptionalism, etc., can have anything to offer towards a solution of the problem I have in mind.)

(2)

Armed with the technical terms introduced in section (1), we now turn to the analysis of the idea of a valid (deductive) inference.

By an inference, valid or invalid, we shall understand, in this paper, a number of statements, at least two, of some language, for example English, of which one is marked out for a conclusion and the others for premises (for example, by writing the conclusion last, below a horizontal line, etc.).

Our problem will be to analyse, in the most general way possible, the conditions under which such an inference (or argument) is called "valid"; or using a slightly different terminology, the conditions under which the relationship of deducibility actually holds between some premises and a conclusion.

We shall take most of our examples, for the sake of simplicity, from syllogistic logic; but this should not create the impression that we are more concerned with syllogistic logic than with any of the more developed systems (including the so-called "alternative" or "non-Aristotelian" systems, and those which use modalities).

262 | We begin by considering two simple examples of inferences; (a) a valid one, and (b) an invalid one.

(a)	(b)
All kittens are green	*All men are mortal*
Joe is a kitten	*Socrates is mortal*
Joe is green	*Socrates is a man*

Our task is to explain in a general way what we mean by saying that (a) is valid while (b) is not – without appeal to any recognized system of rules of inference of which (a) may be an observance or application. How could we try to explain to somebody who has not studied such a system of logical rules that (a) is valid and (b) invalid?

We might try to argue on the following lines:

It is conceivable that the conclusion of (b) is false, *even if the premises are both true*, while this is not conceivable in the example (a). For, assume that all kittens are really green, and that Joe is really a kitten, and assume nothing else; then, clearly, Joe must be green. But assume that all men are really mortal, and that Socrates is really mortal, but do not assume anything else (more especially, do *not* assume that Socrates is a man any more than, say, Joe); then, clearly, it is conceivable that the premises of (b) are true, and the conclusion false, since Socrates may be mortal and, for example, a kitten. In other words, for (b) a state of affairs (in which "Socrates" is the name of a kitten) is possible which renders both premises true and the conclusion false, while with (a) every state of affairs which renders the premises true would also render the conclusion true.

These considerations lead to our first preliminary and tentative definition (D1):

(D1) *An inference is valid if, and only if, every possible state of affairs which renders all the premises true also renders the conclusion true.*

We may call a state of affairs which renders all the premises of an inference

true and, at the same time, the conclusion false, a counter-example of that inference.
263 (A counter-example of (b) is provided, for example, by a state | of affairs in which "Socrates" is the name of a kitten.) Using this term, we may re-formulate (D1) in this way:

(D1′) *An inference is valid if, and only if, no counter-example of it exists.*

The main objection to this first tentative definition is the vagueness of the term "state of affairs", and more especially "possible state of affairs". This latter term may even be suspected of introducing a vicious circularity. For we are discussing logical validity, i.e., logic. But "possible" may very well mean "logically possible", and thus presuppose what we wish to define. (The same may be said of words like "conceivable", etc.)

In order to avoid these question-begging terms, we shall make use of the technical terms introduced in section (1). We can either use the comparatively simple term "logical skeleton" or the more complicated term "logical form" (or, in its stead, the term "form-preserving interpretation"). Let us first use, tentatively, the following two logical skeletons of (a) and of (b):

(a+)	(b+)
All . . . . are ——	*All . . . . are* ——
, , , , is a	*, , , , is* ——
, , , , is ——	*, , , , is a . . . .*

Applying to (a+) and (b+) similar considerations as we applied to (a) and (b), we arrive at our second tentative definition:

(D2) *An inference is valid if, and only if, every inference with the same logical skeleton whose premises are all true has a true conclusion.*

We may now re-define our term "counter-example" as follows:

A counter-example of an inference is an inference with the same logical skeleton whose premises are all true and whose conclusion is false.

If we use the term "counter-example" in this second sense (if necessary, we can distinguish it by the attribute "skeleton-preserving"), then we can give an alternative
264 | formulation (D2′) of (D2), such that the wording of (D2′) is identical with that of (D1′), although its meaning of course, has changed with the meaning of "counter-example".

Another possibility is that we use the term "logical form" instead of "logical skeleton", leaving everything else unchanged. Owing to the fact, however, that "logical form" is, in its turn, defined with the help of the term "form-preserving interpretation", it turns out that it is preferable to use the latter term instead. We thus arrive at (D3). This is the definition described in the beginning of this paper as our first result, and as a generalization of Tarski's definition:

(D3) *An inference is valid if, and only if, every form-preserving interpretation of it whose premises are all true has a true conclusion.*

(D3′) is taken to have the same wording as (D1′) and (D2′); but "counter-example" is now defined in this way:

A counter-example (or, more fully, a form-preserving counter-example) of an inference is a form-preserving interpretation whose premises are all true and whose conclusion is false.

An immediate result of our definition (D3) is the following theorem (T1) which has the same wording as (D2), except that the term "logical form" takes the place of "logical skeleton":

(T1) An inference is valid if, and only if, every inference of the same logical form whose premises are all true has a true conclusion.

(Similarly, if we re-define "counter-example" again, so that it means an inference of the same logical form with true premises and a false conclusion, then we obtain from (D3) a theorem (T1′) with the same wording as (D1′) etc., but, of course, a different meaning.)

In view of (T1), we can say that the main difference between (D2) and (D3), i.e. our second and third definition, is that (D2) refers to "logical skeleton" and (D3) – indirectly – to "logical form".

265 | The considerable merits of these three definitions, and especially those of (D3), will be discussed in the next section; and in section (4), we shall present those objections against (D3) which will lead us to the construction of an improved definition.

(3)

Proceeding to a discussion of our three definitions (D1), (D2), and (D3), I shall first explain the points in which they are all strong; next the strong points of (D2) and (D3); and ultimately, those in which (D3) is superior to the others.

All three definitions make use of the fundamental idea of *transmission of truth from the premises to the conclusion*; that is to say, of the idea that, if the premises are true, the conclusion *must* be true also. And all the definitions except the first succeed in explaining, or rather avoiding, this "must" (which is one of those dangerous question-begging terms) by pointing out that the transmission of truth *depends* solely upon the logical skeleton, or logical form, of the argument; and the term "depends" (which is also dangerous since it may mean "Logically depends") is avoided by the simple method of referring to *all* inferences of the same logical skeleton or form.

Both the reference to the transmission of truth and to the logical skeleton or form seem to me intuitively highly satisfactory. It is the main point of the practical usefulness of deduction that, if we know that the premises are true and the inference valid, we can rely on the conclusion being true. In this way, inference allows us to obtain from reliable primary information reliable secondary information; and it allows us, by using as premises primary information from different sources, to derive secondary information unknown to any one of the sources.

Moreover, the transmission of truth from the premises to the conclusion which in itself is pragmatically as well as intuitively such an important point, means that, whenever the conclusion of a valid inference is false, at least one of the premises
266 must be false; for otherwise we would have a | counter-example and the inference would be invalid. In other words, the transmission of truth from the premises to the conclusion means also the *re-transmission of falsity from the conclusion to (at least one of) the premises*. This is, from the pragmatic point of view, just as important an aspect of a valid deduction as the obtaining of reliable secondary information. It enables us to reject prejudices by falsifying their consequences; and it allows us to test a hypothesis by the method[5] of trying to refute some of the conclusions which follow from it; for, if one of these is not true, the hypothesis cannot be true either.

It may be objected here that the term “true” which plays such a role in our definitions as well as in these considerations is vague; also, that we often do not know whether a certain statement is true or not. While this latter point must be admitted, the vagueness of the idea of truth need not be admitted; on the contrary, most people know that the word “true” can be easily eliminated from any such context as (1) “The statement ‘the snow is white’ is true” or (2) “The statement ‘the snow is red’ is true”. They know that the whole of (1) asserts precisely the same as the statement (1′) “The snow is white” and that (2) asserts precisely the same as (2′) “The snow is red”. But such a knowledge about the way in which a term can be eliminated from a simple context is, precisely, a knowledge of its meaning.

Why then, it may be asked, do we not eliminate the word “true” from our definitions? The answer is that it is easily eliminated from such simple but fundamental contexts where it refers to certain single statements, but not from contexts in which, more generally, we speak *about kinds of statements of some language* – say, about all statements of a certain logical skeleton or form, etc. It is especially in such a context that the term “true” – meaning precisely the same as in the other context – becomes useful.[6]

267 | I am therefore not prepared to admit that the employment of the term “true” in our definition is objectionable. Nevertheless, it will be seen that, in our final and improved definition (see also (D5)), there is a possibility of avoiding this term. At the present moment, however, I wish to emphasize that it must be the main point of every definition of validity which is intuitively satisfactory that it provides for the transmission of truth (whether or not the idea of truth enters the definition); and our definitions ⟨do⟩[a] provide for it.

Apart from referring to the transmission of truth, our second and third definitions refer to the *logical skeleton or form* of the argument in question. This too can be shown to agree with our intuitive idea of a valid inference. One immediate consequence

[5] Cp. my *Logik der Forschung* ⟨Popper, 1935⟩, and “The Poverty of Historicism, III”, ⟨Popper, 1945b⟩, esp. pp. 78ff.

[6] The theory of truth underlying these remarks has been developed by Tarski in his analysis on the concept of truth (in Polish, ⟨Tarski, 1933b⟩; in German, “*Der Wahrheitsbegriff in den formalisierten Sprachen*”, ⟨Tarski, 1935a⟩).

[a] Originally: “to”.

which is part of this intuitive idea is that, if a certain inference is valid, all other inferences of the same logical skeleton or form must be valid too. It is this fact which allows us to lay down *rules of inference* which give a description of the logical form of an argument. A rule of inference asserts that from premises of a certain kind, a conclusion of a certain kind can be deduced.

We shall call such a rule of inference "valid" if, and only if, every inference drawn in observance of the rule is valid.

The fact that, whenever an inference is valid, there will also be a valid rule of inference (describing either the skeleton or the form of the argument; e.g., "Barbara") is also an immediate consequence of our second and of our third definition.

Ultimately I may mention very briefly, as a further advantage common to our second and third definitions, the fact that they can be both easily extended so as to be applicable not only to statements but also to *statement functions*.

A statement function (e.g. "He is green") can be obtained by replacing a descriptive sign ("Joe") of a statement by an appropriate variable, e.g. a pronoun ("He"). Such a function is neither true nor false. Nevertheless, it is useful for certain developments
268 to treat, say, |

(a–)

All kittens are green
He is a kitten

He is green

as a valid inference.

In order to extend our second and third definition so as to cover such cases, their wording need not be changed. All that is necessary is to include statement functions wherever we have so far considered statements, or groups of statements, and to specify, if necessary, the real statements – i.e., those which alone can be true or false – by some adjective; we shall call them, say, "proper statements". Accordingly, a *true* or a *false* example of the same logical form or skeleton as (a–) can only be an example consisting of proper statements, since functions can be neither true not false; and the same will hold for a "form-preserving interpretation whose premises are all true", etc.: such an interpretation will have to consist of proper statements.

We turn now to the weak points of our definition (D2), and the main reason for preferring the third definition to the second. The main difference between the two definitions is that (D2) refers to other arguments *of the same logical skeleton* as the argument in question and thereby confines its reference to other arguments *belonging to the same language*; (D3), on the other hand, refers to all form-preserving interpretations and therefore to *an unspecified number of different languages*, viz., to all those into which the formative signs can be properly translated. We shall show that this difference has two consequences:

(1) Our second definition (D2) is satisfactory only if the language *L* to which the argument under consideration belongs is sufficiently rich in descriptive signs.
(2) Our third definition is, besides, superior to the second because it yields the immediate consequence that the validity or otherwise of an inference or rule of

inference is independent of the language in which it is formulated, in the sense
269 that if it is valid in one language, then every one of | its proper form-preserving translations into another language will be valid also.[7]

While point (2) does not need an explanation, point (1) does, and the rest of this section will be devoted to construct a simple example of a language *L* in which, owing to the poverty of its dictionary, an invalid inference would appear as valid from the point of view of (D2), simply because no counter-example exists within *L*.

Let us assume a language *L* with the usual formative signs, “all”, “some”, “is”, “are”, “not” etc. We may also include the words “and”, “or”, as well as the prefix “non-” (e.g. say, “non-tall”) and the word “who” among the formative signs, so that we may construct out of the simple descriptive signs “tall”, “fair”, and “Greeks” such complex signs as “fair or tall”, “non-tall and fair”, “Greeks who are tall”, etc., which are made up of both formative and descriptive signs.

As to the descriptive dictionary of *L*, we may admit proper names of persons,
270 such as “Socrates”, etc., and | property-names or class names such as “Greeks”, “Englishmen”, “Frenchmen” etc., as well as, say, “tall”, “short”, “wise”, “foolish”, “fair” and “dark”. The main point is that our descriptive dictionary must not contain any *simple* descriptive name synonymous with, say, “short and foolish” or “short and not-foolish” etc.; nor must it contain any simple descriptive name of a property which is common to all persons. (We may consider the class of all persons as our universe of discourse; and we may say that we must not have in *L* a *purely descriptive* name of the universal class.) The upshot of all this is that we achieve in this way that our language does not possess two different simple names of two classes of which the one is included in the other.

Consider now this example of an inference in *L*:

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[7] Cp. the end of section (1) and note 4. It is even possible to avoid the words “form-preserving” here, by a further generalization of (D3). For this purpose, we may replace the word “valid” in (D3) – or, for that matter, in our final definition (D6) – by “*formally valid*”, and then add the following definitions of “*informally (or materially) valid*” and of “valid”; “An inference is informally valid if, and only if, it is formally invalid and there exists a proper translation which is formally valid.” – “An inference is valid if it is formally or informally valid.” – An example of an informally valid inference is:

(a–)

Peter’s speed surpasses Richard’s
Richard’s speed surpasses Spencer’s
____
Peter’s speed surpasses Spencer’s

For a formal counter-example, replace “speed” by “dog” and “surpasses” by “passes” or “surprises”. On the other hand, we may say that the argument is properly translated if we replace “surpasses” by “is greater than” and that in this translation it is formally valid. What is often called the “rational reconstruction” of an argument is usually an attempt to show – e.g. by giving translation rules such as definitions, etc. – that it is informally valid. Of course the question whether a certain argument is informally valid or not can hardly ever be answered without further assumption of a more or less questionable character, such as intuitive translation rules – and it is hardly possible to give an answer in the negative except on intuitive grounds. (Informal inference in our sense, it may be noted, has played a certain role in the discussions of the idea of “entailment”.) In the remainder of the present paper, we shall confine ourselves to formal validity without explicit mention of the word “formal”.

(b′)

All Greeks are wise
Socrates is wise

Socrates is a Greek

This is, clearly, of the same form as (b) and (b+) and should therefore be recognized as *invalid*. However, *no counter-example can be formulated within L*. For, a counter-example in L must have true premises of the same skeleton as (b′). But although true universal statements such as "All Greeks are tall or non-tall", "All Greeks are non-Frenchmen", or "All Greeks who are tall are non-short" etc., exist in L, no true statement with the simpler skeleton "All . . . are —" happens to exist. (There exist true statements with the skeleton "All . . . are . . ." in L, but only such as would make the conclusion true.) Thus, within L, no counter-example exists to (b′), and (b′) would have to be described as valid from the point of view of our second definition, contrary to our logical intuition, and contrary also to our third definition which in this point and, it seems, in all others, agrees with our logical intuition.[8] (Of course, if we enrich the descriptive vocabulary of L a little, for example, by introducing the new simple term "personalities", defined, say, by "tall or short or both", then we can at once give a counter-example within L.)

(4)

Our third definition is, I believe, perfectly satisfactory on intuitive grounds. Nevertheless, there are serious objections. These were first urged by Tarski against his own definition.[9]

The weak point of our definition is this. Our definition is based, in the last analysis, upon the fundamental *distinction between formative and descriptive signs*. (Of all the technical terms introduced by us, only "statement-preserving interpretation" – and, of course, "interpretation" – do not presuppose this distinction.) Now this distinction is an intuitive one, and accordingly vague and uncertain. For example, the words "greater" and "smaller" (and "equal") are usually classed (with "identical" and "different") as

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[8] The fact that a definition such as (D2) cannot be satisfactorily applied to languages with a poor
271 dictionary is mentioned in Tarski's lecture quoted | here in note 1. Tarski's own definition (which was the starting point of my investigation) avoids this drawback by combining, as it were, features of (D1) and (D2). In our terminology, it might be perhaps rendered: "An inference is valid if, and only if, every state of affairs which satisfies the logical skeleton of the premises satisfies that of the conclusion." And by a "state of affairs which satisfies a logical skeleton" (Tarski would call it a "model" of the skeleton), we would have to think of real things, and their properties and relations (not of the *names* of things, or of properties, or of relations, for these may be wanting). Tarski's definition is free of the disadvantage (1) discussed in the text, but since it is bound to the logical skeleton of some (one) language L, it does not fully share with (D3) the advantages (2) and the possibilities discussed in the foregoing footnote – at least, not without something like a "rational reconstruction" which might assimilate it to our conceptual apparatus.

[9] For Tarski's definition, see notes 8 and 1.

formative, while the intuitively similar words “taller” and “shorter” are classed as descriptive. This may be all right, but no satisfactory principle is forthcoming by which the classification can be justified.

Besides, it is by no means the case that the distinction can be drawn in every language. Thus most verbs, such as “runs” combine formative and descriptive functions
272 (of “is” and “running”). But this is only another way | of saying that the distinction is not applicable to “runs”. Since we have languages in which statements occur (“Achilles runs”) to which the distinction is inapplicable, it is clear that we may have whole languages without any term which strikes us intuitively as either purely formative or purely descriptive.

That there is something more needed than a reliance on an intuitive classification of the sign of a language may be illustrated by this example:

(c)

Bob sits in the cinema in the same row as Pat, and to the left of Pat
Pat sits in the cinema in the same row as Tim, and to the left of Tim

---

Bob sits in the cinema in the same row as Tim, and to the left of Tim.

This seems, intuitively, valid; even after constructing something like a counter-example by substituting “surprise” for “left” do we feel, intuitively, that (c) may be in order; we feel that the apparent counter-example only sounds like one, and that “to the left of” and “to the surprise of” have really not the same logical form, because there is something formative in “left”, as it were. But it is not encouraging to find that we have no principle at our disposal that may help us to clear up the matter directly.

One thing we have to do is to operate with a somewhat restricted list of formative signs, and to make sure that there is no ambiguity in our use of these formative signs. Aristotle moved first in this direction, by restricting his investigation to the language of “categorical propositions”. He introduced thereby an artificial limitation and rigidity which is quite foreign to naturally grown languages, but, it seems, necessary if we wish to construct a theory of inference. A language with such an unnatural rigidity of its rules may be called an “artificial language” or “calculus”. Modern logicians have followed Aristotle in confining their discussions to one or the other artificial language or calculus, and the method, I believe, is practically unavoidable.

273 | But it does not solve our fundamental problem. The logician who constructs an artificial language usually lays down a list of formative signs whose meanings he explains, and he treats all signs which are not in this list, and not definable on the basis of the list, as descriptive. This method is very successful and need not be criticized. But the choice of his signs (and their meaning) appears very largely as arbitrary, or as justified mainly on unanalysable intuitive grounds.

On this choice, however, everything depends in logic. It is clear that, by transferring a sign – e.g., the word “left” in our last example – so far considered as descriptive, into the list of formative signs, or *vice versa* (without altering the intuitive meaning attached to it), we may alter the logical form of some, or even of all our arguments, with the result that arguments so far valid become invalid, and *vice versa*. And this can be done as long as we have no objective criterion of the formative or descriptive character of a sign.

Our objection may be summed up as follows:

The proposed definition of valid inference may permit us to reduce the intuitive problem whether a certain inference is valid or not to the problem whether a certain sign is formative or not. But this problem remains intuitive.

I do not intend to create the impression, by this formulation, that I consider our definition of validity as futile. On the contrary, it is very valuable. Nevertheless, the situation is serious. If there is no clear-cut distinction between valid and invalid inferences, then, it can be shown, there is also no clear-cut distinction between logical and factual (or empirical) statements, or, to use a more traditional terminology, between analytic or synthetic propositions. This is a question of major philosophical importance. One might even say that it involves the whole question of empiricism, and of scientific method.[10]

(5)

274 | Is our problem insoluble? I do not think so. One way to a possible solution follows here in outline. It is explained, more fully, in the subsequent sections.

There are inferences – admittedly, so trivial ones that they have hardly ever attracted much notice – which can be shown, on our definition of validity, to be valid *whatever the logical form of the statements involved*. Their validity is thus independent of that distinction between formative and descriptive signs which we have seen to be of such a problematic character. We shall say of these inferences that they are *absolutely valid*. Absolute validity can be defined in terms of statement-preserving interpretations, and therefore without referring to the distinction between formative and descriptive signs.

Next we observe that once we have a certain system of absolutely valid rules of inference at our disposal, it is possible to define the logical force or import of the various formative signs in terms of deducibility (i.e., in terms of the predicate: "from the statements a, b, c . . ., the statement d *can be deduced*"). We shall call a definition of a formative sign in terms of this relation an *inferential definition*.

With the help of the idea of an inferential definition, we can characterise the formative signs as signs which can be defined by an inferential definition. In this way we can meet the objections raised against the distinction between formative and descriptive signs, and re-establish (D3), as it were. Ultimately, we shall propose a definition (D6) which makes use only of the ideas of absolute validity and of an inferential definition, and which no longer refers to truth.

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[10] These important problems, raised by our (or rather by Tarski's) definition of valid inference, have been only little discussed, except by Tarski himself – who seems to consider the problem insoluble – and by Carnap, who takes a more hopeful view of the matter; cp. R. Carnap, *Introduction to Semantics* ⟨Carnap, 1942⟩, esp. p. vii, where "the distinction between logical and descriptive signs" is mentioned, and pp. 56ff.

(6)

There are inferences which are valid according to all our definitions, in spite of the fact that the logical form of the statements involved is irrelevant. They are very trivial inferences indeed – so trivial that some logicians refused to accept them. Accordingly, our first task will be to argue that they are valid.

275 | Consider the example:

(d)

$$\frac{\textit{Joe is a kitten}}{\textit{Joe is a kitten}}$$

From the point of view of all our definitions (and from the point of view of most modern systems of logic) this is definitely a valid inference – even though it is something like an extreme case, or a "zero-case", as it were. For it is clear that no counter-example of (d) can exist. Some logicians have objected to calling these cases "valid inferences" on grounds such as these: inference is a movement of the mind; but our mind does not move in such cases as (d); thus this cannot be an inference. But since even these logicians hardly suggest that (d) is actually invalid, not much harm can be done by calling it an inference. It seems that their objections are due to the fact that in such zero-cases our intuition is not a reliable guide. (Is zero a number, or is it not, rather, nothing? Is one a number? No, one thing is just one thing, and not a number of things! Even two is not a number, but a couple. A number of things are at least three, it seems.) But there are several reasons why we should call (d) a valid inference – quite apart from our definition.

One is that it would be awkward to forbid that the middle and major term of a syllogism may ever coincide, as in the example:

(e)

$$\frac{\begin{array}{l}\textit{All kittens are kittens}\\ \textit{Joe is a kitten}\end{array}}{\textit{Joe is a kitten}}$$

But if (e) is an inference (and, of course, valid), then we may clearly omit the first premise as redundant; and in this way we obtain (d).

But there are stronger reasons for accepting (d). There are very strong reasons for not excluding the possibility of cases of *mutual inference* (i.e., from a premise to a
276 conclusion | and also from this conclusion to its premise). A mutual inference can, of course, have only *one* premise (since an inference can have only one conclusion), but we can hardly forbid inferences from one premise, since we can combine several premises into one by merely joining them with the help of the word "and". If we forbid mutual inference, then we can never be sure whether an inference, however convincing, is valid; for if anybody should later discover an equally convincing argument running from the conclusion to the premise (or to the conjunction of the premises) then we would have to declare both inferences invalid.

But if mutual inferences are possible, then (d) must be possible too. For there is a principle of inference which has never been challenged – the transitivity principle which says that the conclusion c of a conclusion b of the premise a is itself a conclusion of the premise a; and this principle if applied to a mutual inference, yields:

(6.1) "From the statement a we may deduce a",

that is, a rule of inference of which (d) is an observance. In other words, whoever is not prepared to admit (d) and (6.1) must either forbid mutual inference, which has the most awkward consequences, or he must give up the following transitivity principle:

(6.2) If from the statement a we can deduce b, and if from b we can deduce c, then we can also deduce c from a.

We shall therefore rely on our definition of validity and accept (d) and (6.1) as valid.

But the validity of (d) and (6.1) does clearly not depend on the distinction between formative and descriptive signs. *The fact that no form-preserving counter-example exists is here a direct consequence of the fact that no statement-preserving counter-example exists* – together with the fact that, of course, all form-preserving interpretations must be statement-preserving.[11]

Thus there are valid inferences whose validity does not depend upon the distinction
277 between formative and | descriptive signs. We shall call these inferences "absolutely valid"; and we define, tentatively:

(D4) *An inference is absolutely valid if, and only if, every statement-preserving interpretation whose premises are true has a true conclusion.*

Or alternatively:

(D4′) *An inference is absolutely valid if no statement-preserving counter-example of it exists.*

The field of absolutely valid inferences, and of absolutely valid rules of inference[12], covered by this definition, is somewhat trivial but not at all unimportant. It can be shown[13] that all absolutely valid rules of inference which we shall need for the logic of statements – and there are many and even somewhat complicated rules among them – can be reduced to two – the one a generalization of (6.1) and the other ⟨a⟩ generalization of (6.2). By "reduced", I mean here: every inference which is an observance of some of the rules in question can be shown to be an observance of these two rules – obtained, possibly, by applying them many times in succession, which is permitted by the generalized transitivity rule (6.2g) itself.

In order to formulate these two rules more easily, we introduce the following abbreviation: we shall write

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[11] The definition of "statement-preserving counter-example" (cp. "form-preserving counter-example") is obvious, in view of section (2).

[12] The definition is again obvious: a rule of inference is absolutely valid if, and only if, every inference drawn in observance of it is absolutely valid.

[13] Cp. my "New Foundations for Logic," forthcoming in *Mind*, 1947 ⟨as Popper, 1947c⟩.

$$\text{“}a, b, c \ldots / d\text{”}$$

instead of “from the premises $a, b, c \ldots$, the conclusion d can be deduced”; and we shall write

$$\text{“}a_1, a_2, \ldots, a_n / b\text{”}$$

instead of “from the premises $a_1, a_2, \ldots, a_n$, the conclusion b can be deduced”. (Note that our variables “a”, “b”, “a_1”, etc., are variable *names* of statements, i.e., that only *names* of statements may be substituted for them, but not the statements themselves; this distinguishes these variables from the so-called propositional variables of the calculus of propositions.)

278 | With the help of this notation, we can write our two generalized rules:

(6.1g) $$a_1, a_2, \ldots, a_n / a_i,$$

provided a_i is identical with one of the n statements $a_1, a_2, \ldots, a_n$ (in symbols: provided that $1 \leq i \leq n$).

(6.2g) If, from the n premises $a_1, a_2, \ldots, a_n$ taken together, each of the m statements $b_1, b_2, \ldots, b_m$ can be separately derived, then,

$$\text{if } b_1, b_2, \ldots, b_m / c \text{ then } a_1, a_2, \ldots, a_n / c.$$

These two rules, or rather the system consisting of these two rules, can be easily shown to be absolutely valid and therefore, *a fortiori*, valid. They are all the absolutely valid rules needed for the proof that our final definitions are adequate for the logic of proper statements. In order to extend our results to statement functions, we need another set of trivial rules which can easily be shown to be absolutely valid, and which allow us to operate with the idea of *the result of substituting, in some statement function one variable for another*. In order to indicate the character of these rules, I shall use the symbol “$a\binom{x}{y}$” as an abbreviation of “the result for substituting the variable y for the variable x in the statement function a”.[14] The trivial rules mentioned are all of the character of

(6.3) $$\text{If } x = y, \text{ then } a / a\tbinom{x}{y} \text{ and } a\tbinom{x}{y} / a.$$

The triviality of this rule will be realized by considering that, if $x = y$, the result of substituting y for x in a leaves a completely unchanged; or in other words, that

(6.4) $$a\tbinom{x}{x} / a.$$

Accordingly, $a\binom{x}{x}$ and a will be mutually deducible, whatever the logical form of
279 a may be; and this is what | (6.3) asserts. If we abbreviate “mutually deducible” by “//”, defining:

(6.5) $$a // b \text{ if, and only if, } a / b \text{ and } b / a,$$

then we can write (6.3) as follows

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[14] The symbols “x” and “y” are (variable) *names* of variables.

$$a\binom{x}{x}//a. \tag{6.6}$$

The triviality of (6.6) is, in view of (6.4), quite obvious.

The rules of inference (6.3) and (6.6) are clearly not only valid, but absolutely valid. For there can be no interpretation which preserves statements, including statement functions, and which renders a by a true statement, but not $a\binom{x}{x}$ which, in view of (6.4) is only another name for a.[15]

Our concept of absolute validity, we have seen, is independent of the problematic distinction between formative and descriptive signs. In addition, it is also possible to define it in such a manner that the much less problematic *concept of truth is also avoided*. For it turns out that a great many properties of statements can replace "true" in our definition – among them such trivial properties as "containing less than five words (or signs)", for which we shall briefly say "short". The properties which may replace truth can be called *freely interpretable properties*, or briefly, *free properties*. Intuitively speaking,[16] a property is called "free" if we are free to choose (by an
280 appropriate choice | of the translation rules) for any individual statement a_1 of L_1, whether or not it shall be interpreted by a statement which possesses the property in question. The strict definition of "free property" (given in note 16) makes use of the term "statement-preserving interpretation", but does not use any specific name of a free property, such as "short" or "true".

Now it can be shown[17] that if every statement-preserving interpretation transmits *one* free property in a certain argument, then it transmits *all* free properties in this argument. This allows us to replace (D4) by:

(D5) *An inference is absolutely valid if, and only if, there exists at least one free property such that every statement-preserving interpretation whose premises all possess this free property has a conclusion with the same free property.*

This definition avoids the term "truth"; but in the presence of the definition of "free property", and in view of the fact that "true" designates a free property, we can obtain (D4) from (D5).[18]

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[15] Note that, in these considerations, we never assume anything about the logical form of a (not even that it is a function rather than a proper statement). For a list of similarly trivial rules which are needed for developing the logic of functions and which can all be shown to be absolutely valid, see rules (6.1) to (6.6) of my *New Foundations for Logic* ⟨Popper, 1947c⟩. Cp. note 13.

[16] The strict definition is: "A property of statements is a free property if, and only if, to every arbitrary division of the statements of L_1 into two exclusive classes A and B, there exists a statement-preserving interpretation which interprets all statements of A by statements which possess the property in question and all statements of B by statements which do not possess it." – As an illustration, I may also mention two examples of properties of a statement a_m which are *not* free: (1) "a_m is a statement of L_m." – (2) "a_m does not occur in text x after b_n."

[17] With the help of the fact that the relation designated by "statement-preserving interpretation" is transitive.

[18] The transition from (D4) to (D5), including its effect on (D6), corresponds roughly to the transition from Truth Tables to abstract Matrices, in the sense of Łukasiewicz and Tarski. In Carnap's terminology, it may be described as one from *General Semantics* to *General Syntax*, since "true" is a Semantical concept, while "short" is Syntactical; "free property" is also Syntactical, for it is defined merely with the help of "statement-preserving interpretation" which, in view of section

Absolute validity cannot replace validity; its main advantage is that its definition is free from the objections we raised (following Tarski's suggestions) against our definition of validity. But before going any further, we may ask whether there are not analogous objections left. Admittedly, absolute validity does no longer depend on the
281 distinction between formative and descriptive signs. But | does it not depend upon the distinction between statements and non-statements? And is this distinction not as intuitive as the other?

The answer to this last question must be "yes". But the situation is, nevertheless, not analogous to the one we have laboured so hard to avoid. Intuitive doubts about the distinction of formative and descriptive signs may arise even in connection with an artificial language. As opposed to this, any doubt whether or not a certain expression of an artificial language is a statement can hardly arise on intuitive grounds. (If at all, then it arises as a very definite problem of reconstruction, for example, in order to avoid certain paradoxes.) But the main difference between the two problems is this: the distinction between formative and descriptive languages affects our central problem – validity. A given inference may be valid or invalid, according to the way we draw the line. This is never the case with the other distinction. It cannot affect the decision as to the validity or invalidity of some given inference; it can only affect the question whether a certain sequence of expressions is an inference (valid or invalid) or no inference at all.

The problem of validity is always: given that this is an inference – is it valid? It is clear that the two questions – the one of characterizing the formative signs, the other of characterizing the statements – have a very different status relative to this problem. And while the solutions of the problems of validity and of formative signs are closely interdependent, I do not believe that the problem of validity and that of statements are likely to affect each other in any way.[19]

(7)

Absolute validity cannot, of course, replace validity. Its definition may be free from flaws, but this advantage is bought at a high price. Absolute validity, one is tempted to say, is a property of inferences which are absolutely trivial.

282 | We have therefore to return to the problem of validity, and with it, to that of formative signs. But we return to these problems with means of solving them. By our definition of absolute validity, we have acquired the right to operate freely with all

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(1), appears to by a Syntactical term (as opposed to "form-preserving interpretation" into whose definition the Semantic idea of a proper translation enters). But, it may be asked, what about our assertion that truth is a free property? And what about the derivation of (D4) from (D5)? Altogether, I am very doubtful whether the distinction between Semantics and Syntax really fits our approach which is very largely meta-metalinguistic. (Our term "valid" belongs to the meta-metalanguage.) Yet we may perhaps describe the transition in question as a change of emphasis from the Semantics of Semantics to the Semantics of Syntax.

[19] Cp. note 4.

kinds of rules of inference, *as long as these do not refer to the logical form of the statements involved.*

With these means at our disposal, we can not yet define what we mean when we say:

"a is the negation of b"

or perhaps

"a is the disjunction of b and c".

But we do possess the means of defining what we mean when we say:

"a has the same (logical) force as a negation of b"

or perhaps

"a has the same (logical) force as a disjunction of b and c".

We can express the phrase "a has the same (logical) force as b" (or "a is equivalent to b") with the help of the symbol "//", by "$a//b$", which has been defined above (6.5) as mutual deducibility. There is an alternative definition of equivalence, viz.:

(7.1) $a//b$ if, and only if, for every c: a/c if, and only if, b/c.

Now just as this defines the phrase: "a has the same force as b, *whatever the logical form of a and b may be*", so we can also define the phrase: "a has the same force as the negation of b, *whatever the logical form of a and b may be*" by:[20]

(7.2) $a//$the negation of b if, and only if, for every c: $a, b/c$ and, if $a, c/b$ then c/b.

283 | Similarly we can define:

(7.3) $a//$the disjunction of b_1 and b_2 if, and only if,
for every c_1 and c_2: $a, c_1/c_2$ if, and only if, $b_1, c_1/c_2$ and $b_2, c_1/c_2$.

I shall not discuss the adequacy of these and the other definitions mentioned here, since this follows from material I have given elsewhere[21]. I may mention, however,

[20] (7.2) defines classical negation; if we replace the last phrase of (7.2) by "if $b, c/a$ then c/a" we obtain the negation belonging to Heyting's intuitionistic calculus. Both definitions can be proffered at the same time, but if in one language, a classical as well as an intuitionistic negation exists of every statement, then the latter becomes equivalent to the former, or in other words, classical negation then absorbs or assimilates its weaker kin. This remark follows from the observations made in my paper quoted in note 13 upon rule 4.2e, and modifies one made in that paper; but the negation belonging to Johansson's "Minimum Calculus", is not absorbed by classical negation; and the same holds for the following definition of a negation which seems to agree excellently with intuitionistic intentions, although it is not equivalent with that of Heyting's calculus: "$a//$the impossibility of b if, and only if, for every c: a/c or b/c, and if b/a, then c/a." (This definition is no longer "purely derivational"; this fact marks the transition to modal logic.)

[21] Cp. note 13. For propositional logic, one definition – apart from (7.1) – is sufficient: "$a//$the alternative denial of b_1 and b_2 if, and only if, for every c_1: c_1/b_1 and c_1/b_2 and $b_1, b_2/c_1$, if and only if, for every c_2: $a, c_1/c_2$ and if $a, c_2/c_1$ then c_2/c_1." Combined with (7.4) this suffices for functional logic; and combined with the definition at the end of note 20, for modal logic (whether propositional or functional).

that the two definitions (7.2) and (7.3), form a sufficient basis for propositional logic – say, for Russell's system, and that we can give similar definitions for all the known compounds of propositional logic.

In order to provide for functional logic, we define universal and existential quantification by a similar method. One of them is sufficient, in the presence of (7.2); we choose existential quantification:[22]

(7.4) Provided x is distinct from y,
$a\binom{y}{x}$ //the result of the existential quantification with respect to x of $b\binom{y}{x}$ if, and only if, for every c: $a\binom{y}{x}/c\binom{y}{x}$ if, and only if, $b\binom{x}{y}/c\binom{y}{x}$.

The definitions given here form a sufficient basis of propositional and (the lower) functional logic, in a sense which will be explained; but they are given only as
284 illustrations of our method. We can, by this method, give many | more definitions; not only of the logical force of certain formative signs, but also definitions of such matters as the exclusiveness (or contradictoriness) of a number of statements, and of their exhaustiveness (or "logical disjunctness", to use Carnap's expression). For simplicity's sake, we confine ourselves to two statements, a_1 and a_2:

(7.5) a_1 and a_2 are exclusive if, and only if,
for every b_1 and b_2: if b_1/a_1 then, if b_1/a_2 then b_1/b_2.

(7.6) a_1 and a_2 are exhaustive if, and only if,
for every b_1 and b_2: if a_1/b_2 then, if a_2/b_2 then b_1/b_2.

These concepts[23] are interesting since they provide us with a definition of complementarity:

(7.7) a//the complement of b if, and only if, a and b are exhaustive as well as exclusive.

Now on the basis of (7.2) and (7.7) it can be shown that, if a_1//the negation of b, and a_2//the complement of b, then $a_1//a_2$.

Had we known this before, we need not have defined "complement"; "negation" would have done just as well. Until, however, an equivalence between two definitions such as (7.2) and (7.7) is established, we must always be careful to use different names. *But this, indeed, is the main precaution necessary.* We need not make sure, in any other way, that our system of definitions is consistent. For example, we may define (introducing an arbitrarily chosen name "opponent"):

(7.8) a//the opponent of b if, and only if, for every c: b/a and a/c.

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[22] It is understood that two rules are at our disposal (because of their absolute validity) which are described, in my "New Foundations," as "rules of substitution", and which read, in the notation of that paper:

"$(Eyb)\binom{y}{x}//Ex(b\binom{y}{x})$" and "If y/z then: $(Ezb)\binom{y}{x}//Ez(b\binom{y}{x})$".

Alternatively, a supplementary definition yielding these two rules may be added to (7.4).

[23] Their extension to n statements may be indicated by: "$a_1, a_2, \ldots, a_n$ are exhaustive if, and only if, for every b_1 and b_2, if a_1/b_2 then, if a_2/b_2 then, ... if a_n/b_2, then b_1/b_2."

This definition has the consequence that every language which has a sign for "opponent of *b*" – analogous to the sign for "negation of *b*" – will be inconsistent (i.e. every one of its statements will be paradoxical). But this need not lead us to abandon (7.8); it only means that no consistent language will have a sign for "opponent of *b*".

285 | Each of our definitions gives rise to a number of rules of inference. For example, (7.1) gives rise to the rule:

(7.91) If $a/\!/b$, then b/a.

Similarly (7.2) gives rise to

(7.92) If $a/\!/$the negation of b, then if $a, c/b$, then c/b.

And (7.3) gives rise to

(7.93) If $a/\!/$the disjunction of b_1 and b_2, then b_1/a and b_2/a.

To every of these rules we may add: "*whatever the logical form of the statements involved may be*".

All these rules are absolutely valid. (That is to say, if they are added to 6.1g and 6.2g, the resulting system is absolutely valid.) Why? Because no statement-preserving counter-example can be found. Consider (7.91): since we have laid down, in effect, that we can replace "$a/\!/b$" by "a/b and b/a", we can in (7.91) replace "If $a/\!/b$ then" by "If a/b and b/a then"; and it is clear that no statement-preserving counter-example can be found to any inference drawn in observance of the resulting rule. The same holds for all other rules of this kind.

Since we may consider the definitions themselves as rules, we may say, therefore, that they are all absolutely valid. Whether the concepts they define are adequately defined, is a different matter; this will depend upon our intentions. If, for example, we intend to define conjunction (or rather, its logical force) and use the right hand side of our definition of disjunction (7.3) for this purpose, then we shall have an inadequate definition. But the definition will, nevertheless, define something – namely just what we usually would call "disjunction" rather than "conjunction".

So much about these definitions. We now turn to the problem of formative signs, and of valid inference. We define:

(7.2$^{\mathrm{D}}$) The language L_1 contains a (preceding or succeeding) *sign of negation* if, and only if, it contains a sign which, if joined to any statement b of L_1 (placed before b, or placed after b) forms a new statement a of L_1, such that this resulting statement $a/\!/$the negation of b, in the sense of (7.2).

286 | Note that, according to this definition, L_1 may contain more than one sign of negation.

(7.3$^{\mathrm{D}}$) The language L_1 contains a (preceding, or intervening, or succeeding) sign of disjunction if, and only if, it contains a sign which, if joined to any pair of statements b_1 and b_2 of L_1 (placed before b_1 followed by b_2, or placed between b_1 and b_2, or placed after b_2 preceded by b_1) forms a new statement

a of L_1, such that this resulting statement $a//$the disjunction of b_1 and b_2, in the sense of (7.3).

In the same way, we may define the other formative signs; we can define them as *signs which, in certain contexts, form statements of a defined logical force.*

Definitions such as (7.2^{D}) and (7.3^{D}) may be called inferential definitions. They are characterized by the fact that they define a formative sign by its logical force which is defined, in turn, by a definition in terms of inference (i.e. of "/").

It is now easy to define the term "formative sign".

A sign s of a language L_1 is a formative sign if, and only if, s can be defined by an inferential definition.

According to this definition of the term "formative sign", the question whether a certain sign *s* used in a certain way is formative remains an open question until someone either produces an inferential definition of *s* or shows, by some method or other, that such a definition does not exist (as can indeed be done in certain cases, for example, the negation sign of Johansson's minimum calculus). But the remark that this question may remain open does not constitute an objection against our definition; and especially the fact that inferential definitions of the intuitively recognized formative signs of propositional, functional, and even modal logic can be given (as has been shown) establishes, it seems, the adequacy of our definition of the term "formative sign".

I believe that this definition solves, fundamentally, our crucial problem. It provides a rational basis for the distinction between formative and descriptive signs; and with this, the objections of section (4) can now be met. Our definition achieves,
287 however, more than this result. It | also shows the rationality of the idea of a form-preserving interpretation, since it makes it clear that a form-preserving interpretation does not depend on the intuitive knowledge possessed by a translator – his intuitive knowledge of the meaning of the formative signs of a language. For we see now that precise translation rules for formative signs can be obtained from their definitions, because they are defined with the help of rules of inference which are absolutely valid, and because absolute validity, in its turn, depends merely upon the idea of statement-preserving interpretations, which does not presuppose a knowledge of languages.

We can therefore now adopt the *wording of* (D3) *as our final definition of valid inference.* Of course, the words of (D3) have, in view of our new results, acquired a somewhat different meaning, namely, a more precise one.

But it seems that we can, if we wish, go further; it seems that we can avoid, in view of (D5), even the reference to truth which occurs in the wording of (D3), by defining:

(D6) *An inference is valid if, and only if, it is either absolutely valid, or it can be shown, on the basis of the inferential definition of the formative signs, to have been drawn in observance of absolutely valid rules (including those which define the logical force of the statements involved).*

The wording of this definition should be capable of some improvement, but the idea, I think, is clear; and considering the actual techniques of establishing valid rules of inference (indicated in my paper quoted in note 13) it seems to be adequate. A new problem arises, however, in connection with this definition, viz. to prove, as can be done in the case of (D5) and (D4), that (D6) actually yields (D3), that is, guarantees the transmission of truth. But this problem must be left for another occasion.

(8)

An objection which may have troubled the reader for some time may now be briefly discussed. Is not our procedure rather circular? We assert that the validity of the
288 logical rules of inference *follows* from certain definitions. | This may be so; but does not this *derivation* assume the validity of some rules of inference – and probably just of those which are to be derived?

This objection cannot be discussed here in full, for that would mean a disquisition on the distinction between *languages* and *metalanguages*, of the general status of any metalinguistic investigation, and, more particularly, of any logical investigation. But a very brief answer may be attempted.

If we investigate such problems as the way in which two statements, *a* and *b*, are related – whether the one follows from the other, or contradicts the other, or is complementary to it, etc. – then we are investigating certain objects which are parts of some language, i.e. *linguistic objects*. If we wish to study linguistic objects, we must discuss them, and make statements about them, as with all other objects. If we use our language in order to discuss linguistic objects, then we say that we use our language as a *metalanguage*. The language which we study is called the *object language* (or language under investigation).

Now it is important to realize that we cannot at the same time *use* a word, or a statement, or an argument, and *study* it; and also that the study of words or statements presupposes the unhampered, although careful, use of some language in precisely the same way as the study of trees or of mental processes or of music.

It is for this reason that we should never attempt to reflect on our metalanguage *while* we are analysing an object language; and it is for this reason that we should not attempt to analyse the arguments we are using while engaged in analysing the rules of argumentation.

Indeed, if this were not so, then *all* logical investigation would be impossible. For if we wish to study something, we can, clearly, not begin by giving up the use of all argumentation; and logic is no exception to this rule.

In deriving rules of inference from definitions, we cannot, of course, avoid *using* inferential arguments, just as in studying plants or animals. *After* we have done our
289 job, we may | then analyse the inferential arguments used. This is a legitimate and interesting new problem, but it is quite different from the original problem – that of deriving certain rules from certain definitions.

If we actually analyse (using a meta-metalanguage) the procedure used before in the metalanguage, then we find that there is no circularity involved. Surely, we must *know how* to use our metalanguage when studying the rules of inference of certain object languages (as we did), and surely, we must know how to draw inferences in the metalanguage while engaged in these studies. But we do not formulate the rules we are using, and *these rules, therefore, never appear as premises in our derivations*. But only if they did could we speak of circularity.

This situation would not be affected if, for example, we might have to *use* negation (say, the word "not") while *discussing* negation. But we actually find, when we investigate our metalanguage, that it is possible to carry out our investigations in the metalanguage without using negation or rules of inference pertaining to negation; and this example, surely, reaffirms the view that our investigation is not circular.

(9)

We have, so far, only indicated how the well-known *rules of inference* of logic can be derived, not from assumptions such as primitive rules, but from definitions. In conclusion, I shall briefly mention how the *logical theorems* of a language (for example the well-known tautologies of the calculus of propositions, including, of course, the primitive propositions or axioms) can be obtained from our definitions without further assumption (such as the assumption of primitive propositions or axioms).

The procedure is based on a new definition – the definition of logical demonstrability, or more precisely, of "*a* is (logically) demonstrable":

(9.1) *a* is demonstrable if, and only if, *a* can be validly inferred from any statement *b* whatsoever.

290 | This definition may seem at first intuitively rather puzzling. But it only gives a precise form, in terms of valid inference, to the somewhat vague intuitive idea that a statement *a* is logically demonstrable if, and only if, it is true merely because of its logical form. That *a* is true merely because of its logical form can be expressed, more clearly, by saying that all statements of the same logical form must be true. But this means that no form-preserving interpretation of *a* can be false, and therefore, that no inference with the conclusion *a* can be invalid, whatever the premises may be; for if no form-preserving interpretation of *a* can be false, then it is clear that *no form-preserving counter-example can be found*.

There are other arguments to show that our definition (9.1) is in keeping with the intuitive idea of demonstration or proof, and that it brings out a distinction between derivation (i.e., non-demonstrative inference) and demonstration which has been often neglected by logicians.[24]

A derivation never establishes the truth of the conclusion. It only establishes the

[24] The distinction between derivation and proof has been emphasized by Carnap (in his *Logical Syntax*, ⟨Carnap, 1934a⟩ and ⟨Carnap, 1937⟩, as well as in his *Introduction to Semantics* ⟨Carnap, 1942⟩). Our treatment agrees fundamentally with his results but goes beyond them, in so far as our treatment of the theory of derivation is quite independent of the fact whether or not there are demonstrable statements in the language under consideration, while Carnap's treatment of derivation makes essential use of certain rules of derivation which are only re-formulations of axioms; cp. his *Introduction*,⟨Carnap, 1942⟩ p. 167, and his *Formalization of Logic*, ⟨Carnap, 1943,⟩ p. 9.

fact that the question whether or not the conclusion is true can be shifted back to the question whether or not the premises are all true. Thus, if we find that the conclusion is false, this does not show that the argument, the derivation is invalid – it only shows that at least one of the premises must be false.

With a proof it is different. If somebody tells us that he has proved a, and we find that a is false, then, clearly, something must have been wrong with the argument, with the proof. In other words, we use the word "proof" intuitively in such a way that a proof must be invalid if the proved statement is false. A counter-example establishing
291 | the invalidity of a derivation must consist of true premises and a false conclusion. For a counter-example establishing the invalidity of a proof, it suffices to show that the conclusion (or a form-preserving interpretation of it) is false.

We see that a proof, as opposed to a derivation, does not assert the truth of the conclusion *provided* the premises are true, but that it asserts the truth of the, conclusion *absolutely* – independently of the question whether any particular other statement is true. The dependance upon any particular premise is, as it were, thrown off. Our definition shows how this is possible. If a statement is provable whenever it is derivable from any premise whatsoever then, if we suspect the truth of one premise, we can always replace it by another. The conclusion does no longer depend on any particular premise or set of premises, or on any particular assumption – it is established under all possible assumptions, under all possible circumstances. Since it can be derived from any description of facts, it must be true whatever the facts are, or independently of all facts.

But the strongest reason for accepting (9.1) as a satisfactory definition of logical demonstrability is that, if we accept this definition, we can show, in our metalanguage, on the basis of our definitions and without any further assumption, that all the well-known primitive propositions or axioms of the various logical calculi, and with them, of course, the theorems, are demonstrable.

Besides we can show that those methods which have been usually called proofs (such as the indirect proof, or the *reductio ad absurdum*) are indeed demonstrations in the sense of our definition, that is to say, that they establish the demonstrability of their conclusions, as opposed, for example, to syllogisms, the *modus ponens*, etc., which are not demonstrations but merely derivations.

As an interesting minor result it turns out that the dilemma differs from most of the other classical arguments in being a method or figure of demonstration and not a figure of derivation, such as the syllogism – in spite of the fact that it has been
292 treated, under the name of "*syllogismus* | *cornutus*", as if it had the same status as the categorical or the hypothetical syllogism.

Philosophically of greater interest is perhaps the result that, while valid *demonstrations must not be circular* – they must throw off any dependence upon any specific premise –, *derivations are always circular*; indeed, our analysis shows that all derivations are based upon absolutely valid rules, which are clearly circular (cp. example (d) and rule (6.1) in section 6), and upon inferential definitions, which cannot be the means of establishing anything new. The point is interesting in view of Mill's much discussed criticism of the syllogism.[b] Syllogisms are, of course, circular; they must be so since they are derivations; and any suggestion that the circle is vicious only reveals that they are mistaken for demonstrations.

To sum up, it is possible to construct a general metalinguistic theory of logic, applicable to any ⟨objectlanguage⟩[c], without assuming a distinction between formative and descriptive signs, without assuming anything like the customary primitive rules of inference, or any primitive propositions or axioms; and this construction is based on definitions which, in the last analysis, go back to the somewhat trivial concept of a statement-preserving interpretation.

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[b] Cf. Mill (1843, Vol. I, Book II, Ch. 3, "Of the Functions, and Logical Value, of the Syllogism").

[c] Correction according to § 30.5.1, this volume.

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Chapter 3
New Foundations for Logic (1947)

Karl R. Popper

Abstract This article is a corrected reprint of K. R. Popper (1947c). New Foundations for Logic. In: *Mind* 56 (223), pp. 193–235. Errata and additional footnotes have been inserted from K. R. Popper (1948e). Corrections and Additions to "New Foundations for Logic". In: *Mind* 57 (225), pp. 69–70.

Editorial notes: We have changed (D *n*) to (D*n*) throughout. The footnotes are now numbered consecutively, whereas in the original footnotes were numbered per page. Popper may have discussed the content of this article with Bernays, considering the last passage of their joint work "On Systems of Rules of Inference", Chapter 14 of this volume, where one can also find an explanation of footnote 9. The term "minimum calculus" refers to Johansson's (1937) "Minimalkalkül".

1. Introduction[1]

193 Logic ought to be simple; and, in a way, even trivial. Complications in logic all arise from two sources. Reiterated applications of trivialities may result, in the end, in such complexity that the thread is lost, as it were, and that one has to resort to the laborious method of carefully checking every step in order to ensure that all is well. A minute analysis is thus often made necessary, and this creates the other source of complication – the need for a high degree of precision in the formulation even of trivialities.

[1] This paper is a report and a discussion of some results obtained by the author during the last ten years. Formal proofs are avoided here but they will be published elsewhere. A knowledge of symbolic logic is not assumed; on the contrary, it may be necessary to warn readers against the attempt to identify our theory of compound statements and quantification with the calculi of propositions or functions. Our theory is more general: we do not construct one calculus or language, but we investigate a wide range of languages, among them some that do not contain any distinct symbols corresponding to those symbols which play such a role in the calculus of propositions and in similar calculi.

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It is assumed here that the central topic of logic is the *theory of formal or deductive inference* (or of derivability; or of deducibility; or of logical consequence: all these expressions are here taken to mean the same thing). We shall first attempt to determine this notion of logical derivation or derivability by laying down a few very simple primitive rules for it. This will be done in sections 2 and 3.

In spite of the triviality of these rules, the concept of derivability thus determined turns out to provide an exceedingly powerful instrument. It will be shown that with
194 its help, we can draw up systems of rules of inference which cover not only | the theory of compound statements (corresponding to the "calculus of propositions") but also the theory of quantification, that is to say, of universal and existential statements (corresponding to the "lower functional calculus").

But we shall go even further. Surveying our system of rules of inference, we shall find that it is possible to lay down definitions of all the logical concepts, if we take derivability as our sole undefined specifically logical concept. But this means that all these other concepts,[2] in principle even though not in practice, can be dispensed with, and that the primitive rules of sections 2 and 3, in spite of their triviality, somehow cover the whole of the theory of inference.

2. General Theory of Derivation

If we wish to discuss rules which tell us under what circumstances a certain statement – the conclusion – follows from certain other statements – the premises –, then we have first to come to an agreement how to write such rules.

Take "*a*"; "*b*"; "*c*"; . . . to be names of statements; then we can write[a]

$$\begin{array}{c} a \\ b \\ c \\ \hline d \end{array}$$

or something like it, in order to express the assertion: "From the premises, a, b, and c the conclusion d can be derived." This manner of writing a rule of derivation is known from traditional logic, where one often writes rules of inference (or "moods") in this way; for example, the *modus ponendo ponens* is often written:

$$\begin{array}{c} \textit{If A then B} \\ A \\ \hline B \end{array}$$

[2] Except the concept "the result of substituting x for y in the formula a" which is introduced, in section 6, by way of a few exceedingly trivial primitive rules of inference (and which may be said to be implicitly definable in terms of derivability).

[a] In Popper (1947c) the following two derivations are inside quotation marks.

which may be read: “From the premise ‘*If A then B*’ together with the premise A, the conclusion B can be derived.”[3]

195 | In order to save space, and to make our way of writing less clumsy, we shall use here a slightly different notation; the main difference being that we shall write everything in one line, using the symbol “/” instead of the horizontal line; we shall, furthermore, use commas in order to divide the various premises, instead of writing them in different lines. In other words, the notation:

$$\text{“}a, b, c/d\text{”}$$

will be used in order to express the assertion: “From the statements a, b, and c, the statement d can be derived.”

Very often we shall have to discuss inferences from many premises. Let n be the number of the premises we are considering. Then the expression:

$$\text{“}a_1, \ldots, a_n/b\text{”}$$

will be used for conveying the assertion: “From the statements $a_1, a_2, a_3, \ldots, a_n$, the statement b can be derived.”

The sign “/” designates the *specifically logical concept of deducibility* whose importance was emphasised in the *Introduction*. It should be noted that, although we may operate with as many premises as we like, we draw only *one* conclusion at a time; in other words, an expression such as “$a_1, \ldots, a_n/b, c$” has no meaning for us, as we are using our notation. If we wish to assert that b as well as c follows from $a_1, \ldots, a_n$, then we have to say: “$a_1, \ldots, a_n/b$ and $a_1, \ldots, a_n/c$”.

Having explained our notation, we proceed to the discussion of actual rules of derivation. The most trivial and at the same time the simplest rule of inference is undoubtedly

$$a/a \tag{2.1}$$

in words: “From the statement a, the statement a can be derived.”

This rule is so trivial that many classical logicians have hesitated to admit it. They have felt that nothing could be gained by admitting it, and that the dignity of the procedure of inference would be imperilled if this were called an inference. But these considerations are irrelevant. Since the rule is, obviously[4], not invalid, we should have to admit it even if it were useless. But, contrary to first impressions, it is far from being useless: within those more subtle analyses of more complicated threads to which we referred in the first section, it turns out to be exceedingly useful; and it is even characteristic, altogether, of the triviality of the fundamental rules from which the edifice of logic arises. We should, furthermore, keep in mind that triviality of the

[3] For a precise rendering, Quine’s signs for so-called “quasi quotation” should be used instead of our single quotation marks. But since we shall not make any use of this device in what follows, it seems preferable not to burden the reader with it.

[4] In the sense of every adequate definition of inference such as Tarski’s; *cp.* his and my papers mentioned in ⟨footnote 9⟩. ⟨Footnote added in the Errata.⟩

196 basic | assumptions is an advantage rather than a disadvantage, provided that what we obtain, at the end, is adequate for our purpose.

Another trivial valid rule is the following:

$$\text{(2.2)} \qquad \text{If } a_1, \ldots, a_n/b \text{ then } a_1, \ldots, a_n, a_{n+1}/b$$

which can be translated into: "If an inference is valid, then adding a new premise does not make it invalid." Indeed, the addition of new premises can only strengthen the premises, – just as an omission, in general, weakens them. ("Strengthening" and "weakening" will be defined and discussed later in this section.)

A third triviality is that the *order* of the premises does not affect the validity of the inference. This leads us to lay down the rule

$$\text{(2.3)} \qquad \text{If } a_1, \ldots, a_n/b \text{ then } a_n, \ldots, a_1/b.$$

This rule permits us to reverse the order of the premises; by itself, it does not allow us to put the premises into any desired order. But, combined with the previous two rules, and with one (the principle of transitivity) which we shall soon discuss, it does, in fact, allow us to achieve precisely this. (This fact can be established; but we omit all such proofs in this paper.)

Before proceeding to the discussion of a more complicated rule, the principle of the transitivity of derivability, it should be mentioned that the three rules so far explained, *i.e.*, 2.1, 2.2, and 2.3, can (in the presence of the transitivity principle) be replaced by one single rule, of about equal simplicity, *viz.*, the following:

"From the premises $a_1, \ldots, a_n$, we can derive, as a conclusion, any statement a_i which is one of the premises, *i.e.*, for which $1 \leq i \leq n$." This can be expressed in our notation by

$$\text{(2.4)} \qquad a_1, \ldots, a_n/a_i \qquad (1 \leq i \leq n).$$

The notation might perhaps irritate some who are not used to this kind of thing; but it is only the notation which makes this rule appear less trivial than the others; the idea is simplicity itself, and the rule, obviously, is valid. For if (as we asserted) the order of the premises is irrelevant; if adding to them only strengthens them, and does not invalidate the inference; and if

$$a/a$$

is valid, then, obviously,

$$\text{(2.41)} \qquad a_1, \ldots, a_n/a_1$$

must be valid, and also

$$a_1, \ldots, a_n/a_2$$

197 | provided n is, at least, equal to two; and also

$$a_1, \dots, a_n / a_3$$

provided n is at least equal to three; and generally we have 2.4, *i.e.*,

$$a_1, \dots, a_n / a_i$$

provided n is at least equal to i, which is precisely what we express by the condition "$1 \leq i \leq n$". We shall call 2.4 the "generalised principle of reflexivity" and refer to it by "(Rg)", *i.e.*, generalised reflexivity principle.

We now turn to the principle of transitivity. In its simplest form, this rule can be expressed by

(2.5) If a/b, then: if b/c then a/c

or in words: "If b follows from a, then: if c follows from b, then c follows from a." We shall refer to it by "(Ts)", *i.e.*, "simplest Transitivity principle".

This rule (Ts), although perhaps slightly less trivial, is still very simple. In the next section, we shall indicate[5] a method by which it can be retained in the simple form in which we have introduced it here. This method, however, makes use of a new concept, that of the conjunction of two statements, – say, the statement a and the statement b. The introduction of this concept simplifies certain things very considerably – this is, indeed, the reason why it is introduced. But it goes hand in hand with a restriction of the generality of our approach. This is a serious step which should not be taken before we have seen how far we can get with our present method. Besides, only if we see that things get a little complicated can we appreciate the simplification achieved by the new method. We shall therefore proceed to explain the more complicated forms of the rule of transitivity which are needed in many inferences.

But we shall first introduce here some linguistic abbreviations. We shall write in our rules sometimes "→" instead of "if . . . then . . ."; for example, we shall write a rule like 2.2 sometimes:

(2.2+) $a_1, \dots, a_n / b \rightarrow a_1, \dots, a_n, a_{n+1} / b$

or the rule 2.3:

(2.3+) $a_1, \dots, a_n / b \rightarrow a_n, \dots, a_1 / b.$

The introduction of the sign "→" for "if . . . then . . ." is a very different matter from the introduction of a new logical concept. It is *merely* an abbreviation; and in order to emphasise this fact, we shall quite frequently revert to our ordinary way of writing.
198 | It is important to keep this in mind, and not to think that we introduce a logical symbolism in an underhand manner. (A new logical sign must be introduced explicitly by a rule or by rules determining its use. The only specifically logical sign we have so far introduced is "/", and we are still busy explaining the rules, such as 2.1, 2.2, 2.3, etc., which determine its use.)

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[5] *Cp.* ⟨footnote 13⟩. ⟨Footnote added in the Errata.⟩

If we use our new abbreviation, rule 2.5, that is to say (Ts), becomes:

$$a/b \rightarrow (b/c \rightarrow a/c). \tag{2.5+}$$

The brackets correspond to the colon of the original formulation.

Apart from "→" we shall make use of "&", in the usual way, as abbreviation for "and"; again, we shall indicate the informal character of this arrangement by reserving the right of continuing to write "and" whenever we like.

We now proceed to the more complicated forms of the transitivity principle.

The simple form discussed so far allows us only to handle cases of inference from *one* premise. It can be extended to the case of more than one premise by writing, instead of 2.5+:

$$a_1, \ldots, a_n/b \rightarrow (b/c \rightarrow a_1, \ldots, a_n/c). \tag{2.5e}$$

In words: "If the statement b follows from the premises $a_1, a_2, \ldots, a_n$, then, if c follows from b, c also follows from $a_1, a_2, \ldots, a_n$." We shall refer to this form of the principle by "(Te)", *i.e.*, "extended transitivity principle". It is a very important principle, and it will be used, in the next section, together with (Rg) and another rule, to form what we shall call our "basis II", which makes use of conjunction. But at present, we do not wish to use conjunction, and we need, therefore, a stronger and more complicated principle which we shall call the "generalised principle of transitivity" and which we shall denote by "(Tg)".

This generalised principle of transitivity is, indeed, not so simple as our other principles, and this is the reason why we shall attempt to do without it, that is to say, why we shall design a method for avoiding its acceptance as a primitive rule.

Why we need it here is due to the following reason:

We may be able to derive the statements b_1 as well as b_2 from the premises $a_1, \ldots, a_n$; *i.e.*, we may have

$$a_1, \ldots, a_n/b_1 \ \& \ a_1, \ldots, a_n/b_2.$$

At the same time, we may have

$$b_1, b_2/c.$$

199 | Clearly, we have then also

$$a_1, \ldots, a_n/c.$$

Or, putting it in one line, the following rule will be generally valid:

$$(a_1, \ldots, a_n/b_1 \ \& \ a_1, \ldots, a_n/b_2) \rightarrow (b_1, b_2/c \rightarrow a_1, \ldots, a_n/c).$$

Now this is perhaps not much more complicated than (Te). But we cannot stop here. It is possible that c does not follow from b_1 and b_2 together, but that it follows from the m premises $b_1, \ldots, b_m$; or in other words, it may be that

$$b_1, \ldots, b_m/c$$

holds; and it is quite possible that *each* of these m premises of c, *i.e.*, *each* of the statements, $b_1, b_2, \ldots, b_m$, follows in turn as a conclusion from the premises $a_1, a_2, \ldots, a_n$. In this case,

$$a_1, \ldots, a_n/c$$

will hold also.

But how shall we write down a rule stating this more complicated kind of transitivity? There is no very simple way of writing it down.

One way of writing it is this:

(2.5g) $$(a_1, \ldots, a_n/b_1 \ \& \ a_1, \ldots, a_n/b_2 \ \& \ldots \& \ a_1, \ldots, a_n/b_m) \rightarrow \\ (b_1, \ldots, b_m/c \rightarrow a_1, \ldots, a_n/c).$$

Another way of writing it is this:

(2.5g[1]) $$\left\{ \begin{array}{ll} & a_1, \ldots, a_n/b_1 \\ \& & a_1, \ldots, a_n/b_2 \\ \vdots & \quad \vdots \\ \& & a_1, \ldots, a_n/b_m \end{array} \right\} \rightarrow (b_1, \ldots, b_m/c \rightarrow a_1, \ldots, a_n/c).$$

Or we may introduce a new abbreviation for any expression like

$$a/b_1 \ \& \ a/b_2 \ \& \ a/b_3 \ \& \ldots \& \ a/b_m$$

(whatever the number of the premises may be) by using instead the brief expression:

$$\prod_{i=1}^{m} a/b_i$$

This will be found repulsive by many, and we shall not continue to use anything as
200 complicated as this in the remainder of the | paper. But we may mention that our rule 2.5g is in this way much easier to write:

(2.5g†) $$\prod_{i=1}^{m} a_1, \ldots, a_n/b_i \rightarrow (b_1, \ldots, b_m/c \rightarrow a_1, \ldots, a_n/c).$$

Another way out would be to write:

(2.5g‡) If, for all values of i between 1 and m (*i.e.*, for $1 \leq i \leq m$), we have $a_1, \ldots, a_n/b_i$, then we also have: if $b_1, \ldots, b_m/c$ then $a_1, \ldots, a_n/c$.

But whatever way of writing we may adopt – there is no doubt that this general principle of transitivity is considerably less simple and convincing than, say, (Te), *i.e.*,

(2.5e) $$a_1, \ldots, a_n/b \rightarrow (b/c \rightarrow a_1, \ldots, a_n/c).$$

Before we proceed, in the next section, to develop a method which allows us to dispense with anything more complicated than the rules (Te) or even (Ts), I wish to make clear that the system consisting of the two rules (Rg) and (Tg), *i.e.*, 2.4 and 2.5g, or alternatively, the system consisting of the four rules, 2.1; 2.2; 2.3; and (Tg), is sufficient as a basis for the construction of propositional and functional logic, as undertaken in sections 3 to 7. (The precise sense in which these sets form a "basis" will become clear in the sequel.)

The two systems are of *equal effect* in the sense that it can be shown that every inference which is valid in the one is also valid in the other. Furthermore, each of the two systems is independent in the sense that it can be shown of any one of the rules so far mentioned that, if it is omitted, the system in question is no longer of equal effect with the other system. (They can also easily be shown to be consistent.)

With the help of each of the two systems, a large number of what we may call "secondary rules of inference" can be shown to be valid. A rule is called "secondary" to some other rules which are "primary" relative to it if it does not add anything new to these primary rules; that is to say, if every inference which is asserted as valid by the secondary rule could be drawn merely by force of the primary rules alone, for example, by reiterated application of one of the primary rules, or by applying one after the other. ("Primitive" rules are rules of a set of rules such that all the other rules considered are secondary to the rules of this set; and an independent set of primitive
201 rules is called a "basis".) Many of the secondary rules of our system | are, of course, trivial, but some are not. Among the more trivial ones is the following (which we already know):

(2.41) $$a_1, \ldots, a_n / a_1$$

This is clearly a valid rule whenever 2.4 is valid.

(2.6) $$\text{If } a_1, \ldots, a_n / b \text{ then } a_{n+1}, a_1, \ldots, a_n / b.$$

This rule is very similar to rule 2.2, but it does not follow from 2.2 alone. (To say that a rule "follows" from certain other rules is merely an elliptical way of saying that it is secondary to them.) On the contrary, the system consisting of rules 2.1 to 2.3 and (Tg) remains independent if 2.3 is replaced by 2.6 which shows that 2.6 is not only independent of 2.2 but also of 2.2 together with 2.1 and (Tg).[6]

Another secondary rule is one which we mentioned when introducing the rule 2.3 (which permits the reversal of the order of the premises). It is the rule that the premises may be rearranged in any desired order. This simple rule, if formulated in our usual way, looks a little complicated – again a case where the complication is not due to the contents of a rule but to the need of expressing it precisely, and in a handy and standardised way. The rule is:

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[6] I owe to Professor Paul Bernays the suggestion of replacing 2.3 by 2.6. It can be shown, however, that our original set 2.1 to 2.3 and (Tg) is in the following sense weaker than the new system (with 2.6 instead of 2.3). If, in our original system, 2.1 is replaced by the stronger rule 2.41, then the system remains independent. In the new system, if 2.1 is so replaced, 2.2 becomes dependent. (We thus obtain a system of three primitive rules, *viz.*, 2.41; 2.6; 2.5g.)

(2.7) $a_1, \ldots, a_{n+m}/b \to a_n, \ldots, a_1, a_{n+1}, \ldots, a_{n+m}/b \qquad (0 \le n \le n+m).$

Rule 2.7, together with 2.5g and 2.41, may be taken for another basis of equal effect with the ones mentioned.

An obvious but very important secondary rule is the following:

(2.8) If in any valid inference, one or more of the premises are such that they follow from the others, then they can be omitted as redundant, *i.e.*, without invalidating the inference.

In view of

$$a/a,$$

this rule 2.8 allows us to omit from a set of premises any premise which occurs more than once.

It will have been noticed that we have rules of two kinds. Some, such as "a/a"
202 or "$a_1, \ldots, a_n/a_n$" may be called *absolute* | rules of derivation; others such as "$a/b \to a, c/b$" (a secondary rule to 2.2) may be called *conditional* rules of derivation. In a conditional rule, we may distinguish between the antecedent rule and the consequent rule. Thus in the conditional rule

$$a, b/c \to b, a/c,$$

the rule "$a, b/c$" is the antecedent rule, while "$b, a/c$" is the consequent rule.

Some important and no longer trivial principles can be shown to hold of all such conditional rules. In order to formulate them, we first explain the terms "strengthening" and "weakening" as applied to premises or conclusions.

If a conclusion can be validly derived from some premise(s), we say that it is therefore logically *weaker* than the premise(s) or at most equal (in strength) to them, and that the premise(s) are logically *stronger* than the conclusion or at least equal to it. We shall, for brevity, say simply "weaker" and "stronger", but we have to keep in mind that these terms are here intended to be used in such a way as *not to exclude equal strength*; for the validity of "a/a" reminds us that a conclusion may, very obviously, be as strong as the premise from which it is drawn.

We further say that we "strengthen" (or "weaken") the premise(s) or the conclusion of a rule if we replace some or all of them by others in such a way that as a result they become logically stronger (or weaker, as the case may be); equal strength, again, is not excluded.

Now we can formulate some of the secondary rules which can be shown to hold in our system as follows:

(2.9) In a valid conditional rule,
- (a) the premises of the antecedent rule may be weakened,
- (b) the conclusion of the antecedent rule may be strengthened,
- (c) the premises of the consequent rule may be strengthened,
- (d) the conclusion of the consequent rule may be weakened,

without affecting the validity of the conditional rule.

It is possible to show that any change of any valid conditional rule of our system, effected in accordance with rule 2.9, leads to a new valid rule which, too, is within our system.

Another interesting principle is connected with *mutual deducibility* (*or logical equivalence*) of statements. If a and b are mutually deducible, we write

$$a//b.$$

203 | "//" is a new concept of our system, and can be easily defined on the basis of "/", by the following obvious Definition:

$$\text{(D//)} \qquad a//b \text{ if, and only if, } a/b \ \& \ b/a.$$

This definition may be abbreviated by using "↔" for "if, and only if":

$$\text{(D//+)} \qquad a//b \ \leftrightarrow \ (a/b \ \& \ b/a).$$

We shall use "↔" quite often, but, in the same way as "→" and "&", as an abbreviation only.

Now it can be shown of our system that, whenever $a//b$, we can, without impairing its validity, substitute a for b, in some or all places of the occurrence of b, in any valid inference. If we call two mutually deducible statements "logically equivalent", then we can call this very general rule the *substitutivity principle for logical equivalence*. And we can read "$a//b$" not only "a is logically equivalent to b", but also "a is substitutionally equal to b"; and we can call mutual deducibility also "*substitutional equality*". (The problem of the substitutivity of logical equivalence will be mentioned again in later sections.)

But is our system of rules valid?

We can define validity of inference (following in the main Tarski[7]) in the following way: An inference is valid if every interpretation (*i.e.*, for our present purpose: every actual set of statements which can be used as an example of one of our rules) which makes the premises true also makes the conclusion true.

If we call an interpretation which makes all the premises true and the conclusion false a *counterexample*, then we can also say: An inference is valid if no counterexample exists.[8]

Now it can be very easily shown of all our rules that they are valid in this sense. Take, for example, the rule 2.41:

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[7] *Cp.* A. Tarski, "Ueber den Begriff der logischen Folgerung" ⟨Tarski, 1936b⟩. We shall not discuss here the concept of interpretation. (Tarski speaks of "models".) But the fact, established in this paper, that formative signs can be defined in terms of a concept of deducibility which does not assume any formative signs in turn, opens a way to applying Tarski's concept without difficulty. I have in mind the difficulty, mentioned by Tarski, of distinguishing between formative ("logical") and descriptive signs. This difficulty seems now to be removed. (*Cp.* my "Logic without Assumptions," forthcoming ⟨as Popper, 1947b⟩.)

[8] See also my "Why are the Calculuses of Logic and Mathematics Applicable to Reality?" ⟨Popper, 1946c⟩, esp. pp. 47ff.

$$a_1, \ldots, a_n / a_1.$$

204 | Assume that a counterexample exists. Then there must be a certain interpretation which makes all the premises true and, at the same time, the conclusion false; thus, in the light of this interpretation, the conclusion a_1 will be a false statement. But since a_1 occurs in the premises also, they cannot possibly be all true statements. Accordingly, no counterexample can exist.

In a similar way, all the other rules can easily be shown to be valid, and this explains their triviality: the degree of their triviality is, as it were, in proportion to the ease with which their validity can be shown.

3. Another Approach. Conjunction

We now proceed to discuss the replacement of the complicated rule (Tg) ⟨or⟩ 2.5g by the simpler rule (Te) or 2.5e. But in order to do this, we must somewhat restrict the generality of our approach.

Indeed, the rudiment of a theory of inference sketched in the foregoing section[9] is about as general as such a theory can possibly be. It can be applied to any language in which we can identify statements (or sentences, or propositions – there is no need for us here to enter into the problem of the possibility or necessity of distinguishing between these entities), that is to say, expressions of which we might reasonably say that they are true or that they are false. Nothing is presupposed of our $a, b, c, \ldots$ except that they are statements, and our theory shows, thereby, that there exists a rudimentary theory of inference for any language that contains statements, whatever their logical structure or lack of structure may be. (But this means, for any *human* language, that is to say, for any language which is not only expressive and evocative of response, as animal languages are, but also descriptive, *i.e.*, containing means for describing facts, or statements of facts, which may be true or false.)

We may mention here in passing that our symbols "*a*"; "*b*"; "*c*"; etc., are *variables*, similar to those used by mathematicians in algebra; and as the values of the mathematicians' variables are *numbers*, so the values of our variables are statements.
205 | The student of algebra studies a universe of discourse consisting of numbers (of some kind – say, natural numbers, or real numbers). We study a universe of discourse consisting of statements (of some kind – say, the statements of a certain language). The mathematician allows that certain *figures* (not numbers) – for example, the figures "1", "2", etc., may be substituted for his variables; that is to say, what may replace his variables is any *name* of a number. Similarly, we must assume that *names of*

[9] Professor Bernays has drawn my attention to the close relationship that exists between this theory and the system of five axioms for the theory of consequence developed by A. Tarski in his "Ueber einige fundamentale Begriffe der Metamathematik" ⟨Tarski, 1930b⟩. Bernays designed a method of translating Tarski's *prima facie* very different and more abstract approach into a more elementary one, very similar to the approach presented in this section. (He has also drawn my attention to a somewhat similar system; see ⟨Hertz, 1923⟩ and ⟨Hertz, 1931⟩.) With the help of this method it can be easily shown that our system is equivalent to the first four of Tarski's five axioms.

statements (not the statements themselves) may be substituted for our variables. This is what we mean when we say that the mathematicians' symbols "a"; "b"; etc., are variable names of numbers, *i.e.*, variables which designate unspecified numbers; in the same sense, our symbols "a"; "b"; etc., are variables which designate unspecified statements, and they may be described, in the sense indicated, as variable names of statements.

Now the universe of statements envisaged in the foregoing section may consist of the statements of any language, whatever its logical structure. But in the present section, a certain restriction is imposed on the admissible languages – they must contain what we shall call *conjunctions*.

We shall define conjunction in what follows. The intuitive meaning of our definition can be conveyed perhaps in this way: We say that a language L contains conjunctions (or that it contains the operation of conjunction) if we can join any two statements – the statement a, say, and the statement b – in such a way as to form a new statement c which is logically equivalent to a and b together. This is done, in English, by linking them together with the help of the word "and". But we need not suppose that any such word exists: the link may be effected in very different ways; moreover, the new statement need not even contain the old ones as recognisable separate parts (or "components"). We may therefore say:

A language L contains the operation of conjunction if, and only if, it contains, for any two statements, a and b, a third statement c which is logically equivalent to them (*i.e.*, a little more precisely, equivalent to the first two statements taken together).

Now the remark on logical equivalence definitely needs here some clarification. According to the way in which we have introduced "//", we cannot write "$a, b//c$"; rather, only *one* statement is allowed to occur on the left- as well as on the right-hand side of "//" only. For we must remember that our conclusions always consist of *one* statement only; and in the case of "//", conclusions (as well as premises) stand on both sides.

206 | Nevertheless, the statement c may be said to be equivalent to the two statements a, b if we have:

$$c/a \tag{1}$$

$$c/b \tag{2}$$

$$a, b/c \tag{3}$$

We are thus led to a definition along the following lines:

(D3.01) The statement c is a conjunction of the two statements a and b if, and only if, c/a, c/b, and $a, b/c$.

(D3.02) A language L contains the operation of conjunction if, and only if, it contains, with every pair of statements, a, and b, a third statement c which is the conjunction of a and b.

Definitions D3.01 and D3.02 are as such quite in order, and adequate. But in view of our aim – the simplification of the transitivity principle – we shall *not* adopt D3.01. Rather, we shall replace D3.01 by a similar but slightly stronger definition.

The situation is this: D3.01 makes, of course, very essential use of our specifically logical concept "/". Its force will depend, therefore, upon the force of this concept "/"; but this means upon the primitive rules of derivation assumed to hold. A closer analysis shows that D3.01 is perfectly adequate as long as we assume all those rules of inference which we are accustomed, intuitively, to use; which means, in effect, the systems of primitive rules discussed in section 2. But if we drop some of these rules, or if we replace 2.5g by the weaker form of the transitivity principle 2.5e, then our definition is no longer quite adequate. It turns out, in this case, that it does not entitle us to assert that *more than two* separate statements can be joined into a conjunction or, in other words, that there exists, for any three statements a, b, and c, an equivalent statement d. On the basis provided by section 2, this can be shown to hold, but not if 2.5g is replaced by 2.5e or 2.5. (It is in these somewhat subtle points that our trivial system of logic becomes complicated.)

But the main point of using conjunction is just that it should permit us to *link up any number of statements into one*. If it does this, then we can always replace the m conclusions of m different inferences from the same premises by one inference. But if we can do this, then (Te) or perhaps even (Ts) will be, clearly, sufficient. At the same time, many other simplifications would be possible. For example, we could use "//" in order to express equivalence between $a_1, \ldots, a_n$ and $b_1, \ldots, b_m$ – namely by first replacing each of the two sets by the conjunction of all statements belonging to it.

207 We see here that it is a practical need – the need for | simplification, abbreviation, greater lucidity of certain formulations, etc. – which leads us to introduce conjunction; and we may safely assume that it was precisely such a need which led, in the actual development of most human languages to the invention of a linguistic device, such as the use of the word "and", which guarantees that the language contains conjunction in the sense defined above.

Since our definition is, as we have said, only adequate on the basis of the whole system of section 2, we shall have to modify it in order to achieve our purpose. But before proceeding to do so, we shall make some comments upon another aspect of our definitions D3.01 and D3.02. These comments will go into some detail, but our labours will be rewarded, for our findings concerning conjunction apply equally to the hypothetical, the disjunction, the negation, the quantifiers, etc.

If we designate the conjunction of the statements a and b by the letter "c", it is necessary for us always to add, explicitly, whenever we mention c, that this c is a conjunction of the statements a and b. We can avoid this by introducing a new sign, such as "$\wedge(a, b)$" – which may be read: "the conjunction of the statement a and the statement b", – or, more briefly, "$a \wedge b$", – which may be read "(the statement) *a-and-b*", or simply "*a-and-b*". Using this method, D3.01 becomes:

(D3.01+) The statement $a \wedge b$ is a conjunction of a and b if, and only if, $a \wedge b/a$, $a \wedge b/b$, and $a, b/a \wedge b$.

But this can be split up into two parts: a definition D3.0 and three primitive rules of inference:

(D3.0) "$a \wedge b$" is the (variable) name of the conjunction of the two statements a and b (called the two components of the conjunction).

The three rules of inference for $a \wedge b$, are:

(3.1) $$a \wedge b/a.$$

(3.2) $$a \wedge b/b.$$

(3.3) $$a, b/a \wedge b.$$

It is clear that (D3.0) is nothing but the introduction of a new label, and that only the three rules give the new label a meaning, – by relating it to deducibility.

A warning should be given here: as we have introduced it, *the symbol* "$\wedge$" *has no separate meaning at all*. It would be a mistake to consider it as an abbreviation of the word "and", – perhaps even as an alternative abbreviation to "&". We do not even assume that the language we are discussing – the language to which our statements $a, b, c, \ldots$ belong – possesses a special sign for linking statements into conjunctions.
208 We can | easily imagine a language L, consisting, say, of simple subject-predicate statements, in which the end of a whole statement is always indicated by the word "STOP". In such a language, a conjunction of two or more statements may be expressed simply by omitting the word "STOP" between them, rather than by inserting a word corresponding to our "and". Furthermore, a language may dispose of different means for expressing conjunction. Our language may perhaps express conjunction not only by the omission of the word "STOP" but alternatively (taking a clue from Latin) by replacing the word "STOP" by the word "QUE"; and this may mean that the two preceding statements are intended to form a conjunction. (If three statements are to be linked, "QUEQUE" may conclude the third, etc.)

Any number of different methods of expressing conjunction may be invented, and may actually occur, even within one and the same language.

It is for this reason that we say that $a \wedge b$ is a conjunction of a and b, rather than that it is *the* conjunction of a and b. But however many conjunctions of a and b we may have in a language, they all must be equivalent if they satisfy our rules 3.1 to 3.3; for any two conjunctions of a and b which satisfy these rules (and those of section 2) can be shown to be mutually deducible.

The upshot of all this is that what we have defined is not so much the conjunction of a and b but the precise *logical force* (or the logical import) of any statement c that is equal in force to a conjunction of a and b. This may be made clearer, perhaps, by re-formulating our definition D3.01 in this way:

(D3.03) The statement c is equivalent (or substitutionally equal) to any conjunction of the statements a and b, if, and only if, c/a and c/b and $a, b/c$.

Or in an alternative way of writing:

(D3.03+) $$c//a \wedge b \leftrightarrow (c/a \;\&\; c/b \;\&\; a, b/c).$$

From this definition, we can obtain the three rules 3.1, 3.2 and 3.3 in the following way: if c and $a \wedge b$ are substitutionally equal, then: (1) $c/a \;\&\; c/b \;\&\; a, b/c$ (by force of D3.02), and (2) we may substitute "$a \wedge b$" for "c" (by force of substitutional equality). But the result of this substitution leads precisely to the rules 3.1 to 3.3.

It will now be reasonably clear, I hope, what we mean if we say that we can define conjunction on the basis of the system of section 2, with "/" as our only specific logical term. It may be mentioned, in addition, that if we wished, we could easily proceed further, and define, with the help of our definition of a conjunction, the *sign of*
209 *conjunction itself* (in English, the word "AND") | along the following lines. We could distinguish between preceding, intervening, and succeeding signs of conjunction (*i.e.*, signs preceding the two statements to be linked, or placed between them – like "AND" – or succeeding them – like "QUE" in our example above). Then we can define: A sign S of a language L is a preceding sign of conjunction in L if, and only if, for any pair of statements a and b of L, an expression consisting of S followed by a and by b is, in L a conjunction of a and b. Corresponding definitions would have to be laid down for intervening and succeeding signs of conjunction. Next, we could define: A language L contains a (preceding, etc.) sign of conjunction if, and only if, it contains a sign S such that S is a (preceding, etc.) sign of conjunction.

These definitions are intended to assume as little as possible about the particular method by which a particular language may express conjunction. But they are not applicable to *all* languages containing conjunction; they cannot achieve generality. It is always possible to design new methods of expressing conjunction which are not covered by any definition of this kind (for example, underlining in written language, intonation in spoken language, etc.); and I have mentioned this way of defining the term "sign of conjunction in L" merely in order to show that we do not, perhaps, pay for the generality of our approach by an incapacity of applying our theory to the usual types of language.

These comments upon the character of our definitions, and on the possibilities of extending them to the definition of certain signs of some language, apply, as indicated, not only conjunction, but equally to disjunction, negation, hypotheticals, the quantifiers, etc., that is, to all *formative signs*[10] of a language.

We now turn back to the particular problems of conjunction. First we may mention that our definitions can be made much neater; for it so happens that the rules 3.1 to 3.3 can be replaced, in the presence of the rules of section 2, by either of the two following rules:

(3.4) $$a \wedge b/c \;\leftrightarrow\; a, b/c.$$

(3.5) $$c/a \wedge b \;\leftrightarrow\; c/a \;\&\; c/b.$$

210 | Using 3.4, instead of using 3.1 to 3.3 (as we did in our definition D3.03+) we can replace D3.03+ by the following definition:

(D3.04+) $$c /\!/ a \wedge b \;\leftrightarrow\; (\text{for any statement } d\text{: } c/d \;\leftrightarrow\; a, b/d).$$

Similarly, 3.5 would give rise to the following definition:

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[10] I prefer to speak of the "formative signs" of an object language where Carnap and Tarski speak of "logical signs". The reason is that I consider logic as a purely metalinguistic affair; on the other hand, the signs in question (of the object language) characterise the formal structure of that language.

(D3.05+) $c//a \wedge b \;\leftrightarrow\;$ (for any statement d: $d/c \;\leftrightarrow\; d/a$ & d/b).

Each of these two definitions is adequate, on the basis of the system of section 2, but each of them presupposes (Tg), *i.e.* 2.5g, for adequacy.

But there is a method of generalising all these rules, by introducing many premises, $a_1, \ldots, a_n$, instead of one premise a. The various generalised rules look like this:

(3.1g) $$a_1, \ldots, a_n, b \wedge c/b.$$

(3.2g) $$a_1, \ldots, a_n, b \wedge c/c.$$

(3.3g) $$a_1, \ldots, a_n, b, c/b \wedge c.$$

(3.4g) $$a_1, \ldots, a_n, b \wedge c/d \;\leftrightarrow\; a_1, \ldots, a_n, b, c/d.$$

(3.5g) $$a_1, \ldots, a_n/b \wedge c \;\leftrightarrow\; a_1, \ldots, a_n/b \;\&\; a_1, \ldots, a_n/c.$$

On the basis provided by section 2, each of the last two rules,[11] 3.4g as well as 3.5g, turns out to be replaceable by the three rules 3.1g to 3.3g, and *vice versa*; but this result is in so far trivial as, on this basis, it also turns out that the generalisation is superfluous, that is to say, that we can everywhere omit the premises $a_1, \ldots, a_n$, which brings us back to our original forms 3.1 to 3.5. But this situation changes completely if we drop the generalised transitivity principle (Tg) and replace it by the simpler form (Te). In this case, all the rules 3.1 to 3.5 and 3.1g to 3.4g still follow from 3.5g;[b] but the opposite is not the case.

Thus rule 3.5g is more powerful[12] than the others; and indeed, it turns out that it allows us to dispense with (Tg); that is to say, it can be shown that, in the presence of 3.5g, (Tg) is secondary to (Te) and (Rg).

We shall briefly sketch the proof of this contention. The assumption of the generalised transitivity rule (Tg) is that the following m rules are valid:

$$a_1, \ldots, a_n/b_1 \;\&\; a_1, \ldots, a_n/b_2 \;\&\; a_1, \ldots, a_n/b_3 \;\&\; \ldots \;\&\; a_1, \ldots, a_n/b_m \tag{1}$$

Now we obtain, from the first of these, *i.e.*, from

$$a_1, \ldots, a_n/b_1 \;\&\; a_1, \ldots, a_n/b_2 \tag{2}$$

by rule 3.5g,

$$a_1, \ldots, a_n/b_1 \wedge b_2 \tag{3}$$

211 | accordingly, we also have, by (1),

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[11] Rule 3.4g has been suggested to me by Professor Bernays. This led me to adopt the simplified rule 3.4 (as rule 4.1 in section 4).

[12] It is more powerful relative to those transitivity principles which we are considering here (with a view to avoiding an unspecified number of antecedents). ⟨Footnote added in the Errata.⟩

[b] This statement is not correct; 3.4g does not follow from 3.5g. The error will lead to the failure of Basis II; cp. 4.6. Cp. also the remark by Bernays in his letter to Popper, 12 May 1948 (this volume, § 21.7).

$$a_1, \ldots, a_n/b_1 \wedge b_2 \;\&\; a_1, \ldots, a_n/b_3 \tag{4}$$

and, applying rule 3.5g again, we obtain

$$a_1, \ldots, a_n/(b_1 \wedge b_2) \wedge b_3 \tag{5}$$

and so on, until all the various conclusions $b_1, \ldots, b_n$ are incorporated into *one*, as components of a conjunction. Now this process, which makes use only of 3.5g, allows us to replace (1) by one single rule of inference. To this single rule we can apply (Te), the extended form of the transitivity rule.[c]

We may sum up our results as follows:

We have now two approaches for deducibility.

Approach I consists of the systems of section 2 – in its simplest form, a basis consisting of two primitive rules: rule (Rg), the generalised principle of reflexivity, and rule 2.5g the generalised principle of transitivity, denoted by (Tg). We repeat these two rules, which constitute what we call the basis I:

(Rg) $$a_1, \ldots, a_n/a_i \qquad (1 \leq i \leq n)$$

(Tg) $$\prod_{i=1}^{m} a_1, \ldots, a_n/b_i \rightarrow (a_1, \ldots, a_n/c \rightarrow b_1, \ldots, b_m/c)$$

This approach is applicable to any language.

Approach II is less generally applicable: it assumes that the language under consideration contains (in accordance with definition D3.02) the operation of conjunction. In its simplest form, called "basis II", it consists of a postulate (PC), expressing this assumption and replacing the definition D3.02, and of three primitive rules, *viz.* of (Rg) and (Te), the simple form of the principle of transitivity previously called 2.5e, and of 3.5g, which we shall call the generalised principle of conjunction. This is basis II:

(PC) If a is a statement and b is a statement, then $a \wedge b$ is a statement.

(Rg) $$a_1, \ldots, a_n/a_i \qquad (1 \leq i \leq n).$$

(Te) $$a_1, \ldots, a_n/b \rightarrow (b/c \rightarrow a_1, \ldots, a_n/c).$$

(Cg) $$a_1, \ldots, a_n/b \wedge c \;\leftrightarrow\; (a_1, \ldots, a_n/b \;\&\; a_1, \ldots, a_n/c).$$

These two approaches[13] are, of course, not equivalent; basis II is richer than basis I, since it contains, for example, all the rules 3.1 to 3.5, and 3.1g to 3.5g (which basis I
212 does not contain), and | many other rules which may be derived from it; but they become equivalent in force if we add to basis I postulate (PC) together with, for

[13] There are a number of alternative bases in each of the two approaches. I mention only basis I′, consisting of 2.1; 2.2; 2.3; either (Tg) or 2.8; basis II′ uses only (Ts), together with 2.1; 2.2; 2.3; Cg, and 3.4g.

[c] Popper's proof is not correct; $b_1, b_2/c \rightarrow b_1 \wedge b_2/c$ does not follow from the rules of Basis II. Cf. this volume, § 4.6.

example, the rules 3.1 to 3.3 (or with one of the rules 3.4 or 3.5, or else with one of the Definitions D3.03 to D3.05).

4. The Logic of Compound Statements

Either of the two bases described in the foregoing sections suffices for the construction of modern logic,[14] *i.e.*, of the logic of compound statements[15] and of the logic of quantification (or the propositional and the lower functional calculus), and therefore *a fortiori* for constructing classical logic, so far as it is valid.

The logic of compound statements comprises the rules of inference pertaining to conjunction, to the hypothetical or conditional statement, its converse, and the bi-conditional, to disjunction, negation, etc. The system of these rules can be constructed by precisely the same method as was used in the previous section for introducing conjunction:

We first assume postulates (such as PC in section 3), one for each of the compounds we wish to introduce, in order to assure, for every pair of statements, say a and b, the existence of the corresponding compound statement. In this way, we may postulate for every pair of statements a and b the existence of a statement $a > b$ (if we want our language to contain hypothetical compounds); of a statement $a < b$; (if we want converse hypotheticals); of a statement $a \mathrel{\widehat{=}} b$ (if we want bi-conditionals); and of a statement $a \vee b$ (if we want disjunctions). Furthermore, we postulate that for every statement a, a statement $\neg a$ exists (if we want negations).

These postulates have the function of confining our investigation to languages in which such compounds exist. They can be replaced by definitions such as D3.02; or alternatively we may formulate our theory hypothetically, by introducing an assertion about deducibility by a remark such as "If a language contains negation, then the following rules of inference hold: . . ." In other words, the postulates or definitions do not really form a part of our theory of inference – they are there solely to indicate
213 explicitly that the application of our theory is limited, if we | wish to operate with certain compounds, to languages which contain these compounds.

The meaning of the compounds will be determined by *primitive rules of derivation* (4.1 to 4.6), one primitive rule for each of the six compounds mentioned.

It may be asked whether any actually spoken language contains these compounds? The answer, I believe, is: they do not; but they contain them approximately. The actually spoken languages are a kind of natural growth, with rules of use which are not clearly defined, and which cannot be clearly defined; if we define them in a definite way, then we replace the actually spoken and growing language by something else –

[14] I exclude the theory of membership (as Quine calls it) as mathematical. Classical syllogistic (so far as valid) appears as a small part of the logic of statement functions with one variable.

[15] The term "compound" is in so far misleading as we do not assume here that the so-called "components" are actually parts of the statements (the conjunction, etc.) whose components they are; see section 3. That is, we do not assume statement-composition, although our theory is applicable to it.

by a kind of artificial reconstruction of it, or to put it bluntly, by a kind of artificial calculus.

This cannot be otherwise; for there is no reason why all people who speak a certain naturally grown language should conform, in their habits of speech, to a precise system of rules of use. On the contrary, they will, at best, develop that degree of precision which is requisite to the ordinary problems of the day (rather than to scientific and, more especially, mathematical and logical problems). A grammarian who attempts to legislate for them in fact attempts to impose an artificial calculus upon them; as a rule, he obtains his calculus by a process of idealising the speaking habits of what he considers to be well educated people. The logician also constructs an artificial model language or calculus, but as a rule (I hope) without attempting to impose it upon anybody.

Why does he do it? As I indicated above, the fundamental problem of logic is the theory of inference – in the most elementary form, the drawing up of rules of valid inference. In this task, he cannot succeed without introducing some artificial precision into the language in which these inferences are formulated. For what he aims at is, at the very least, a distinction between valid and invalid inferences. But whether or not a certain concrete instance of an inference is valid will depend, very often,[16] upon the meaning of certain words which may occur in the premises or in the conclusion; or more precisely, on the rules determining the use of these words. If, therefore, these rules are fluid, then the question whether or not a certain inference is valid cannot be answered.

214 | Thus the logician cannot fulfil his most elementary task without constructing something like an artificial model language; and every logician from Aristotle onward has done so – although few were quite clear about what they were doing.

The primitive rules of derivation with the help of which we are about to determine the meaning of our compounds, in a way go beyond this method of constructing an artificial model language – they characterise a very wide class of (artificial) languages as conforming to certain definite and precise rules – rules which make it possible for us to develop a theory of inference.

Still, when all is said and done, it so happens that to each of our artificial compounds, there exists a closely corresponding compound proposition in English. The correspondence between the conjunction $a \wedge b$ and the English statements constructed with "and", for example "It rains and it is wet", are particularly close. But even the conditional statement $a > b$ corresponds very closely indeed to some current English expressions. The correspondence with a phrase such as "If it rains, then it is wet" or "Provided it rains, it is wet" is quite good, but that with the more complex statement "It does not rain without its being wet" is better. Accordingly, we can pronounce the symbol "$a > b$" perhaps "if-a-then-b" or better "a-not-without-b", remembering, however, that it is *not* an abbreviation for the English phrase but a *variable name* of any statement which stands in a certain logical relationship to the two statements a and b.

[16] But not always; for the theory of section 2 (or of our approach I) is independent of the meaning of any particular *word*; all it presupposes is that we can identify *statements*.

Similarly, we can read "$a < b$" perhaps "*a-if-b*" or "*a-provided-b*", and "$a \mathrel{\widehat{=}} b$" perhaps "*a-if-and-only-if-b*".

The disjunction $a \vee b$ does not correspond badly to a phrase such as "It rains or it is wet or both". The correspondence with the briefer phrase "It rains or it is wet" is very bad indeed, but we may for the sake of brevity pronounce "$a \vee b$", nevertheless "*a-or-b*", having in mind that this is short for "*a-or-b-or-both*".

There is no very fitting English phrase corresponding to the negation $\neg a$. The negation of the statement "It rains", corresponds very well to "It does not rain" (rather than to the impossible phrase "Not it rains"), yet the negation of "All men are mortal" does not correspond to "All men are not mortal" but rather to "Not all men are mortal". A generally applicable phrase which is not too bad in English and which corresponds very well to $\neg a$ is "It is not the case that . . .". Abbreviating this, we can read "$\neg a$" perhaps as "*not-a*".

215 | We now state our primitive rules:

(4.1) $$a \wedge b/c \;\leftrightarrow\; a, b/c.$$

Or: "From the one premise *a-and-b* we can derive c if, and only if, from the two premises a and b we can derive c." (This is the same as 3.4. Note the trivial character of the rule. Nevertheless, it is important and very powerful in connexion with basis I; with basis II it is, of course, redundant.)

(4.2) $$a/b > c \;\leftrightarrow\; a, b/c.$$

Or: From the statement a we can derive the statement *b-not-without-c* if, and only if, we can derive from the two statements a and b the statement c. (Example: From "It rains incessantly every week-end" we can derive "Provided I frequently walk during week-ends, I frequently walk while it rains" if, and only if, we can derive from the two premises "It rains incessantly every week-end" and "I walk frequently during week-ends" the conclusion "I walk frequently while it rains".)

(4.3) $$a/b < c \;\leftrightarrow\; a, c/b.$$

(4.4) $$a/b \mathrel{\widehat{=}} c \;\leftrightarrow\; a, b/c \;\&\; a, c/b.$$

(4.5) $$a \vee b/c \;\leftrightarrow\; a/c \;\&\; b/c.$$

(4.6) $$\neg a, b/\neg c \;\leftrightarrow\; c, b/a.$$

Rules 4.1 to 4.5 constitute *the positive logic of compound statements*, that is to say, they suffice for that part of propositional logic which is independent of negation. If our rule 4.6 for negation is added, we obtain the whole of compound statements (or of propositional logic). If, instead, the two rules for quantification discussed in section 7 are added, we obtain the whole of positive logic.

Positive logic as defined by these rules does not yet contain all valid rules of inferences in which no use is made of negation: there is a further region which we may call the "extended positive logic" (or, if we exclude quantification, the "extended positive logic of compound statements"). This extension is achieved by adding to the system without 4.6 an additional rule for hypothetical statements, *viz.*:

$$a, b > c/b \leftrightarrow a/b. \tag{4.2e}$$

This rule may be, loosely, characterised as asserting that, if we add to a premise another one which merely informs us that something depends on our conclusion, then we do not thereby strengthen in any way the power of our original premise to yield
216 that particular conclusion. An example is: From the two | premises "All men are mortal" and "If Plato is mortal then he cannot be a god", we can derive the conclusion "Plato is mortal" if, and only if, we can derive this conclusion from the first premise alone.

If rule 4.2e is added as primary to our rules 4.1 to 4.5, then all valid rules of the logic of compounds in which negation does not occur become secondary rules. Also we do not need, in the presence of rule 4.2e, the whole force of our rule of negation 4.6, but can obtain this rule as a secondary rule from some weaker rules[17] (from the rules of the so-called intuitionist logic). On the other hand, if we have as primary the two rules 4.2 and 4.6, then 4.2e becomes a secondary rule.

Considering the triviality of these rules – especially of the "positive" rules 4.1 to 4.5 – their power is truly amazing. To give a few examples:

From 4.1 alone (of course, the basis is always assumed), we can obtain as a secondary rule the associative law for conjunction

$$(a \wedge b) \wedge c // a \wedge (b \wedge c); \tag{4.01}$$

furthermore, on the basis I, all the rules of section 3.

From 4.2 alone, we obtain, on either of the two bases, first of all

$$a, a > b/b, \tag{4.020}$$

that is to say, the *modus ponens*; furthermore, the important rules

$$a/b > a. \tag{4.021}$$

$$a > (a > b)/a > b. \tag{4.022}$$

On the basis II, or if we add either rule 4.1 or 4.6, we obtain furthermore

$$a > b/(b > c) > (a > c). \tag{4.023}$$

$$a, b/c > d \leftrightarrow a, b, c/d. \tag{4.024}$$

This last rule is a kind of generalisation of 4.2 which cannot, on the basis I, be obtained from 4.2 alone, but which can also be obtained with the help of 4.6 (instead of 4.1 or of basis II).

From the rule 4.024 alone, all the rules of positive logic containing no compound

[17] In fact, 4.6 assumes more than we need: we can replace in it "↔" by "→", and still get 4.6 as secondary rule. It may be mentioned that our rule 4.6 amounts in effect only to a principle underlying the classical theory of "indirect reduction".

except hypotheticals are derivable without any other help, – of course, always assuming
217 at least basis I. | Among these rules are

(4.025) $$a > (b > c)/b > (a > c).$$

(4.026) $$a > (b > c)/(a > b) > (a > c).$$

If 4.2e is added, we obtain *all* valid rules containing no compound other than hypotheticals. If we combine 4.1, 4.5 and 4.6, then we obtain, for example

(4.5) $$a \wedge (b \vee c)//(a \wedge b) \vee (b \wedge c).$$

From 4.6 alone we obtain, on either of the two bases, for example:

(4.061) $$\neg a, b_1/a \rightarrow \neg a, b_1/b_2 \ \& \ b_1, b_2/a,$$

a rule which, in its effect, is equal to 4.6; furthermore

(4.062) $$a//\neg\neg a.$$

(4.063) $$a, \neg a/b.$$

(4.064) $$a/b \ \& \ \neg a/b \rightarrow c/b.$$

(4.065) $$\neg c/b \rightarrow (a, b/c \ \leftrightarrow \ a/c).$$

These rules can hardly be called trivial; 4.063, for example, is a rule whose validity has been questioned in this journal[d] not very long ago; 4.065 is, in spite of its comparative simplicity, hardly self-evident.

These examples are here given mainly with the intention of illustrating our thesis that we should not despise trivial primitive rules: they may give rise to secondary rules which are far from being trivial.

Regarding the question of the triviality of our primitive rules, it may further be observed that the rules 4.1 to 4.5, that is, the rules of the positive logic of compounds, are not only very obvious, but have, besides, the character of definitions (so-called contextual definitions) of the compounds which they introduce. The newly introduced symbol (the definiendum) occurs only once, on the left-hand side, while on the other side our specific logical term "/" is used, as defining term, apart from certain phrases of ordinary English, or their abbreviations.

The position is different with the rule of negation, 4.6. Here *two* negations occur on the left at the same time: the one is somehow linked up with the other, and can, therefore, not always be eliminated alone. Rule 4.061 is, in this respect, perhaps slightly preferable. But it has a very similar defect. The variable "*a*" occurs twice, and the rule is therefore not necessarily an effective means of eliminating the symbol "¬" from any context. But a definition should be able to achieve this.

218 | The peculiar situation of negation in this respect is connected, I believe, with the fact that various systems of logic have been developed which use a weaker form of negation (notably Heyting's intuitionistic logic which formalises Brouwer's views on

[d] Popper here refers to Jeffreys (1942).

logic). It is, therefore, of considerable interest to search for an explicit definition for negation which might permit us to eliminate it from any context, and which might bring it into line with the other compounds.

5. Explicit Definitions of the Compounds

It can be easily shown that the introduction of the compounds cannot in any way impair the substitutivity of logical equivalence. That is to say, whenever a and b are mutually deducible, we may substitute a for b, or b for a, at some or all places of occurrence, in any rule of inference, however complicated, without affecting the validity or invalidity of the rule. If we, therefore, use the method already exemplified by definitions D3.03+ to D3.05+, then we cannot fail to obtain definitions which allow us to eliminate the defined sign without difficulty from any context.

All the definitions so obtained are rather obvious adaptations of the primitive rules of the last section, except the definition of negation which is based on 4.061 rather than on 4.6.

The Conjunction:

(D5.1) $$a//b \wedge c \;\leftrightarrow\; (\text{for any } c_1\text{: } a/c_1 \;\leftrightarrow\; b, c/c_1).$$

The Hypothetical:

(D5.2) $$a//b > c \;\leftrightarrow\; (\text{for any } a_1\text{: } a_1/a \;\leftrightarrow\; a_1, b/c).$$

The Converse Hypothetical:

(D5.3) $$a//b < c \;\leftrightarrow\; (\text{for any } a_1\text{: } a_1/a \;\leftrightarrow\; a_1, c/b).$$

The Bi-Conditional:

(D5.4) $$a//b \mathrel{\widehat{=}} c \;\leftrightarrow\; (\text{for any } a_1\text{: } a_1/a \;\leftrightarrow\; a_1, c/b \;\&\; a_1, b/c).$$

The Disjunction:

(D5.5) $$a//b \vee c \;\leftrightarrow\; (\text{for any } c_1\text{: } a/c_1 \;\leftrightarrow\; b/c_1 \;\&\; c/c_1).$$

The Negation[18]:

(D5.5) $$a//\neg b \;\leftrightarrow\; (\text{for any } a_1 \text{ and } b_1\text{: } a, a_1/b \rightarrow (a, a_1/b_1 \;\&\; a_1, b_1/b)).$$

[18] In this formula, the last occurrence of "b_1" may be omitted. It is added only in order to make a certain inherent symmetry (which is nothing but the symmetry between the laws of contradiction and excluded middle) more obvious. For an alternative definition, see ⟨footnote 20⟩.

While none of these definitions use a defined term in the definiens, we may add some
219 others which make use of defined terms: |

The Exclusive Disjunction ("*a-or-b-but not both*"):

(D5.7) $$a /\!/ a \neq b \;\leftrightarrow\; a /\!/ \neg(a \mathrel{\widehat{=}} b).$$

The Alternative Denial ("*not-a-or-not-b*"):

(D5.8) $$a /\!/ a \curlywedge b \;\leftrightarrow\; a /\!/ \neg(a \wedge b).$$

The Conjoint Denial ("*not-a-and-not-b*"):

(D5.9) $$a /\!/ a \downarrow b \;\leftrightarrow\; a /\!/ \neg(a \vee b).$$

The Tautology:

(D5.00) $$a /\!/ t(b) \;\leftrightarrow\; a /\!/ b \vee \neg b.$$

The Contradiction:

(D5.0) $$a /\!/ f(b) \;\leftrightarrow\; a /\!/ b \wedge \neg b.$$

For the last two, we can easily prove the rules:

(5.001) $$t(a) /\!/ t(b).$$

(5.01) $$f(a) /\!/ f(b).$$

That is to say, all tautologies are substitutionally equal, and the same holds for all contradictions. This shows that we may omit as irrelevant the reference to "a" or "b", and simply write "t" and "f" instead of "$t(a)$" and "$f(a)$".

In a way, all these definitions are quite unnecessary. True, they may replace the primitive rules of section 4, but we can work just as well or better with the much simpler primitive rules. Nothing is added to our system or taken away if we replace the rules of section 4 by the definitions of the present section.

The only reason why we nevertheless present these definitions here is that they establish beyond any doubt the fact that our primitive rules of section 5 have the character of definitions, even the rule for negation.

This fact is, no doubt, interesting. It may help to clear up the discussion of the so-called "alternative systems of logic". From our point of view, these systems are not alternative systems of logic, but alternative ways of using certain labels such as, for example, the label "negation". For it is plain that, by using different primitive rules, or by using different definitions, we can define different concepts. There is no problem involved here. If we introduce instead of 4.6 a similar primitive rule like

(5.06_{M}) $$a, b / \neg c \;\leftrightarrow\; c, b / \neg a,$$

then this rule defines a concept similar to our concept of negation, as defined by 4.6

(the "classical" concept of negation), but also a little different. This concept may be called "negation$_M$" (it is, in the presence of the other rules except 4.6, identical with
220 the | negation of Johansson's "Minimum Calculus"). It so happens that negation$_M$ is so closely related to our classical negation that all rules of inference based on negation$_M$ are also valid for classical negation, but not *vice versa*. (In this sense negation$_M$ can be said to be weaker than classical negation.) This is due to the simple fact that rule 5.06 is a valid secondary rule of our classical system. If we add to rule 5.06 another rule (which is not secondary to 5.06 but secondary to our classical system), *viz.*:

$$(5.06_I) \qquad \neg a, \neg\neg a / a,$$

then these two rules together define another concept – we call it "negation$_I$" – the intuitionist concept of negation.

All these concepts can co-exist, in one and the same model language, as long as they are distinguished.[19] Thus there is no quarrel between alternative systems. The rules of inference pertaining to the various concepts of negation are not identical, to be sure. But this is very satisfactorily explained by the simple fact that the various concepts have been given a meaning by rules which are not identical.[20]

The question whether ordinary spoken language uses the one or the other of these concepts does not, I believe, arise. Ordinary language is not sufficiently precise for us to ask such questions about it. As far as the somewhat more precise and therefore more artificial language of science is concerned, we are still asking too much, without doubt, if we ask such a question in connexion with it. But I suggest that there are good reasons why, for most purposes of science, the classical concept should be preferable to the others – simply because it is stronger, more explicit. This does not prevent us from using, for certain purposes, especially in parts of mathematics, the interesting concept negation$_I$ side by side with the classical one.

The upshot of all these considerations is this: If we have an artificial model language with signs for conjunction, the conditional ... etc. (we have called them "formative signs" of the language in question) then the meaning of these formative signs can be exhaustively determined by the rules of inference in which the signs

[19] It is, however, not always possible to distinguish them, or to prevent one from becoming absorbed or assimilated by another. If, for example, a language L contains classical negation and the conditional, then there exists, for every b and c, a statement $b > c$ such that 4.2e holds for every a (besides 4.2). But in the presence of 4.2e (and 4.2) all the rules which are valid for classical negation can be derived from those for intuitionist negation (as indicated on ⟨p. 133f⟩). This shows that if L contains definitions of both classical and intuitionist negation, the latter is unavoidably absorbed or assimilated by the former. ⟨Footnote added in the Errata.⟩

[20] We can use the following signs: "$\langle \neg_c\, a \rangle$" for the classical negation of a, "$\langle \neg_i\, a \rangle$" for the intuitionist negation of a. We can then introduce the following two explicit definitions, of which the first is a somewhat preferable alternative to (D5.6):

$$(\text{D5.6c}) \qquad a /\!/ \langle \neg_c\, b \rangle \leftrightarrow (\text{for any } b_1\text{: } a, b / b_1 \ \&\ (a, b_1 / b \rightarrow b_1 / b)).$$

$$(\text{D5.6i}) \qquad a /\!/ \langle \neg_i\, b \rangle \leftrightarrow (\text{for any } b_1\text{: } a, b / b_1 \ \&\ (b, b_1 / a \rightarrow b_1 / a)).$$

occur; this fact is established by defining our definitions of these formative signs
221 explicitly in terms of rules of inference. | There is good reason to believe that, in our ordinary language, we learn the intuitive meaning of formative signs in the same way; for no doubt, we learn their intuitive meaning by learning *how to use them*. In other words, we learn their meaning by learning the rules of their use, and these rules are mainly the rules of *their use in connexion with the drawing of inferences*.

Thus it is trivial that once we understand intuitively the meaning of formative signs, the proper handling of the rules of inference pertaining to them is part of this knowledge.

This is why rules of inference have that compelling character which mislead idealistic logicians to speak of laws of thought. Instead of taking this compulsion as the unanalysed and unanalysable *datum* of logic, we can indeed explain it quite easily. Once we know how to use the words "and", "provided", etc., that is to say, once we have learned the language and have got an intuitive grasp of its use, we feel compelled to admit that from a and $a > b$, b can be deduced; in exactly the same way as we feel compelled to admit that this animal here is a cow (provided it is a cow), once we have learned how to use the word "cow", and have got an intuitive grasp of its use.

6. Statement-Functions. Substitution as a Logical Operation

We now extend the scope of our inquiry. So far, we have considered only one kind of expressions, namely statements. Now we are going to consider, in addition to statements, so-called "statement-functions".

An example of a statement-function is: "He is a charming fellow". Whether this is true or false will depend on the question whom we mean by "he", or in other words, whose name we substitute for the pronoun[21] "he"; whether we substitute "Ernest", say, or "Edward". The expression "he is a charming fellow" is as such neither true nor false, it is *no statement*, but it can be converted into a statement by a fitting substitution – of a name, or of a descriptive phrase such as "Ernest's best friend". Other examples are: "This is an ugly fireplace"; "He is his younger brother"; "He owes him the legacy left to him by his father". The last two examples show the need for making distinctions: they become clearer if we write, say, "He_1 is the younger brother of he_2", and "He_1 owes he_2 the legacy left to he_3 by the father of he_4" – of course, something else may be meant, for example, "He_1 owes he_2 the legacy left to he_3 by the father of he_1", or "He_1 owes to he_2 the legacy left to he_2 by the father of he_3", etc.

222 | The need for a sufficiently long list of distinct pronouns is obvious, and we shall assume, of the languages under consideration, that they dispose of such a sufficient list. (See below, postulate PF3.)

Now consider again the last example. It is clear that we can *derive* from it the statement-function "He_1 owes to he_2 some legacy". Similarly, we can *derive* from the

[21] The excellent suggestion to treat name-variables as pronouns is, according to Quine ⟨Quine, 1940, p. 71⟩, due to Peano.

statement-function "He is not only a charming fellow but an excellent physician" the statement-function "He is an excellent physician". In other words, if our treatment of deducibility is to be as general as we intend it to be, then a treatment of the deducibility of statement-functions becomes necessary. At the same time, it will be necessary to treat combinations of statements and statement-functions.

We shall, in fact, assume that the theory of derivation developed in previous sections holds not only for statements but also for statement-functions.

Although statements and statement-functions should be clearly distinguished, they are, of course, rather similar things. A statement-function is, as it were, a statement with certain holes or openings in it, which can be filled up or closed up by inserting names. For this reason, statement-functions are often called "*open statements*", and statements proper are often called "*closed statements*". The terminology has great advantages, and we shall adopt it; not, however, without issuing a warning that the term "open statement" must not mislead anybody into believing that open statements are statements in the ordinary sense. They are not, for they are neither true nor false. "Open statement" is only an alternative, and often a more convenient, term for "statement-function". Only closed statements are statements proper.

Since we do not want to say that open statements are statements, we shall use another term – "formulae" – to describe the class of expressions which consists of open statements as well as of closed statements. Thus we shall say that every open statement is a formula, and that every closed statement is a formula.

But apart from formulae, we have another class of expressions to consider – the pronouns "he", "he$_1$", "he$_2$" ... "it", etc. We shall call these pronouns the *name-variables* under consideration, since names may be substituted for them.

It is obvious that these two classes of expressions, *formulae* and *pronouns or name-variables* have no member in common. This will be expressed in our postulate PF1.

223 | Thus our universe of discourse will consist, from now on, of two entirely distinct classes of expressions – formulae, F, and name-variables, N. We shall introduce the convention to use from now on "a"; "b"; "a_1"; "b_1" ... as variable names of formulae rather than as variable names of closed statements only (as before), and furthermore, to use "x"; "y"; "z"; "u"; "w"; etc., as variable names of pronouns. It so happens that "x"; "y"; etc., can be described as variable names of name-variables. This is perhaps a little confusing, for a moment or two, but quite in order.

The symbols "x"; "y"; etc., are variables which refer to certain symbols such as "he"; "he$_1$"; "it"; "it$_1$"; etc., of the languages under consideration. The symbols "he", "he$_1$", etc., refer in their turn to certain men – to Ernest, to Edward, etc. These *men* – not perhaps their *names* – constitute the "realm of individuals" or the "universe of individuals" to which the language L under consideration refers. This universe of the discourse of L is not the universe of *our* discourse; *we*, rather, discuss languages – L, for example – and especially certain classes of expressions.

The name-variables of L may occur in the formulae of L in two ways: either as *free variables* or as *bound variables*.

In the open statements which we used as examples, all the variables occurred freely. But we can *bind* a freely occurring variable by placing before the formula in which it occurs the phrase

(A) “Whosoever he_1 may be . . .”

or the phrase

(E) “There exists at least one he_1 such that . . .”

The phrase (A) is called the “universal quantifier (binding he_1)”, the phrase (E) is called “the existential quantifier (binding he_1)”.

The theory of quantifiers will be sketched in the next section; in the present section, we deal merely with formulae and variables in general. But it may be mentioned that, if all variables of a function are bound, the function is a closed statement, *i.e.*, a statement proper. Or in other words, an open statement may give rise to a closed one in either of the two following ways: by substituting names for the free variables, or by binding the free variables with the help of quantifiers. For example, the open statement “If he is a good physician, then he is a good man” may give rise to the statements:

(1) “If Ernest is a good physician then he is a good man.”

224 (2) (A) “Whosoever he may be – if he is a good physician then | he is a good man.” (This may be said to be the same as “All good physicians are good men”: a universal statement.)

(2) (E) “There exists some he such that if he is a good physician then he is a good man.” (This may be interpreted as: “There exists somebody who, if he is a good physician, is also a good man”: an existential statement.)

We said before that the quantifiers may be placed before a function; but in another language (or in the same), they may be placed after the function, etc. In other words, the same problems arise as those we discussed in section 3 in connexion with conjunction, and the position of the sign “and”. We shall, therefore, not assume that all the languages we wish to discuss contain distinguishable signs which we can call quantifiers. What we shall discuss, in the next section, is merely the way in which the formulae, denoted by

$$a$$

and by

$$Axa$$

are related, if Axa is equivalent to the result of applying universal quantification (with regard to the variable x) to the formula a.

The sign “Ax” has therefore no more independent meaning for us than the sign “$\wedge$”; nevertheless, we shall loosely speak of “Ax” as of the name of a universal quantifier binding x, and of “Ex” as of the name of an existential quantifier binding x.

Leaving the quantifiers and the rules of inference which are to determine their meaning to the next section, we now turn to the theory of another and even more fundamental logical operation, also comparable to conjunction, *viz.*, the *operation of substituting the variable y for the variable x in the formula a*. The intuitive idea of substituting the variable “he_1” for the variable “he_2” in the formula “he_1 loved he_2” or “he_1 killed he_2”, and the difference such a substitution should make to the meaning of a formula, seems to be perfectly clear. In fact, this idea seems to be so obvious that it is usually accepted as a kind of descriptive term that is not in need (and perhaps not capable of) logical analysis – least of all, of an analysis in terms of derivability.

But the operation of substitution plays a very important part in quantification theory, and an attempt to reduce it to the concept of derivability seems highly desirable if we wish to build up a coherent theory.

For this purpose we first introduce the symbol "$a\binom{x}{y}$" as a convenient name for the formula which is the *result* of the substitution of the variable y for the variable
225 x in the formula a, in | all places of the occurrence of x. (If x does not occur in a, then $a\binom{x}{y}$ is the same as a.) As in the case of the compounds, we are not going to characterise this result itself, but rather its *logical force* – *i.e.*, any formula which is equivalent to the result.

To this end, we first postulate, as a preparation, PF1, PF2, and PF3.

(PF1) If L is one of the languages under consideration, then the formulae of L and the name-variables of L have no member in common.

(PF2) $a\binom{x}{y}$ is a formula (of L) if, and only if, a is a formula (of L), and x and y are name-variables (of L).

(PF3) If a is a formula then there exists at least one name-variable x such that $a /\!/ a\binom{x}{y}$ for any name-variable y.

The contents of the first two of these postulates are plain. The third has mainly the function of ensuring that, for every formula (especially for every open statement), there exists at least one variable which does not occur in it.[22] (We mentioned before that, if x does not occur in a, then $a\binom{x}{y}$ is the same as a.)

We now proceed to the primitive rules of derivation which lay down the precise meaning in which the term "$a\binom{x}{y}$" is used within the theory of inference. These rules look complicated, but this is only due to their notation: their content is trivial indeed.

$$\text{(6.1)} \qquad \text{If, for every } z,\ a /\!/ a\tbinom{y}{z} \text{ and } b /\!/ b\tbinom{y}{z}, \text{ then } a /\!/ b \rightarrow a\tbinom{x}{y} /\!/ b\tbinom{x}{y},$$

that is, in brief: equal substitutions in equal formulae have equal results. This rule assures the continued substitutivity of equivalence in the theory of statement-functions and of quantification.

$$\text{(6.2)} \qquad a\tbinom{x}{x} /\!/ a.$$

This is plain.

$$\text{(6.3)} \qquad \text{If } x \neq y, \text{ then } \left(a\tbinom{x}{y}\right)\tbinom{x}{z} /\!/ a\tbinom{x}{y},$$

that is: once we have substituted y for x, then x no longer occurs (provided $y \neq x$), and further substitutions for x have, therefore, no effect.

$$\text{(6.4)} \qquad \left(a\tbinom{x}{y}\right)\tbinom{y}{z} /\!/ \left(a\tbinom{x}{z}\right)\tbinom{y}{z},$$

226 | that is: if we substitute first y for x and then z for y, then the result is the same as if we had substituted at once z for x and, besides, z for y.

[22] Or, at least, upon which it does not depend. ⟨Footnote added in the Errata.⟩

(6.5) $$(a\tbinom{x}{y})\tbinom{z}{y}/\!/(a\tbinom{z}{y})\tbinom{x}{y},$$

that is, if we substitute for two variables x and z, the same variable y, then it is irrelevant which of these two substitutions we undertake first.

(6.6) If $w \neq x$; $x \neq u$; and $u \neq y$, then $(a\tbinom{x}{y})\tbinom{u}{w} /\!/ (a\tbinom{u}{w})\tbinom{x}{y}$.

This says that, if we substitute for two *different* variables, x and u, two other variables, y and w, then it is irrelevant which of the two substitutions we undertake first – provided always that the variable to be substituted for x does not coincide with u, and *vice versa*.

These six primitive rules determine the meaning of the symbol "$a\tbinom{x}{y}$" in a way precisely analogous to the way in which, say, rules 3.1 to 3.3 determine the meaning of conjunction. And the meaning of "$a\tbinom{x}{y}$" is determined, precisely as was that of conjunction, with the help of the concept of derivability, "/".

To give a few examples of secondary rules: we can obtain first, from postulate PF3

(6.01) If, for any x and y, $a\tbinom{x}{y}/\!/b\tbinom{x}{y}$, then $a/\!/b$.

From this and 6.1, we obtain

(6.02) If, for every z, $a/\!/a\tbinom{y}{z}$ and $b/\!/b\tbinom{y}{z}$, then
$$a/\!/b \leftrightarrow (\text{for every } x\text{: } a\tbinom{x}{y}/\!/b\tbinom{x}{y}).$$

Another example, secondary to 6.2; 6.4; 6.5, is:

(6.03) $$(a\tbinom{x}{y})\tbinom{y}{x}/\!/a\tbinom{y}{x}.$$

Quite important is the fact that, with the help of "$a\tbinom{x}{y}$", we can define a new concept – the non-occurrence of x in a, or, perhaps more precisely, the non-relevant occurrence of x in a (or the conception: "*a-does-not-depend-on-x*"). We denote this idea by "$a_{\grave{x}}$" (read: "*a-without-x*") and define it as follows:

(D6.1) $$a/\!/a_{\grave{x}} \leftrightarrow (\text{for any } y\text{: } a/\!/a\tbinom{x}{y}).$$

227 | That is: a is equivalent to $\langle a\rangle$-*without-x* if, and only if, a is equivalent to whatever may be the result of substituting in $\langle a\rangle$ for x.

If we neglect this concept then the result is, as a rule, that some formulae can be proved in the systems in question which, in effect, amount to the assertion that there exists only *one* individual, in the realm of individuals to which the name-variables of L refer, that is in the universe of discourse of the language under consideration. This result is highly undesirable, and in order to make explicit that such a result is frankly contradictory to our intentions, I suggest including in our list of postulates for the logic of functions the following:[e]

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[e] Cp. the remark by Bernays in a letter to Popper (this volume, § 21.7, end of page 4) and Popper's reply (this volume, § 21.8).

(PF4) In every language under consideration, there exists at least one a, one x, and one y, such that[23]

$$a/a\binom{x}{y} \rightarrow t/f.$$

Since "t/f" can be shown to be contradictory, this postulate has the result that a theory in which the rule

$$a/a\binom{x}{y} \tag{1}$$

or even

$$a_{\grave{y}}/a_{\grave{y}}\binom{x}{y} \tag{2}$$

holds becomes frankly contradictory. But these rules (1) or (2) are just the rules asserting in effect that there exists only *one* individual. For they assert that *all* formulae such as "he_1 loves he_2" are equivalent to "he_1 loves he_1"; and this can be so only if "he_1 is not identical with he_2" is equivalent to "he_1 is not identical with he_1", which is only the case if there exists only one he.

This indicates that we may be able to express *identity and difference of individuals* with the help of the means at our disposal; and although we cannot, of course, define the sign of identity in L (there may be no such sign in L), we can, again, as in the case of conjunction, define the logical force of any statement-function expressing identity between the variables x and y. If we denote such a function by "$Idt(x, y)$" we can define:[24]

(D6.2) $$a//Idt(x,y) \leftrightarrow \text{(for every } b \text{ and } z\text{: } ((b//b_{\grave{x}} \;\&\; b//b_{\grave{y}}) \rightarrow a, b\tbinom{z}{x}/b\tbinom{z}{y}) \;\&$$
$$((\text{for every } c \text{ and } u\text{: } ((c//c_{\grave{x}} \;\&\; c//c_{\grave{y}}) \rightarrow b, c\tbinom{u}{x}/c\tbinom{u}{y}))) \rightarrow b/a))$$

Difference is easy to define:

(D6.3) $$a//Dff(x,y) \leftrightarrow a//\neg Idt(x,y).$$

This shows that these two concepts are logical concepts, and that the corresponding signs – if any – of the languages under consideration are formative signs.

[23] Here we could write, more simply, "b/c" instead of "t/f". ⟨Footnote added in the Errata.⟩

[24] To be exact, we have, of course, to add the following postulate as a preliminary to the definition. (A similar postulate would have to be added for difference.)

(P *Idt*) If x and y are name variables, then $Idt(x, y)$ is a formula.

Furthermore, our defining rules of substitution should be extended by introducing such rules as:

228 (A) $$(Idt(x,y))\tbinom{x}{z}//Idt(z,y).$$ |

(B) $$(Idt(x,y))\tbinom{y}{z}//Idt(x,z).$$

(C) If $x \neq u \neq y$, then $Idt(x,y)\binom{u}{z}//Idt(x,y)$.

7. Quantification

The theory of quantification is too subtle to be analysed in detail within the framework of this paper. All I shall attempt is to show that our two rules, 7.1 and 7.2, are not more complicated than previous ones, and that they can be considered as *definitions of universal and existential quantifications*.

Our two primitive rules are[25]

(7.1) If $b\binom{x}{y}$ is a formula, then: $a_{\grave{y}}/Axb_{\grave{y}} \leftrightarrow a_{\grave{y}}/b_{\grave{y}}\binom{x}{y}$

or in a different way of writing:

$$\text{If } a \text{ and } b\tbinom{x}{y} \text{ are formulae, then: } a/Axb \leftrightarrow a/b\tbinom{x}{y} \quad (a = a_{\grave{y}};\ b = b_{\grave{y}})$$

(7.2) If $a\binom{x}{y}$ and b are formulae, then: $Exa_{\grave{y}}/b_{\grave{y}} \leftrightarrow a_{\grave{y}}\binom{x}{y}/b_{\grave{y}}$

or in alternative formulation:

$$Exa/b \leftrightarrow a\tbinom{x}{y}/b \qquad (a = a_{\grave{y}};\ b = b_{\grave{y}}).$$

If we wish, we can transform these rules into explicit definitions:

229 (D7.1) $a_{\grave{y}}//Axb_{\grave{y}} \leftrightarrow$ (for every $c_{\grave{y}}$: $c_{\grave{y}}/a_{\grave{y}} \leftrightarrow c_{\grave{y}}/b_{\grave{y}}\binom{x}{y}$)

(D7.2) $a_{\grave{y}}//Exb_{\grave{y}} \leftrightarrow$ (for every $c_{\grave{y}}$: $a_{\grave{y}}/c_{\grave{y}} \leftrightarrow b_{\grave{y}}\binom{x}{y}/c_{\grave{y}}$).

The definitions are, as in previous cases, no improvement – they only establish the fact that quantification can be defined directly on the basis provided by sections 2 or 3, and the primitive rules 6.1 to 6.6.

I do not claim that the two new primitive rules are trivial in the sense in which our basic rules of sections 2 and 3, or even the rules of section 4, are trivial. There is, without doubt, a certain subtlety involved in the way these rules make use of the concepts "$a\binom{x}{y}$" and "$a_{\grave{x}}$"[26] defined in section 6, – a subtlety which defies a simple

[25] Postulates and rules of substitution have also to be assumed, corresponding to those mentioned in the last footnote.

[26] The use of "$a_{\grave{x}}$", however, is avoidable in all the primitive rules and definitions; *i.e.*, we can write, more lengthily but more precisely and clearly ($x \neq y$ must always be assumed here):

(7.1) $a\binom{y}{x}/Ax(b\binom{y}{x}) \leftrightarrow a\binom{y}{x}/b\binom{x}{y}$

(7.2) $Ex(a\binom{y}{x})/b\binom{y}{x} \leftrightarrow a\binom{x}{y}/b\binom{y}{x}$

(D7.1) $a\binom{y}{x}//Ax(b\binom{y}{x}) \leftrightarrow$ (for every c: $c\binom{y}{x}/a\binom{y}{x} \leftrightarrow c\binom{y}{x}/b\binom{x}{y}$)

(D7.2) $a\binom{y}{x}//Ex(b\binom{y}{x}) \leftrightarrow$ (for every c: $a\binom{y}{x}/c\binom{y}{x} \leftrightarrow b\binom{x}{y}/c\binom{y}{x}$)

Of the "rules of substitution" referred to in the last note we may state explicitly:

$$(Axa)\tbinom{x}{y}//Ay(a\tbinom{x}{y});$$

explanation. Yet anybody who is conversant with quantification theory will, I am sure, be astonished that rules of this comparative degree of simplicity – and, for those who are accustomed to this kind of thing, even triviality – are sufficient. For the hitherto known comparable systems of quantification theory are much more complicated. For example, the well-known system of Hilbert and Ackermann contains (in place of our two rules) two axioms; two rules rather similar to our own; four very complicated rules for the handling of variables; and at least one rule forbidding certain operations (which are permitted in our system). All these rules, except the last mentioned, can be shown to be secondary to our two rules. Or to take another system – the very interesting and suggestive one of Quine's *Mathematical Logic*. This system takes only the universal quantifier as primitive. (The existential quantifier is introduced by a well-known definition with the help of the universal quantifier and negation.) Quine's system corresponds therefore to our *one* rule 7.1. But Quine needs five axioms – partly rather complicated ones – which can, of course, all be shown to be secondary to our rule 7.1.

Among the secondary rules which can be shown to be valid in every adequate theory of quantification, there are extremely complicated ones – here logic definitely ceases to be trivial. But it is our contention that these complications are merely due to the wealth of possible iterations and combinations of essentially trivial rules. For even if our latest defining rules are no longer very trivial, they are only definitions, that is to say, nothing but abbreviations; and they all go back to one specific logical concept – the concept of derivability, "/", introduced by the undoubtedly trivial rules of sections 2 and 3.

8. Derivation and Demonstration

230 | If we omit from our system the definitions of tautology and contradiction, D5.0 and D5.00, then our whole system is one of *purely derivational logic*. By this we mean the following. Take any language in which ordinary descriptive statements occur, such as, in English, statements like "If it is raining then the streets are slippery" whose truth or falsity cannot be decided by merely logical considerations (in contra-distinction to the tautology "If it is raining then it is raining"). Call these descriptive statements "non-logical statements". Then we can define:

(D8.1) A system of primitive rules of derivation is called "purely derivational" if, and only if, we can give such examples for each primitive rule that all of the statements (whether components or compounds) which occur in the examples are non-logical.

The point of a purely derivational logic is this: it is a system intended from the

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and:

$$\text{If } x \neq y, \text{ then } (Axa)\binom{y}{z} // Ax(a\binom{y}{z}).$$

Corresponding rules of substitution hold for existential quantification. ⟨Footnote added in the Errata.⟩

start to be a theory of inference in the sense that it allows us to derive from certain informative (non-logical) statements other informative statements.

Most systems of modern logic are not purely derivational, and some (for example in the case of Hilbert Ackermann) are not derivational at all. They operate not so much with rules of inference as with axioms or with rules of proof (axiom schemata). That is to say, they take as primitive such assertions as “All statements designated by ‘$a > a$’ are true” (or “are provable” or “follow from the empty class of premises”, etc.; these are just so many ways of stating axiom schemata).

These procedures are in themselves unexceptionable. But they are liable to blur the distinction between derivation and proof; they are liable to create the impression that every logical proof is a derivation whose first premises are logical axioms.

But this impression is definitely wrong. It is true that a derivation whose premises are logical axioms is a proof. But this is so only (1) because every derivation whose premises are provable is a proof, and (2) because the so-called logical axioms are in fact provable.

Let us consider for a moment one of the intuitive ways which we use in proofs, – say in an indirect proof.

Assume that we succeed in deriving, from one premise a

$$a/b \tag{1}$$

$$a/\neg b \tag{2}$$

231 | Then we argue in this way: a must be absurd, for from it follows b as well as $\neg b$, and therefore $b \wedge \neg b$, which is certainly false. Thus a is *logically refuted*; but its negation, $\neg a$, must be true. Thus we have proved $\neg a$.

If we look at this argument, then we see that it refers to two derivations, (1) and (2); and that it argues from derivability to refutability and demonstrability.

In this way, *all proofs refer to derivations*; the derivations are the most conspicuous parts in proofs; but the derivation is not the proof.

Take another example.

If we find that the following derivations are valid

$$a/b \tag{1}$$

$$\neg a/b \tag{2}$$

then we say that we have proved b. For we say, one of the two premises, a or $\neg a$, must be true; and if both the derivations are valid, the truth of the true premise must be transmitted to the conclusion. Thus b must be true – we have proved b.

Now what these proof schemata have in common is this: They use derivation.

But while *derivation* only establishes that the conclusion is true *provided* that the premises are true, the proof seeks to establish the truth of its conclusion independently of the question whether the premises are true or false. It tries to establish the truth of the conclusion unconditionally.

But is such a thing possible? Yes, it is possible, although only for comparatively rather uninformative statements – for what we may call “logical truisms”.

We can now give a definition of a logical proof or demonstration:

(D8.2) The statement a is demonstrable $\leftrightarrow$ the statement a can be validly derived from any premise whatsoever.

This means, indeed, that the statement can be shown, by means of derivations, to be true unconditionally – independently of any particular premise. (If we derive it from b and doubt the truth of b, we can at once derive it from $\neg b$ also, or from c, etc.)

We shall write

$$\vdash a$$

for "the statement a is demonstrable". Then D8.2 becomes:

(D8.2+) $$\vdash a \leftrightarrow \text{(for any statement } b\text{: } b/a);$$

we can, similarly, define refutability, writing

$$\urcorner\!\!/\, a$$

for "a is refutable":

(D8.3) $$\urcorner\!\!/\, a \leftrightarrow \text{(for any statement } b\text{: } a/b).$$

232 Now on the basis of our purely derivational logic, it is possible to show strictly: |

(8.1) All the so-called axioms of the logic of propositions and propositional functions are provable; for example, those of *Principia Mathematica*, etc.

(8.2) Every statement that can be validly derived from a demonstrable statement is demonstrable, *i.e.*,

$$(\vdash a \;\&\; a/b) \vdash b.$$

(8.3) If we add demonstrable statements to the premises of any derivation, then the validity or invalidity of the derivation remains unaffected.

All these are principles which have been more or less intuitively used so far, but which can be strictly established as consequences of our definition of proof.

But there are further results. For example we can easily show

(8.4) $$a/b \to (\vdash a \to \;\vdash b),$$

which is only another form of 8.2; *but at the same time, the converse is not true*, that is to say, we must be careful not to assert that

$$(\vdash a \to \;\vdash b) \to a/b.$$

(Take a to be non-logical and b to be contradictory: this is a counterexample.)

This warning is very important, for there are valid *rules of proof* of the theory of statement-functions such that

(8.5) $$\vdash a_{\hat{y}} \leftrightarrow \;\vdash a_{\hat{y}}\binom{x}{y}$$

for which the corresponding *rule of derivation*

$$a_{\grave{y}} / a_{\grave{y}} \binom{x}{y}$$

is invalid.[27] Among the valid rules of proof are the important rules

$$(8.9) \qquad a_{\grave{x}} / b \rightarrow a_{\grave{x}} / Axb.$$

$$(8.91) \qquad a_{\grave{y}} / b_{\grave{y}} \binom{x}{y} \rightarrow a_{\grave{y}} / Axb.$$

Now these – for example 8.9 – may be transformed in this way:

$$(8.92) \qquad \vdash a_{\grave{x}} > b \rightarrow \vdash a_{\grave{x}} > Axb.$$

But they must not be transformed in this way:[28]

$$a_{\grave{x}} > b / a_{\grave{x}} > Axb,$$

233 | because otherwise

$$\vdash a > Axa$$

becomes provable, and with it,

$$a // a \binom{x}{y}$$

could be shown to be valid – which violates our postulate PF4, of section 6.

Now all the mistakes here warned against do actually vitiate some otherwise very excellent books on modern logic – an indication that the distinction between (conditional) rules of proof or rules of demonstration on the one side and rules of derivation on the other cannot be neglected without involving oneself in contradictions.

It may be mentioned, in passing, that derivational logic *must* operate with a theory of open sentence functions, while demonstrational logic may neglect this part of logic.

We may perhaps sum up by saying that a purely derivational logic can get over an awkward dualism which demonstrational logic cannot avoid. Demonstrational logic must start with axioms – at least one – and rules of inferences – at least something like the *modus ponens*. As opposed to this, derivational logic operates monistically, *i.e.*, with rules of inference alone. The so-called axioms of the various systems of the logic of demonstration can be proved without assuming one of them.

9. Metalanguage and Object Language

Why do we study logic? We all understand our language, which means that we all know how to draw inferences, as we have seen. We do not need logical theory for

[27] *Cp.*, as opposed to this, Carnap, *Formalization of Logic* ⟨Carnap, 1943⟩, p. 137, D. 28–2, rule 10.

[28] *Cp.* Carnap, *ibid.*, p. 138, rule 11; this is undoubtedly suggested by Hilbert-Bernays' way of writing these rules in the same style as the *modus ponens* (*cp.* pp. 105f.) which suggests that they are rules of derivation. Hilbert Ackermann, on the other hand, write p. 57 the *modus ponens* as if it were a conditional rule of proof.

being able to draw inferences; nor does the study of logical theory lead to a method of making all inferences fool-proof.

There are two obvious reasons for studying logic. One is curiosity, theoretical interest in languages and ⟨in⟩ their rules of use. This needs no apology. The other is practical. Our naturally grown languages have not been designed for the use to which we put them in scientific and mathematical investigations. They definitely do not always stand up too well to the strain of modern civilisation: paradoxes arise, spurious theories; the conclusiveness of some of our subtle arguments becomes doubtful. Such practical needs lead us to study the rules of language in order to design an instrument fit for use in science.

But for the study of language, we have to *use* language; and we have to use it freely and fully, without being afraid of using it. There is no study of any object whatever
234 without the full | use of language if we need it. As the zoologist uses language to describe the behaviour of reptiles or lions, so the student of language must use language in order to describe the behaviour of statements, formulae, variables, etc.

But is there not a danger that our studies may be vitiated if we use and study language at the same time? Undoubtedly there is. But there exists a simple device which seems to get over these difficulties beautifully. It is, simply, always to distinguish the language which is the object of our studies (the "object language") from the language we are using. The latter, if used for the discussion of other languages, is called the "metalanguage".

The fundamental idea of the distinction between object language and metalanguage is this: we simply cannot use *and* discuss one and the same language at the same time. Thus we decide to acknowledge the impossibility, and *we do not worry too much about the language we are using* – as long as we are using it. If we feel doubtful whether our way of using it was quite in order, then nothing will prevent us from proceeding, later, to an investigation of our metalanguage, using thereby what has been called a "meta-metalanguage" (etc., as long as we like to go on).

I mentioned that we should use the metalanguage freely. By this I mean that we must not be deterred, for example, from using conjunction in the metalanguage when defining conjunction in the object language. We *must* speak, if we wish to define. It is impossible to speak properly in our language of ordinary use without using conjunction, the conditional, and indeed all its formative signs. Here is no vicious circularity: we do not attempt to define the words we are using, but we use words in order to define very general concepts referring to other languages.

The demand that, in order to avoid circularity, we should avoid, in defining signs of the object language, the use of formative signs in the metalanguage, is no better than the impossible demand that we should avoid the use of statements in the metalanguage; for in most languages we can have no statements at all if we avoid formative signs. Incidentally we can, in our definitions, avoid even the appearance of circularity – we can for example define negation without making use of negation in the metalanguage, and we can define "$a \wedge b$" without using "&" in the metalanguage, and "$a \vee b$" without using "or".

As to our own investigations, we find, if we proceed to study our metalanguage in
235 the meta-metalanguage, that our meta-language | can be easily formalised. It turns

out that we do not need more than positive logic, including quantifiers (if we exclude the postulate PF4 which need not be assumed). Thus it happened that we defined negation, without using it in the meta-language (incidentally and without intending it). This fact may be interesting for some reason or other; but had we to make use of negation in the metalanguage, in order to define what we mean by negation in the various object languages we were then studying, there would have been no circularity involved.

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Chapter 4
Functional Logic without Axioms or Primitive Rules of Inference (1947)

Karl R. Popper

Abstract This article is a corrected reprint of K. R. Popper (1947d). Functional Logic without Axioms or Primitive Rules of Inference. In: *Koninklijke Nederlandse Akademie van Wetenschappen, Proceedings of the Section of Sciences* 50, pp. 1214–1224.

Editorial notes: The article was reprinted with the printing plates for Popper (1947d) as K. R. Popper (1947e). Functional Logic without Axioms or Primitive Rules of Inference. In: *Indagationes Mathematicae* 9, pp. 561–571. In the latter, the title is followed by the notes "(Communicated by Prof. L. E. J. Brouwer.)" and "(Communicated at the meeting of October 25, 1947.)", referring to the meeting of the Koninklijke Nederlandse Akademie van Wetenschappen. These notes are omitted in the present version. The title of the former was newly typeset and the addition "(Communicated by Prof. L. E. J. Brouwer.)" was dropped from the title. The original pagination (pp. 1214–1224) was preserved in brackets, and the new pagination (pp. 561–571) was added. Page numbers in our margins thus refer to Popper (1947d) but the page numbers of Popper (1947e) are also provided in the concordance. For better readability we have reformatted lists (a)–(d), (a′)–(c′) and (1)–(2) from normal paragraphs to items with block text. A list of errata in footnote 1 for Popper (1947c) is omitted here. These errata are contained in the more complete list of errata Popper (1948e) and were corrected in this volume. The symbol *V* for the metalinguistic "or" has been replaced throughout by $\bigvee$. Details concerning the publication of this article can be found in Popper's correspondence with Brouwer; cf. Chapter 22 of this volume.

I

1214 The customary systems of modern lower functional logic, such as *Principia Mathematica*, or the systems of Hilbert-Ackermann, Hilbert-Bernays, or Heyting, etc., proceed in the following way.

(a) Undefined primitive *formative signs* of the system under construction are laid down – at least *one* (either alternative denial or conjoint denial) for propositional logic and *one* (the universal or existential quantifier) for quantification theory.

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(b) Unproved primitive propositions or axioms are laid down – at least one for propositional logic, and one for quantification theory.
(c) Primitive rules of inference (such as the *modus ponens*) for the formative signs are laid down, at the very least one for propositional logic and one for quantification theory.
(d) Some further very general primitive rules of inference (such as some principles stating that the inference relation is transitive and reflexive) which do not refer to formative signs are assumed, either explicitly or, more often, tacitly.

The procedure here described is not always strictly adhered to, to be sure; the axioms (b) for example, may be expressed in the form of rules of inference (c), etc. – But to my knowledge, the scheme sketched is the one most frequently adopted.

In an earlier publication[1], I have outlined a method by which the procedure can be considerably simplified. This method consists, in the main, in this.

It is observed that the traditional procedure must make use of a language of
1215 communication – the *metalanguage* – in which it discusses and | describes the
linguistic system under consideration – the *object language*. It is further observed that all systems are bound to use metalinguistic concepts such as "The conclusion a is derivable from the premises $a_1, a_2, \ldots, a_n$", and "If a is demonstrable then b is demonstrable", etc.

Now it can be shown that, if we have at our disposal the first of these two metalinguistic ideas, i.e., the concept of deducibility (deducibility from n premises is an $n + 1$-termed metalinguistic predicate), characterized by a few trivial rules of inference of the kind (d), such as the transitivity and reflexivity principles which are assumed in all the customary systems, then the following holds:

(a′) No sign such as a sign for alternative denial or for a quantifier need be laid down as primitive, since names for the alternative denial and the quantifiers and all the other formative signs of an object language can be defined in terms of deducibility.
(b′) No unproved axioms or primitive propositions need to be assumed, since the axioms or primitive propositions of the usual systems can be shown to be all demonstrable (on the basis of an explicit definition of demonstrability in terms of deducibility).
(c′) No primitive rules of inference of kind (c) need to be laid down, since all the rules of inference pertaining to the formative signs of the system follow from the definitions of these signs.

In other words, given the concept of deducibility and a few primitive rules of kind (d) to characterize it (I have called an independent system of such rules a "basis"), everything else can be obtained by way of explicit definitions in terms of deducibility ("inferential definitions").

In this way, a considerable simplification, as compared with the customary systems, is achieved. But, as will be shown here, we can even go further.

[1] Cp. *New Foundations for Logic* ⟨Popper, 1947c⟩. In this paper there are a number of *errata*, of which some that may cause trouble to readers of the present paper may be listed here: ⟨We omit Popper's list. These errata have been corrected in this volume.⟩

The inferential definitions of the conjunction (or the alternative denial) and of the result of one of the quantifications, for example, of universal quantification, can be reformulated in such a way as to incorporate *all* the rules of inference mentioned. In this way, we can get rid of even the few trivial primitive rules (d) which were left in the previous approach; in other words, we obtain the whole formal structure of logic from metalinguistic inferential definitions alone.

II

Our theory is completely metalinguistic.

Our universe of discourse consists of expressions of certain object languages; we consider especially the *statements* (open or closed) and the *name-variables* of these languages.

We shall use, in the metalanguage, "a"; "b"; "c"; etc.; "a_1"; "a_2"; "a_n"; "b_1"; "b_2"; "b_m"; etc., as variables whose values are the statements (open or closed) of an object language under consideration, and "u"; "v"; "w"; "x"; "y"; "z"; as variables whose values are the name-variables of that object-language.

1216 | We shall use two undefined concepts: (1) deducibility and (2) substitution.

(1) The notation

$$\text{“}a_1, \ldots, a_n/b\text{”}$$

will be used in order to express the assertion "From the statements $a_1, a_2, \ldots, a_n$ of the object language, the statement b of that language can be deduced." The sign "/" is an $n + 1$-termed predicate of the metalanguage.

(2) The notation

$$\text{“}a\binom{x}{y}\text{”}$$

will be used as a (variable) metalinguistic name of the statement which is the result of substituting, in the statement a (open or closed), the variable y for the variable x, wherever it occurs. $a\binom{x}{y}$ is identical with a if x does not occur in a.

These two are the only specific metalinguistic primitive signs used in our metalanguage.

Apart from the specific metalinguistic or logical signs "/" and "$a\binom{x}{y}$", we shall use, in the metalanguage, the normal means of expression, including "and", "if and only if", etc.

The following *abbreviations* will be used in the metalanguage:

"... → ..." for "if ... then ..."
"... ↔ ..." for "... if, and only if ..."
"... & ..." for "... and ...".

Also, the expression "for every ..." (followed by variables) will be used, and "=" and "≠" (flanked by "x", "y", etc.). Brackets will be used in the customary way.

We do not undertake here to formalize our metalanguage. But it may be mentioned that the only logical rules needed in the metalanguage (except where we treat modalities) are those of the positive part of the Hilbert-Bernays calculus of propositions as far as they pertain to "if-then", "if, and only if", and to "and" (i.e. groups I, II, IV), and the rules for identity. The rules for negation need not be assumed (only some of their applications to "$x \neq y$"); but we need rules for universal quantification, especially the rule of specification: "If (for every $a_1, \ldots, a_n$: $(F(a_1, \ldots, a_n))$) then $F(b_1, \ldots, b_n)$."

III

Apart from the primitive signs, *defined* signs are introduced by definitions. These are, except the sign "//" for mutual deducibility, variable metalinguistic names of certain statements (open or closed), viz., either of so-called compounds or of the results of quantification.

1217 We shall use the following signs, to be defined later: |

1. "$a//b$" will be used to express that a/b and b/a.
2. "$a \wedge b$" will be used as name of the conjunction of a and b.
3. "$a > b$" will be used as name of the conditional statement with the antecedent a and the consequent b.
4. "$a \vee b$" will be used as name of the disjunction of a and b.
5. "a^c" will be used as name of the classical negation of a.
6. "a^i" will be used as name of the intuitionist negation of a.
7. "$(Aza)\binom{x}{y}$" will be used as name of the statement which results from first applying to a universal quantification with regard to z and then substituting, in the result, y for x wherever it occurs.
8. "$Az(a\binom{x}{y})$" will be the name of the statement which results from the universal quantification of $a\binom{x}{y}$ with regard to z.
9. "$(Eza)\binom{x}{y}$" is like 7, but with existential quantification.
10. "$Ez(a\binom{x}{y})$" is like 8, but with existential quantification.

We shall not lay down primitive rules or axioms etc. for our undefined terms, but we shall, instead, frame three of our explicit definitions in such a way that the few rules needed to characterize our undefined terms emerge from these definitions. These three definitions will be fundamental to the others, and will be called "Basic Definitions" (DB).

The definition DB1, of "//", is basic for all others; DB2, of "$\wedge$", is basic for propositional (and therefore also for functional) logic; and DB7, of "$(Axb)\binom{x}{y}$", is basic for quantification theory. Each of the other definitions of propositional logic presupposes DB1 and DB2, and D8 to D10 presuppose, in addition, DB7. None of them except D10 presupposes other than Basic Definitions. The only definitions in which defined terms (other than "//") are used are D8 and D10, which use terms defined in D7 and D9 respectively. (The relation between D8 and D7, or D10 and D9, is somehow akin to recursiveness; but these are the only definitions needed in which anything resembling recursiveness occurs.)

IV

We now proceed to the definitions of the concepts mentioned. The first definition, of "//", uses only "↔" apart from "/". All the later ones make use of "//" also. They have all the form

$$\text{"}a/\!/\text{the definiendum} \leftrightarrow \text{(for every} \ldots\text{: (}\ldots\text{))"}$$

That is to say, we do not define, e.g., conjunction, but rather the logical force of conjunction. (I have discussed this problem at length elsewhere[1,2].)

Here is the list of the definitions.

General.

(DB1) $$a/\!/b \leftrightarrow (\text{for every } c\text{: } (a/c \leftrightarrow b/c))$$

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(DB2) $$a/\!/b \wedge c \leftrightarrow (\text{for every } a_1, \ldots, a_n\text{: } ((a_1, \ldots, a_n/a \leftrightarrow (a_1, \ldots, a_n/b \;\&\; a_1, \ldots, a_n/c)) \;\&\; (b/c \rightarrow (a_n, \ldots, a_1/b \rightarrow a_1, \ldots, a_n/c)) \;\&\; (a_1, \ldots, a_n/c \rightarrow a_1, \ldots, a_n, b/c) \;\&\; a_1/a_1))$$

(D3) $$a/\!/b > c \leftrightarrow (\text{for every } d\text{: } (d/a \leftrightarrow b, d/c))$$

(D4) $$a/\!/b \vee c \leftrightarrow (\text{for every } d\text{: } (a/d \leftrightarrow (b/d \;\&\; c/d)))$$

(D5) $$a/\!/b^c \leftrightarrow (\text{for every } d\text{: } (a, b/d \;\&\; (a, d/b \rightarrow d/b)))$$

(D6) $$a/\!/b^i \leftrightarrow (\text{for every } d\text{: } (a, b/d \;\&\; (b, d/a \rightarrow d/a)))$$

Note[3]: In the presence of D5, D6 is equivalent to D5; in the absence of D5, D6 is weaker than D5.

Quantification Theory.

(DB7) Provided $x \neq y$,

$$\begin{aligned} a\tbinom{x}{y}/\!/(Axb)\tbinom{x}{y} \leftrightarrow\ & (\text{for every } c, \text{ and every } u, v, \text{ and } w\text{:} \\ & ((c\tbinom{x}{y}/a\tbinom{x}{y} \leftrightarrow c\tbinom{x}{y}/b\tbinom{y}{x}) \\ & \& \left((v \neq x \;\&\; y \neq u) \rightarrow (c\tbinom{x}{u})\tbinom{y}{v}/\!/(c\tbinom{y}{v})\tbinom{x}{u}\right) \\ & \& \; (c\tbinom{y}{x})\tbinom{y}{w}/\!/c\tbinom{y}{x} \;\&\; (c\tbinom{u}{v})\tbinom{v}{w}/\!/(c\tbinom{u}{w})\tbinom{v}{w} \\ & \& \; (c\tbinom{u}{v})\tbinom{w}{v}/\!/(c\tbinom{w}{v})\tbinom{u}{v} \end{aligned}$$

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[2] Cp. *Logic without Assumptions* ⟨p. 83⟩, esp. section (7).

[3] This remark, which follows from the comments made in *New Foundations* upon rule 4.2e, qualifies an assertion made there on ⟨p. 137⟩.

$$\& \; ((\text{for every } z\text{: } (b /\!/ b\tbinom{u}{z} \;\&\; c /\!/ c\tbinom{u}{z}))$$
$$\rightarrow (b /\!/ c \rightarrow b\tbinom{v}{u} /\!/ c\tbinom{v}{u}))) \;\&\; c\tbinom{u}{u} /\!/ c))$$

(D8) $$(Aua)\tbinom{x}{y} /\!/ Az(a\tbinom{x}{y}) \;\leftrightarrow\; ((u = x \rightarrow y = z) \;\&\; (u \neq x \rightarrow u = z))$$

(D9) $$a\tbinom{x}{y} /\!/ (Exb)\tbinom{x}{y} \;\leftrightarrow\; (\text{for every } c\text{: } (a\tbinom{x}{y}/c\tbinom{x}{y} \;\leftrightarrow\; b\tbinom{y}{x}/c\tbinom{x}{y}))$$

(D10) $$(Eua)\tbinom{x}{y} /\!/ Ez(a\tbinom{x}{y}) \;\leftrightarrow\; ((u = x \rightarrow y = z) \;\&\; (u \neq x \rightarrow u = z))$$

V

I shall now briefly outline the method by which we obtain, from this system of inferential definitions, the systems of rules of inference which were sketched in my earlier papers referred to.

Since

(5.01) $$a/c \;\leftrightarrow\; a/c$$

is true whatever the meaning of the predicate "/" may be, we obtain, substituting a for b, from DB1

(5.02) $$a /\!/ a$$

and therefore

(5.03) $$b \wedge c /\!/ b \wedge c$$

This allows us to obtain from DB2, by substituting "$b \wedge c$" for "a", the set of five rules

(5.20) $$a_1, \ldots, a_n/b \wedge c \;\leftrightarrow\; (a_1, \ldots, a_n/b \;\&\; a_1, \ldots, a_n/c)$$

1219 (5.201) $$b/c \rightarrow (a_n, \ldots, a_1/b \rightarrow a_1, \ldots, a_n/c)$$ |

(5.202) $$a_1, \ldots, a_n/c \rightarrow a_1, \ldots, a_n, b/c$$

(5.203) see below

(5.204) $$a_1/a_1$$

Rule 5.203, viz.:

(5.203) $$a_n, \ldots, a_1/c \rightarrow a_1, \ldots, a_n/c$$

can be easily obtained from 5.201 and 5.204. Now it is quite obvious that the system of the four rules 5.201 to 5.204 is equivalent to the following four rules 5.21 to 5.24:

(5.21) $$a_1, \ldots, a_n/b \rightarrow (b/c \rightarrow a_1, \ldots, a_n/c)$$

(5.22) $$a_1, \ldots, a_n/b \rightarrow a_1, \ldots, a_{n+1}/b$$

(5.23) $$a_1, \ldots, a_n/b \rightarrow a_n, \ldots, a_1/b$$

(5.24) $$a/a$$

But this set, together with 5.20, is equivalent to one which I have elsewhere[1] described as "Basis II", and which can be shown to provide a sufficient basis for propositional logic, and especially for the definitions D3 to D6. In the presence of 5.20, the rules 5.21 to 5.24 characterize our first undefined concept, "/", sufficiently for operating with it successfully.

By the same method, i.e., by substituting "Axb" for "a" (and "a" for "c") in DB7, we obtain the rule

(5.70) $$a\binom{x}{y}/(Axb)\binom{x}{y} \leftrightarrow a\binom{x}{y}/b\binom{y}{x}$$

and together with it the following six rules which characterize our second undefined concept, "$a\binom{x}{y}$", sufficiently for operating with it successfully. These rather trivial rules are

(5.71) If $v \neq x$; $x \neq y$; and $y \neq u$, then: $(a\binom{x}{u})\binom{y}{v} // (a\binom{y}{v})\binom{x}{u}$

(5.72) If $x \neq y$, then: $(a\binom{x}{y})\binom{x}{z} // a\binom{x}{y}$

(5.73) $$(a\binom{x}{y})\binom{y}{z} // (a\binom{x}{z})\binom{y}{z}$$

(5.74) $$(a\binom{x}{y})\binom{z}{y} // (a\binom{z}{y})\binom{x}{y}$$

(5.75) If, for every z, $a//a\binom{y}{z}$ and $b//b\binom{y}{z}$, then: $(a//b \rightarrow a\binom{x}{y}//b\binom{x}{y})$

(5.76) $$a\binom{x}{x}//a$$

If we now consider also definitions other than basic ones then we find that, in the presence of 5.20 to 5.24, each of the definitions DB1 to D6 is equivalent to the corresponding rule of inference in the following list:

(5.1) $$a//b \leftrightarrow (a/b \;\&\; b/a)$$

(5.2) $$a \wedge b/c \leftrightarrow a, b/c$$

(5.3) $$a/b > c \leftrightarrow a, b/c$$

(5.4) $$a \vee b/c \leftrightarrow a/c \;\&\; b/c$$

(5.5) $$a^c, a/b \;\&\; (a^c, b/a \rightarrow b/a)$$

(5.6) $$a^i, a/b \;\&\; (a, b/a^i \rightarrow b/a^i)$$

1220 | Turning to the non-basic definitions of Quantification Theory, we find that from D8, we obtain the two rules

(5.81) If $x \neq z$, then: $(Azb)\binom{x}{y}//Az(b\binom{x}{y})$

(5.82) If $x = z$, then: $(Azb)\binom{x}{y}//Ay(b\binom{x}{y})$

which I have described elsewhere[4] as “rules of substitution”, and which relate the two concepts defined in DB7 and D8 to one another. Corresponding remarks hold for D9 and D10.

It will be seen that the two Basic Definitions DB2 and DB7 are somewhat complicated. Yet I think that the procedure of incorporating not only 5.20 into a definition of “$\wedge$” but also the rules which make it possible to operate with 5.20, is perfectly natural. Take DB1. The meaning of “$\wedge$” is determined not by 5.20 alone, but only by 5.20 in connection with 5.21 to 5.24; it seems therefore appropriate to incorporate 5.21 to 5.24 into our definition of conjunction. Analogous remarks hold for the six rules incorporated, together with 5.70, in DB7. (See also the next section, VI.)

It may be asked why conjunction is singled out, of all the propositional compounds, to serve as basic. The answer is given in an earlier publication[1]: conjunction (more precisely, rule 5.20) allows us to simplify the transitivity principle, and such a simplification is necessary if we wish to incorporate the transitivity principle into a definition which can be formulated with the means of expression described in section II. Nevertheless, conjunction is not the only compound which can be used in this way. Alternative Denial can be used as well.

In the presence of DB1 and DB2, alternative denial can be defined in various ways, for example:

$$
\begin{aligned}
(\mathrm{D}\curlywedge)\quad a//b \curlywedge c \;\leftrightarrow\; &(\text{for every } d_1\text{: }((\text{for every } d_2\text{: } (d_2/d_1 \;\leftrightarrow\; (d_2/b \;\&\; d_2/c)))\\
&\leftrightarrow\; (\text{for every } d_3\text{: } (a, d_1/d_3 \;\&\; (a, d_3/d_1 \rightarrow d_3, d_3/d_1)))))
\end{aligned}
$$

All that is necessary for strengthening this definition in such a way that it can serve as basic instead of DB2 is (1) to replace “d_2” by “$a_1, \ldots, a_n$”, and (2) to incorporate the rules 5.201 to 5.204 in a manner precisely analogous to the way in which they are incorporated in DB2.

VI

Against our procedure of incorporating the rules 5.201 to 5.204 into DB2, and 5.71 to 5.76 into DB7, the objection may be raised that it does not lead to homogeneous definitions. I do not think this objection sufficiently important to discuss it here at length. But I wish to mention that at least one attempt to define the intuitive idea of homogeneity which underlies this objection leads to the following result.

Rules 5.20 and 5.204 alone are (in the presence of DB1) sufficient for proving
1221 that the left hand side of DB2 follows from the right hand side; | the other rules, 5.201 and 5.202, which are together with 5.20 and 5.204 incorporated in DB2, are not needed for this purpose. This shows, it may be said, that they should be taken out of DB2, and stated separately, as primitive rules. Corresponding remarks may

[4] Cp. *New Foundations for Logic*, ⟨footnotes 24 and 25⟩.

be made about DB7. Here rule 5.70 alone is sufficient (in the presence of DB1 and DB2) for establishing that the left hand side of DB7 follows from the right hand side. 5.71 to 5.76 are superfluous for this purpose.

In order to meet this objection, we can easily transform DB2 and DB7 in such a way that the objection no longer holds. All that is necessary for this purpose is to replace, in DB2, the expression

$$\text{“}a_1, \ldots, a_n/b\text{”}$$

by the expression[a]

$$\text{“}((b/c \rightarrow (a_1, a_2/b \rightarrow a_3, \ldots, a_n/c)) \rightarrow a_n, \ldots, a_1/b)\text{”}.$$

The resulting definition is then "homogeneous" in the sense that all rules (components) tied up in the definiens are now needed for showing that the left hand side follows form the right hand side. Similarly, in DB7, we need only replace the expression

$$\text{“}\left(c\binom{x}{y}/a\binom{x}{y} \leftrightarrow c\binom{x}{y}/b\binom{y}{x}\right)\text{”}$$

by the expression

$$\text{“}\left(x \neq u \neq y \rightarrow \left((c\binom{u}{u})\binom{y}{x}/(a\binom{u}{u})\binom{y}{x} \leftrightarrow (c\binom{y}{y})\binom{y}{x}/(b\binom{y}{y})\binom{x}{y}\right)\right)\text{”}$$

in order to achieve the corresponding result. In other words, homogeneity of the kind here indicated (and the same seems to hold for some other kinds) can always be attained in a trivial way, at the price of making the formulae more complicated. It is therefore hardly worth worrying about.

It may be mentioned, in this connection, that there are quite a number of equivalent ways of writing DB2 and DB7. The shortest formula to take the place of DB2 known to me is the following. (We assume that $n + r \geq n \geq 1$.)

$$\text{(DB2}'\text{)} \quad a /\!/ b \wedge c \leftrightarrow \text{(for every } a_1, \ldots, a_{n+r}\text{:}$$
$$((a_1, \ldots, a_n/a \leftrightarrow (a_1, \ldots, a_n/b \ \& \ a_1, \ldots, a_n/c))$$
$$\& \ (a_n, \ldots, a_1/b \rightarrow (b/c \rightarrow a_1, \ldots, a_{n+r}/c)) \ \& \ b/b)).$$

VII

Our system is sufficient for lower functional logic in the sense that its rules of inference permit all logical transformations which are permissible by force of the usual lower functional calculi, and that they allow us to establish the demonstrability of all formulae which are demonstrable in these calculi.

In order to establish sufficiency, it is advisable to introduce the one-termed metalinguistic predicate

---

[a] Originally, this expression reads: $((b/c \rightarrow (a_1, a_2/b \rightarrow a_3, \ldots, a_1/c)) \rightarrow a_n, \ldots, a_1/b)$. We have replaced $a_3, \ldots, a_1/c$ by $a_3, \ldots, a_n/c$.

$$\text{“}\vdash a\text{”}$$

1222 | which we read as “*a* is demonstrable”. It can be defined by

(D11) $\vdash a \leftrightarrow$ (for every b: (b/a))

With the help of the means at our disposal, it is easy to show that this definition is equivalent to the formula

(7.11) $\vdash a \leftrightarrow a > a/a$

This formula could be used as a definition. Provided we do not object to the use of the defined term “$a > a$” in the definiens, it may even be said to be preferable because it uses *one* variable only, and avoids metalinguistic quantification.

The sufficiency of our system can now be established in the following sense.

Whenever a “proposition” such as

$$\text{“}p \supset p\text{”}$$

can be “asserted” in the system, say, of *Principia Mathematica*, then the corresponding formula, in our case

$$\text{“}\vdash a > a\text{”}$$

can be obtained in our metalinguistic system, by using only our definitions.

Whenever the formula in question is intuitionistically valid, it is only necessary to use, besides Basic Definitions, the definitions of the symbols actually occurring in the formula. If it is intuitionistically invalid but classically valid, then, even if no symbol of negation occurs in the formula, D5, the definition of classical negation, is needed for establishing its demonstrability.

VIII

If we wish to apply our system to some object language *L*, then certain existential postulates must be added which correspond to the definitions, such as

(P2) If b and c are both statements of L, then there exists a statement a of L such that $a//b \wedge c$, and that DB2 is satisfied.

Or:

(P5) If b is a statement of L, then there exists a statement a of L such that $a//b^c$, and that D5 is satisfied.

(And so on.)

Of course, P2 and P5 suffice for propositional logic, if we wish to use classical negation, or even *one* postulate demanding the existence of $a//b \curlywedge c$ for every b and c. (This is the real advantage of using alternative denial.)

IX

1223 | Our method can be easily extended so as to comprise modal functions. We can introduce the six symbols[5]:

(N) "$N\,a$" for "a is necessary".

(I) "$I\,a$" for "a is impossible".

(L) "$L\,a$" for "a is logical" (i.e. either necessary or impossible).

(C) "$C\,a$" for "a is contingent" (or factual).

(P) "$P\,a$" for "a is possible".

(U) "$U\,a$" for "a is uncertain" (or not necessary).

These explanations are inexact, since the six symbols are actually intended to serve as *names* of statements of the object language. "$N\,a$", for example, should be explained, more lengthily but more precisely, as the name of a statement which asserts the necessity of the state of affairs described by a (or the necessity of the proposition expressed by a).

For the definitions, we must use "or" in the metalanguage (abbreviated by "$\bigvee$"), based on the appropriate rules. We shall also use "$\vdash a$" and "$\urcorner a$" (i.e. "a is refutable"), defined by

(D12) $$\urcorner\, a \leftrightarrow \text{(for every } b\text{: } (a/b))$$

But "$\vdash$" and "$\urcorner$" are used here as abbreviations only, and should be eliminated with the help of D11 and D12.

(For the development of the theory of modality, metalinguistic negation is also needed, together with at least its intuitionistically valid rules.)

Our six definitions are:

(DN) $a//N\,b \leftrightarrow ((\vdash a \bigvee \urcorner\, a) \;\&\; (\vdash a \leftrightarrow \;\vdash b))$

(DI) $a//I\,b \leftrightarrow ((\vdash a \bigvee \urcorner\, a) \;\&\; (\vdash a \leftrightarrow \;\urcorner\, b))$

(DL) $a//L\,b \leftrightarrow ((\vdash a \bigvee \urcorner\, a) \;\&\; (\vdash a \leftrightarrow (\vdash b \bigvee \urcorner\, b)))$

(DC) $a//C\,b \leftrightarrow ((\vdash a \bigvee \urcorner\, a) \;\&\; (\urcorner\, a \leftrightarrow (\vdash b \bigvee \urcorner\, b)))$

(DP) $a//P\,b \leftrightarrow ((\vdash a \bigvee \urcorner\, a) \;\&\; (\urcorner\, a \leftrightarrow \;\urcorner\, b))$

(DU) $a//U\,b \leftrightarrow ((\vdash a \bigvee \urcorner\, a) \;\&\; (\urcorner\, a \leftrightarrow \;\vdash b))$

The main interest of these definitions lies in the fact that our method allows us here again to avoid the use of defined concepts, such as negation, in the definiens.

[5] This list of six modal functions corresponds to the table given by R. Carnap, *Meaning and Necessity* ⟨Carnap, 1947⟩, p. 175. Cp. also my *Logic without Assumptions* ⟨Popper, 1947b⟩ where, in ⟨footnote 20⟩, a definition of impossibility is given which is equivalent to the one given here, and simpler. The more complicated definition given here has however the advantage of belonging to a complete set of six analogous definitions for all the modal functions.

Accordingly, any sub-set of these definitions can be combined, for example, with positive logic, avoiding the use of classical or of any negation. It is also possible, for
1224 example, to introduce only “$I\,a$” | (impossibility), and to compare the result with the intuitionist use of negation, etc. Our formulae yield, for example,

(9.1) $$I(I\,a)//P\,a$$

thus justifying an intuitively obvious theorem (much used by intuitionism).

It appears that from these and our previous definitions taken together, all the valid rules of modal logic (including quantification theory) can be obtained).

X

The question may be raised whether our method is not so powerful merely because we employ fairly strong logical means in our metalanguage. It is therefore worth mentioning that, if we replace our system of definitions (e.g. DB2) by the system of rules which can be obtained from them (e.g. 5.20 to 5.24; note also that DB1 and D11 can be replaced by 5.1 and 7.11 respectively which do not need any quantifiers, and that corresponding adjustments can easily be made in the definitions of the modalities), then there is no need whatever to operate with quantifiers in the metalanguage. (This result is, perhaps, of some interest in connection with the decision problem). The logical rules of operating in the metalanguage may be reduced, in this way, to the rules pertaining to “→” and to “&”. (If we split up our rules 5.1 to 5.4 etc. sufficiently, even “&” may be avoided.) But no system of logic can be constructed without using at least “→” in the metalanguage (just as we have to use “/”); for the transitivity principle – in its simplest form, “$a/b \rightarrow (b/c \rightarrow a/c)$” – must be capable of being stated, in some way or other, in the metalanguage of every system; for example, in those systems which (like Nicod’s and Quine’s) use *one* undefined primitive sign of the object language for propositional logic.

Our results show that *no* such undefined sign of the object language (nor anything representing it in the metalanguage) need be assumed in addition to these necessary metalinguistic means of expression and to our two undefined specific logical concepts, and that these two concepts themselves, although undefined, can be, indirectly, characterized by two explicit definitions, viz. those of conjunction (or alternative denial) and universal quantification, respectively.

We can therefore say:

Every metalanguage which possesses the means of expression necessary to formulate the most general rules of inference (such as the principle of transitivity of inference in the simple form 5.21) possesses all the means necessary for constructing the whole propositional logic, including the logic of modalities.

Every metalanguage which, in addition, possesses a variable name of the statement which is the result of the substitution of x for y in a statement a, also possesses all the means necessary for constructing quantification theory.

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Chapter 5
On the Theory of Deduction, Part I. Derivation and its Generalizations (1948)

Karl R. Popper

Abstract This article is a corrected reprint of K. R. Popper (1948a). On the Theory of Deduction, Part I. Derivation and its Generalizations. In: *Koninklijke Nederlandse Akademie van Wetenschappen, Proceedings of the Section of Sciences* 51, pp. 173–183.

Editorial notes: The article was reprinted with the printing plates for Popper (1948a) as K. R. Popper (1948b). On the Theory of Deduction, Part I. Derivation and its Generalizations. In: *Indagationes Mathematicae* 10, pp. 44–54. In the latter, the title is followed by the notes "(Communicated by Prof. L. E. J. Brouwer.)" and "(Communicated at the meeting of November 29, 1947.)", referring to the meeting of the Koninklijke Nederlandse Akademie van Wetenschappen. These notes are omitted in the present version. The title of the former was newly typeset and the addition "(Communicated by Prof. L. E. J. Brouwer.)" was dropped from the title. The original pagination (pp. 173–183) was preserved in brackets, and the new pagination (pp. 44–54) was added. Page numbers in our margins thus refer to Popper (1948a) but the page numbers of Popper (1948b) are also provided in the concordance. "On the Theory of Deduction" was submitted as one manuscript but had to be divided into two parts due to demands from the printers or the publisher; cf. the correspondence between Popper and Brouwer in § 22.4 and § 22.5 of this volume. In his letter to Brouwer of 18 November 1947 (this volume, § 22.4), Popper emphasized the importance of his results and put them into context. References like ⟨New Foundations for Logic, p. 119⟩ refer to the corrected reprint in this volume.

173 In sections I and II of this paper, new primitive rules for derivational logic will be formulated and proofs will be given of the principal rules of derivational logic proposed in three earlier papers[a],[1], ⟨Popper, 1947b,c,d⟩, to which the present paper is a sequel.

[1] See also the "Additions and Corrections" ⟨Popper, 1948e⟩ to ⟨Popper, 1947c⟩, and note 1 to ⟨Popper, 1947d⟩. In its general approach, terminology, and symbolism, the present paper is based on these earlier papers.

[a] To which Popper refers to as P_1, P_2 and P_3; they are Popper (1947c, this volume, Chapter 3), Popper (1947b, this volume, Chapter 2) and Popper (1947d, this volume, Chapter 4), respectively. We have replaced P_1, P_2 and P_3 by the latter references.

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In section III, a few concepts of the general theory of derivation will be introduced. Some of these will be used in the subsequent sections in which certain problems concerning the definitions of classical and intuitionist negation will be discussed.

I

The notation

$$a_1, \ldots, a_n/b$$

will be used, as in the earlier papers mentioned, to express the assertion: "The conclusion b is derivable from the premises $a_1, a_2, \ldots, a_n$." We shall call this the "/-notation". In addition to this notation, we shall use in the present paper another notation to express the same assertion. This new notation, viz.:

$$D(b, a_1, \ldots, a_n)$$

will be called the "D-notation".

We shall here use the D-notation as our fundamental notation; that is to say, we shall take

$$D(a_1, \ldots, a_n)$$

as our fundamental undefined concept, and we shall assume that the /-notation has been introduced with the help of the definition[2]:

$$(D/) \qquad a_2, \ldots, a_n/a_1 \leftrightarrow D(a_1, \ldots, a_n)$$

174 | The use of the D-notation has the following advantages. First, it gives rise to a generalisation by suggesting that

$$D(a_1, \ldots, a_n)$$

may be meaningful for $n = 1$; and we shall indeed find (in section II) that

$$D(a)$$

turns out to be equivalent to "$\vdash a$", i.e., to "a is demonstrable" (as defined in the earlier papers). Secondly, the D-notation assimilates our metalinguistic symbolism still more closely to that of Hilbert and Bernays. It makes it more obvious that "$D(a_1, \ldots, a_n)$" is an n-termed predicate of our metalanguage; also that it is our only[3] undefined

[2] As in ⟨Popper, 1947c⟩ and ⟨Popper, 1947d⟩, "$\leftrightarrow$" abbreviates the metalinguistic use of "if and only if". Similarly, "$\rightarrow$"; "&"; and "$\bigvee$" abbreviate "if-then"; "and"; and "or-or both", respectively.

[3] Our undefined symbol "$a\binom{x}{y}$" (cp. ⟨Popper, 1947b,c,d⟩) is not, of course, a predicate, but a function whose values are statements (including statement functions) and whose arguments are a statement and two individual variables. But this symbol will not be used in the present paper (except in footnote 11, below).

predicate, since the class of statements (including statement functions) can be defined as the domain of the relation $D(a_1, \ldots, a_n)$. Ultimately, the D-notation throws some light upon the function of the primitive rules which characterize the relation $D(a_1, \ldots, a_n)$, or upon the function of those secondary rules which take the place of the primitive rules if we employ, as in ⟨Popper, 1947d⟩, the method of laying down Basic Definitions. For in the D-notation, it becomes clear that the main function of these rules is to relate n-termed deducibility to 2-termed and $n+r$-termed deducibility (including, more especially, $n+1$-termed deducibility, in the case of BI.1–2; cp. also D3.4).

In ⟨Popper, 1947c⟩, two methods of laying down primitive rules were distinguished, called "Basis I" and "Basis II", respectively. In the present section, a set forming a Basis I will be discussed, and in section II, a rule which (together with two definitions) suffices for Basis II.

The set of primitive rules for Basis I consists of the following two rules[4], BI.1 and BI.2. (It is assumed, for all rules written in the D-notation, that $1 \leq n$ and $1 \leq r$.)

(BI.1) $$D(a_1, a_2) \leftrightarrow D(a_1, a_2, a_2).$$

(BI.2) $$D(a_1, \ldots, a_n) \leftrightarrow (a_{n+1}) \ldots (a_{n+r})\big(D(a_{n+r}, a_1, \ldots, a_n) \rightarrow D(a_{n+r}, \ldots, a_2)\big).$$

We shall first sketch the derivation of the most important rules belonging to the earlier sets.

Putting $n = 2$ and $r = 1$, we obtain from BI.2:

(1.1) $$D(a, b) \leftrightarrow (c)\big(D(c, a, b) \rightarrow D(c, b)\big),$$

and from this, by substitution and BI.1:

(1.2) $$D(a, a);$$

(1.3) $$D(a, a, a)$$ (by 1.2; BI.1).

175 | Furthermore, we obtain from BI.2 alone

(1.4) $$D(a_1, \ldots, a_n) \rightarrow \big(D(a_{n+r}, a_1, \ldots, a_n) \rightarrow D(a_{n+r}, \ldots, a_2)\big).$$

Except for the proof of

(1.5) $$D(a) \leftrightarrow (b)D(a, b)$$

which we shall give in section II, in order to establish the equivalence of "$D(a)$" and "$\vdash a$" (as defined in the earlier papers), we shall give the following derivations in the $/$-notation, and we shall base them exclusively upon 1.2′, 1.3′ and 1.4′, i.e., upon 1.2, 1.3 and 1.4 expressed in the $/$-notation.

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[4] In the present paper (in contradistinction to the earlier ones) I use the quantifiers "(a)" and "(Ea)" as abbreviations of the metalinguistic phrases "for every a" and "there exists at least one a such that".

$$(1.2') \qquad a/a$$

$$(1.3') \qquad a, a/a$$

$$(1.4') \qquad a_1, \ldots, a_n/b \rightarrow (b, a_1, \ldots, a_n/c \rightarrow a_{n+r}, \ldots, a_1/c).$$

Our contention is that $1.2'$ to $1.4'$ are equivalent to the Basis I as characterized in the earlier papers.

We obtain, for $n = 1$ and $n + r = m$:

$$(1.41) \qquad a_m, \ldots, a_1/a_1 \qquad (1.2'; 1.3'; 1.4')$$

and therefore

$$(1.411) \qquad a_1, \ldots, a_n/a_n; \qquad (1.41)$$

$$(1.412) \qquad b, a_1, \ldots, a_n/a_n; \qquad (1.411)$$

$$(1.42) \qquad a_{n+r}, \ldots, a_1/a_n, \qquad (1.411; 1.412; 1.4')$$

which can also be written

$$(1.42') \qquad a_1, \ldots, a_n/a_i \qquad (1 \leq i \leq n),$$

and which yields, more especially,

$$(1.421) \qquad b, a_1, \ldots, a_n/b.$$

We further obtain

$$(1.43) \qquad a_1, \ldots, a_n/b \rightarrow a_{n+r}, \ldots, a_1/b; \qquad (1.421; 1.4')$$

$$(1.44') \qquad a_1, \ldots, a_n/b \rightarrow a_n, \ldots, a_1/b; \qquad (1.43)$$

$$(1.45') \qquad a_1, \ldots, a_n/b \rightarrow a_1, \ldots, a_{n+r}/b; \qquad (1.43; 1.44')$$

From $1.4'$ and $1.44'$ we also obtain:

$$(1.46') \qquad a_1, \ldots, a_n/b \rightarrow (b, a_1, \ldots, a_n/c \rightarrow a_1 \ldots, a_n/c).$$

Rule $1.46'$ may be called the "principle of the redundant first premise"; it can be formulated in words: "If the first premise of an inference is derivable from the remaining premises, then its omission does not invalidate the inference." For clearly, $1.46'$ is the same as

$$(1.46'') \qquad a_2, \ldots, a_n/a_1 \rightarrow (a_1, \ldots, a_n/b \rightarrow a_2 \ldots, a_n/b),$$

or in our D-notation:

$$(1.46''') \qquad D(a_1, \ldots, a_n) \rightarrow \big(D(b, a_1, \ldots, a_n) \rightarrow D(b, a_2 \ldots, a_n)\big).$$

176 | Now 1.2′, 1.44′, 1.45′ form, together with the principle I have called[5] "generalized transitivity principle", that is to say, with 1.47

(1.47)

$$(a_1, \ldots, a_n/b_1 \;\&\; \ldots \;\&\; a_1, \ldots, a_n/b_m) \rightarrow (b_1, \ldots, b_m/c \rightarrow a_1, \ldots, a_n/c)$$

a set of rules which constitutes a form of Basis I.

Our contention is that, in the presence of 1.44′ and 1.45′, the generalized transitivity principle 1.47 can be derived from 1.46′.

The proof of our contention will be given in the /-notation.

We first obtain, from 1.44′ and 1.45′

$$b_1, \ldots, b_m/c \rightarrow a_1, \ldots, a_n, b_1, \ldots, b_m/c. \tag{1.48}$$

In the presence of of this principle, we only need 1.45′ for our proof; for we need only to prove

$$(a_1, \ldots, a_n/b_1 \;\&\; \cdots \;\&\; a_1, \ldots, a_n/b_m) \rightarrow \\ (a_1, \ldots, a_n, b_1, \ldots, b_m/c \rightarrow a_1, \ldots, a_n/c) \tag{1.49}$$

in order to obtain 1.47, since, clearly, 1.47 can be obtained by 1.48 from 1.49. We now sketch the proof of 1.49 which makes use of 1.45′ only (apart, of course, of 1.46′).

We observe that 1.46′ may be written:

$$a_1, \ldots, a_n, b_1, \ldots, b_{m-1}/b_m \rightarrow (a_1, \ldots, a_n, b_1, \ldots, b_m/c \rightarrow \\ a_1, \ldots, a_n, b_1, \ldots, b_{m-1}/c). \tag{1.491}$$

Applying to this 1.45′, we obtain

$$a_1, \ldots, a_n/b_m \rightarrow \\ (a_1, \ldots, a_n, b_1, \ldots, b_m/c \rightarrow a_1, \ldots, a_n, b_1, \ldots, b_{m-1}/c). \tag{1.492}$$

From this we obtain, by substitution,

$$a_1, \ldots, a_n/b_{m-1} \rightarrow \\ (a_1, \ldots, a_n, b_1, \ldots, b_{m-1}/c \rightarrow a_1, \ldots, a_n, b_1, \ldots, b_{m-2}/c). \tag{1.493}$$

Combining 1.412 and 1.413, we get:

$$(a_1, \ldots, a_n/b_m \;\&\; a_1, \ldots, a_n/b_{m-1}) \rightarrow (a_1, \ldots, a_n, b_1, \ldots, b_m/c \rightarrow \\ a_1, \ldots, a_n, b_1, \ldots, b_{m-2}/c). \tag{1.494}$$

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[5] Cp. ⟨New Foundations for Logic, p. 119⟩, rule 2.5g.

We can continue this procedure for $m - 2$ steps, i.e., until the premises b_i are exhausted. The result is 1.49.

It may be remarked that a Basic Definition of "$b \wedge c$" (analogous and equivalent to DB2′ in ⟨Popper, 1947d⟩) may be obtained by making use of 1.2′; 1.3′; and 1.4′; this definition is DB2$^{\mathrm{I}}$:

$$(\mathrm{DB2^I})\quad a//b \wedge c \leftrightarrow (a_1)\ldots(a_{n+r})\big((a/a_n \leftrightarrow b, c/a_n) \mathbin{\&} \big(a_1,\ldots,a_n/b \rightarrow (b, a_1,\ldots,a_n/c \rightarrow a_{n+r},\ldots,a_1/c)\big) \mathbin{\&} b/b \mathbin{\&} b, b/b\big).$$

This definition may be simplified by replacing "$b/b \mathbin{\&} b, b/b$" by "$a_1,\ldots,a_n/a_1$"; or else, by "$a_1,\ldots,a_m/a_1$" together with the restriction "$1 \leq m \leq 2$"; but even in the latter case, the simplified definition is still a little stronger than necessary, that is to say, stronger than DB2$^{\mathrm{I}}$.

177 We have derived 1.2′; 1.44′; 1.45′ and 1.47 from our BI.1 and BI.2; | the derivation of BI.1 and BI.2 from the rules mentioned, and therefore the equivalence of the two sets of rules for Basis I, is trivial[6].

II

We now proceed to Basis II. This is not equivalent to Basis I, but to Basis I combined with the two definitions

$$(\mathrm{DB1})\qquad a//b \leftrightarrow (c)(c/a \leftrightarrow c/b)$$

and

$$(\mathrm{DI}\wedge)\qquad a//b \wedge c \leftrightarrow (d)(a/d \leftrightarrow b, c/d).$$

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[6] The proof that we can obtain 4.7 (called in ⟨Popper, 1947c⟩ "generalized transitivity principle") from 4.6′ solves the problem of constructing a *complete* basis for the *general* theory of derivation – such as Basis I, as opposed to Basis II – without having to use 4.7 as primitive. The objection against 4.7 is that it makes use of an unspecified number of conjunctive components in its antecedent; this may be considered as introducing a new metalinguistic concept – something like an infinite product. The problem of avoiding 4.7 was discussed, but not solved in ⟨Popper, 1947c⟩. The lack of a solution led me there to construct Basis II, the need for which, as it were, has now disappeared. But Basis II turned out to be of interest in itself; cp. especially the derivation of 2.2 from B II, below, and the remark (in the paragraph before the last, of section II) on the definition-like character of BII.

The solution mentioned makes it possible to construct Basic Definitions like those of ⟨Popper, 1947d⟩, but with Basis I as underlying basis. (⟨Popper, 1947d⟩ uses Basis II for this purpose.) Such Basic Definitions – cp. DB2$^{\mathrm{I}}$ – may define conjunction, but any other compound serves just as well, since Basis I, as opposed to Basis II, is neutral with regard to the various compounds. – It may be remarked that we can by various methods reduce the number of Basic Definitions from three to two, viz., to DB1 plus a Basic Definition which, apart from incorporating the definition of some compound C and the rules of the basis chosen, also includes the six rules pertaining to substitution (i.e. ⟨Popper, 1947d⟩, rules 5.71 to 5.76). If Basis I is chosen, C may be one of the quantifiers, which would make this procedure more "natural".

(The second of these definitions is incorporated in DB2$^{\mathrm{I}}$; cp. the end of the foregoing section.)

The simplest form of Basis II known to me consists of the *one primitive rule* BII (built in analogy to BI.2), together with the two definitions, DBI and DII∧.

$$\text{(BII)} \quad D(a_1, \ldots, a_n) \leftrightarrow (a_{n+1}) \ldots (a_{n+r})\big(D(a_{n+r}, a_1) \rightarrow D(a_{n+r}, \ldots, a_2)\big).$$

The difference between BII and BI.2 consists in writing "$D(a_{n+r}, a_1)$" instead of "$D(a_{n+r}, a_1, \ldots, a_n)$"; and although this difference makes it possible to derive BI.1 from BII, it makes it impossible to derive from BII the generalized transitivity principle without at least assuming, in place of DI∧, the more complicated definition (DII∧)

$$a//b \wedge c \leftrightarrow (a_1) \ldots (a_n)\big(a_1, \ldots, a_n/a \leftrightarrow (a_1, \ldots, a_n/b \;\&\; a_1, \ldots, a_n/c)\big).$$

178 If this definition is incorporated, together with BII, into a Basic | Definition, then we obtain the following Basic Definition DB2$^{\mathrm{II}}$, which is equivalent to DB2$^{\mathrm{I}}$:

$$\text{(DB2}^{\mathrm{II}}\text{)} \quad a//b \wedge c \leftrightarrow (a_1) \ldots (a_{n+r})\Big(\big(a_1, \ldots, a_n/a \leftrightarrow (a_1, \ldots, a_n/b \;\& \; a_1, \ldots, a_n/c)\big) \;\&\; \big(a_1, \ldots, a_n/b \rightarrow (b/c \rightarrow a_{n+r}, \ldots, a_1/c)\big) \;\&\; b/b\Big).$$

(This is equivalent to DB2′, given in ⟨Popper, 1947d⟩).

I shall confine myself to showing that we can obtain, from BII, the rules 1.42′; 1.44′; 1.45′; and, besides, 2.46′, i.e.:

$$\text{(2.46}'\text{)} \quad a_1, \ldots, a_n/b \rightarrow (b/c \rightarrow a_1, \ldots, a_n/c)$$

which is, in Basis II, the transitivity principle that corresponds to the (stronger) principle 1.46′ of Basis I.

The main interest of BII is that, by putting $n = 2$ and $r = 1$, we obtain

$$\text{(2.1)} \quad D(a, b) \leftrightarrow (c)\big(D(c, a) \rightarrow D(c, b)\big)$$

which, without any further help, leads to

$$\text{(2.2)} \quad D(a, a);$$

for "$D(c, a) \rightarrow D(c, a)$" must be true, whatever the meaning of "$D(a_1, a_2)$" may be.

We obtain from BII immediately

$$\text{(2.4)} \quad D(a_1, \ldots, a_n) \rightarrow \big(D(a_{n+r}, a_1) \rightarrow D(a_{n+r}, \ldots, a_2)\big).$$

We thus have, in the /-notation:

$$\text{(2.2}'\text{)} \quad a/a$$

$$\text{(2.4}'\text{)} \quad a_1, \ldots, a_n/b \rightarrow (b/c \rightarrow a_{n+r}, \ldots, a_1/c).$$

Putting here $n = 1$ and $n + r = m$, we obtain:

$$(2.41) \qquad a_m, \ldots, a_1/a_1, \qquad (2.2'; 2.4')$$

and from this

$$(2.3') \qquad a, a/a.$$

We further obtain:

$$(2.43) \qquad a_1, \ldots, a_n/b \rightarrow a_{n+r}, \ldots, a_1/b; \qquad (2.2'; 2.4')$$
$$(2.44') \qquad a_1, \ldots, a_n/b \rightarrow a_n, \ldots, a_1/b; \qquad (2.43)$$
$$(2.45') \qquad a_1, \ldots, a_n/b \rightarrow a_1, \ldots, a_{n+r}/b; \qquad (2.43; 2.44')$$
$$(2.46') \qquad a_1, \ldots, a_n/b \rightarrow (b/c \rightarrow a_1, \ldots, a_n/c). \qquad (2.4'; 2.44')$$

The principle

$$(2.42) \qquad a_1, \ldots, a_{n+r}/a_n,$$

or

$$(2.42') \qquad a_1, \ldots, a_n/a_i \qquad (1 \leq i \leq n),$$

is obtained from 2.41 together with 2.45′.

Since 2.42′; 2.44′; 2.45′ are identical with the rules 1.42′; 1.44′; 1.45′, we have derived all the rules we wanted.

179 | We now proceed to show that

$$(2.5) \qquad D(a) \leftrightarrow (b)D(a, b),$$

which establishes the equivalence of "$D(a)$" and "$\vdash a$".

Putting in BII $n = 1$ and $r = 2$, we obtain

$$(2.51) \qquad D(a) \leftrightarrow (b)(c)\big(D(c, a) \rightarrow D(c, b)\big),$$

and further

$$(2.52) \qquad D(a) \rightarrow \big(D(a, a) \rightarrow D(a, b)\big). \qquad \text{(by 2.51)}$$

From this and 2.2 we obtain

$$(2.53) \qquad D(a) \rightarrow D(a, b)$$

and thus

$$(2.54) \qquad D(a) \rightarrow (b)D(a, b).$$

In order to obtain from 2.51 the converse of 2.54, we need only

$$(2.55) \qquad (b)D(a,b) \rightarrow (b)(c)\big(D(c,a) \rightarrow D(c,b)\big),$$

which is an immediate consequence of the transitivity principle in its simplest form, i.e. of

$$(2.551) \qquad D(a,b) \rightarrow \big(D(c,a) \rightarrow D(c,b)\big).$$

This concludes the proof of 2.5. In order to show that 1.5, which is the same as 2.5, can be derived from BI.1 and BI.2, we first consider that we can obtain 2.51 from BI.2 in the same way as from BII. Since we have also 2.2, i.e., 1.2, we obtain 2.45; and since we also have 2.55, which is obtainable from 1.47 by $n = 1$ and $m = 1$, we obtain 2.5, i.e. 1.5.

Concerning BII, it may be remarked that, in view of the method of deriving 2.2, it very closely resembles DB1 (cp. ⟨Popper, 1947d⟩). It might therefore, perhaps, be described as a "quasi-definition"; for it defines, as it were, the n-termed relation $D(a_1, \ldots, a_n)$ for $1 \leq n$, in terms of the two-termed relation $D(a_1, a_2)$ and the $n + r$-termed relation $D(a_1, \ldots, a_{n+r})$, for $1 \leq r$.

Concerning our two Basic Definitions $\mathrm{DB2^I}$ and $\mathrm{DB2^{II}}$, it may be noted that they can be made homogeneous (in the sense of ⟨Popper, 1947d⟩) simply by incorporating BI.1 and BI.2, or BII, respectively, as they stand. In this case, a second set of quantifiers appears within the right hand side of the definitions, which makes them more complicated; but we achieve, besides homogeneity, the derivability of 1.5 and 2.5. If, on the other hand, we wish to avoid this set of quantifiers within the right hand side, then 1.5 and 2.5 cannot be derived, and we have to revert to our earlier method of defining "$\vdash a$". But with this, the main advantage of the D-notation disappears. (Thus it seems that there is not much point in formulating $\mathrm{DB2^I}$ and $\mathrm{DB2^{II}}$ in the D-notation.)

III

We can distinguish between the *general* and the *special* theories of derivation (and
180 proof). The special theories are the theories of the | compounds, quantifiers, and modalities; closely connected with them are the theories obtained by adding existential postulates to our bases. (Such postulates may demand, for example, the existence of provable or refutable statements, or perhaps the existence of a conditional statement c to every pair of statements a and b, that is to say, of a weakest statement which, together with a, yields b). The general theories are erected on either BI.1 and BI.2, or on BII, without, however, introducing compound argument variables for $D(a_1, \ldots, a_n)$, or existential postulates. But such problems as the equivalence of existential postulates fall within the scope of the general theory.

In the present section, we shall first sketch that part of the general theory which is concerned with the complementarity or exhaustiveness (or disjunctness) and with the contradictoriness or exclusiveness (or conjunctness) of n statements. We shall write

$$\vdash (a_1, \ldots, a_n)$$

for "the statements $a_1, \ldots, a_n$, taken together, are complementary or exhaustive" and

$$\urcorner\!\!\!\!/ \; (a_1, \ldots, a_n)$$

for "the statements $a_1, \ldots, a_n$, taken together, are contradictory or exclusive". The definitions are:

$$\text{(D3.1)} \qquad \vdash (a_1, \ldots, a_n) \;\leftrightarrow\; (b)(c)\big((a_1/c \;\&\; \ldots \;\&\; a_n/c) \rightarrow b/c\big).$$

$$\text{(D3.2)} \qquad \urcorner\!\!\!\!/ \; (a_1, \ldots, a_n) \;\leftrightarrow\; (b)(c)\big((b/a_1 \;\&\; \ldots \;\&\; b/a_n) \rightarrow b/c\big).$$

An alternative and equivalent way of defining "$\urcorner\!\!\!\!/$" is this:

$$\text{(D3.2}'\text{)} \qquad \urcorner\!\!\!\!/ \; (a_1, \ldots, a_n) \;\leftrightarrow\; (b)(a_1, \ldots, a_n/b).$$

Introducing the convention that the brackets after "$\vdash$" and "$\urcorner\!\!\!\!/$" may be omitted, we obtain, for $n = 1$,

$$\text{(3.1)} \qquad \vdash a \;\leftrightarrow\; (b)(b/a); \qquad (2.2'; 2.46'; \text{D3.1})$$

$$\text{(3.2)} \qquad \urcorner\!\!\!\!/ \; a \;\leftrightarrow\; (b)(a/b). \qquad (2.2'; 2.46'; \text{D3.1})$$

This shows that, for $n = 1$, the two concepts coincide with demonstrability and refutability respectively, as defined in my earlier papers; furthermore, that for $n = 1$, $D(a_1, \ldots, a_n)$ and $\vdash (a_1, \ldots, a_n)$ coincide.

The two concepts may be generalized or relativized by introducing the following definition. (We assume $0 \leq n$; $0 \leq m$; $1 \leq n + m$.)

$$\text{(D3.3)} \quad (a_1, \ldots, a_n) \vdash (b_1, \ldots, b_m) \;\leftrightarrow\; (c)(d)\Big((b_1/d \;\&\; \ldots \;\&\; b_m/d) \rightarrow \big((c/a_1 \;\&\; \ldots \;\&\; c/a_n) \rightarrow c/d\big)\Big).$$

An alternative formulation can be obtained as before (cp. D3.2′):

$$\text{(D3.3}'\text{)} \quad (a_1, \ldots, a_n) \vdash (b_1, \ldots, b_m) \;\leftrightarrow\; (c)\big((b_1/c \;\&\; \ldots \;\&\; b_m/c) \rightarrow a_1, \ldots, a_n/c\big).$$

We again introduce the convention that brackets can be omitted, before and after "$\vdash$".

181 | The concept defined by D3.3 may be called "relative demonstrability" (or "relative refutability"), and "$a_1, \ldots, a_n \vdash b_1, \ldots, b_m$" may be read: "The statements $b_1, \ldots, b_m$ are complementary relative to the demonstrability (or self-complementarity) of all the statements $a_1, \ldots, a_n$." For it is clear, from D3.3 and D3.3′, that every one the the a_i which stand in front of "$\vdash$" may be omitted if it is demonstrable (or self-complementary), without affecting the force of the whole expression. This

shows that for $n = 0$ relative demonstrability degenerates into complementarity (or demonstrability) as defined by D3.1.

A similar consideration shows that every b_i which stands after "⊢" may be omitted if it is refutable or self-contradictory. If all the b_i are so omitted, we obtain, for $m = 0$, an expression which is equivalent to D3.2. That is to say, we obtain:

(3.3) $$a_1, \ldots, a_n \vdash \;\leftrightarrow\; \text{⁊}\, a_1, \ldots, a_n.$$

Thus "$a_1, \ldots, a_n \vdash b_1, \ldots, b_m$" may be read, alternatively: "The statements $a_1, \ldots, a_n$ are contradictory, relative to the refutability (or self-contradictoriness) of all the statements $b_1, \ldots, b_m$."

For certain purposes – especially if we wish to emphasize the duality or symmetry between "⊢" and "⁊" – the use of "$(\ldots) \vdash$" turns out to be preferable to that of "⁊ $(\ldots)$".

It should be noted that, for $m = 1$, relative demonstrability degenerates, as it were, into derivability; that is to say, we have

(3.4) $$a_1, \ldots, a_n \vdash b \;\leftrightarrow\; a_1, \ldots, a_n / b.$$

If, however, $m > 1$, then relative demonstrability means something else. (It is thus a further generalisation of "/".) Its meaning can be intuitively explained by remarking that, whenever disjunction is available,

(3.5) $$a_1, \ldots, a_n \vdash b_1, \ldots, b_m \;\leftrightarrow\; a_1, \ldots, a_n / b_1 \vee b_2 \vee \ldots \vee b_m.$$

that is to say, the intuitive meaning (even if disjunction is not available) is the same as that of the derivability of the disjunction of the statements standing to the right of "⊢" from the statements standing to the left of "⊢". Relative demonstrability is thus about the same as Gentzen's "Sequences" or Carnap's "Involution"[7].

182 | Once we have adopted D3.3 or D3.3′, we obtain D3.1 and 3.1 as theorems, as well as theorems corresponding to D3.2 and 3.2 (with "$(\ldots) \vdash$" instead of "⁊ $(\ldots)$"); and we can, if we wish, even dispense with the further use of "/", in view of 3.5.

In the next section, we shall make use of "$a_1, \ldots, a_n \vdash b_1, \ldots, b_m$" more especially for $0 \le n \le 2$ and $0 \le m \le 2$. Implicit use will be made of the obvious theorems:

[7] Cp. ⟨Gentzen, 1935a,b⟩; and ⟨Carnap, 1943⟩, esp. § 32, pp. 151ff. – My remark that relative demonstrability is "*about*" the same as these earlier concepts alludes to the following differences. (1) It is not quite clear whether Gentzen's concept is, like ours, a metalinguistic *predicate* asserting some kind of inference (it looks as if his horizontal stroke rather than his sequences were meant in this way) or a *name of an object-linguistic notation*. (2) Carnap's concept on the other hand (which is not open to any objection of this kind) may be described as a *generalization* of "$\ldots \vdash \ldots$", in so far as *classes* of statements are admitted as arguments, besides statements. (3) Attention may be drawn, furthermore, to the fact that Gentzen identifies the difference between classical and intuitionist logic with the difference (I am using my own terminology) between permitting m to be greater than 1, and taking 1 as the upper limit of m. This does not agree with our results, and seems to be due only to Gentzen's choice of his primitive rules for negation. We operate freely with $m \ge 2$, even within intuitionist logic.

$$(3.61) \qquad a \vdash a, b$$

$$(3.62) \qquad b \vdash a, b$$

$$(3.63) \qquad a \vdash b, c \rightarrow \big(c \vdash d \rightarrow (b \vdash d \rightarrow a \vdash d)\big).$$

(In the expression in brackets, "$\vdash$" may, of course, be here replaced by "/".)

The rules ⟨3.61 to 3.63⟩ show that we may characterize "$a \vee b$" by a characterizing rule which is precisely analogous to and dual of the simplest characterizing rule[8] for "$a \wedge b$":

$$(3.71) \qquad a \wedge b \vdash c \;\leftrightarrow\; a, b \vdash c$$

$$(3.72) \qquad a \vdash b \vee c \;\leftrightarrow\; a \vdash b, c.$$

Similarly, we can characterize a new compound, called "anti-conditional" and denoted by "$a \ngtr b$", in precise analogy to (and as the dual of) $a > b$:

$$(3.81) \qquad a \vdash b > c \;\leftrightarrow\; a, b \vdash c$$

$$(3.82) \qquad a \ngtr b \vdash c \;\leftrightarrow\; a \vdash b, c.$$

"$a \ngtr b$" is, intuitively (and in the presence of classical negation), a name for the negation of $a > b$, or for the conjunction of a and the negation of b. But in the absence of classical negation, its meaning is less familiar. (Cp. 5.32 and 5.42, and note 15, below.)[b]

These characterizing rules can all be transformed into explicit definitions; for example, 3.71 into DI$\wedge$ (quoted at the beginning of section II).

All the rules and definitions given apply equally to closed and open statements. But as long as we confine ourselves to closed statements, "... $\vdash$ ..." may, intuitively, be interpreted as asserting that at least one of the statements on the right of "$\vdash$" is true, provided all statements on the left are true. It should be noted, on the other hand, that two or more open statements – e.g. an open statement and its classical negation – may be complementary (or relative complementary) in our sense even though their A-closures, i.e. the results of universal quantification, are not complementary (or relative complementary). This is due to fact that A-closures and E-closures (i.e. results of existential quantification), are duals of each other, like conjunction and disjunction. For it follows from our definitions that every valid $\vdash$-formula remains valid if any of its statements to the left of "$\vdash$" are replaced by their A-closures,
183 and any of its statements to the | right by their E-closures; and that, if there are no

[8] Cp. ⟨New Foundations for Logic, p. 132, rule 4.1⟩; ⟨Functional Logic without Axioms or Primitive Rules of Inference, 5.2⟩ – See also DI$\wedge$ (in section II, above).

[b] In a letter to Alonzo Church (this volume, § 24.3) Popper suggested the insertion of the following: Conjoint denial and alternative denial also are, in this System, dual to each other

$$(3.91) \qquad a \downarrow b \vdash c \;\leftrightarrow\; \vdash a, b, c$$

$$(3.92) \qquad a \vdash b \curlywedge c \;\leftrightarrow\; a, b, c \vdash$$

open statements to the left or to the right respectively, the disjunction or conjunction respectively of the remaining open statements may be replaced by either its A-closure or its E-closure.

So far we have considered generalizations of the idea of derivability; but in view of 3.4, which may be taken as a definition of derivability in terms of relative demonstrability, and in view of D3.3, which defines the latter with the help of only *two-termed derivability* (or inference from *one* premise), it is clearly possible to use two-termed derivability, "$D(a, b)$", characterized by (ordinary) reflexivity and transitivity, as our *sole undefined predicate*. That is, we may introduce "$D(a, b)$" by the *one* rule 2.1, or, for example, by the following rule (constituting a "basis III"):

(BIII) $$D(a, b) \leftrightarrow (c)\big(D(b, c) \rightarrow D(a, c)\big).$$

The explicit definition of "$D(a_1, \ldots, a_n)$" in terms of "$D(a, b)$" becomes, considering D3.3 and 3.4:

(D3.4) $$D(a_1, \ldots, a_n) \leftrightarrow (b)\Big(\big(D(a_n, b) \mathbin{\&} \ldots \mathbin{\&} D(a_2, b)\big) \rightarrow D(a_1, b)\Big).$$

But this formula turns out to yield BIII. It therefore, surprisingly enough, suffices (without BIII) for a Basis I.

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Chapter 6
On the Theory of Deduction, Part II. The Definitions of Classical and Intuitionist Negation (1948)

Karl R. Popper

Abstract This article is a corrected reprint of K. R. Popper (1948c). On the Theory of Deduction, Part II. The Definitions of Classical and Intuitionist Negation. In: *Koninklijke Nederlandse Akademie van Wetenschappen, Proceedings of the Section of Sciences* 51, pp. 322–331.

Editorial notes: The article was reprinted with the printing plates for Popper (1948c) as K. R. Popper (1948d). On the Theory of Deduction, Part II. The Definitions of Classical and Intuitionist Negation. In: *Indagationes Mathematicae* 10, pp. 111–121. In the latter, the title is followed by the notes "(Communicated by Prof. L. E. J. Brouwer.)" and "(Communicated at the meeting of November 29, 1947.)", referring to the meeting of the Koninklijke Nederlandse Akademie van Wetenschappen. These notes are omitted in the present version. The title of the former was newly typeset and the addition "(Communicated by Prof. L. E. J. Brouwer.)" was dropped from the title. The original pagination (pp. 322–331) was preserved in brackets, and the new pagination (pp. 111–121) was added. Page numbers in our margins thus refer to Popper (1948c) but the page numbers of Popper (1948d) are also provided in the concordance. References like ⟨New Foundations for Logic, p. 145⟩ refer to the corrected reprint in this volume. For an explication of the matrices (truth tables) in § V cf. this volume, Chapter 1, Definition 8.1.

IV

322 We now turn to the discussion of negation – more especially, of the intuitionist negation of Brouwer and Heyting, and its relation to other negations. In the present section, we shall discuss the simplest characterizing rules for, and definitions of, the intuitionist negation of a, denoted by "a^i"; the classical negation of a, denoted

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by[9] “a^k”; and a third negation of a, denoted by “a^m” i.e., the “minimum definable (non-modal) negation of a”, or the “weakest definable (non-modal) negation of a”.

The definitions and characterizing rules of a^i and a^k given in my earlier papers are adequate, but a little complicated and intuitively not as obvious as, for example, 3.71 to 3.82. A characterizing rule for a^i which in its simplicity is comparable to 3.71 etc. is this[10]:

$$a \vdash b^i \;\leftrightarrow\; a, b \vdash . \tag{4.1}$$

This may be put into the words: “The intuitionist negation of b is the weakest of those statements which are strong enough to contradict b.” The corresponding explicit definition is:

$$a /\!/ b^i \;\leftrightarrow\; (c)(c \vdash a \;\leftrightarrow\; c, b \vdash). \tag{D4.1}$$

Thus intuitionist negation is, as it were, characterized by contradictoriness alone. One might be tempted to think that classical negation is similarly related to complementarity; but this is not the case. The dual of 4.1 leads to a new kind of negation which is weaker than classical negation. We call this negation “a^m” (the “minimum definable negation of a”; its characterizing rule may be written:

$$a^m \vdash b \;\leftrightarrow\; \vdash a, b; \tag{4.2}$$

323 | that is to say a^m can be characterized as the strongest of those statements which are weak enough to be complements of a. We can[11] transform (4.2) into the corresponding

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[9] In ⟨New Foundations for Logic, footnote 20⟩, I have used instead of “a^i” and “a^k” two more complicated symbols. In ⟨Popper, 1947d⟩, sections III ff., I have used the symbols “a^i” and “a^c”. I have now replaced “a^c” by “a^k” because I found that the “c” in “a^c” was misleading, occurring as it does together with the statement-variable “c”.

[10] Rule 4.1 and all other rules in this section are *purely derivational* (in the sense of ⟨New Foundations for Logic, p. 145⟩), i.e. they can all be non-vacuously satisfied by non-logical (or factual) statements. Were we to write “$a \wedge b \vdash$” instead of “$a, b \vdash$”, or “$\vdash a \vee b$” instead of “$\vdash a, b$”, our rules would no longer be purely derivational, since these rules would need logical (demonstrable or refutable) statements to satisfy them. The reason why my earlier definitions are more complicated then the present ones is that I did not then use “$\vdash$” and “$\overline{/}$” with more than one argument; this makes it impossible to obtain rules as simple as our present ones *if we wish to remain within derivation logic*, which I consider highly desirable for non-modal logic. (The definitions of the modalities, of course, *cannot* be purely derivational; cp. ⟨Logic without Assumptions, p. 104⟩, end of note 20.)

[11] The method here used to obtain relatively simple formulae incorporating the condition “the weakest (or strongest) statement such that ...” is capable of fairly wide application. This may be illustrated by the example of the definition of identity. We use “$Idt(x, y)$” as the metalinguistic name of a statement-function expressing identity between individuals represented by the individual variables x and y. (Note that, in this characterization, “x” and “y” must not be put into quotes.) We introduce the abbreviating notation:

$$a /\!/ a_{\hat{x}\hat{y}} \;\leftrightarrow\; (w)\left(a /\!/ a\binom{x}{w} \;\&\; a /\!/ a\binom{y}{w}\right)$$

We can define $Idt(x, y)$ as the weakest statement strong enough to imply what ⟨Hilbert and Bernays, 1934, p. 65⟩ call the “second identity axiom”, as follows (cp. ⟨New Foundations for Logic, p. 143⟩, D6.2):

definition

$$\text{(D4.2)} \qquad a/\!/b^m \leftrightarrow (c)(a \vdash c \leftrightarrow \ \vdash b, c).$$

There exist two simple rules for classical negation (see 4.311, f., below) which are analogous to 4.1 and 4.2, but less striking. The simplest and most striking characterization I have been able to find is the following explicit definition[12]

$$\text{(D4.3)} \qquad a/\!/b^k \leftrightarrow a, b \vdash \ \& \ \vdash a, b.$$

That is to say, the classical negation of b can be defined (as Aristotle might have defined it) as that statement which is at once contradictory and complementary to b.

Classical negation, according to this definition, will exist in a language L if, and only if, there exists in L to every statement a a statement a^k which is both contradictory and complementary to a. It is fairly clear, from this definition, that intuitionist negation a^i or its dual a^m, or perhaps both, may exist in a language in which classical negation does not exist; and we shall prove all this by an example (in section V).

Some characterizing rules for classical negation, if written in terms of relative demonstrability, are only slightly different from 4.1 and 4.2, and may be described as generalizations of these rules; 4.31 is equivalent to D4.3, and so is 4.32, which is the dual rule of 4.31:

$$\text{(4.31)} \qquad a \vdash b^k, c \leftrightarrow a, b \vdash c.$$

$$\text{(4.32)} \qquad a, b^k \vdash c \leftrightarrow a \vdash b, c.$$

324 | The close relationship to 4.1 and 4.2 respectively, is clear, but the rules are intuitively less satisfactory than D4.3 – especially in view of the fact that D4.3 is an explicit definition. Two other characterizing rules, each of them equivalent to D4.3, may be obtained by inverting 4.1 and 4.2:

$$\text{(4.311)} \qquad a, b^k \vdash \ \leftrightarrow a \vdash b.$$

$$\text{(4.312)} \qquad \vdash a^k, b \leftrightarrow a \vdash b.$$

The first of these may be expressed in words: “The classical negation of b is a statement that contradicts every statement a which is at least as strong as b”. This is

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$$a/\!/Idt(x, y) \leftrightarrow$$
$$(b)(z)((b/\!/b_{\dot{x}\dot{y}} \rightarrow a, b\tbinom{z}{x}/b\tbinom{z}{y}) \ \& \ (((c)(u)(c/\!/c_{\dot{x}\dot{y}} \rightarrow b, c\tbinom{u}{x}/c\tbinom{u}{y}))) \rightarrow b/a)).$$

Adopting the method used in 4.1 of formalizing “the weakest statement such that”, this may be replaced by the rule (or the corresponding definition).

$$a/Idt(x, y) \leftrightarrow (b)(z)(b/\!/b_{\dot{x}\dot{y}} \rightarrow a, b\tbinom{z}{x}/b\tbinom{z}{y}).$$

[12] The presence of 3.71 or of 3.72 is assumed. For the derivational character of D4.2 see note 10 above. An identical definition is given in ⟨Logic without Assumptions, p. 105⟩, rule 7.7.

similar to 4.1, but, surely, more involved and less striking. 4.312 may be read: "The classical negation of a is a statement which is complementary to every statement b which is at most as strong as a."

Each of the rules and definitions 4.1; D4.1; D4.3 (in the presence of conjunction or disjunction; 4.31 to 4.312; can be shown to be equivalent to the corresponding definitions given in my earlier papers.

V

In the presence of classical negation, that is to say, of characterizing rules for, or of a definition of, classical negation, it is possible to prove

$$a^i /\!/ a^k \tag{5.11}$$

$$a^m /\!/ a^k \tag{5.12}$$

and therefore also

$$a^i /\!/ a^m \tag{5.13}$$

This follows simply from the fact that the characterizing rules for a^i and a^m allow us to prove equivalence for every statement which satisfies these rules. (This is, precisely, the point which makes what may be called a "fully characterizing rule" equivalent to a definition.) But a^k satisfies the characterizing rules for a^i and a^m. Thus we obtain 5.11 to 5.13.

This result may be generalized. Whenever we have two logical functions of statements (or two formative signs) S_1 and S_2, which have been introduced by way of two sets of primitive rules, R_1 and R_2, such that R_2 is obtained by the omission of some rules of R_1, then we can prove in the presence of S_1, the equivalence of (the full expressions of) S_1 and S_2 *whenever both are definable.* For example, if we introduce "$a > b$" by rule 3.71 and another function, say "$a \supset b$", by the two[13] rules (of which the first is like 3.71)

$$a \vdash b \supset c \;\leftrightarrow\; a, b \vdash c; \tag{5.21}$$

$$a, b \supset c \vdash b \;\leftrightarrow\; a \vdash b; \tag{5.22}$$

325 | then, since 3.71 or 5.21 can be transformed into a definition, we can prove

$$a > b /\!/ a \supset b, \tag{5.23}$$

in spite of the fact that 5.22 is independent of 5.21, that is to say, that its addition changes the meaning of the sign "$\supset$".

[13] The second of these two rules is discussed in section VII below (rule 7.3^k); for another discussion of the same rule see ⟨New Foundations for Logic, p. 133⟩, (rule 4.2e).

The problem arises whether, in the presence of both a^i and a^m in a language L_1, a^k is always present, so that 5.13 holds, or whether a^i and a^m can coexist in L_1 without becoming equivalent.

We shall prove that the second alternative holds, by constructing, as an example, a language L_1 in which a^i is equivalent to $I\,a$, i.e. to the modal statement which asserts that the state of affairs described by a is *impossible*[14], and in which a^m is equivalent to $U\,a$, i.e. to the modal statement asserting that the state of affairs described by a is *uncertain* (not necessary).

We have, of course, to choose a language L_1 in which at least *one* non-logical statement exists, since we have otherwise $I\,a//U\,a$, which would lead to $a^i//a^m$, i.e. to the case we wish to avoid. We shall construct a language L_1 which contains *one* factual statement s, together with all the compounds which may be constructed from it with the help of the four functions characterized by 3.71 to 3.82 and of the definitions for a^i, a^m, $I\,a$ and $U\,a$. (a^k, of course, does not occur.) We shall have in L_1 (1) demonstrable statements such as $s > s$; $s \vee U\,s$; (2) factual statements such as s; $s \wedge U\,s$; $s \vee I\,s$, and (3) refutable statements such as $s \wedge I\,s$; $s \not> s$.

326 | In order to show that a^i may be here without contradiction identified with $I\,a$, and a^m with $U\,a$, we construct a mathematical model of our metalanguage, interpreting our variables "a", "b", etc., as variables whose values are the three numbers 1, 2, and 3, and "$a_1, \ldots, a_n/b$" as the statement asserting that the greatest of the numbers $a_1, \ldots, a_n$ is at least equal to b.

On the basis of our characterizing rules for $I\,a$; $U\,a$; $a \wedge b$; $a \vee b$; $a > b$, and $a \not> b$, this interpretation forces us to accept the following: "$a \wedge b$" is to be interpreted as the greater of the two numbers a and b; "$a \vee b$" as the smaller of them; etc. We obtain the matrices (also useful for showing that D4.3 is independent of 4.1, etc.):

a	$I\,a$	$U\,a$	$a \wedge b$	123	$a \vee b$	123	$a > b$	123	$a \not> b$	123
1	3	3	1	123	1	111	1	123	1	311
2	3	1	2	223	2	122	2	113	2	332
3	1	1	3	333	3	123	3	111	3	333

In order to show that in L_1, $a^i//I\,a$ and $a^m//U\,a$, we can use the generally valid dual rules:

(5.31) $$a^i//a > I\,a$$

(5.32) $$a^m//U\,a \not> a$$

or alternatively[15]

[14] $I\,a$, $U\,a$, and the other modal functions are defined in ⟨Popper, 1947d⟩, section VIII. A simpler definition (taken from ⟨Logic without Assumptions, footnote 20⟩) of $I\,a$ is given in section VII below, D7.I. The dual of this definition is:

$$a//U\,a \leftrightarrow (c)((c/a \bigvee c/b) \;\&\; (a/b \rightarrow a/c)).$$

[15] It is remarkable that both a^i and a^m can be defined in terms of "$>$" and its dual "$\not>$", while a^k

(5.41) $$a^i // a > (a \not> a)$$

(5.42) $$a^m // (a > a) \not> a.$$

Evaluating with the help of our numerical tables either 5.31 and 5.32 or 5.41 and 5.42, the equivalences $a^i // I\, a$ and $a^m // U\, a$ can easily be shown to be valid in L_1.

This result is not in general true, but the fact that it holds for L_1 establishes that, without risking a contradiction, we may postulate $a^i // I\, a$, provided that classical negation does not exist in the language under consideration. This justifies the well known intuitionistic identification of a^i with "a is impossible".

Our result is by no means trivial since from our definition of a^i (which is equivalent with the implicit characterization of Heyting's calculus) we obtain

(5.5) $$a, b / a^i \rightarrow b / a^i$$

while we cannot obtain the corresponding formula for $I\, a$ from its definition; if we could, the equivalence of $I\, a$ and a^k could be established in the presence of a^k, which is obviously not possible, in view of our definitions of $I\, a$ and a^k. The place of 5.5 is taken by the weaker rules

(5.51) $$a, b / I\, a \rightarrow b / a^i.$$

(5.52) $$a / I\, a \rightarrow b / I\, a.$$

The fact that a rule which is like 5.5 but with "$I\, a$" instead of "a^i" cannot be shown to follow from our definition of $I\, a$ might be considered, at first sight, as speaking against the intuitionistic identification of a^i with $I\, a$. But the problem is whether a^i is equivalent to *$I\, a$ in the absence of a^k*. That this is the problem may be seen from the fact that intuitionism admits that the law of the excluded middle holds in a wide range of cases, which means, in our way of speaking, that for certain statements a it may happen that a^i is complementary to a and thus coincident with a^k.

327 | A complete formal justification of the intuitionist identification of a^i with the impossibility of a could be obtained by proving the following very general

Conjecture: If a language L does not contain the classical negation of every of its statements but does contain a^i and $I\, a$ for every statement a, then the following holds: if, for some b, the does not exist b^k, then $I\, b$ is the weakest statement contradictory to b, so that b^i is equivalent to $I\, b$.

I have so far not been able to prove or disprove this conjecture. (The problem is

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cannot be so defined (as proved by our example L_1). We see that every language containing ">" and "$\not>$" necessarily contains a^i and a^m (possibly undistinguishable from a^k) while it need not contain a^k. – It may be mentioned that the theory of the anti-conditional "$\not>$" is quite interesting. We have, for example, the dual of 7.40 (see section VII, below), i.e.:

$$a \vdash b \not> c, d \leftrightarrow (e)(b \vdash e, c \rightarrow a \vdash e, d).$$

We also obtain, with the *modus ponens*, its dual:

$$a, a > b \vdash b \qquad a \vdash a \not> b, b$$

a straightforward calculation in our metalinguistic calculus, and most probably not difficult to solve; cp. its formal presentation at the end of section VII.) Meanwhile, our example of a language L_1 for which this conjecture holds establishes that, for a language which does not contain classical negation, we may postulate that a^i is identical with $I\,a$, without fear of contradiction.

It should be noted that 5.5 is the only one of the more important rules which a^i and $I\,a$ do not share. Among the more important rules which can be derived from the definition of $I\,a$ and which hold for a^i as well are:

(5.6) $$a, I\,a \vdash$$

(5.7) $$a \vdash I\,a \rightarrow\ \vdash I\,a$$

(5.71) $$I\,a \vdash a \rightarrow\ \vdash I\,I\,a$$

(5.8) $$a \vdash I\,I\,a$$

(5.9) $$a \vdash I\,b \rightarrow b \vdash I\,a$$

Equally important is that the following classical principles (which also hold for a^m and $U\,a$) cannot be shown to hold generally of either a^i or $I\,a$:[a]

(5.6′) $$\vdash a, a^m$$

(5.7′) $$a^m \vdash a \rightarrow\ \vdash a$$

(5.8′) $$a^{mm} \vdash a$$

(5.9′) $$a^m \vdash b \rightarrow b^m \vdash a.$$

VI

We shall now extend our considerations to other kinds of negation, even to fairly remote and unusual ones. Neglecting (1) $I\,a$ and $U\,a$, which were considered in the last section, we shall now consider (2) a^k; (3) a^i; (4) a^m; (5) fa, i.e. the self-contradictory compound of a (for which $fa//fb$ holds) definable, for example, by

$$a//fb \leftrightarrow (c)(a/b \leftrightarrow a/c);$$

and ultimately (6) three negations, a^j; a^l; and a^n, each of which is to be considered as introduced with the help of one of the following three primitive rules, respectively:

(6.1) $$a, b/c^j \rightarrow a, c/b^j$$

(6.2) $$a, b^l/c \rightarrow a, c^l/b$$

(6.3) $$a, b/c \rightarrow a, c^n/b^n$$

328 | (These three rules will be shown *not* to be equivalent to definitions, that is to say, they are not fully characterizing rules in our sense.) 6.1 is equivalent to the axioms which

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[a] In 5.7′ and 5.8′ we have replaced occurrences of "⊣" by "⊢".

introduce the negation a^j of Johansson's so-called "Minimalkalkül". In view of 6.2 we may call a^l the "left-hand side negation of a" (in contradistinction to Johansson's a^j which, in view of 6.1, is a "right hand side negation"). a^n may be called the "neutral negation"; it is neutral with respect to right-sidedness and left-sidedness, as is a^k which, indeed, can be fully characterized by the converse of 6.3, viz. by:

$$(6.4) \qquad a, b^k/c^k \rightarrow a, c/b$$

The following diagramme indicates the way in which the rules of the six negation a^k, a^i, a^m, a^j, a^l, and a^n are satisfied.

$$\begin{tikzcd}
 & & a^k \arrow[dll] \arrow[ddl] \arrow[ddd] \arrow[ddr] \arrow[drr] & & \\
a^i \arrow[dr] & & & & a^m \\
 & a^j \arrow[dr] & & a^l \arrow[dl] & \\
 & & a^n & &
\end{tikzcd}$$

The arrows indicate, for example, that a^k satisfies the rules holding of all the others, or that a^i satisfies the rules holding of a^j and a^n, and that the latter ones are satisfied by all others (except a^m which is so weak[16] that it does not satisfy even 6.3).

We shall now show that the three negations listed under (6) cannot be defined.

In order to show this for a^j, we introduce the following auxiliary definition of "tb":

$$a//tb \leftrightarrow (c)(b/a \leftrightarrow c/a)$$

"tb" may be called the "self-complementary compound of b". We obtain ta/tb.

It can now be shown that tb satisfies 6.1. On the other hand, a^k also satisfies 6.1. Thus, if a^j were definable, we would obtain $ta//a^j$ and $a^k//a^j$ and therefore $ta//a^k$. But this is possible in contradictory languages only; since we have $(a > a)^k \vdash$ and $\vdash t(a > a)$, it is clear that $ta//a^k$ would lead to $a//b$. Thus there cannot be a definition equivalent to 6.1, and a^j cannot be defined[17].

Similarly, 6.2 is satisfied by fa and a^k; thus a^l cannot be defined.

329 | 6.3 is satisfied by ta, fa and a^k; thus a^n cannot be defined.

The fact that Johansson's negation a^j cannot be formally distinguished from ta (which is not, by any stretch of imagination, to be called a negation) speaks strongly against its adoption. a^l seems slightly preferable since fa has something in common with Ia; for fa is equivalent to Ia whenever a is either factual or demonstrable. a^n is, perhaps, the best of the three negations under (6).

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[16] There are, of course, dual rules of 6.1, 6.2, and 6.3, two of which are satisfied by a^m, just as 6.1 and 6.3 are satisfied by a^i. Note that our diagramme does not contain the duals of a^j, a^l, and a^k, and that it is therefore not fully symmetrical.

[17] For Johansson's calculus, see ⟨Hilbert and Bernays, 1939, p. 449f⟩. The fact that his negation cannot be defined, and that this impossibility can be proved, is mentioned in ⟨Logic without Assumptions, p. 107⟩.

But the fact that none of the three is definable speaks very strongly against all of them; indeed, I suggested in ⟨Popper, 1947d⟩ that the term "formative sign" should be applied only to signs whose meaning is definable by definitions in terms of deducibility. Should this suggestion be accepted, then we would have to say that those signs of a language L which represent Johansson's negation, or the others under (6), are not formative.

On the other hand, the very fact that these three signs cannot be defined makes it possible to combine them in the same language L with classical negation without running the risk of destroying their distinguishableness; which is not possible for the signs under (3) and (4).

VII

We now turn to some concluding remarks about the existential assumptions connected with intuitionist and classical negation. They will provide, at the same time, examples of the kind of existential problems which are far from trivial and which arise, and can in principle be solved, within the general theory of derivation.

It is well known that intuitionism does not assert that there does not exist a classical negation of any statement, on the contrary, Brouwer has not only asserted that the law of excluded middle is valid for certain entities, but has given a proof[18] of its validity for a certain range of entities. This means, from our point of view, that there exist some statements a to which statements b exist which are both contradictory and complementary to a. Where intuitionism deviates from classical logic is in its assertion that such statements b do not exist to *every* statement a.

In other words, intuitionism asserts, for the language which it considers (the language in which mathematicians deal with infinite sets):

$$(7.1^i) \qquad (a)(Eb)(c)(c \vdash b \;\leftrightarrow\; a, c \vdash).$$

The corresponding assertion or postulate of classical logic is

$$(7.1^k) \qquad (a)(Eb)(c)((c \vdash b \;\leftrightarrow\; a, c \vdash) \;\&\; (\vdash a, c \;\leftrightarrow\; b \vdash c)).$$

which, of course, implies 7.1^i.

Now intuitionism does not only assert 7.1^i, but it denies 7.1^k; that is to say, it asserts besides 7.1^i the following principle which is a negation (an intuitionist one) of 7.1^k:

$$(7.2^i) \quad (Ea)(b)(Ec)(d)(e)(((c \vdash b \;\leftrightarrow\; a, c \vdash) \;\&\; (\vdash a, c \;\leftrightarrow\; b \vdash c)) \rightarrow d \vdash e).$$

330 | In other words, the fundamental assertion of intuitionism – the one by which it is distinguished from classical logic – is an existential assertion; it asserts the existence

[18] Cp. ⟨Brouwer, 1928⟩.

of a statement a for which the application of the classical principle 7.1^k leads to a *contradiction*. (This is the force of the clause "$d \vdash e$" which allows us to deduce a contradiction from a tautology.)

Intuitionism does not consider an existential assertion such as 7.2^i as legitimate if it cannot be supported by an actual construction of an example; it is therefore a crucial task for intuitionism to give an example of a statement which, if treated classically, leads to a contradiction. In other words, the proof[19] of the contradictoriness of classical mathematics must be of crucial importance for intuitionism. It is ⟨in⟩ agreement with this result that Brouwer considers the proof of the contradictoriness of classical mathematics as one of the most central problems of intuitionist mathematics.

We now proceed to state some principles equivalent to 7.2^i.

One such principle is obtained by asserting the existence of an intuitionist negation which is not classical. This may be written, considering 7.1^i and 7.1^k:

$$(7.21^i)\quad (Ea)(Eb)(c)(d)(e)((c \vdash b \leftrightarrow c, a \vdash) \;\&\; ((b \vdash c \leftrightarrow c \vdash a) \rightarrow d \vdash e))$$

In the presence of 7.1^i, 7.21^i is equivalent to 7.2^i.

A less obvious equivalence can be obtained if we remember[20] that, in in the presence of intuitionist negation and the conditional, classical negation can be derived from the following principle 7.3^k (cp. also 5.22):

$$(7.3^k)\qquad a, b > c/b \rightarrow a/b$$

In this form, the existential character of 7.3^k is not very obvious, but it becomes obvious if we eliminate here the sign ">", with the help of

$$(7.40)\qquad a, b > c/d \leftrightarrow (e)(b, e/c \rightarrow a, e/d)$$

which an alternative characterizing rule[21] for ">", obtainable from 3.81 (or from the definition of "$b > c$"). Applying 7.40, we can transform 7.3^k into 7.4^k:

$$(7.4^k)\qquad (a)(b)(c)(Ed)((b, d/c \rightarrow a, d/b) \rightarrow a/b)$$

which shows its existential character. Negating 7.4^k, we obtain a principle 7.4^i which (in the presence of 7.1^i and of a formula asserting the existence of the conditional) is equivalent to 7.2^i:

$$(7.4^i)\qquad (Ea)(Eb)(Ec)(d)(e)(f)(((b, d/c \rightarrow a, d/b) \rightarrow a/b) \rightarrow e/f).$$

In this case, the equivalence is by no means obvious, but demands a fairly complicated proof, based, of course, upon BI.1 and BI.2. It shows that the consequences derivable from this basis are not all trivial.

[19] This proof must, of course, be intuitionistically valid; and therefore, *a fortiori*, classically valid also.

[20] Cp. note 13, and rule 5.22, above, and ⟨New Foundations for Logic, p. 133⟩ (rule 4.2e).

[21] For the dual of rule 7.40, see note 15 above.

331 As an example of an interesting and perhaps more difficult problem, | I may refer to the conjecture, formulated in section V, that $a^i /\!/ I\,a$ whenever a^i and $I\,a$ exist but not a^k. This conjecture can be proved or disproved by proving or disproving a metalinguistic conditional with the conjunction of the closures of BI.1, BI.2 and the definition of "$\vdash a, b$" and "$a, b \vdash$" as antecedent and a consequent which asserts: "If a is (not equivalent to b^m and therefore) not the classical negation of b, then, if a is the intuitionist negation of b, then $a /\!/ I\,b$." In order to formalize this assertion, we shall make use of the following comparatively simple definition[22] of $I\,b$:

(D7.1) $$a /\!/ I\,b \;\leftrightarrow\; (c)((a/c \vee b/c) \;\&\; (b/a \rightarrow c/a)).$$

With the help of this, we can write the assertion in question as follows

$$(a)(b)(c)(d)(Ee)(Ef)(Eg)(((a/d \;\leftrightarrow\; \vdash b, d) \rightarrow e/f) \rightarrow ((g/a \;\leftrightarrow\; b, g \vdash) \\ \rightarrow ((a/c \vee b/c) \;\&\; (b/a \rightarrow c/a))).$$

If the metalinguistic conditional here described is refutable, and a counter example can be constructed,[23] i.e., a language for which it is not valid, then the problem arises of formulating the necessary and sufficient conditions under which it holds; for that it holds for some languages has been established by our example in section V.

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[22] Cp. note 14, above.

[23] *Note added in the proofs.* I have now been able to construct various simple counter examples which refute the conjecture.

Popper, K. R. (1948d). On the Theory of Deduction, Part II. The Definitions of Classical and Intuitionist Negation. In: *Indagationes Mathematicae* 10, pp. 111–121.

Chapter 7
The Trivialization of Mathematical Logic (1949)

Karl R. Popper

Abstract This article is a corrected reprint of K. R. Popper (1949a). The Trivialization of Mathematical Logic. In: *Proceedings of the Tenth International Congress of Philosophy*. Ed. by E. W. Beth, H. J. Pos, and J. H. A. Hollak. Vol. 1. North-Holland, pp. 722–727, including the corrections from K. R. Popper (1949b). Errata. In: *Proceedings of the Tenth International Congress of Philosophy*. Ed. by E. W. Beth, H. J. Pos, and J. H. A. Hollak. Vol. 1. North-Holland, p. 1259.

Editorial notes: Kleene (1949, this volume, § 13.13) cites a preprint of this article in his review. Popper uses the notation $a_1, \ldots, a_n/b_1, \ldots, b_m$ also in his unpublished "A Note on the Classical Conditional" (this volume, Chapter 17).

(1)

722 More than two decades ago, the method of truth tables or matrices seemed to offer some prospect of trivializing mathematical logic, as represented, say, by *Principia Mathematica*. Two hopes were raised.

One was that it may be possible to show that the system of mathematical logic rests entirely upon the *definitions of the formative linguistic signs involved*. Since the truth tables or matrices may be taken as definitions of these signs, it was hoped that the truth of all the propositions of the system could be shown to rest upon these definitions.

Another of the hopes raised was that the whole system could be shown to be decidable or computable by methods analogous to evaluations with the help of truth tables.

It is known that both these hopes failed. The second hope, more especially, was shattered in consequence of Gödel's discoveries and finally given up in view of proofs by Church and Turing. No attempt to revive it will be made in this paper.

But the first hope also failed. The difficulty was that only a narrow part of logic

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could be tackled by the matrix method. Even quantification theory proved to go beyond it. But apart from the quantifiers, other formative signs – or at least, other signs which most logicians considered as formative – appeared to be hopelessly out of reach of the truth table or matrix methods; especially the *signs of identity*.

But other and perhaps more radical doubts appeared. The truth table method itself threatened the very security it first appeared to produce. It gave rise to the so-called many valued systems, and to doubts whether the definitions obtainable in the two valued system were the only adequate ones, or even whether they were at all adequate.

Still more radical, from the point of view here taken, were the doubts entertained by those who gave up the truth table method altogether – for various reasons, among which two may be mentioned here.

(a) The truth table of the conditional (I shall avoid throughout the term “implication”) was intuitively unconvincing. This led to several alternative systems, such as that of Lewis, or Heyting’s intuitionistically valid rules for the conditional.
(b) Classical negation and its truth table were, by intuitionists, considered unsatisfactory, in consequence of Brouwer’s discussion of the law of the excluded middle. This led especially to the systems of Heyting, Kolmogoroff, and Johansson.

723 In view of this situation, it is no longer possible to accept the method of | truth tables ⟨or⟩ matrices as *the* fundamental method of mathematical logic. But I do not think that there is any need to give up the hope for a trivialization of mathematical logic in the sense that the whole of it becomes obtainable from definitions of the formative signs.

(2)

As the sole definiens of our definitions, the idea of deducibility or derivability will be used. We can restrict ourselves to using deducibility from *one* premise. We write

$$“D(a, b)”$$

for “a is deducible from b”. The letters “a” and “b”, etc., are used as variables whose values are statements (i.e., “a” and “b” are variables that may be supplanted by names of statements, but not by statements themselves). Deducibility is thus first introduced as a two-termed (metalinguistic) predicate. We do not need, for the derivation of mathematical logic, to assume more about deducibility than that it is transitive and reflexive. But for the purpose of intuitive grasp as well as for the purpose of comparing our definitions with the truth table definitions, we shall remember that the assertion “the statement a follows from the statement b” will be true if, and only if, no counter example exists, i.e., no example of statements (of the same logical form) such that the conclusion a is false while the premise b is true.

With the help of “$D(a, b)$” it is easy to define deducibility from n premises. We write

$$“a_1, \ldots, a_n / b”$$

in order to express that b is deducible from the n premises $a_1, a_2, \ldots, a_n$. It is, furthermore, easy to define a generalization of this. We write

$$\text{“}a_1, \ldots, a_n/b_1, \ldots, b_m\text{”}$$

in order to express ⟨that, if the n premises⟩ $a_1, a_2, \ldots, a_n$ are all true, at least one of the statements $b_1, \ldots, b_m$ must be true too. The definition must be so framed that “a/b” means the same as “$D(b, a)$”; and it can be so framed that, for $m = 0$, we obtain a formula “$a_1, \ldots, a_n/$” which means that the statements $a_1, \ldots, a_n$ are contradictory to each other, i.e., that they cannot all be true; and that, accordingly, “$a/$” means that a is self-contradictory, or logically refutable. And we obtain, for $n = 0$, a formula “$/b_1, \ldots, b_m$” which means that the statements $b_1, \ldots, b_m$ are complementary to each other, i.e., that they cannot all be false; and that, accordingly, “$/b$” means that b is self-complementary or tautologous, or logically demonstrable. We also define

$$\text{“}a//b\text{”}$$

in such a way that it expresses the idea that “$D(a, b)$” and “$D(b, a)$” both hold i.e., that a and b are mutually deducible.

All this can be easily achieved by using “$D(a, b)$” as our only undefined term, characterized only by transitivity and reflexivity.

724 | We can now say, of two statements a and b, that a is weaker than b (we use the term “weaker”, in order to express more precisely, that a is weaker *or at most as strong* as b) if and only if $D(a, b)$, i.e., if a follows from b. And we can say, under the same circumstances, that b is stronger than a (that is, more precisely, at least as strong as a).

It is now easy to define those compounds for whose definitions truth tables can be used, and also the other formative signs which cannot be defined by truth tables. I shall not give all definitions but I shall confine myself to the most interesting cases.

The conjunction of the statements a and b can be defined in various ways, for example (a) as the weakest of the statements from which a as well as b can be derived, or (b) as the strongest of the statements derivable from every statement from which a as well as b can be derived; etc.

Using our “/”-notation, we can put this as follows. (We use “$a \wedge b$” as a *name* for the conjunction of a and b.)

(2.1) “$a \wedge b/c$” means the same as “$a, b/c$”.

This is surely trivial, but it suffices.

The conditional of b and c (we use “$b > c$” as its name) can be similarly defined as the weakest statement strong enough to satisfy the *modus ponens*. This, again, is trivial. It can be said without exaggeration that everybody who has used the words “if . . . then” has used them in such a way that, given the if-then-statement and the antecedent, the consequent is assumed to follow. We can write the *modus ponens*:

(2.02) “$b > c, b/c$”

To define the conditional "$b > c$" as the weakest statement that satisfies the *modus ponens* means to say that *every* statement a which is stronger than the ⟨conditional $b > c$⟩, that is, for which "$a/b > c$" holds, – and *only* such a statement – is strong enough to replace $b > c$ in 2.02. But this amounts to saying that our definition can be expressed by

(2.2) "$a/b > c$" means the same as "$a, b/c$".

It is clear that every adequate definition of $b > c$ must demand that it satisfies the *modus ponens*; and it is also clear that every statement a which is stronger than $b > c$ must satisfy it. Thus our definition is, again, trivial. What is remarkable is only that it suffices for all our purposes.

We proceed to negation. Here we have the choice of various possibilities. The intuitionist negation (of Heyting's calculus) can be completely characterized as follows: b^i, the intuitionist negation of b, is the weakest statement strong enough to contradict b; i.e.,

(2.3^i) "a/b^i" means the same as "$a, b/$".

There is a dual of intuitionist negation which I shall not discuss. The classical
725 negation of b, denoted by "b^k", can be characterized as the statement | which is both contradictory and complementary to b. Using "$a//b$" we can write

(2.3^k) "$a//b^k$" means the same as "$a, b/$ and $/a, b$".

It is possible to show that our definitions of the conjunction and of classical negation are (if we make use of the intuitive meaning of "a/b"⟨)⟩ equivalent to the classical two-valued truth tables for these compounds. But our definition of the conditional (and, obviously, that of intuitionist negation) is not equivalent ⟨to⟩ any definition by means of truth tables. All we can obtain, in this respect, from our definition of the conditional is that, if the antecedent is true and the consequent false, the conditional must be false too, and – in view of the fact that we obtain the formula "$c/b > c$" – that if the consequent is true, the conditional must be true too.

This is in so far quite satisfactory as it agrees well with our intuitive use of "if … then …" – in any case better than the classical truth tables. But how is it that our definition 2.2 suffices for our purposes, including classical logic, even though it is so much weaker than the classical truth table?

The answer is that the classical truth table for the conditional can be obtained, although not from our definition of the conditional alone, but from it together with our definition for classical negation. A fairly remote consequence of these two definitions is that 2.2, in the presence of 2.3^k, is equivalent to

(2.2^k) "$a/b > c, d$" means the same as "$a, b/c, d$"

and that this formula can indeed be shown to yield the classical truth table for the conditional. For it yields, in view of the reflexivity of "$D(a, b)$" (and since we can get rid of "a" on both sides), the formula

(2.21^k) "$/b > c, b$"

which demands that, whenever b is false, $b > c$ must be true.

We briefly turn to quantification and to identity, both undefinable in the truth value system.

The result of universal quantification of a statement a can be defined as the weakest statement strong enough to satisfy the law of specification, that is to say, the law "what is valid for all instances is valid for every single one".

The result of existential quantification of the statement a can be defined as the strongest statement weak enough to follow from every instance of a. (In both cases, we can easily explain what "instance" means.)

The identity statement "$Idt(x, y)$" can be defined as the weakest statement strong enough to satisfy the following formula (we use here "$a(x)$" as name of a statement-function in which a variable occurs whose *name* is "x")

$$\text{“}Idt(x, y), a(x)/a(y)\text{”}$$

726 | that is to say, the formula corresponding to what Hilbert-Bernays call the second identity axiom. (Hilbert-Bernays's first axiom follows from our demand that the identity statement must be the *weakest* statement satisfying this formula.) Difference can be defined, without presupposing negation, by a dual definition.

In precisely the same way as the Identity of the entities denoted by x and y we can define "$\{x\}(y)$" i.e., the name of a statement expressing that the entity denoted by y is a member of the unit class of the entity denoted by x (or that these two share all properties).

(3)

These definitions in terms of inference – they may be called inferential definitions – suffice for constructing the lower functional logic. They suffice for Boolean algebra, and for classical Aristotelian logic. We shall give a brief sketch of how these may be obtained in a simple way.

We use now "$P(x)$", "$Q(x)$" etc. as names of statements with one variable x; and we use "(x)" as universal operator. Then we can define "$P \subset Q$" by the following formula

(3.1) "$P \subset Q//(x)(P(x) > Q(x))$"

We further define $P = Q$ by

(3.2) "$P = Q//P \subset Q \wedge Q \subset P$"

and the Boolean product "PQ" by

(3.3) $$\text{“}PQ = R /\!/ (x)(((P(x) \wedge Q(x)) > R(x)) \wedge (R(x) > (P(x) \wedge Q(x))))\text{”}$$

We define P^-, the (classical) complement of P, by

(3.4) $$\text{“}P^- = Q /\!/ (x)((P(x)^k > Q(x)) \wedge (Q(x) > P(x)^k))\text{”}$$

and introduce the constant “O” by

(3.5) $$\text{“}P = O /\!/ P = PP^-\text{”}$$

This is all we need for Boolean algebra, since the associative and commutative laws for the Boolean product and the law of idempotence follow from the definition of the product, i.e. from 3.3.

Part of classical Aristotelian logic can be obtained in the usual way, as part of Boolean algebra, with “$P \subset Q$” as classical “a-proposition”, and with the classical “e-proposition” defined by

(3.6) $$\text{“}PeQ /\!/ PQ = O\text{”}$$

We obtain, as is well known, only 15 of the syllogistic moods. But we can obtain the whole of the classical theory of the syllogism (and thus prove its consistency) if we decide to define the a-proposition by

(3.7) $$\text{“}PaQ /\!/ P \subset Q \wedge (P = O)^k\text{”}$$

The classical “i-proposition” (denoted by “PiQ”) and the classical “o-proposition” are, of course, defined as the classical negations of the e-proposition and a-proposition, respectively.

727 | The only classical – or semi-classical – rules which now do not remain valid (but only conditionally valid) are the rules of obversion, in the direction from negative to affirmative propositions.

A system in which an axiom schema “PiP” is accepted has been published by ⟨Bocheński, 1948⟩, and similar systems were previously published, according to Bocheński (*op. cit.*, notes 3 and 5a) by ⟨Łukasiewicz, 1929⟩ and ⟨Ajdukiewicz, 1926–1927⟩. But such a system, although consistent, cannot be adequate, since it is not compatible with the two demands that (a) there should be sufficient terms in the system to have true instances of all four categorical propositions and (b) that “$PiQR / PiQ \wedge PiR$” should hold (i.e. that from “Some men are tall and fat” we can deduce “Some men are tall and some men are fat”). These two demands seem to be clearly needed for adequacy. But if they are accepted, then “PiP” leads to a contradiction. For we obtain from it “$PRiPR$” and thus, by conversion and (b), “PiR”, which contradicts (a) since it says that no example of an e-proposition can be true. No such difficulty arises in our system which establishes the fact that all the syllogistic rules can be obtained from appropriate definitions of the four categorical propositions.

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Chapter 8
Are Contradictions Embracing? (1943)

Karl R. Popper

Abstract This article is a corrected reprint of K. R. Popper (1943). Are Contradictions Embracing? In: *Mind* 52 (205), pp. 47–50.

Editorial notes: The article is an elaboration on a footnote to "What Is Dialectic?", Popper (1940, fn on p. 408); . Popper announced it in a letter to Harold Jeffreys of 26 April 1942 (this volume, § 27.1). In a letter from Popper to Carnap of 15 October 1942 (this volume, § 23.1) he calls it an "unimportant Note", and explains why he uses the word "embracing" instead of Carnap's word "comprehensive": he did not yet know Carnap's *Introduction to Semantics* (1942). However, in the last passage the possibility of a negation too weak to be a logical constant is indicated, which raises the question of logicality. The original publication contains the address "*Canterbury University College, New Zealand*" and the name "K. R. Popper" at the end, which we have omitted here.

(A)

47 Under the title *Does a Contradiction Entail Every Proposition?* ⟨Jeffreys, 1942⟩ Mr. Harold Jeffreys writes that he was "satisfied at first, but on further thought ... again doubtful", when considering my affirmative answer to this question (sketched, in a non-technical paper, in ⟨Popper, 1940⟩. *Cp.* also the historical references to Łukasiewicz and Duns Scotus, made in ⟨Popper, 1941⟩[a]). Mr. Jeffreys' doubt appears to be due to the following consideration. He believes (*a*) that the very argument used to show that every sentence can be inferred from a contradiction *assumes* the inadmissibility of some (or the same) contradiction; and (*b*) that this way of arguing is circular. Before re-analysing my argument in order to show that I did not make the

[a] Popper (1941) reads: "Apart from this correction I should like to add, as an acknowledgement, to the footnote on p. 408: 'The following more technical considerations, pp. 406–408, were known, in essence, to Duns Scotus († 1308), as shown by J. Łukasiewicz, the famous Polish logician and historian of logic; cf. ⟨Łukasiewicz, 1935⟩'". Łukasiewicz (1935, p.124) gives Duns Scotus' proof (by means of an example) of *ex contradictione sequitur quodlibet* which uses disjunction introduction and the disjunctive syllogism.

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alleged assumption, I should like to mention that even if I had, my argument would not be circular, so far as Mr. Jeffreys' problem is concerned. For there may be people who are prepared to grant the falsity or inadmissibility of contradictions, but who are not satisfied that every contradiction must be *embracing*, *i.e.* that every sentence can be inferred from it. (In fact, this attitude is precisely Mr. Jeffreys' if I am not mistaken.) In other words, even if I were to admit (*a*), or in Mr. Jeffreys' words, that the denial of "the possibility that *p* and *non-p* can both be true . . . assumes that the system does not contain the contradiction . . .", even then I would only assume that the contradiction is false, not that it is embracing; so that the argument would not be circular.

However, I really tried in my paper to show more than the "embracingness" of contradictions. I aimed also at showing that the embracing character of contradictions is a *practical* reason for not admitting them. This further aim could not be realized if (*a*) had to be granted, *i.e.* if we had to *assume* the inadmissibility of contradictions, in order to show that they are embracing. In connection with this aim, Mr. Jeffreys would indeed be right in believing that (*b*) follows from (*a*), *i.e.* my argument would indeed be circular, if I had made the assumption (*a*). But I do not agree with Mr. Jeffreys that I have made it, or that I have to make it.

Mr. Jeffreys' argument appears to be this. I postulated in my paper two *rules of inference*:

(1) From any given premise, a conclusion can be drawn which is a disjunction containing the premise as one component and any sentence whatsoever as its second component.

(2) From two premises of which the first is a disjunction, and the second the negation
48 of one component of that disjunction, | the second component of that disjunction can be drawn as a conclusion.

Nothing beyond these two rules need be assumed in my proof. Mr. Jeffreys argues, however, that (2) implicitly assumes that a sentence and its negation cannot both be true; and he can point to a statement in my paper (the last on p. 409) which seems to admit that this assumption is implied. But this is a misunderstanding. In logic, as well as in mathematics, we must always clearly distinguish between the formal trend of the argument and the personal intuitive procedure by which we try to "grasp" it. For instance, we can build up a system of Geometry in a strictly deductive way, without the use of illustrative *diagrams*, but few people would intuitively "grasp" such a system without using them, privately, as it were. It is only in this illustrative way that (2) "assumes" the inadmissibility of contradictions. In order to convince those who are reluctant to admit (2) I argued *ad hominem*: "The first premise, being a disjunction, maintains that at least one of its components is true. The second premise, being the *negation* of one of the components, *maintains that this component cannot be the true one*; therefore, the other must be true." This "argument" indeed makes use of the intuition that a sentence and its negation cannot both be true. But it is only an illustration; like the illustrative diagrams, it cannot prove anything at all. Our rule (2) is not deducible from, or replaceable by, the assumption that a sentence and its negation cannot both be true (*i.e.* the so-called Law of Contradiction). Nor does it imply this assumption. In fact, so little is needed to show the embracingness of and thereby the practical uselessness of contradictory premises, that we can, if we like, proceed in our construction of a system of logic in the following way: we can first assume a *rudimentary system* of the "calculus of propositions" which is so incomplete, or *logically weak*, that it is part of practically all logical systems, and that the so-called laws of contradiction, excluded middle, identity, and double negation, as formulated within the calculus of propositions, can be proved to be entirely *independent* of it. With the help of this rudimentary system, we can next establish the embracingness and uselessness of contradictions. (And only after having done this, do we proceed, if we wish, to strengthen our system, until all non-embracing formulae are deducible; thus we may add next, for instance, the "law of contradictions".) To prove that this is possible, I enter into some technicalities.

(B)

Our two rules of inference (1) and (2) must *not* be taken, of course, as *formulae* of a *propositional calculus*. But they can be replaced by certain formulae (of the system of Material Implication). (2) would have to be replaced by the formula (2′), *and* by the "Principle of Inference" (*cp.* PM, *i.e. Principia Mathematica* 1·1; LL, *i.e.*, p. 126, and 14·29, which enables us to carry out all proofs here needed also within a *System of Strict Implication* or *Entailment*; *cp.* also LL 19·72)[b]. The rule (1) can be
49 | replaced by the formula (1′). We thus arrive at a rudimentary system consisting of the primitive formulae (1′) and (2′):

(1′) $$p \supset (p \vee q)$$

(2′) $$(p \vee q) \supset (\sim p \supset q)$$

(1′) is identical with PM 2·2; (2′) with PM 2·53. The two formulae (1′), (2′) constitute a system sufficient to prove the embracingness of any "pair of contradictory sentences", *i.e.* any two formulae of which one is the negation of the other.

By combining (1′) with (2′), we can eliminate the disjunction. We thus obtain another example of a rudimentary system with the same property, namely the single primitive formula

(PM 2·24) "$p \supset (\sim p \supset q)$"

which is also Carnap's PSIIi, and corresponds to his PSIi[c]. (Carnap's PSIi is not, as Mr. Jeffreys says, "$\sim p \supset (p \supset q)$", *i.e.* (PM 2·21), which has, however, analogous properties. It is important to note that *none* of these formulae can be said to be objectionable or "drastic" as Mr. Jeffreys suggests; not even from the standpoint of a theory of Strict Implication or Entailment, since such a theory may contain the whole theory of Material Implication, as Lewis has shown.)

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[b] Cf. Whitehead and Russell (1910–1913, 1925–1927) and Lewis and Langford (1932).

[c] The formulas PSIIi and PSIi can be found in Carnap (1934a, 1937).

(C)

The last system, *viz.* the single formula (PM 2·24) can be used to throw some light on the *weakness* of these rudimentary systems. For it is clear that it can be replaced for our purpose by the following *System A* of three primitive formulae:

(1″) $$p \supset (q \supset p)$$

(2″) $$(q \supset p) \supset (\sim p \supset \sim q)$$

(3″) $$\sim\sim q \supset q$$

These formulae correspond to Hilbert-Bernays I[d], p. 66 (I, 1); (V, 1); (V, 3). With the help of proofs supplied by them, pp. 76ff., it can easily be shown that, even after adding their formulae defining conjunction (whereupon for instance "$p \,.\, \sim p$" can be shown to be embracing), and disjunction, the laws of contradiction, excluded middle, identity, and the first law of double negation (V, 2) are all *independent* of *System A*.

(D)

If we now omit (3″), *i.e.* the second law of double negation, we still retain a rudimentary system sufficient to prove the practical uselessness of any pair of contradictory premises. In this *System N*, which consists only of (1″) and (2″), we can no longer prove full embracingness of contradictions, only what may be called their *n-embracingness*; *i.e.* we can prove that any *negation of any formula whatsoever* can be derived. But this is enough to show their practical uselessness. (Another *System N′*, with the same properties as *N*, is, of course, the single formula "$p \supset (\sim p \supset \sim q)$".)

Of other "rudimentary systems" I mention only the so-called *positive logic*, which
50 does not operate with negations. In such a | system, contradictions (*i.e.* sentences or classes of sentences from which a "pair of contradictory sentences" can be deduced) do not exist; but there are still embracing formulae (*e.g.* the single formula "$p \supset q$").

To sum up: in any but the most rudimentary logical systems, and certainly in any system rich enough for mathematical derivations, *embracingness, n-embracingness, and contradictoriness coincide.* Systems containing the operation of *negation* may be so much weakened that contradictoriness only implies n-embracingness. It appears, however, that we cannot weaken them further without depriving *negation* of the character of a logical operation.

There is little hope for Hegelian dialectics to find support in even the weakest of logics . . ..

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[d] Cf. Hilbert and Bernays (1934).

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Chapter 9
A Note on Tarski's Definition of Truth (1955)

Karl R. Popper

Abstract This article is a corrected reprint of K. R. Popper (1955a). A Note on Tarski's Definition of Truth. In: *Mind* 64 (255), pp. 388–391.

Editorial notes: The article was also reprinted in Popper (1972) and has been translated into Italian (cf. Lube, 2005 for bibliographic details). We number footnotes consecutively, whereas in the original footnotes are numbered per page. The original publication contains the name "Karl R. Popper." and the address "London School of Economics" at the end, which we have omitted here.

388 In his famous paper on the Concept of Truth[1], Tarski describes a method of defining the idea of truth, or more precisely, the idea of "x is a true statement (of the language L)". The method is first applied to the language of the class calculus, but it is a method of very general application to many different (formalized) languages, including languages which would allow the formalization of some empirical theories. The characteristic of his method is that the definition of "true statement" is based upon a definition of the *relation of fulfilment*, or more precisely, of the phrase "the infinite sequence f fulfils the statement-function x".[2] *This relation of fulfilment is of interest on its own account*, quite apart from the fact that it is crucial for the definition of truth (and that the step from the definition of fulfilment to that of truth presents hardly any problem). The present note is concerned with the problem of employing, in the definition of fulfilment, *finite instead of infinite sequences*. This is, I believe, a desideratum both from the point of view of an application of the theory to the empirical sciences, and also from a didactic point of view.

Tarski himself briefly discusses two methods[3] which employ finite sequences of

[1] *Cf.* ⟨Tarski, 1935a⟩. I understand that Tarski now prefers to translate "Aussage" and "Aussagefunktion" by "sentence" and "sentence-function" (while I am using here "statement" and "statement-function") and that these terms are used in Professor J. H. Woodger's translation of Tarski's logical papers, soon to be published by the Clarendon Press, Oxford ⟨cf. Tarski, 1956⟩.

[2] *Cf.* ⟨Tarski, 1935a⟩ pp. 311, 313. Note that the class of statement-functions includes that of statements, *i.e.* of *closed* statement-functions.

[3] The first alternative method is sketched in Tarski's note 40 on pp. 309f. (It is not explicitly stated that this method may be used for the purpose of avoiding infinite sequences, but it is clear that it

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varying length instead of infinite sequences. But he points out that these alternative methods have certain disadvantages. The first of them, he indicates, leads to "considerable complications" (*ziemlich bedeutenden Komplikationen*) in the definition of fulfilment (Definition 22), while the second has the disadvantage of "a certain
389 artificiality" (*eine gewisse Künstlichkeit*), | insofar as it leads to a definition of truth (Definition 23) with the help of the concept of an "empty sequence" or a "sequence of zero length".[4] What I wish to point out in this note is that a comparatively slight variation of Tarski's procedure allows us to operate with finite sequences without getting involved in complications, or in such artificialities (*e.g.* empty sequences) as Tarski had in mind. The method allows us to preserve the very natural procedure of the condition (δ) of Tarski's Definition 22 (and thus to avoid the detour of introducing relations – or attributes – of a degree equal to the number of the *free* variables of the statement-function under consideration). My variant of Tarski's method is a slight one; but in view of the fact that Tarski refers to other variants which have considerable disadvantages, but not to this one, it may be worth while to describe what is perhaps a small improvement.[5]

In order to do so, it is useful to mention, informally, first the idea of a place number n (or the nth place) of a finite sequence of things, and secondly, the idea of the length of a finite sequence f, *i.e.* the number of places of f (in symbols, $Np(f)$) which is identical with its greatest place number, and of the comparison of different finite sequences with respect to their length. We mention, in the third place, that a thing may occupy a certain place – the nth, say – in the sequence, and may, therefore, be described as the nth thing or the nth member of the sequence in question. It should be noted that one and the same thing may occur in different places of a sequence, and also in different sequences.[6]

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can be so used.) The second method is described in note 43, pp. 313f. The method suggested in this note of Tarski's, which is technically different from the one used by Tarski in his text, is used by Carnap in his *Introduction to Semantics* (1942), pp. 47f. Although Carnap refers to Tarski, he overlooks Tarski's anticipation of this particular method. (There is even a third method, indicated by Tarski in note 87 on p. 368. This device is very simple, but undoubtedly highly artificial, in Tarski's sense of artificiality; moreover, this method only relates to the definition of truth itself, not to that of fulfilment, which has an interest of its own.)

[4] This artifical concept is also used by Carnap.

[5] The main difference between my method and those suggested by Tarski (mentioned in note 3 above) consists in this. Tarski suggests to correlate, with a given function (either infinite sequences or) finite sequences of a definite length (dependent on the function) while I use finite sequences which are "of sufficient length" (Definition 22a), *i.e.* not too short for the function in question. Accordingly, my finite sequences can be of *any length* (beyond a certain minimum which depends on the function). But the admission of finite sequences of any length (provided it is sufficient) does not involve any vagueness, since we easily obtain a *theorem* (*cf.* Tarski's Lemma A, p. 317) according to which, if f fulfils x, then every extension g of f also fulfils x (where g is an extension of f if and only if for every f_i there exists a g_i such that $g_i = f_i$). Thus the theorem informs us that we only need to consider the *shortest* finite sequences of those which are adequate to the function under consideration (to be sure, to the total compound function under consideration, as opposed to its components).

[6] The things considered in this section of Tarski's work are classes; in view of the development of Tarski's §§ 4 and 5, I shall here speak of "sequences of things" instead of sequences of classes, assuming that a relation $f_i \subset f_k$ is defined for all things f_i and f_k.

Like Tarski, I use "f_1", "f_2", ..., "f_i", "f_k", ..., "f_n", as names of the things
390 which occupy the 1st, 2nd, ith, kth, ..., nth | place of the sequence f. I use the same notation as Tarski does, except that I use "$P_k y$" as the name of the universalization of the expression y with respect to the variable v_k.[7] It is assumed that to Tarski's definition (11)[8] is added a definition of "v_k occurs in the statement-function x" – an assumption which in no way goes beyond Tarski's methods, and which in fact is implicit in Tarski's own treatment.

We can now proceed to replace Tarski's Definition 22. We shall replace it by two definitions, the preliminary definition 22a, and the definition 22b which corresponds to Tarski's own definition.

Definition 22a

A finite sequence of things f is adequate to the statement-function x (or of sufficient length with respect to x) if, and only if,

for every natural number n,

if v_n occurs in x, then the number of places of f is at least equal to n (*i.e.* $Np(f) \geq n$).

Definition 22b[9]

The sequence f fulfils the statement-function x if, and only if, f is a finite sequence of things, and x is a statement-function, and

(1) f is adequate to x,
(2) x satisfies one of the following four conditions:
 (α) There exist natural numbers i and k such that $x = \iota_{i,k}$ and $f_i \subset f_k$.
 (β) There exists a statement-function y such that $x = \overline{y}$, and f does not fulfil y.
 (γ) There exist two statement-functions y and z such that $x = y + z$ and f fulfils either y or z or both.
 (δ) There exists a natural number k and a statement-function y such that
  (a) $x = P_k y$,
  (b) every finite sequence g whose length is equal to f fulfils y, provided g satisfies the following condition: for every natural number n, if n is a place number of f and $n \neq k$, then $g_n = f_n$.

Tarski's Definition 23 can now be replaced by one of the following two equivalent[10] definitions.

391 | *Definition* 23+

x is a true statement (*i.e.* $x \in Wr$) if and only if (a) x is a statement ($x \in As$) and (b) every finite sequence of things which is adequate to x fulfils x.

Definition 23++

x is a true statement (*i.e.* $x \in Wr$) if and only if (a) x is a statement ($x \in As$) and (b) there exists at least one finite sequence of things which fulfils x.

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[7] *Cf.* Tarski's definition (6) on p. 292.

[8] *Op. cit.* p. 294. Tarski defines explicitly only the phrase "The variable v_k occurs freely in the statement-functions x".

[9] This is exactly like Tarski's definition 22, except that (1) is added to Tarski's condition (in order to replace his infinite sequences by finite ones), and that our $(\delta)(b)$ contains a minor adjustment, insofar as it refers to the length of f (and of g).

[10] The equivalence emerges from Tarski's consideration; *cf. op. cit.* p. 313, lines 13 to 16.

It may be noted that the formulation of 23++ does not need to refer to the adequacy of the sequence. It may be further noted that in 23+ (which corresponds exactly to Tarski's definition), but not in 23++, the condition (a) may be replaced by "x is a statement-function", thus achieving a certain generalization by including certain statement-functions with free variables, for example, the function $\iota_{i,i}$; *i.e.* the universally valid (*allgemeingültige*) statement-functions.[11]

In an analogous way, 23++, if extended to functions, leads to the notion of a satisfiable (*erfüllbare*) statement-function.

I may finally mention that, in its application to an empirical theory (at least partially formalized), and especially to non-quantified statement-functions of such a theory, the definition of *fulfilment*, *i.e.* Definition 22b, seems to be perfectly "natural" from an intuitive point of view, mainly owing to the avoidance of an infinite sequence.[12]

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[11] *Cf. op. cit.* 320, Definition 27 and *seq.*

[12] We may use it, for example, to define an instantiation of a law (not written as a universalization, *i.e.* written without universal prefix) as a finite sequence of things which fulfils it; or, in my opinion more important, to define a *refuting instance* of any statement-function (open or closed) as a finite sequence of things which does *not* fulfil it.

Chapter 10
On a Proposed Solution of the Paradox of the Liar (1955)

Karl R. Popper

Abstract This article is a corrected reprint of K. R. Popper (1955b). On a Proposed Solution of the Paradox of the Liar. In: *Journal of Symbolic Logic* 20 (1), pp. 93–94.

Editorial notes: The article is an abstract of Popper's contribution to the *Amsterdam Meeting of the Association for Symbolic Logic* (1945), which was published in the proceedings Beth and Feys (1955). The proceedings contain the following preface:

> THE AMSTERDAM MEETING OF THE ASSOCIATION FOR SYMBOLIC LOGIC
>
> A meeting of the Association for Symbolic Logic was held in Amsterdam, on September 1, 1954, in conjunction with the International Congress of Mathematicians.
>
> The morning session for the invited address on *Algebraization of the Predicate Logic* by Alfred Tarski and for contributed papers, was presided over by W. V. Quine, the afternoon session, for contributed papers and for the invited address on *A Proposed Definition of Mathematical Logic* by Alonzo Church, was presided over by Robert Feys; these two meetings were held in the "Trippenhuis", the residence of the Royal Netherlands Academy of Sciences and Letters. At the end of the afernoon session, the participants were the guests of Professor and Mrs. E. W. Beth at a reception in the same building.
>
> An evening session, for contributed papers, was held in the Royal Tropical Institute and presided over by Leon Henkin. The room for this session was kindly provided by the Organizing Committee of the International Congress of Mathematicians.
>
> E. W. Beth
> Robert Feys

93 In his ***Symbolic logic, an introduction*** (New York 1952),[a] Professor F. B. Fitch
94 | attempts to solve the paradox of the liar by proposing what I shall refer to as his *hypothesis*: "the sentence, 'This proposition is false'" is "an indefinite proposition" (2.14). This hypothesis is based upon a "classification of propositions" (2.13) into (A) *definite propositions* which are either (1) True (T), or (2) False (F), an (B) *indefinite propositions* (I) "which are not to be asserted as true or false" (2.12).

[a] Fitch (1952).

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Using "a" as a variable name of a proposition, this classification becomes (1) $a \in I \equiv (\sim(a \in T) \,\&\, \sim(a \in F))$. Using "$i$" as a constant name of a proposition (i.e. of the paradox), we put (2) $i =$ "$i \in F$". Professor Fitch's hypothesis then becomes: (3) $i \in I$. Now assume this hypothesis to be true. Then (4) $\sim(i \in F)$ must be true in view of (1); and since in view of (2), (4) is the negation of i, we find that (5) (*the negation of* i) $\in T$. But (5) may also be written (6) $i \in F$.

From the hypothesis that (e) is true we have thus derived two contradictory propositions, (4) and (6). Thus (3), i.e. Professor Fitch's hypothesis, cannot be true; it must be either false or indefinite; and for his system to be consistent, (3) *must* be indefinite: it then escapes our *reductio*. But the consistency of the system results from weakness: it fails in that kind of refutation it was designed (pp. 217–225) to admit.

By interpreting "I" as the class of meaningless non-propositions, our argument down to (6) can be used to refute the hypothesis that i is meaningless.

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Chapter 11
On Subjunctive Conditionals with Impossible Antecedents (1959)

Karl R. Popper

Abstract This article is a corrected reprint of K. R. Popper (1959a). On Subjunctive Conditionals with Impossible Antecedents. In: *Mind* 68 (272), pp. 518–520.

Editorial note: The original publication contains the name "Karl R. Popper" and the address "University of London" at the end, which we have omitted here.

518 By a subjunctive conditional I mean here a statement of the following kind:

"Whether or not p is true in fact – if it *were* true, then q would certainly (or necessarily) be true"; or, perhaps, "should p hold, then q would hold", etc.

One can use the symbol

$$\text{“}p \underset{s}{\supset} q\text{”}$$

as an abbreviation of a statement of this kind. (This may also be read, if one prefers, "p subjunctively implies q", in analogy to reading "$p \supset q$" as "p materially implies q".)

The question to which this note is devoted is this: Let us use the name "opposite subjunctive conditionals" ⟨for⟩ two subjunctive conditionals with the same antecedents and directly opposite consequents, such as

(i) $$p \underset{s}{\supset} q$$

(ii) $$p \underset{s}{\supset} {\sim}q$$

We may then ask: can two opposite subjunctive conditionals be true together?

There seems to be considerable disagreement on this question. In an appendix to my book *The Logic of Scientific Discovery*[a] (new appendix ∗x), I implicitly assumed, without argument, that all those interested in subjunctive conditionals would agree that (i) and (ii) could well be true together, provided p was (physically) *impossible*. (I

[a] Popper (1959b).

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even assumed more than this.) I soon had to learn that some who had thought much on the subject disagreed with me on this point.[1]

Without claiming to say anything new here I shall therefore attempt to establish, by purely intuitive argument, two things.

(1) We can construct consistent sets of premises from which two opposite subjunctive conditionals, related as (i) and (ii), may be deduced by valid intuitive arguments.

(2) As a consequence of (1), we are bound to admit that two opposite subjunctive conditionals can be true together. If this admission is felt, by some, to be counter-intuitive, then we may ask them to remember that there are certain unusual and
519 rarely used | cases ("zero-cases" or "points of singularity") upon which it is unwise to bring intuition to bear immediately.

Before proceeding to an empirical example (II), I shall sketch an arithmetical example (I).

Consider the following three compatible premises, expressed in what may be called the "subjunctive style".

(I) Should a certain number a be a natural number between 3 and 6 (*i.e.* $3 < a < 6$), then the following would hold: should $a \neq 4$, then $a = 5$, so that a would be a prime number; and should $a \neq 5$, then $a = 4$, so that a would not be a prime number.

(IA) Should $a > 10$, then $a \neq 4$

(IB) Should $a > 10$, then $a \neq 5$

Moreover, we state categorically as a fact (fourth premise):

(I+) $a > 10$

Now two people, Allen and Brown, are asked to consider, in the light of these premises, the following question: if (*per impossibile*) a were between 3 and 6, would it be prime or not?

Allen may then argue from (I), (IA), and (I+), that a would be prime; and Brown from (I), (IB), and (I+) may argue that a would not be prime.

I believe that both deductions, however trite, are correct. I further hold that the situation is precisely analogous to the case where we can deduce two opposite material conditionals $p \supset q$ and $p \supset \sim q$ from consistent premises. There is no problem here: we simply conclude in this case that, in the light of the premises, p must be *false*. Similarly with subjunctive conditionals: we simply conclude that, in the light of the premises, p must be *impossible*.

To this argument, the reply has been made that reasoning from arithmetical examples is unconvincing in questions of this kind. I therefore proceed now to an empirical example. We consider the following three subjunctive premises:

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[1] See the challenging paper ⟨Downing (1959).⟩. Note especially the remark on p. 126: "It seems clear, though it is perhaps impossible to prove, that two subjunctive conditionals with the same antecedent and contradictory consequents cannot both be true." If I am right, then this assumption, though intuitively persuasive, can be disproved.

(II) If Peter were now in his flat, he would be either in his bedroom or in his bathroom, and he could not be in both at the same time, and he could be in no other room (say, because the flat has no other rooms).
(IIA) If Peter were now about twenty miles from his flat, he could not possibly be in his bedroom now.
(IIB) If Peter were now about twenty miles from his flat, he could not possibly be in his bathroom now.

Moreover, we have the factual premise (fourth premise):

(II+) Peter is now about twenty miles from his flat.

We ask Allen and Brown to consider, in the light of these premises, whether or not Peter would be in his bathroom now if (*per impossibile*) he were now in his flat. Allen argues from (II), (IIA), and (II+) that, if Peter were now in his flat, he would now be in his bathroom; while Brown argues from (II), (IIB), and (II+), that if Peter were now in his flat, he could not possibly be now in his bathroom.

Again, I think that both arguments are valid.

520 | Now it is clear that our two sets of premises are both consistent, since they are both satisfiable: if we put $a = 11$, our first set of premises becomes true. Similar, the second set of premises was, in fact, true of my friend Peter M. some short time ago. Since from true premises only true conclusions can be validly derived, we are bound to admit that two opposite subjunctive conditionals, related like (i) and (ii), can be true together.

To my own mind, this fact is far from counter-intuitive. But since there are people who find it counter-intuitive that two opposite material conditionals can be true together (or perhaps that "Every Pegasus has wings" and "No Pegasus has wings" might be true together), it is to be expected that there will be people who find our result intuitively unacceptable.

Yet I feel that our intuitions are not always to be trusted in cases which simply do not arise, or which are avoided, in ordinary life, or in ordinary parlance. Perhaps the simplest case of failure arises in connection with the question: is zero a number? There is no doubt that, in ordinary parlance, it is not. "I have a number of pennies in my pocket, and the number is zero" would be eccentric in ordinary parlance. (In fact the smallest number that makes "a number of" in ordinary parlance, is three.) This example is not as trite as it looks: the extension of the idea of number from *many* to *two*, and to *one*, and ultimately to *zero*, is a feat of abstraction which was not easy to perform: we still count "one, two, three", rather than "zero, one, two, three". Yet while the question whether we should call zero a number is a comparatively unimportant terminological question, grave and startling conclusions may be drawn from the assumption (as may be drawn from any logically false assumption) that two opposite subjunctive conditionals, since they cannot be true together, cannot both follow from consistent premises. One such startling conclusion would be that Brown has succeeded in refuting Allen.

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Chapter 12
Lejewski's Axiomatization of My Theory of Deducibility (1974)

Karl R. Popper

Abstract This article is a corrected reprint of K. R. Popper (1974b). Lejewski's Axiomatization of My Theory of Deducibility. In: *The Philosophy of Karl Popper*. Ed. by P. A. Schilpp. The Library of Living Philosophers. La Salle, Illinois: Open Court, pp. 1095–1096.

Editorial note: Popper replies to Lejewski's (1974) contribution in Schilpp (1974).

I

1095 Professor Lejewski's contribution is a very generous reconstruction of a number of formal-logical (or rather, metalogical) papers which I wrote in the years from 1946 to 1948. The amount of work devoted by Lejewski to these papers is great, and his criticism is admirable: it is always fair and always fully justified; and, I suppose, only too often it is too moderate.

Professor Lejewski has done his best for me – more than I thought possible. Taking up and developing further a suggestion of Bernay's, he has proved that a system which he denotes by P is inferentially equivalent to one which he denotes by T, where the two letters stand for Popper and Tarski. This he does by (a) first eliminating certain blunders from my system, and (b) formulating both systems "in the language which we owe to Lesniewski". He thus arrives at a result which does me more than justice, I fear, for nobody will question the interest of Tarski's system. I am immensely grateful to Professor Lejewski for this result, for it shows that what I wrote (with much enthusiasm) was not a complete failure.

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II

However, much as I am indebted to Lejewski for thus saving my honour, as it were, he has not brought out, or even briefly hinted at, what my intentions were in writing these bad and ill-fated papers. By putting them in Lesniewski's language he has, in fact, done something extremely interesting, but at the same time something that
1096 goes contrary to my intentions. For my | main intention was to simplify logic by developing what has been called by others "natural deduction". I suppose that as an effort to build up a simple system of natural deduction (a commonsense logic, as it were), my papers were just a failure. However, Evert Beth told me that he was inspired partly by them to build up his own logic (his "semantic tableaux"). He told me so after I had received much discouragement, which had depressed me and had made me give up the attempt at developing my ideas further.

III

My papers were, largely, the result of an attempt to reform the teaching of elementary logic – the propositional calculus and the lower functional calculus; and I had some encouraging results, especially in quantification theory, which Lejewski for lack of space was unable to discuss. But my papers were, at the same time, inspired by the hope of solving a problem which Tarski (in "On the Concept of Logical Consequence"), a paper which I knew well in its German original[a]: a paper different from those with which Lejewski shows that my papers can be compared) indicates as insoluble; rightly, I now suspect. This was the problem of distinguishing between logical (or as I prefer to call them, "formative") signs and descriptive signs. Logical or formative signs are words like "not", "or", "and", "all", "there is at least one". Descriptive signs are "atom", "dog", "green", and so on. The task of distinguishing between the two types of signs or words seems at first sight simple; but for example the phrase "is identical with" does not obviously belong to one group or the other. Tarski showed that the concept of logical consequence can be easily elucidated (with the help of the concept of truth or of a "model") once we have decided upon a list of logical or formative signs. My idea was very simple: I suggested we take the concept "logical consequence" as primitive and try to show that those signs are logical or formative which can be defined with the help of this primitive concept. Most of my work was an attempt to carry this out. It is only fair to say that my papers did not succeed in this (as emerges from Lejewski's analysis). But I should add that Lejewski works within an elaborate system – that of Lesniewski – which demands a degree of explicitness which few other logicians live up to; indeed, which few logicians ever mastered, or even know of in these days. And also that my papers were "essays" or "attempts", trying to sketch a way; I had little hope of finalizing it.

But I should make plain that I now think that Tarski's scepticism concerning a clear

[a] Tarski (1936b).

demarcation between logical and descriptive signs is well founded. Demarcations are needed; but they are usually not sharp. This seems always to be so, interestingly enough; and it is perhaps not to be regretted.

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Chapter 13
Reviews of Popper's Articles on Logic

Wilhelm Ackermann, Evert W. Beth, Haskell B. Curry, Gisbert Hasenjaeger, John G. Kemeny, Stephen C. Kleene, John C. C. McKinsey, and Ernest Nagel

Abstract This chapter contains the contemporary reviews of Popper's articles on logic by W. Ackermann, E. W. Beth, H. B. Curry, G. Hasenjaeger, J. G. Kemeny, S. C. Kleene, J. C. C. McKinsey and E. Nagel.

Editorial notes: The English reviews appeared in Mathematical Reviews and in the Journal of Symbolic Logic. The German reviews were published in Zentralblatt für Mathematik und ihre Grenzgebiete; the English translations are by us. Original references to articles and earlier reviews are here given in $\langle\,\rangle$.

13.1 Ackermann (1948): Review of "Functional Logic without Axioms or Primitive Rules of Inference" (Popper, 1947d)

Verf. schlägt zum Aufbau der Logik einen wesentlich anderen Weg als den üblichen ein. Anstatt die logischen Grundbeziehungen wie die des Aussagenkalküls durch Axiome und Ableitungsregeln zu charakterisieren, wird gewissermaßen eine syntaktische Definition dieser Operationen gegeben. Grundlegend sind zwei undefinierte Begriffe der Metasprache, nämlich die Ableitbarkeit von b aus $a_1, \ldots, a_n$ und die Substitution einer Variablen für eine andere innerhalb eines Ausdrucks. Mit Hilfe dieser Begriffe wird nun z. B. nicht eigentlich die Konjunktion definiert, sondern gewissermaßen die logische Rolle der Konjunktion, d. h. es wird definiert, unter welchen Umständen ein Ausdruck a zu der Konjunktion von b und c in der Beziehung der gegenseitigen Ableitbarkeit steht. Entsprechende Definitionen werden für Disjunktion, Implikation, klassische und intuitionistische Negation, universale und existentielle Quantifikation, sowie auch für die modalen Verknüpfungen der Notwendigkeit, Möglichkeit usw. aufgestellt. Die Hilfsmittel, die in der Metasprache benutzt werden, um aus diesen Definitionen die notwendigen Folgerungen zu gewinnen, sind die positive Logik und die Regeln zum Gebrauch der Identität und der universalen Quantifikation.

D. Binder et al. (eds.), *The Logical Writings of Karl Popper*,
Trends in Logic 58, https://doi.org/10.1007/978-3-030-94926-6_13

The author proposes for the construction of logic an essentially different way to the usual one. Instead of characterizing the basic logical relations like those of the propositional calculus through axioms and derivation rules, in a way, a syntactic definition of those operations is given. Two undefined concepts of the metalanguage are fundamental, namely the derivability of b from $a_1, \ldots, a_n$ and the substitution of one variable for another in an expression. With the help of these concepts it is not the conjunction, but, in a sense, the logical role of conjunction that is defined, i.e. the circumstances by which an expression a stands in the relation of mutual deducibility to the conjunction of b and c. Corresponding definitions are given for disjunction, implication, classical and intuitionistic negation, universal and existential quantification, as well as for the modal connectives of necessity, possibility, etc. The means that are used in the metalanguage to obtain from these definitions the necessary conclusions are the positive logic and the rules for the use of identity and of universal quantification.

13.2 Ackermann (1949a): Review of "On the Theory of Deduction, Part I" (Popper, 1948a)

In bezug auf die hier vorliegende allgemeine Tendenz vgl. die Besprechung einer früheren Arbeit des Verf. ⟨Ackermann, 1948; dieser Band, § 13.1⟩. Für die Ableitbarkeit von b aus $a_1, \ldots, a_n$ werden hier neue Grundregeln aufgestellt und daraus die in früheren Arbeiten benutzten bewiesen. Verf. zeigt ferner, wie sich gewisse allgemeine, zur Theorie der Deduktion gehörige Begriffe wie Unverträglichkeit und vollständige Disjunktivität von n Aussagen u. a. m. in seine Theorie einordnen.

Regarding the general trend present here, cp. the review of an earlier work by the author ⟨Ackermann, 1948; this volume, § 13.1⟩. New basic rules for the deducibility of b from $a_1, \ldots, a_n$ are here postulated, and those used in the previous articles are proven from them. The author furthermore shows how certain universal concepts of the theory of deduction such as incompatibility and complete disjunctivity of n propositions, among others, fit into his theory.

13.3 Ackermann (1949b): Review of "On the Theory of Deduction, Part II" (Popper, 1948c)

Verf. setzt hier seine metalogische Untersuchung der Aussageverknüpfungen ⟨Ackermann 1948; dieser Band, § 13.1⟩ fort, indem er die klassische und die intuitionistische Negation unter diesem Gesichtspunkt betrachtet. Die intuitionistische Negation von b wird als die schwächste der Aussagen definiert, die stark genug sind, b zu widersprechen. Die klassische Negation von b wird definiert als diejenige Aussage, die zu b gleichzeitig kontradiktorisch und komplementär ist. Ausgehend von diesen Definitionen wird das Verhältnis der beiden Negationen zueinander näher beleuchtet. Interessant ist, daß die Negation im intuitionistischen Minimalkalkül von Johansson sich nicht mit den benutzten Mitteln definieren lässt.

The author continues his metalogical investigation of the propositional connectives ⟨Ackermann, 1948; this volume, § 13.1⟩ by considering classical and intuitionistic negation from this point of view. The intuitionistic negation of b is defined as the weakest proposition strong enough to contradict b. The classical negation of b is defined as the proposition which is at the same time contradictory and complementary to b. Starting from these definitions the relation of the two negations is examined more closely. It is interesting that the negation of the intuitionistic minimal calculus of Johansson cannot be defined with the means being used.

13.4 Beth (1948): Review of "New Foundations for Logic" (Popper, 1947c)

This is an exposition, by means of successive tentatives, of a theory of inference which reminds one of the systems given by G. Gentzen, S. Jaśkowski, and O. Ketonen, but is also influenced by Tarski's well-known work on the concept of logical consequence. This theory is not dependent on logical axioms; the difficulty, mentioned by Tarski, of distinguishing between formative (or logical) and descriptive (or extra-logical) signs, is removed. An inference is called valid if every interpretation which makes the premises true also makes the conclusion true; we can also say: an inference is valid if no counter-example exists. The expression "$a_1, \ldots, a_n/b$" is used in order to express the assertion: "From the statements $a_1, a_2, \ldots, a_n$, the statement b can be derived." The notion of mutual deducibility can be defined as follows: "$a//b$ if, and only if, a/b and b/a."

A number of valid rules of inference are mentioned. The system consisting of the rules "$a_1, \ldots, a_n/a_i$ $(1 \le i \le n)$" and

$$\text{“if} \left\{ \begin{array}{ll} & a_1, \ldots, a_n/b_1 \\ \text{and} & a_1, \ldots, a_n/b_2 \\ \ldots & \ldots \quad \ldots \\ \text{and} & a_1, \ldots, a_n/b_m \end{array} \right\} \text{and if } b_1, \ldots, b_m/c, \text{ then } a_1, \ldots, a_n/c\text{”}$$

as well as some other systems, is sufficient as a basis for the construction of propositional and functional logic.

The logic of compound statements is based on a number of primitive rules of derivation. Conjunction, for instance, is characterized by the rule "$a \wedge b/c$ if, and only if, $a, b/c$." It may, however, also be introduced by means of the explicit definition

"$a//b \wedge c$ if, and only if, for any $c_1 : a/c_1$ if and only if $b \wedge c/c_1$." The theory of quantification is said to be too subtle to be analysed in detail in this paper. A more systematic and more complete exposition of the system is to be given elsewhere. The paper concludes by some considerations on derivation and demonstration and on metalanguage and object language.

13.5 Curry (1948a): Review of "Functional Logic without Axioms or Primitive Rules of Inference" (Popper, 1947d)

In an earlier paper ⟨Popper, 1947c; this volume, § 13.4⟩ the author proposed a formulation for logical calculus in which the fundamental relation is one of deducibility expressed by "$a_1, \ldots, a_n/b$." This relation is similar to one postulated by P. Hertz ⟨1929b⟩ and used by Gentzen ⟨1935a; 1935b⟩ in his system *LJ*. Popper proposed in that paper two "bases" for his formulation, called basis I and basis II, respectively. Basis I is similar to that of Hertz; its rules *Rg* and *Tg* are the rules stated, in that order, in the cited review. Basis II postulates *Tg* for the case $m = 1$ only, but requires that there be an operation of conjunction, expressed by "$a \wedge b$" and a rule *Cg*: $a_1, \ldots, a_n/b \wedge c \;\leftrightarrow\; a_1, \ldots, a_n/b \;\&\; a_1, \ldots, a_n/c$.[a] His contention is that basis I can be derived from basis II. The operations formalizing the logical connectives are introduced on basis I by rules which have, in interpretation at least, the character of definitions. In the present paper these "metalinguistic inferential definitions" are restated so as to incorporate in them the rules of basis II. Of course the "definitions" are not definitions in the usual sense, and the device is at best only a technical subterfuge. But any interest it may have is destroyed by the fact that the contention above mentioned is false. Counterexample: let the $a_1, \ldots, a_n, b$ be rational integers, $b \wedge c$ be the lesser of b and c, and let "$a_1, \ldots, a_n/b$" mean that the minimum of $a_1, \ldots, a_n$ is less than or equal to $b + n - 1$; then the rules of basis II are satisfied, but *Tg* fails for $a = b_1 = b_2 = 1$, $c = 0$.

In view of this and other errors in the previous paper, conclusions based on it must be received with caution. Presumably the author's ideas can be carried through, at least in principle (the chief doubts relate to variables and quantifiers), for basis I. If the object is to simplify the rule *Tg*, Gentzen's "Hauptsatz" goes further in this direction than the author's basis II. The paper closes with a statement of possible definitions for modal functions. These require the use of a disjunction in the "metalanguage."

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[a] We have replaced $\leftrightarrows$ by $\leftrightarrow$.

13.6 Curry (1948b): Review of "Logic without Assumptions" (Popper, 1947b)

This paper discusses the philosophical background of the author's inferential formulation of the logic of propositions and predicates. ⟨See Popper, 1947c,d, 1948e = Popper, 1947e; reviews § 13.4 and § 13.5, this volume; cf. also the papers reviewed below in §§ 13.7–13.9⟩.

It contains no mathematical results; but it throws light on the motivation of the author's other papers. The major point is that logic deals with formative signs, and the latter should be definable in terms of pure deducibility.

13.7 Curry (1948c): Review of "On the Theory of Deduction, Part I" (Popper, 1948a)

In this paper the author revises the system of inferential propositional logic in his previous papers ⟨cited in the preceding review, § 13.6, this volume; Curry, 1948b⟩. The principal innovations are as follows.

(1)[b] He derives the principle *Tg* from Gentzen's elimination rule (Schnitt) without mentioning that Gentzen ⟨1932, in particular, p. 332⟩ did this. Aside from certain idiosyncrasies of the author's formulation, the resulting basis, called basis I, differs from Gentzen's [loc. cit.; or that formed by only the "Struktur-Schluss Figuren" of ⟨Gentzen, 1935a⟩, in particular, p. 192] chiefly in that Gentzen's rule of commutation [Vertauschung in loc. cit. 1934; tacit in loc. cit. 1932] is replaced by more specific assumptions.

(2) He remarks that the change in (1) removes the need for basis II, and that his "basic definitions" can be formulated on basis I. Nevertheless he claims basis II has some interest in its own right and makes some minor improvements in it; but he does not correct the error mentioned in the review of the second of his papers quoted above.

(3) He introduces Gentzen's [loc. cit. 1934] sequences with multiple consequents (along with some related notions), essentially by the definition

$$a_1, \ldots, a_m / b_1, \ldots, b_u \leftrightarrow (c)(b_1/c \;\&\; \ldots \;\&\; b_u/c \rightarrow a_1, \ldots, a_m/c).$$

Here "(c)" indicates universal quantification, "$\rightarrow$" implication and "&" conjunction on the metalinguistic level: such notions are characteristic of the author's work. The principal difference between the resulting system and that of Gentzen [loc. cit. 1934] is that the "Logische-Zeichen-Schluss Figuren" are replaced by quasi-definitions of which the following variants of essentially the same example are typical:

[b] We have replaced $\leftrightarrows$ by $\leftrightarrow$, square brackets by parentheses, | and || by / and //, as well as ⊃ by >, respectively.

$$a/b > c \leftrightarrow a, b/c,$$
$$a/\!/b > c \leftrightarrow (d)(d/a \leftrightarrow d, b/c).$$

Here "//" indicates a kind of equivalence. The system lacks the noneliminative character which leads to Gentzen's Hauptsatz. There may well be other, less obvious differences also.

13.8 Curry (1948d): Review of "On the Theory of Deduction, Part II" (Popper, 1948c)

This is an analysis of negation on the basis of the author's paper reviewed above ⟨Curry, 1948c; this volume, § 13.7⟩. The author considers 6 kinds of negation, viz., a^i, a^m, a^k, a^j, a^l, a^u, with properties as follows:

$$a \vdash b^i \leftrightarrow a, b \vdash; \qquad a^m \vdash b \leftrightarrow \ \vdash a, b; \qquad a, a^k \vdash \ \& \ \vdash a, a^k;$$
$$a, b \vdash c^j \rightarrow a, c \vdash b^j; \quad a, b^l \vdash c \rightarrow a, c^l \vdash b; \quad a, b \vdash c \rightarrow a, c^n \vdash b^n;$$

of these a^i is intuitionist negation, a^m is a form of complementation dual to a^i, a^k is classical negation, a^j is the minimal negation of ⟨Johansson, 1937⟩, a^l its dual[c], and a^u a still weaker form. Of the six, the postulates for a^i, a^m, a^k determine them uniquely if they exist (so that these postulates are quasi-definitions), while for the last three this is not the case. The author argues from this, on the basis of the philosophy in the paper of the second preceding review ⟨Curry, 1948b; this volume, § 13.6⟩, that these are not "formative signs" and therefore not strictly logical. Also if a^k exists in any language, then $a^i = a^m = a^k$; but the author uses three-valued logic as an example to show that a^i and a^m may both exist without there being an a^k. Various alternative quasi-definitions for a^k are considered; also connections with a modal notion of impossibility. At the close there are remarks concerning the existential character of assumptions in regard to negation.

13.9 Curry (1949): Review of "The Trivialization of Mathematical Logic" (Popper, 1949a)

This is a summary, without proofs or even the details of certain definitions, of the author's program for deriving the whole of mathematical logic from definitions of the "formati⟨ve⟩ signs." Parts of this program have appeared previously ⟨Popper, 1947b,c,d, 1948a,c,e = Popper, 1947e, 1948b,d; reviews § 13.4, § 13.6, § 13.5, § 13.7 and § 13.8, this volume⟩; this paper appears to go beyond the previous papers only in

[c] The negation a^l is not the dual of a^j, cp. Binder and Piecha, 2017.

certain minor points regarding identity and Aristotelian logic. The whole program is very obscure, and has not been without serious error ⟨cf. review § 13.5, this volume; Curry, 1948a⟩; likewise it was anticipated, in many respects, by the work of Gentzen and others [cf. cited reviews].

13.10 Hasenjaeger (1949): Review of "The Trivialization of Mathematical Logic" (Popper, 1949a)

Verf. kritisiert die Tragweite und die Angemessenheit der klassischen Matrizen-Methode und skizziert einen Aufbau der mathematischen Logik, der mit Gentzens Kalkülen *LJ* und *LK* ⟨Schmidt, 1935⟩ verwandt ist. Abweichungen: An Stelle des Sequenzenzeichens „→" tritt ein metalogischer Grundbegriff, der manchmal syntaktisch und manchmal semantisch interpretiert wird, ohne daß diese Interpretationen getrennt werden. Die Schlußfiguren werden als Gebrauchsdefinitionen für die logischen Verknüpfungen aufgefaßt und gewinnen dadurch den Charakter von metalogischen Existenzaxiomen, was Verf. anscheinend übersieht. Vgl. auch ⟨Ackermann, 1948, 1949a,b; dieser Band, § 13.1, § 13.2, § 13.3⟩. Inwiefern damit eine „Trivialisierung" erreicht wird, ist nicht klar ersichtlich.

The author criticizes the scope and the adequacy of the classical matrix method and sketches a construction of mathematical logic which is related to Gentzen's calculi LJ and LK ⟨Schmidt, 1935⟩. Differences are as follows: The place of the sequent sign "→" is taken by a metalogical basic concept which is sometimes interpreted syntactically and sometimes semantically, without those interpretations being separated. The derivation rules are considered to be contextual definitions ⟨"Gebrauchsdefinitionen"⟩ for the logical connectives and thereby obtain the status of metalogical existence axioms, which the author apparently overlooks. Cp. also ⟨Ackermann, 1948, 1949a,b; this volume, § 13.1, § 13.2, § 13.3⟩. It is not obvious to what extent this achieves a "trivialization".

13.11 Kemeny (1957): Review of "A Note on Tarski's Definition of Truth" (Popper, 1955a)

The author considers Tarski's concept of the fulfilment of a statement-function by a sequence. In Tarski's definition of truth ⟨Tarski, 1935a⟩ this relation is defined with respect to an infinite sequence. This is necessary to assure that there be an element in the sequence for every variable that may occur in the statement-function. Popper wishes to define fulfilment (or satisfaction) by means of finite sequences. Tarski himself suggests such a procedure, but his method of using sequences of exactly the right length leads to certain artificiality in the resulting definitions. Popper sketches a new definition in which statement-functions are fulfilled by "sufficiently long" finite sequences. It is clear that having sequences which are too long causes no difficulty; one must simply assure that an assignment is available for every free variable, and then all extensions of a sufficiently long sequence will yield the same results. The author states that his procedure is only a slight modification of Tarski's, but one that leads to an attractive reformulation of definitions. The reviewer concurs with this evaluation.

13.12 Kleene (1948): Review of "Functional Logic without Axioms or Primitive Rules of Inference" (Popper, 1947d)

The customary systems of restricted predicate calculus, as the author puts it, proceed by laying down (a) undefined primitive formative signs (e.g., "&", "⊃", "$\forall x$"), (b) unproved primitive propositions or axioms, (c) primitive rules of inference for the formative signs (e.g., *modus ponens* for "⊃"), and (d) some further very general primitive rules of inference (e.g., that the inference relation is transitive). He claims to show that, if we have at our disposal the metalinguistic notion of deducibility, then none of (a)–(d) need be laid down, and the whole formal structure of logic can be obtained by way of explicit definitions in terms of deducibility ("inferential definitions"). (In his previous papers ⟨see McKinsey, 1948; this volume, § 13.14⟩ he supposedly gets rid of (a)–(c); in the present one, of (d) also.)

It seems to this reviewer that his claim entails confusions. Instead of defining, e.g., conjunction, Popper defines the "logical force" of conjunction, by a definition of the form "$a//b \wedge c \leftrightarrow$ the definiens" where "//" expresses interdeducibility. But he substitutes "$b \wedge c$" for "a" (in obtaining 5.03 or 5.20 on ⟨p. 158⟩), where "a" is a variable which ranges over the statements of the object language ⟨p. 155⟩. Thus, to develop his definitions, we seem to require his postulate (P2) asserting the existence of a conjunctive expression, which he apparently intended only for the application of his system to some object language L ⟨p. 162⟩. This is perhaps a minor point; but the following and the point raised by the previous reviewer seem crucial.

After defining "$\vdash a$", he states that, whenever a sentence, such as "$p \supset p$", is provable in the usual systems, then his corresponding formula, in this case "$\vdash a > a$", can be obtained in his metalinguistic system, by using only his definitions. "Whenever the formula in question is intuitionistically valid, it is only necessary to use, besides Basic Definitions, the definitions of the symbols actually occuring in the formula. If it is intuitionistically invalid but classically valid, then, even if no symbol of negation occurs in the formula, D5, the definition of classical negation, is needed for establishing its demonstrability." ⟨p. 162⟩. If his definitions were "explicit," as he claims, it should make no difference whatsoever, in the case of a formula containing only ">", whether or not the definition of classical negation has been stated. Thus evidently the formal structure of logic is not characterized by his definitions simply as explicit definitions. It is fully characterized only when his definitions and his methods of "obtaining" other metalinguistic formulas from them are combined as an inductive definition (or a definition by recursions and an existential quantifier) of the notion of "obtainability" in his metalinguistic system. To do this is to employ, in establishing his system, the ordinary methods, which his system is intended to supplant.

13.13 Kleene (1949): Review of "On the Theory of Deduction, Part I & II" and "The Trivialization of Mathematical Logic" (Popper, 1948a,c, 1949a)

These papers[d] deal with the same ideas as ⟨McKinsey, 1948; this volume, § 13.14⟩ and ⟨Kleene, 1948; this volume, § 13.12⟩. As was pointed out in the first of these reviews, there seem to be at the root of Popper's investigations some new results on systems of logic of the type of Gentzen's *LJ* and *LK* ⟨Gentzen, 1935a,b⟩. These new results are apparently of some interest, e.g. those on six kinds of negation in the second of the present papers. Unfortunately it is not easy to get at exactly what the results are in these terms, because of technical errors in at least some of Popper's papers (as noted by Curry in ⟨1948a; this volume, § 13.5⟩), and because the results are entangled in Popper's claims to be giving a new kind of foundation for mathematical logic, in which the whole of it becomes obtainable from definitions of the formative signs in terms of the idea of deducibility. While it is quite unclear to the reviewer how Popper's "definitions" can be interpreted so as to sustain his claims (cf. the reviews ⟨§ 13.14 and ⟨§ 13.12, this volume⟩; Kleene, 1948; McKinsey, 1948⟩), it seems quite clear (despite Popper's uncertainty, in footnote 7 of the first of the present papers) what Gentzen is doing. Gentzen's "→" is the name of an object-linguistic symbol (or the symbol itself used autonymously). His horizontal stroke is a device to show the articulation of the sequences in a proof-figure, quite on a par with the vertical and slant lines in ⟨Hilbert and Bernays, 1934⟩ (p. 223, the lower figure).

13.14 McKinsey (1948): Review of "Logic without Assumptions" and "New Foundations for Logic" (Popper, 1947b,c)

Since these two papers are concerned with two phases of the same study, they are reviewed together. The first paper is a somewhat popular presentation of ideas which are presented in a more technically developed form in the second.

The problem dealt with, is that of defining valid deductive inference. After some preliminary discussion, the author is led to define a valid inference as one such that "every form-preserving interpretation of it whose premises are all true has a true conclusion." (As the author points out, this definition is rather similar to one previously given by Tarski.) By a "form-preserving interpretation" is here meant a

[d] Kleene cites a preprint of Popper (1949a).

translation of sentences which preserves the meaning of the logical terms (the author calls them "formative terms").

Since this definition depends on the notion of "logical terms," the author attempts to supply a precise definition of this rather vague notion. It appears to the reviewer that (aside from the purely technical contribution, in the second paper, of a new formulation, in the manner of Gentzen, of the restricted predicate calculus) this attempt at a precise definition of logical terms constitutes the chief claim to originality of the two papers. Unfortunately, the definition appears to suffer from an error; in order to make this clear, it is necessary to outline the construction given by the author.

The author begins by defining an *absolutely valid* inference. An inference is absolutely valid if every transformation of the language which always carries statements into statements, and which carries the premises into true statements, also carries the conclusion into a true statement. It is clear, though the author does not explicitly point this out, that an inference is absolutely valid if and only if the conclusion coincides with one of the premises.

The author uses the notation

$$a_1, \ldots, a_n/b$$

to indicate an inference with premises $a_1, \ldots, a_n$ and conclusion b; it is not clear, however, whether this notation is intended to indicate arbitrary inferences, or is to be restricted to absolutely valid inferences. The author writes

$$a//b$$

as an abbreviation for

$$a/b \text{ and } b/a.$$

In terms of this notation, it is asserted, the sentential connectives and quantifiers are definable. For instance, the author defines: "$a//$ the disjunction of b_1 and b_2, if, and only if, for every c_1 and c_2: $a, c_1/c_2$ if and only if $b_1, c_1/c_2$ and $b_2, c_1/c_2$."

A definition of the sort just given is called an *inferential* definition, and the class of logical signs is defined to consist of those which can be defined by inferential definitions.

Now this method of defining logical terms appears to the reviewer inadequate for the following reason. If the sign "/" in inferential definitions is taken to indicate absolute inferences in the originally defined sense (so that $a_1, \ldots, a_n/b$ holds if and only if b is identical with a_i for some i) then the inferential definitions do not give the intended meanings to the symbols introduced by them. Thus, for example, under this assumption, suppose we take, in the above inferential definition of disjunction, a, b_1 and b_2 to be all distinct, c_2 to be identical with a, and c_1 different from a, b_1, b_2 and c_2; then $a, c_1/c_2$ holds, while $b_1, c_1/c_2$ and $b_2, c_1/c_2$ are both false; hence a statement cannot be equivalent to the disjunction of two statements unless it is identical with one or the other of them! If, on the other hand, the sign "/" in inferential definitions is supposed to indicate ordinary inferences, then we seem to be involved in a circle;

for ordinary inferences have not been defined without making use of the notion of logical terms, and hence cannot reasonably be used in order to define logical terms.

13.15 Nagel (1943): Review of "Are Contradictions Embracing?" (Popper, 1943)

In a paper on the nature of mathematics ⟨Jeffreys, 1938⟩[e] Dr. Jeffreys raised the question whether a contradiction entails every proposition. Dr. Popper subsequently offered a demonstration that this was indeed the case ⟨Popper, 1940⟩. Dr. Jeffreys then expressed himself ⟨Jeffreys, 1942⟩ as dissatisfied with the proof, on the ground that it seemed to him circular. Dr. Popper now restates the argument to show that no circularity is involved, and that the conclusion can be obtained in relatively "weak" or "rudimentary" systems of material implication.

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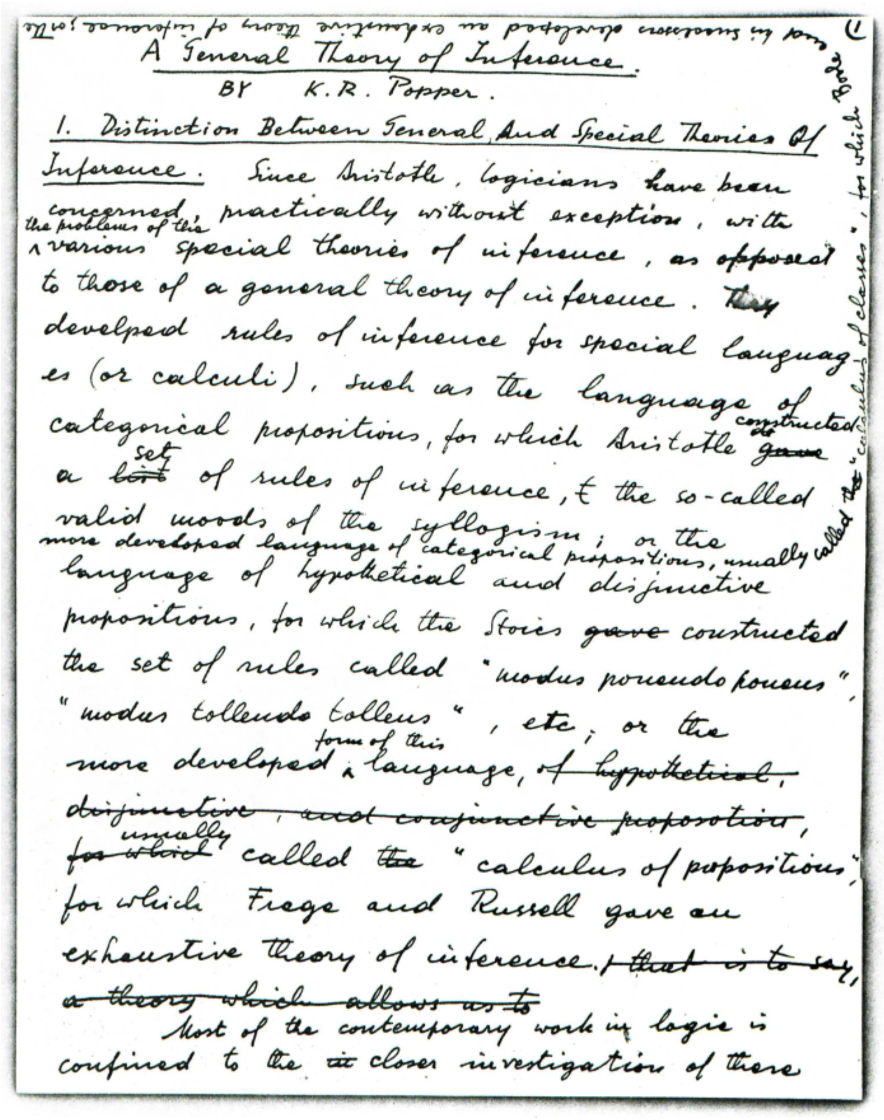
Part II
Unpublished Manuscripts



1

A General Theory of Inference.

BY K. R. Popper.

1. Distinction Between General And Special Theories Of Inference. Since Aristotle, logicians have been concerned, practically without exception, with the problems of the various special theories of inference, as opposed to those of a general theory of inference. ~~They~~ developed rules of inference for special languages (or calculi), such as the language of categorical propositions, for which Aristotle constructed ~~gave~~ a set ~~lot~~ of rules of inference, & the so-called valid moods of the syllogism; or the more developed language of categorical propositions, usually called the "calculus of classes", for which Boole and his successors developed an exhaustive theory of inference; or the language of hypothetical and disjunctive propositions, for which the Stoics ~~gave~~ constructed the set of rules called "modus ponendo ponens", "modus tollendo tollens", etc; or the more developed form of this language, ~~of hypothetical, disjunctive, and conjunctive propositions~~, ~~for which~~ usually called ~~the~~ "calculus of propositions", for which Frege and Russell gave an exhaustive theory of inference. ~~, that is to say, a theory which allows us to~~

Most of the contemporary work in logic is confined to the ~~ex~~ closer investigation of these

First page of Karl Popper's manuscript of "A General Theory of Inference".

Chapter 14
On Systems of Rules of Inference

Paul Bernays and Karl R. Popper

Abstract This is an unpublished joint work by Paul Bernays and Karl Popper.

Editorial notes: The source typescript can be found in several copies in Popper's Nachlass, namely in KPS Box 13, Folder 5 ("Logic Derivations"), KPS Box 14, Folder 8 ("Logic of Negations") and KPS Box 36, Folder 13 ("Bernays"). The copy in the latter has handwritten corrections, and we have used it as a basis for the version presented here. Some corrections, e.g., in the middle of p. 10, may not be Popper's but could be by Bernays; however, we are not sure about this. Some rules are marked with handwritten question marks, which we have omitted here. The typescripts contain footnote marks, but the footnotes are missing. Footnote mark 1 seems to be missing in the original; the paper is partly damaged. We indicate each missing footnote by "⟨Footnote missing⟩". The original typescript has neither a date nor a title, but Popper suggests in a letter to Bernays of 3 March 1947 (this volume, § 21.3) to call it "On Systems of Rules of Inference". We are not sure whether "On Systems of Rules of Inference" was written before, after or at the same time as Popper's unpublished manuscript "A General Theory of Inference" (this volume, Chapter 15). The typescript uses the combined symbols $\not-$ and $\overline{\not-}$, which we replaced by $\vdash$ and $\overline{/}$, respectively. We write $a_1, \ldots, a_n$ (with two commas) instead of $a_1, \ldots a_n$ throughout. In our transcription we use $\bigvee$ and $\vee$ instead of "V" and "v" for metalinguistic and objectlinguistic disjunction, respectively. The page numbers in the left margins refer to the original typescript. The term "minimum calculus" refers to Johansson's (1937) "Minimalkalkül". Rule (5.2X) is Peirce's rule.

1. Notation

1 The individuals which constitute the universe of discourse of this investigation are statements of some object language or other. Since our investigation proceeds entirely in the metalanguage (or meta-metalanguage, etc.), and no signs of any object language are to be quoted, we can dispense with gothic letters; and we use lower case italics from the beginning of the alphabet (as a rule with numerical subscripts) as individual variables.

The metalinguistic formula

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Trends in Logic 58, https://doi.org/10.1007/978-3-030-94926-6_14

(1) $$a_1, \ldots, a_n / a_0$$

is used to express the assertion that the statement a_0 is a conclusion which can be validly inferred from the premises $a_1, a_2, \ldots, a_n$. A formula like (1) is called a "rule of inference".

We shall use, in the metalanguage (but not in the meta-metalanguage, etc.), merely for purposes of abbreviation, the notation of Hilbert and Bernays' functional calculus, with the exception that we use "↔" (instead of "∼") as an abbreviation of the phrase "if and only if".

2. General Theory. Primitive Rules

It is usually assumed that inference is transitive; reflexive; that (since their order is irrelevant) a reversion of the order of the premises does not invalidate it; and that the same holds if we add some new premise to the old ones. We can express this in the
2 following *System of Primitive Rules*, for which we claim independence: |

First System (four rules):

(2.11)
$$((i)1 \leq i \leq m \rightarrow a_1, \ldots, a_n / b_i) \rightarrow (b_1, \ldots, b_m / c \rightarrow a_1, \ldots, a_n / c)$$

(2.12) a/a

(2.13) $a_1, \ldots, a_n / a_0 \rightarrow a_n, \ldots, a_1 / a_0$

(2.14) $a_1, \ldots, a_n / a_0 \rightarrow a_1, \ldots, a_{n+1} / a_0$

For the proof of independence, the following numerical models can be used: 2.11: interpret (1): "$a_0 \leq a_1 + 1 \bigvee a_0 \leq a_2 + 1 \ldots$"; put $a_1 = a_2 = \ldots = a_n$; $b_1 = b_2 = \ldots = b_m = a_1 + 1$; $c = a_1 + 2$.[a] – 2.12: interpret (1): "$a_0 > a_1 \bigvee a_0 > a_2 \ldots$". – 2.13: interpret (1): "$a_0 = a_1$"; put $a_0 = a_1 \neq a_n$. – 2.14: interpret (1): "$a_0 = a_1 \bigvee a_0 = a_n$"; put $a_0 = a_n \neq a_{n+1} = a_1$. –

(Note: The system remains independent if we replace 2.12 by the stronger formula 2.22; use the same interpretation, and put $a_1 = a_2 = \ldots = a_n > a_0$.)

The system of primitive rules is equivalent to the following two systems:

Second System (three rules):

(2.21) the same as 2.11

(2.22) $a_1, \ldots, a_n / a_1$

(2.23) $a_1, \ldots, a_{n+m} / a_0 \rightarrow a_n, \ldots, a_1, a_{n+1}, \ldots, a_{n+m} / a_0 \qquad (0 \leq m < n + m)$

(Rule 2.23 allows us to arrange the premises into any order we like.)

Third System (two rules):

[a] We have corrected b_n to b_m.

(2.31) the same as 2.11

(2.32) $a_1, \ldots, a_n / a_i \qquad (1 \leq i \leq n)$

An alternative way of writing 2.32 is:

(2.321) $a_n, \ldots, a_1, a_{n+1}, \ldots, a_{n+m} / a_1 \qquad (0 \leq m < n + m)$

3 | The independence of these systems can be shown by using the corresponding interpretations given for the first system; besides, the interpretation given for 2.14 can be used for 2.23 and 2.32, and that given for 2.13 for 2.32. – To show that 2.22 cannot be replaced by 2.12, interpret (1): "$a_0 = a_1 = a_2 = \ldots = a_n$"; put the last premise $\neq a_0$.

The proof of the equivalence of the three sets is facilitated by first deriving the following rules 2.4 and 2.5 from the first system:

(2.4) $$a_1, \ldots, a_n / a_0 \rightarrow a_1, \ldots, a_{n+m} / a_0$$

Proof from 2.14, by induction with respect to m. (We show first that, by 2.14, if 2.4 holds for some m, it also holds for $m + 1$; and we show next that, by 2.14, it holds for $m = 1$.)

(2.5) $$a_1, \ldots, a_n / a_0 \rightarrow a_n, \ldots, a_1, a_{n+1}, \ldots, a_{n+m} / a_0$$

Proof from 2.13 and 2.4, $a_i/\overline{a_{n-i}}$ $(1 \leq i \leq n)$. ("$a_i/\overline{a_{n-i}}$" means: "substitute (in 2.4) a_{n-i} for (all the occurrences of) a_i.")

We can now easily show the equivalence of the three sets by showing that the first (i) and the second (ii) are equivalent to the third (iii):

(i)/(iii) We first show that 2.22 can be derived in (i) (and that it is therefore equivalent to 2.12 in (i), since 2.12 from 2.22 by $n = 1$): 2.22 from 2.4, $n = 1$, $n + m/\overline{n}$, $a_0/\overline{a_1}$, and 2.12. – Now 2.321 from 2.5, $a_0/\overline{a_1}$, and 2.22.

(iii)/(i) 2.12 from 2.321, $n = 1$; $m = 0$. – 2.13 from 2.31, $m = n$, $b_i/\overline{a_i}$; $a_i/\overline{a_{n-i}}$; $c/\overline{a_0}$; //3.12. ("//. . ." means: "apply to foregoing *modus ponens* with . . . as second premise.") – 2.14 from 2.31, $m/\overline{n}$; $n/\overline{n+1}$; $b_i/\overline{a_i}$; //2.32, $n/\overline{m+1}$.

(ii)/(iii) 2.321 from 2.23, $a_0/\overline{a_1}$; //2.22, $n/\overline{n+m}$.

(iii)/(ii) 2.22 from 2.32, $a_i/\overline{a_1}$. – 3.23 from 3.31, $m/\overline{n+m}$; $n/\overline{n+m}$; $b_i/\overline{a_i}$; and for $1 \leq i \leq m$, $a_i/\overline{a_{m-i}}$; //2.32, $n/\overline{n+m}$. –

This completes the proof of equivalence.

4 Some further rules may be deduced: |

(Omitting redundant premises:)

(2.6) $$a_n = a_{n+1} \rightarrow (a_1, \ldots, a_{n+1} / a_0 \rightarrow a_1, \ldots, a_n / a_0)$$

Proof: from 2.11 and 2.32.

A more general form of 2.6 is:

(2.7)
$$(n+1 \leq i \leq n+m \rightarrow a_1, \dots, a_n/a_i) \rightarrow (a_1, \dots, a_{n+m}/a_0 \rightarrow a_1, \dots, a_n/a_0)$$

Proof: as above.

(Contraction:)

(2.8) $$(a_1, \dots, a_n/b_0 \;\&\; b_0, b_1, \dots, b_m/c) \rightarrow a_1, \dots, a_n, b_1, \dots, b_m/c.$$

Proof: as above.

The rule of transitivity 2.11, gives rise to several important rules for transforming conditional rules of inference:

(Weakening the premises of the antecedent:)[b]

(2.91) $$(a_1, \dots, a_n/a_0 \rightarrow b_1, \dots, b_m/b_0) \rightarrow ((1 \leq i \leq r \rightarrow a_1, \dots, a_n/c_i) \\ \rightarrow (c_1, \dots, c_r/a_0 \rightarrow b_1, \dots, b_m/b_0))$$

Proof: 2.11, $m/\overline{r}$; $b_i/\overline{c_i}$; $c/\overline{a_0}$; transitivity of "$\rightarrow$".

(Strengthening the premises of the consequent:)

(2.92) $$(a_1, \dots, a_n/a_0 \rightarrow b_1, \dots, b_m/b_0) \rightarrow ((1 \leq i \leq m \rightarrow c_1, \dots, c_r/b_i) \\ \rightarrow (a_1, \dots, a_n/a_0 \rightarrow c_1, \dots, c_r/b_0))$$

Proof: 2.11, $n/\overline{r}$; $a_i/\overline{c_i}$; $c/\overline{b_0}$; transitivity of "$\rightarrow$".

(Strengthening the conclusion of the antecedent:)

(2.93) $$(a_1, \dots, a_n/a_0 \rightarrow b_1, \dots, b_m/b_0) \\ \rightarrow (c/a_0 \rightarrow (a_1, \dots, a_n/c \rightarrow b_1, \dots, b_m/b_0))$$

Proof: 2.11, $m = 1$; $b_1 = b_m/\overline{c}$; $c/\overline{a_0}$; change order of antecedents; transitivity of "$\rightarrow$".

(Weakening the conclusion of the consequent:)

(2.94) $$(a_1, \dots, a_n/a_0 \rightarrow b_1, \dots, b_m/b_0) \\ \rightarrow (b_0/c \rightarrow (a_1, \dots, a_n/a_0 \rightarrow b_1, \dots, b_m/c))$$

Proof: $\langle 2.11 \rangle$, $n/\overline{m}$; $m = 1$; $a_i/\overline{b_i}$; $b_1 = b_m/\overline{c}$; transitivity of "$\rightarrow$" (in the form "$(B_C \rightarrow (C \rightarrow B_C)) \rightarrow ((A \rightarrow B_C) \rightarrow (C \rightarrow (A \rightarrow B_C)))$").

[b] (2.91) and (2.92) should have an (i) before $1 \leq i \leq r$ and $1 \leq i \leq m$, respectively, as in 2.11.

3. Relation between this Method and that of Consequence Classes

We can restate the two rules which constitute our third system in the following way:

(3.1) Provided all the elements of the class of premises X are derivable from the class of premises Y, any conclusion c which is derivable from X is also derivable from Y.

(3.2) Any sentence a_i that belongs to the class of premises X also belongs to the conclusions which are derivable from the class of premises X.

We now use the ordinary notation of the class calculus, and, furthermore, we write "$C(X)$" for "the class of conclusions derivable from the class of premises X". (We shall call $C(X)$ "the consequence class of X" or "the content of X".) Then we can rewrite 3.1 and 3.2:

$$X \subset C(Y) \rightarrow C(X) \subset C(Y) \tag{3.1+}$$

$$X \subset C(X) \tag{3.2+}$$

The first of these formulae, in the presence of the second, is equivalent to the following two formulae:

$$X = C(Y) \rightarrow C(X) \subset C(Y) \tag{3.11}$$

$$X \subset Y \rightarrow C(X) \subset C(Y) \tag{3.12}$$

5 | Proof: 3.11 from 3.1+ by strengthening antecedent. – 3.12 from the trivial formula "$Y \subset C(Y) \rightarrow (X \subset Y \rightarrow X \subset C(Y))$", //3.2+, $X/\overline{Y}$; combine result with 3.1+. – *Vice versa*, 3.1+ from 3.12, $Y/\overline{C(Y)}$, together with the formula "$C(C(Y)) \subset C(Y)$", obtainable from 3.11, $X/\overline{C(Y)}$.

The two formulae 3.11 and 3.12, in turn, are equivalent, respectively, to the following two formulae:

$$C(C(X)) \subset C(X) \tag{3.11+}$$

$$\sum_{X_i \subset Y} C(X_i) = C(Y) \tag{3.12+}$$

Proof: 3.11+ from 3.11, $X/\overline{C(Y)}$, afterwards $Y/\overline{X}$. – 3.12+ with "$\subset$" follows immediately from 3.12; the equality follows from the fact than one of the X_i will be equal to Y. – *Vice versa*, 3.12 from 3.12+ with the help of the trivial formula "$C(X_i) \subset \sum C(X_i)$". – 3.11 from 3.11+, $X/\overline{Y}$, together with the trivial formula "$X = Z \rightarrow C(X) = C(Z)$", $Z/\overline{C(Y)}$. –

The system consisting of the last two formulae and 3.2+ can now be readily compared with the system of five axioms given by Tarski[2] in 1930[c] as the basis of his

[2] ⟨Footnote missing⟩

[c] Cf. Tarski (1930b).

theory of metamathematics. Tarski uses two fundamental concepts, "S", designing the class of all statements (of the objectlanguage under consideration), and "$C(X)$", designating the consequence class of X. His five axioms are, in our notation, ("$\overline{\overline{X}}$" designates the cardinal number of X):

Axiom 1. $$\overline{\overline{S}} = \aleph_0$$

Axiom 2. $$X \subset S \rightarrow X \subset C(X) \subset S$$

Axiom 3. $$X \subset S \rightarrow C(C(X)) = C(X)$$

Axiom 4. $$X \subset S \rightarrow C(X) = \sum_{Y_i \subset X} C(Y_i) \qquad (\overline{\overline{Y_i}} < \aleph_0)$$

Axiom 5. $$(Ea)(a \in S \;\&\; C(\{a\}) = S).$$

Tarski's explicit references to S can be omitted, from our point of view, since we have stated that the elements of our universe of discourse are the statements of some
6 object language. | Similarly, the condition, in Tarski's axiom 4, that $\overline{\overline{Y_i}} < \aleph_0$[d] (i.e. that the premise classes Y_i must be finite) is implicitly realized by our way of writing the premise classes "$a_1, \ldots, a_n$", etc. If we, therefore, omit Tarski's references to S and to finitude (and replace "=" in Tarski's axiom 3 by "$\subset$", which does not change his system in view of Tarski's axiom 2), then we find that Tarski's axiom 1 disappears, that his 2 becomes our 3.2+, his 3 our 3.11+, and his 4 our 3.12+. In other words, each of our three systems in section 2 can be considered as equivalent with the system of the first four of Tarski's axioms.

Concerning his axiom 5, this is not generally valid for all object languages; especially if we consider object languages which either do not contain negation or conjunction (or neither of these), then axiom 5 will not, in general, be satisfied. It has, admittedly, the advantage of excluding certain undesirable interpretations of the system, for example the trivial interpretation which makes "$C(X)$" another way of writing "X". But in order to exclude this interpretation (except in case that S is so chosen that, for all X and Y, $X \subset Y$ or $Y \subset X$), it is sufficient to add the following axiom to Tarski's set (we omit the hypothesis "$X \subset S \;\&\; Y \subset S \rightarrow$"):

(3.3) $$C(X + Y) \subset C(X) + C(Y) \rightarrow X \subset C(Y) \vee Y \subset C(X)$$

This is independent of the others. (Interpret $C(X)$ as the class of all numbers from the smallest to the greatest number in the class X, as long as the latter number does not exceed 3; otherwise, as the class of all numbers up to and including the greatest number in X; and put $X = \{1, 2\}$, $Y = \{2, 3\}$; then 3.3 is not satisfied, but Tarski's axioms are, including axiom 5, if we put $4 \leq a = \overline{\overline{S}} \leq \aleph_0$.) It imposes a limitation on the object language, but it does not appear to narrow the range of admissible object languages quite as much as axiom 5. (It postulates that something resembling conjunction must exist in the object language provided it possesses sentences which

[d] We have corrected $Y_i < \aleph_0$ to $\overline{\overline{Y_i}} < \aleph_0$.

7 are neither provable nor refutable; but it does not, | as does axiom 5, postulate that something like negation exists in the object language.)

There are, of course, other undesirable interpretations which we might wish to exclude, especially an interpretation which identifies $C(X)$ with S; this is not excluded by Tarski's set, but may be excluded by adopting, for example

$$(3.4) \qquad (Ea)(Eb)(a \notin C(\{b\}))$$

which does not narrow the field of admissible object languages, or

$$(3.5) \qquad (Ea)(Eb)(a \notin C(\{b\}) \mathbin{\&} b \notin C(\{a\}))$$

which amounts to postulating that the object language should contain at least two different statements which are both neither consequences of the zero class of premises nor "comprehensive"[3] or "embracing"[4] – for example, some factual sentence and its negation.

The possibility that these two sentences are, in this way, contradictory to each other, may be easily excluded by replacing 3.5 by

$$(3.51) \qquad (Ea)(Eb)(Ec)(a \notin C(\{b\}) \mathbin{\&} b \notin C(\{a\}) \mathbin{\&} c \notin C(\{a, b\})).$$

It is easy to devise existential formulae postulating the existence of any number (even of an infinite number) of mutually independent statements, excluding at the same time undesirable interpretations of $C(X)$.

In considering more specified languages, in the next section, we shall assume something similar to Tarski's axiom 5 (see 4.0f and 4.0$\overline{\diagup}$), but together with its dual assumption (see 4.0 t and 4.0$\vdash$). The function of these assumptions will be, explicitly, to help us to characterize negation.

4. Primitive Rules for Propositional Logic

Returning to the method of treating inference introduced in sections 1 and 2, we now propose to consider object languages which contain, with *every* sentence a, and with *every* pair of sentences a and b, also one or the other or all of the following sentences c_1 to c_6: one sentence $c_1 = \neg(a)$; one sentence $c_2 = \supset(a, b)$; one sentence $c_3 = \equiv(a, b)$; one sentence $c_4 = .(a, b)$; and one sentence $c_5 = \vee(a, b)$. (In other words, we assume that certain functions of one or two variables can be defined in our universe of discourse, so that the values of the functions as well as of their variables are statements.) We shall, as usual, write, in order to save brackets, "$\neg a$"; "$a \supset b$"; "$a \equiv b$"; "$a \,.\, b$"; and "$a \vee b$"; and we shall indicate the structure of more complex compounds in the usual way by brackets. Also, we shall call $\neg a$; $a \supset b$; $a \equiv b$; etc.,

[3] ⟨Footnote missing⟩

[4] ⟨Footnote missing⟩

"the negation of a"; "the conditional of a and b"; "the biconditional of a and b", etc.; and we shall call all these statements "compounds", and we shall say "a is a component of the compound $\neg a$", etc.

We propose to characterize the logical properties of compound statements by the method of laying down, *for each separately*, primitive rules of inference. (Each of these rules determines the deductive power, as it were, of the compound that occurs in it.)

Tarski, in his paper quoted above, has adopted a procedure analogous to that we propose to adopt, by laying down, in his axiom 6^* that S contains with every element a an element $\neg(a)$, and with every pair of elements a, b, an element $\supset(a, b)$, and he characterises the properties of these compounds by way of his axioms 7^* and 8^* (conditional) and 9^* and 10^* (negation). His axioms for the conditional can be transcribed into our notation without difficulty (see the rule 4.2 below). His axioms for negation, however, make essential use of S, and of $C(0)$, i.e. the consequence class of the zero class of premises. They amount to postulating (9^*) that, from the two premises a and $\neg a$, every element of S can be deduced; and (10^*) that, if b can be deduced from a as well as from $\neg a$, then b follows from the zero class of premises
9 (and therefore from any class of premises). If we assume | that S contains at least one true and at least one false statement, then we may say that the force of (9^*) is that, from the premises a, $\neg a$, at least one false statement follows, and the force of (10^*) that, if b follows from a and from $\neg a$, then b follows from a true statement. One method of transcribing this into our way of writing is this: we assume that our metalanguage contains, apart from variables whose values are statements, also two constant names of statements, say "t" and "f", and that the statement t is logically true and the statement f logically false; and by this we mean that the following rules of inference hold for them:

(4.0 t) $$a/t$$

(4.0f) $$f/a$$

i.e., t follows from every statement, and from f, every statement follows.

We shall adopt this method in the present section (the next section will proceed differently), and transcribe Tarski's two axioms for negation:

(4.02) $$a, \neg a/f$$

(4.11) $$a/b \ \& \ \neg a/b \rightarrow t/b$$

In order to achieve symmetry, we replace 4.02 by

(4.12) $$a/b \ \& \ a/\neg b \rightarrow a/f$$

Using fundamentally the same method, we can formulate 4.11 and 4.12 also in the following way: we can write "$\vdash b$" instead of "t/b" ("$\vdash b$" may be read: "b is
10 logically true" or "b is proved" or "b is demonstrable"); and | we can write "$\dashv a$" instead of "a/f" ("$\dashv a$" may be read "a is logically false", or "a is refutable"). We

can introduce these two new signs either by an explicit definition, using "*t*" and "*f*", or by the following rules (which use bound variables) by which we may replace 4.0 t and 4.0 f:

(Demonstration and Refutation:)

(4.0 $\vdash$) $$\vdash a \leftrightarrow (b)\, b/a$$

(4.0 $\urcorner$) $$\urcorner\, a \leftrightarrow (b)\, a/b$$

(These rules have the intended force only if we assume that the language in question contains at least one true and one false statement.) Using the new symbols, 4.11 and 4.12 can be replaced by the following primitive rules of inference:

(Negation:)

(4.11⁺) $$a/b \;\&\; \neg a/b \rightarrow\; \vdash b$$

(4.12⁺) $$a/b \;\&\; a/\neg b \rightarrow\; \urcorner\, a$$

Proceeding to the rules for conditional compounds, we can transcribe Tarski's axioms (8*) and (7*) into two conditional rules of inference of which the one is the converse of the other, and contract them afterwards into one equivalence, viz. into the following primitive rule of inference:

(Conditional:)

(4.2) $$a_1, \ldots, a_n/b_1 \supset b_2 \;\leftrightarrow\; a_1, \ldots, a_n, b_1/b_2$$

In a precisely analogous way, we now can lay down primitive rules of inference for the other compounds of two components:

(Bi-Conditional:)

(4.3) $$a_1, \ldots, a_n/b_1 \equiv b_2 \;\leftrightarrow\; a_1, \ldots, a_n, b_1/b_2 \;\&\; a_1, \ldots, a_n, b_2/b_1$$

(Conjunction:)

(4.4) $$a_1, \ldots, a_n, b_1 \,.\, b_2/c \;\leftrightarrow\; a_1, \ldots, a_n, b_1, b_2/c$$

(Disjunction:)

(4.5) $$a_1, \ldots, a_n, b_1 \vee b_2/c \;\leftrightarrow\; a_1, \ldots, a_n, b_1/c \;\&\; a_1, \ldots, a_n, b_2/c$$

Rules 4.0 $\vdash$, 4.0 $\urcorner$, 4.11⁺, 4.12⁺ (or, alternatively, rules 4.0 t, 4.0 f, 4.11, 4.12)
11 together with the rules 4.2 to 4.5, and | any of the three sets of section 1, constitute a complete system of propositional logic, in the sense that (a) any derivation made possible by the usual systems of propositional logic can be carried out in this system, and that (b) for any tautology of propositional logic, we can derive, in our system, a corresponding schema preceded by "$\vdash$". We shall not, however, prove this assertion

here, since it follows in a more direct way from the considerations of the next two sections. It will be proved in section 7.

5. Pure Rules of Derivation

The system of propositional logic presented in section 4 can be considerably simplified. Such a simplified system will be given in the next section. At the same time, the system can be "purified", in the sense that all those rules are avoided whose formulations make use of "t", "f", "$\vdash$", "$\overline{\diagup}$", or of bound variables, or of rules whose converse is invalid if we do not use bound variables, etc. In the present chapter, we shall briefly explain what we mean by such a "purified" system, or by *a system whose primitive rules are all "pure rules of derivation"*.

Propositional logic (and functional logic) is usually constructed as a system of logically true statements of the object language – in the case of propositional logic often called "tautologies". This system can be characterized, either deductively, by specifying certain statements as primitive propositions or axioms, and by laying down some rule of inference – usually the *modus ponens*. Or the system can be characterized by specifying some decision method, such as truth tables, matrices, etc. which allow us to decide, of any given statement, whether or not it is a tautology. Now one of the main interests of this system of tautologies is that it provides us with a system of rules of derivation. Every conditional tautology allows us, with the help of the *modus ponens*, to derive, from its antecedent, its consequent. Thus, if the *modus ponens* is assumed, we can say that, with every conditional tautology of the form "$A \supset B$", we are given a corresponding rule of inference of the form "A/B". It is mainly in this
12 | way that the tautologies become useful.

Considering this situation it appears a legitimate and even an interesting aim to determine the system of all valid derivation rules of propositional logic in a more direct way, by simply laying down a few primitive rules of derivation, either in addition to the modus ponens or supplanting it, but in any case without recourse to the detour via the logically true or tautologous conditionals. In this way, our logic would become homogenous. If, on the other hand, logically true statements are used, as it is customary at present, then a dualism is unavoidable: statements as well as rules of derivations will have to be used as primitives. A monism of axioms or tautologies would, of course, be insufficient: without the *modus ponens* it would not allow us to make one single derivation. But a purely derivational logic can be monistic. It can ignore the logically true statements; but it allows us, after we have built up an independent pure logic of derivation, to introduce later the logic of demonstration, or of the logically true statements, by simply adding a definition of demonstrability such as 4.0 $\vdash$.

Now it is clear that, if we wish to construct a system which does not introduce as primitive any axioms or axiom schemata but exclusively rules of inference of the character of the modus ponens and the transitivity and reflexivity principles (2.11 and 2.12), then we must clearly state the requirements of a pure rule of derivation.

For otherwise we might easily introduce axioms or axiom schemata in the form of rules of derivation. For example,

(5.001) $$a/b \supset b$$

is surely a rule of derivation, but it is obviously equivalent to

(5.002) $$\vdash b \supset b$$

which asserts that "$b \supset b$" is logically true, and which, if used as a primitive, amounts
13 to laying down an axiom scheme. We thus must | lay down a requirement which excludes not only the use of "t", "$\vdash$", etc., but also rules which assert that statements of a certain form follow from any premise whatsoever, or that they follow from the zero class of premises, etc. For all these are only so many different devices for laying down axioms or axiom schemata.

The requirement we need can be formulated with the help of the following definition:

A rule of inference is called "pure" if and only if we can construct, in some object language L containing non-tautological statements (i.e. statements which are neither tautologically true nor tautologically false), an example which satisfies the rule (in a non-vacuous way if the rule is conditional), and which, moreover, satisfies the requirement that all statements of the example, whether compounds or components, are non-logical.

According to this definition, not only 5.002 but also 5.001 are excluded from the pure rules of derivation, since all statements of the form "$b \supset b$" are logically true; similarly, not only 4.11^+ and 4.12^+ are excluded, but also

(5.011) $$a/b \;\&\; \neg a/b \rightarrow c/b$$

(5.012) $$a/b \;\&\; a/\neg b \rightarrow a/c$$

since these demand that b (in 5.011) and a (in 5.012) are, respectively, logically true and false.

Rules such as 5.001 or 5.01 and 5.02 will be called "mixed rules of derivation". They must not be excluded from our system altogether if it is to be complete in the desired sense. But we can build up a very considerable part of our system (in fact, all rules of derivations which allow us to proceed from non-logical premises to non-logical conclusions) without the use of mixed rules. A system (or a part of a system) which is free of mixed rules we call a "pure logic of derivation"; and a system whose *set of primitives* constitute a pure logic of derivation (but which may contain mixed rules of derivation among its derived rules, but only among these) we call "purely derivational".

14 | The fact that in a purely derivational logic, if it is complete in the desired sense, mixed rules such as 5.01 and 5.02 will be forthcoming, may, at first sight, suggest that such a system is not interesting since it is not really homogeneous. But the interest of the system consists partly just in the fact that mixed rules – i.e. rules which cannot be satisfied by examples that use *only* non-logical statements – are shown to be the

unavoidable limiting cases of pure rules. It is shown, in this way, that certain famous limiting cases such as 4.0 t or 4.0 f, or 5.001; 5.011, 5.012, or

$$a/b \vee \neg b \tag{5.0111}$$

$$a\,.\,\neg a/b \tag{5.0121}$$

are not of the character of conventions – of rules that extend the range of intuitive logic to limiting cases not covered by ordinary language – but that they are indeed unavoidable consequences of any "ordinary logic" if only it covers those rules of inference which are needed, and intuitively used, for the transition from ordinary non-logical premises to ordinary non-logical conclusions. –

By adding a definition of demonstrability such as 4.0 $\vdash$, we add, to our purely derivational logic, the logic of demonstration. Rules such as 5.002, or

$$\vdash a \vee \neg a \tag{5.013}$$

and conditional rules whose antecedent *and* consequent have the form of 5.013, we can call "pure rules of demonstration"; while conditional rules whose antecedent is a rule of derivation while the consequent is a rule of demonstration (or *vice versa*) may be described as "mixed rules of derivation and demonstration". All these may occur in a logic of demonstration, but not in that self-contained part which has been characterized here as purely derivational.

If we now proceed to purify the system of rules presented in section 4, then we find that the technical problems involved are negligible. All we have to do is to replace
15 the mixed rules of | derivation and demonstration, i.e., all the rules preceding 4.2, by the following primitive rule:

(Negation:)

$$a_1, \ldots, a_n, \neg b_1/\neg b_2 \rightarrow a_1, \ldots, a_n, b_2/b_1 \tag{5.1}$$

This rule may be replaced by several sets, such as the two rules

$$a_1, \ldots, a_n, b_1/\neg b_2 \rightarrow a_1, \ldots, a_n, b_2/\neg b_1 \tag{5.11}$$

$$a_1, \ldots, a_n, \neg b_1/b_2 \rightarrow a_1, \ldots, a_n, \neg b_2/b_1. \tag{5.12}$$

The first of these may in turn be replaced by the two rules:

$$a_1, \ldots, a_n, b_1/b_2 \rightarrow a_1, \ldots, a_n, \neg b_2/\neg b_1 \tag{5.111}$$

$$a/\neg\neg a \tag{5.112}$$

In a similar way, rule 5.12 is equivalent, in the presence of 5.111, either to the one unconditional rule

$$\neg\neg a/a \tag{5.120}$$

or to the two rules

(5.121) $$\neg a, \neg\neg a / a$$

(5.122) $$a_1, \ldots, a_n, \neg\neg b_1, \neg\neg b_2 / b_1 \rightarrow a_1, \ldots, a_n, \neg\neg b_1, \neg b_2 / b_1$$

(For proofs of the claimed equivalences see the correspondingly numbered rules of section 6.)

If we omit 5.122 (but retain the rest, i.e. 5.121, and 5.11 or its equivalents), then our system coincides with the “Intuitionistic Calculus” of Heyting. If we further omit 5.121, then the rest, i.e. 5.11 (or its equivalents) becomes equivalent to Johansson’s “Minimum Calculus”.

It may be remarked that, in the presence of 4.2 and the Intuitionistic Rules of Negation (i.e., the rules of negation without 5.122), rule 5.122 is equivalent to

(5.2X) $$a_1, \ldots, a_n, b_1 \supset b_2 / b_1 \rightarrow a_1, \ldots, a_n / b_1$$

16 | In other words, if either 5.122 or 5.2X are added to the Intuitionistic Calculus, then we obtain the complete (classical) system. Rule 5.2X is interesting for a different reason: if 5.2X is added to 4.2, then all those valid rules of inference which employ no compound except the conditional can be obtained with the help of the rules of section 2, while 4.2 alone gives, with the rules of section 2, only those rules of inference which correspond to that subclass of the tautological conditionals which Hilbert and Bernays call “positive identical”. (See 6.72 and 6.720, and section 7, below.)

If 5.2X is added to 4.2, and the rules of negation in section 4 replaced by 5.1 or its equivalents, then the following holds for the so amended system of rules of section 4: every valid rule of inference R which can be expressed in our system can be obtained in it by employing, in addition to the rules of section 2, merely those primitive rules which refer to the compounds occurring in R. Thus, if only conditionals occur in R, rules 4.2 and 5.2X suffice; if negation occurs, 5.1 or its equivalent are sufficient; etc.

In order to make each of the primitive rules (or group of rules which introduce *one* of the various compounds) complete in this sense, we have to make the system redundant; for 5.2X can be obtained from the rules for negation and 4.2.

If, on the other hand, we are not interested in this kind of completeness of every group of rules, then the whole system can be considerably simplified.

6. Simplified Systems

The purely derivational system of propositional logic discussed in the last section can be simplified by limiting the number n of the premises $a_1, \ldots, a_n$ which play a part in the various (conditional) primitive rules of inference. We obtain in this way the following complete system:[e]

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[e] *Handwritten note at the bottom of the page:*
Alternative Denial $a / b \curlywedge c, d \;\leftrightarrow\; a, b, c / d$
Joint Denial $a \downarrow b, d / c \;\leftrightarrow\; d / a, b, c$

(Negation:)

17 (6.1) $$a, \neg b / \neg c \rightarrow a, c / b$$ |

(Conditional:)

(6.2) $$a / b \supset c \ \leftrightarrow \ a, b / c$$

(Bi-Conditional:)

(6.3) $$a / b \equiv c \ \leftrightarrow \ a, b / c \ \& \ a, c / b$$

(Conjunction:)

(6.4) $$a \,.\, b / c \ \leftrightarrow \ a, b / c$$

(Disjunction:)

(6.5) $$a \vee b / c \ \leftrightarrow \ a / c \ \& \ b / c$$

We can, of course, introduce, in the usual way, *some* of these compounds by way of definition rather than by way of primitive rules; if we then take negation and conjunction as primitives, then, for example, rules 6.1 and 6.4 constitute a sufficient basis. This is interesting because this system, consisting in addition to the rule 6.1 which introduces negation only of the extremely simple and even trivial rule 6.4 does not need to be further strengthened by the *modus ponens*; for the rule

$$a, \neg(a \,.\, \neg b) / b$$ (see below, 6.82)

can be derived from 6.4 and 6.1; in this way we can obtain the *modus ponens* immediately with the help of the customary definition:

"$a \supset b$" for "$\neg(a \,.\, \neg b)$"

The system here described – i.e. 6.1 and 6.4 – is in our opinion one of the simplest and, at the same time, most lucid bases of propositional logic so far known. It employs, apart from the rules of section 2 which are, tacitly or otherwise, presupposed by all systems, only two primitive rules. None of the usual systems employs a smaller number of rules, since all employ, at the very least, one rule of inference, and one axiom or one axiom scheme (which amounts to another rule of inference – viz., to a mixed rule asserting that the statements described as axioms follow from any premise whatsoever). At the same time, the known systems which employ only *one* axiom, apart from rules of inference, sacrifice lucidity in order to achieve this aim.

18 | In a similar way, the rules for negation and the conditional, 6.1 and 6.2, suffice as a basis for propositional logic. If, however, negation and disjunction are to be used,

---

for intuitionism, omit "d" in A.D.
for minimum negation, omit "d" in J.D.

then 6.5 is not, it appears, sufficient, but should be replaced by

(6.5+) $\quad a, b \vee c/d \leftrightarrow a, b/d \;\&\; a, c/d$ (see below, 6.91)

from which the *modus ponens* in the form

$$a, \neg a \vee b/b$$

can be obtained by $b/\overline{\neg a}, c/\overline{b}, d/\overline{b}$; for we easily obtain "$a, \neg a/b$" from 6.1. (See below, 6.101.)

Similarly, if we do not wish to employ negation at all, but wish to obtain the rules corresponding to Hilbert-Bernays' "positive identical" conditionals by using only one rule that introduces the conditional, then 6.2 must be replaced by

(6.2+) $\quad a, b/c \supset d \leftrightarrow a, b, c/d$ (see below, 6.721, 6.724)

But if we have conjunction at our disposal, i.e. 6.4, then 6.2+ can be obtained from 6.4 and 6.2, just as it can be obtained from 6.1 and 6.2.

The rule for negation, 6.1, can be split up into independent weaker rules, exactly as indicated in section 5. The rules obtained in this way (numbered so as to correspond to those in section 5) will be proved next.

Again, 6.11 (or its equivalents) yields the negation of the "Minimum Calculus", and, if 6.121 is added to it, that of the Intuitionistic Calculus. –

We now proceed to establish a number of dependent rules of the pure logic of derivation, obtainable from the primitive rules 6.1 to 6.5. In the next section, we shall add mixed rules and prove the completeness of the system.

In deriving these rules, we shall make use of those of section 2. But it is a remarkable fact that we do not need those rules as they stand; all we need are considerably weaker rules, namely the rules of section 2 with a limited number of premises – up to a maximum of three premises. We shall indicate this in the proofs by putting subscripts
19 | to the quoted rules of section 2; for example, "$2.11_{2,3}$" will mean "2.11, with $m = 2$ and $n = 3$", etc.

We shall first prove rules in which only one compound occurs, and only later combine different compounds in the same rules.

(6.101) $\quad \neg a, a/b$ $\quad (6.1, a/\overline{\neg a}; c/\overline{\neg a}, //2.12)$

(6.102) $\quad \neg\neg a/a$ $\quad (6.101, a/\overline{\neg a}, b/\overline{\neg\neg\neg a}; //6.1; 2.6_1)$

(6.1021) $\quad \neg\neg\neg a/\neg a$ $\quad (6.102)$

(6.103) $\quad a/\neg\neg a$ $\quad (6.1, b/\overline{\neg\neg a}; c/\overline{a}; //6.1021; 2.32_2)$

(6.11) $\quad a, b/\neg c \rightarrow a, c/\neg b$ $\quad (6.1, b/\overline{\neg b}; 6.102, a/\overline{b}; 2.14_1; 2.91_{2,2,1})$

(6.12) $\quad a, \neg b/c \rightarrow a, \neg c/b$ $\quad (6.1, c/\overline{\neg c}; 6.103 a/\overline{c}; 2.93_{2,2})$

(6.1101) = 6.103: this can be obtained, by 2.6_1 and 2.32_2, from 6.11, $b/\overline{\neg a}$, $c/\overline{a}$. Similarly, 6.1102 is obtainable from 6.11:

(6.1102) $a, \neg a / \neg b$ $(6.11, c/\overline{a}; 2.32_2)$

Thus we can obtain 6.111 from 6.11, using 6.1102:

(6.111) $a, b/c \rightarrow a, \neg c / \neg b$ $(6.11, c/\overline{\neg c}; 6.1101; 2.93_{2,2})$

(6.112) = 6.103 = 6.1101

(6.120) = 6.102; obtainable from 6.12, $a/\overline{\neg\neg a}$; $b/\overline{a}$; $c/\overline{\neg a}$; together with 6.1201 $(a/\overline{\neg a}; b/\overline{\neg a})$ which is also obtainable from 6.12:

(6.1201) $\neg a, a/b$ $(6.12, c/\overline{a}; 2.32_2; 2.13_2)$

Rule 6.1201 can also be obtained from 6.120, $a/\overline{b}$; together with 6.1102, $b/\overline{\neg b}$; without using 6.12; accordingly, we can obtain the following two rules without using 6.12:

(6.121) $\neg a, \neg\neg a / a$ $(6.1201, a/\overline{\neg a}; b/\overline{a})$

(6.122) $\neg\neg a, \neg\neg b / a \rightarrow \neg\neg a, \neg b / a$ $(6.102; 2.14_1)$

This concludes the derivation of the rules corresponding to 5.11, 5.12, etc. In order to prove our contentions stated in section 5, we have, besides, to establish certain equivalences: 6.120 can be obtained from 6.122, $b/\overline{\neg a}$, together with "$\neg\neg a, \neg\neg\neg a / a$",
20 which in turn follows from | 6.1102, $a/\overline{\neg\neg a}, b/\overline{a}$; and the same, $a/\overline{\neg\neg a}, b/\overline{\neg a}$; together with 6.121. Thus 6.120, in the presence of 6.11, is equivalent to 6.121 and 6.122; but it is also equivalent in the presence of 6.111 instead of 6.11, for 6.1102 can be obtained from 6.111, $c/\overline{a}$. – 6.120, in turn, is equivalent, in the presence of 6.11, to 6.12; for 6.103 = 6.1101 can be obtained from 6.11. – 6.11 from 6.111, $c/\overline{\neg c}$, and 6.112. – 6.1 from 6.11 and 6.120, or from 6.12 and 6.112. – This establishes the claimed equivalences.

Other rules, containing negation only are, for example:[f]

(6.13) $a, b/c \leftrightarrow a, \neg c / \neg b$ $(6.1; 6.111)$

(6.131) $a, b/\neg b \rightarrow b, b/\neg a$ $(6.11; 2.13_2)$

(6.132) $a, b/\neg b \rightarrow b/\neg a$ $(6.131; 2.6_2)$

(6.133) $a, \neg b/b \rightarrow \neg b/\neg a$ $(6.132, b/\overline{\neg b}; 6.103)$

(6.134) $a/b \rightarrow a, \neg b/b$ (2.14_1)

(6.135) $a/b \rightarrow \neg b/\neg a$ $(6.134; 6.133)$

(6.136) $\neg a/\neg b \rightarrow \neg a, b/\neg b$ (2.14_1)

(6.137) $\neg a/\neg b \rightarrow b/a$ $(6.136; 6.132, a/\overline{\neg a}; 6.102)$

(6.14) $a/b \leftrightarrow \neg b/\neg a$ $(6.135; 6.137)$

[f] Between (6.16) and (6.17) there is a handwritten remark, barely readable in the copy: "all these rules, with the exception of the last four, are valid in the intuitionistic logic and even in Johansson's minimum logic, and so are the next three".

(6.15) $\neg a/b \leftrightarrow \neg b/a$ (6.14; 6.102; 6.103)

(6.16) $a/\neg b \leftrightarrow b/\neg a$ (6.14; 6.102; 6.103)

(6.17) $a, \neg b/b \leftrightarrow a/b$ (6.133; 6.14; 6.134)

(6.18) $a, b/\neg b \leftrightarrow a/\neg b$ (6.132; 6.16; 2.14_1)

(6.19) $\neg b/c \rightarrow (a, c/b \leftrightarrow a/b)$ (6.17; $2.91_{2,2,1}$; 2.14_1)

We now turn to rules containing conditionals only. We first obtain the *modus ponens*:

(6.20) $a \supset b, a/b$ (6.2, $a/\overline{a \supset b}$; $b/\overline{a}$; $c/\overline{b}$; 2.12)

and by applying this twice,

(6.201) $a \supset (a \supset b), a/b$ (6.20, $2.11_{2,2}$)

Two important rules are

(6.21) $a/b \supset a$ (6.2, $c/\overline{a}$)

(6.22) $a \supset (a \supset b)/a \supset b$ (6.2, $a/\overline{a \supset (a \supset b)}$; $b/\overline{a}$; $c/\overline{b}$; //6.201)

For what follows, we need

(6.2+) $a, b/c \supset d \leftrightarrow a, b, c/d$ (see below, 6.721 and 6.724)

This can be obtained either with the help of 6.1 or 6.4, and will be so derived later (see 6.721 and 6.724).
We obtain first a generalized *modus ponens*:

(6.220) $a \supset b, c, a/b$ (6.2+, $a/\overline{a \supset b}$; $b/\overline{c}$; $c/\overline{a}$; $d/\overline{b}$; //2.32_2)

and applying this together with the *modus ponens*:

(6.2201) $(a \supset b), (b \supset c), a/c$ (6.220, 6.20, $2.11_{2,3}$)

(6.2202) $(a \supset b), (b \supset c)/a \supset c$ (6.2+, //6.2201)

This establishes the transitivity of the conditional, which we can also write:

(6.23) $a \supset b/(b \supset c) \supset (a \supset c)$ (6.2202; 6.1)

A few more rules of importance are:

(6.231) $a, a \supset c, b \supset c/c$ (6.220; 2.5_3)

21 (6.232) $b, a \supset c, b \supset c/c$ (6.231; 2.5) |

(6.24) $a/(a \supset c) \supset ((b \supset c) \supset c)$ (6.231; 6.2+; 6.2)

(6.25) $b/(a \supset c) \supset ((b \supset c) \supset c)$ (6.232; 6.2+; 6.2)

We now proceed to rules containing the bi-conditional only:

(6.31)	$a \equiv b, a/b$	$(6.3, a/\overline{a \equiv b}; b/\overline{a}; c/\overline{b})$
(6.32)	$a \equiv b, b/a$	(the same)

The next rule needs for its derivation the help of 6.4:

(6.33)	$a, b, c/d \;\&\; a, b, d/c \rightarrow a, b/c \equiv d$	(see below, 6.734)

Rules containing conjunction only:

(6.41)	$a \,.\, b/a$	$(6.4, c/\overline{a})$
(6.42)	$a \,.\, b/b$	$(6.4, c/\overline{b})$
(6.421)	$a, b/a \,.\, b$	$(6.4, c/\overline{a \,.\, b})$

The three rules 6.41, 6.42, and 6.421 are equivalent to $\langle 6.4 \rangle$. (6.421 has sometimes been called "rule of adjunction"; $\langle 6.4 \rangle$ may be called, perhaps, the "full rule of adjunction".)

(6.422)	$a/b \;\&\; a/c \rightarrow a/b \,.\, c$	$(6.421, a/\overline{b}; b/\overline{c}; 2.11_{2,1})$
(6.4221)	$a \,.\, b/c \;\&\; a \,.\, b/d \rightarrow a \,.\, b/c \,.\, d$	(6.422)
(6.43)	$a, b/c \;\&\; a, b/d \rightarrow a, b/c \,.\, d$	$(6.41; 6.42; 2.91_{2,2,2}; 6.4221;$ $6.421; 2.92_{2,2,2})$
(6.431)	$a, b/b \,.\, a$	$(6.421; 2.13_2)$
(6.44)	$a \,.\, b/b \,.\, a$	(6.4; 6.431)
(6.441)	$a, b \,.\, c/a$	(2.22)
(6.442)	$a, b \,.\, c/b$	$(6.41; 2.14_1; 2.13_2)$
(6.443)	$a, b \,.\, c/c$	$(6.42; 2.14_1; 2.13_2)$
(6.444)	$a, b \,.\, c/(a \,.\, b) \,.\, c$	$(6.441; 6.442; 2.11_{2,2}; 6.421;$ $6.443; 2.11_{2,2}; 6.421)$
(6.45)	$a \,.\, (b \,.\, c)/(a \,.\, b) \,.\, c$	$(6.41; 6.42; 2.11_{2,1})$
(6.46)	$(a \,.\, b) \,.\, c/a \,.\, (b \,.\, c)$	$(6.44; 6.45; 2.11_{1,1})$

We also have

(6.47)	$a/b \rightarrow c \,.\, a/c \,.\, b$	$(2.14_1; 2.13_2; 2.92_{1,2,1};$ $6.41; 6.42; 6.421; 2.93_{2,1})$
(6.471)	$a, b \,.\, c/d \rightarrow a, b, c/d$	$(6.421; 2.91_{2,2,3})$
22 (6.472)	$a, b, c/d \rightarrow a, b \,.\, c/d$	$(6.441 \text{ to } 6.443; 2.92_{3,2,3})$ |
(6.48)	$a, b \,.\, c/d \leftrightarrow a, b, c/d$	(6.471; 6.472)

Rules containing disjunction:

(6.51)	$a/a \vee b$	$(6.5, c/\overline{a \vee b})$
(6.52)	$b/a \vee b$	(the same)
(6.53)	$a/c \;\&\; b/c \rightarrow a \vee b/c$	(6.5)
(6.54)	$a \vee b/b \vee a$	$(6.53, c/\overline{b \vee a}; 6.51 \;\&\; 6.52; a/\overline{b}; b/\overline{a})$
(6.541)	$a/a \vee (b \vee c)$	(6.51)
(6.542)	$b \vee c/a \vee (b \vee c)$	$(6.51; 6.54; 2.11_{1,1})$
(6.543)	$b/a \vee (b \vee c)$	$(6.51, a/\overline{b}; b/\overline{c}; 6.542; 2.11_{1,1})$
(6.544)	$a \vee b/a \vee (b \vee c)$	$(6.5, c/\overline{a \vee (b \vee c)}; 6.541; 6.543)$
(6.545)	$c/a \vee (b \vee c)$	$(6.52, a/\overline{b}; b/\overline{c}; 6.542; 2.11_{1,1})$
(6.55)	$(a \vee b) \vee c/a \vee (b \vee c)$	$(6.5, a/\overline{a \vee b}; b/\overline{c}; 6.544; 6.545)$
(6.56)	$a \vee (b \vee c)/(a \vee b) \vee c$	$(6.55; 6.54; 2.11_{1,1})$

The next rule will be derived later, with the help of conjunction and negation:

(6.5+)	$a, b \vee c/d \;\leftrightarrow\; a, b/d \;\&\; a, c/d$	(see below, 6.91)

We now sketch the proofs of a few selected rules, employing more than one compound at a time; first negation and the conditional:

(6.71)	$\neg a \supset \neg b/b \supset a$	$(6.1, a/\overline{\neg a \supset \neg b}; b/\overline{a}; c/\overline{b}; 6.2; 6.20)$
(6.711)	$a \supset b/\neg b \supset \neg a$	(similarly, from 6.111)
(6.712)	$a/\neg a \supset b$	(6.101; 6.2)
(6.713)	$\neg a/a \supset b$	(6.101; 6.2)
(6.714)	$\neg a/a \supset \neg b$	$(6.713, b/\overline{\neg b})$
(6.715)	$\neg b/a \supset \neg b$	(6.21)

From 6.713 and 6.19, we can obtain an important rule in which only the conditional occurs (negation disappears): it is the simplified version of 5.2X:

(6.72)	$a, b \supset c/b \rightarrow a/b$	$(6.713, a/\overline{b}; b/\overline{c}; //6.19, c/\overline{b \supset c})$
(6.720)	$(a \supset b) \supset a/a$	$(6.72, a/\overline{(a \supset b) \supset a}; b/\overline{a}; c/\overline{b}; 6.20)$

In the following rules the conditional is more important than the negation:

23 (6.7201)	$a, a \supset \neg b/\neg b$	(6.20) |
(6.7202)	$a, b/\neg(a \supset \neg b)$	(6.7201; 6.11)
(6.7203)	$\neg(a \supset \neg b)/a$	(6.714; 6.12)
(6.7204)	$\neg(a \supset \neg b)/b$	(6.715; 6.12)
(6.7205)	$\neg(a \supset \neg b)/c \supset d \;\leftrightarrow\; \neg(a \supset \neg b), c/d$	(6.2)
(6.721)	$a, b/c \supset d \;\leftrightarrow\; a, b, c/d$	(2.91; 2.92; 6.7202 to 5)

The last rule is identical with 6.2+; it can be derived, more easily, by using the conditional together with conjunction:

(6.723) $a \,.\, b/c \supset d \;\leftrightarrow\; a \,.\, b, c/d$ (6.2)

(6.724) $a, b/c \supset d \;\leftrightarrow\; a, b, c/d$ (6.723; 6.41; 6.42; 2.91; 6.421; 2.92)

Next the conditional and bi-conditional:

(6.731) $a \equiv b/a \supset b$ (6.31; 6.2)

(6.732) $a \equiv b/b \supset a$ (6.32; 6.2)

(6.7330) $a \supset b, b \supset a/a \equiv b$ (6.33, $a/\overline{a \supset b}$; $b/\overline{b \supset a}$; $c/\overline{a}$; $d/\overline{b}$)

(6.733) $a \supset b/(b \supset a) \supset (a \equiv b)$ (6.7330; 6.2)

We omit rules employing the bi-conditional and negation; but we give a proof of 6.33 which uses the bi-conditional and the conjunction:

(6.7340) $a \,.\, b, c/d \;\&\; a \,.\, b, d/c \rightarrow a \,.\, b/c \equiv d$ (6.3)

(6.734) $a, b, c/d \;\&\; a, b, d/c \rightarrow a, b/c \equiv d$ (6.7340; 6.41; 6.42; 2.91; 6.421; 2.92)

Next the conditional and the conjunction:

(6.7421) $a \supset b, a \supset c, a/b \,.\, c$ (6.220; 6.4201)

(6.7422) $a \supset b, a \supset c/a \supset (b \,.\, c)$ (6.7421; 6.742)

(6.743) $a \supset b/(a \supset c) \supset (a \supset (b \,.\, c))$ (6.7422; 6.2)

The conditional and the disjunction:

(6.750)
$a \vee b/(a \supset c) \supset ((b \supset c) \supset c)$ (6.53, $c/\overline{(a \supset c) \supset ((b \supset c) \supset c)}$; 6.24; 6.25)

(6.751) $a \vee b, a \supset c, b \supset c/c$ (6.750; 6.2; 6.2+)

(6.752) $a \supset c, b \supset c/(a \vee b) \supset c$ (6.751; 6.2+)

(6.753) $a \supset c/(b \supset c) \supset ((a \vee b) \supset c)$ (6.752; 6.2)

Conjunction and negation:

(6.81) $a, \neg b/a \,.\, \neg b$ (6.421)

(6.82) $a, \neg(a \,.\, \neg b)/b$ (6.81; 6.12)

This is the modus ponens in terms of conjunction and negation. We briefly sketch
24 how a rule corresponding to 6.2+ can be derived. |

(6.821) $a, b/\neg(c \,.\, \neg d) \;\leftrightarrow\; a, c \,.\, \neg d/\neg b$ (6.11)

(6.822) $a, b/\neg(c \,.\, \neg d) \leftrightarrow a, c, \neg d/\neg b$ (6.821; 6.448)

(6.823) $a, b/\neg(c \,.\, \neg d) \leftrightarrow a, b, c/d$ (6.822; 6.1; 6.111)

This proves our contention that the rules for conjunction and negation suffice, together with the customary definition of the conditional, to establish the rules for the conditional.

The following rules are of considerable importance:

(6.831) $\neg c/\neg a \;\&\; \neg c/\neg b \leftrightarrow \neg c/\neg a \,.\, \neg b$ (6.422)

(6.84) $a/c \;\&\; b/c \leftrightarrow \neg(\neg a \,.\, \neg b)/c$ (6.831; 6.14; 6.15)

(6.841) $a, \neg d/\neg b \;\&\; a, \neg d/\neg c \leftrightarrow a, \neg d/\neg c \,.\, \neg b$ (6.43)

(6.85) $a, b/d \;\&\; a, c/d \leftrightarrow a, \neg(\neg c \,.\, \neg b)/d$ (6.841; 6.13; 6.12)

(6.851) $a, b/d \;\&\; a, c/d \leftrightarrow a \,.\, b/d \;\&\; a \,.\, c/d$ (6.4)

(6.86)

$a, \neg(\neg b \,.\, \neg c)/d \leftrightarrow \neg(\neg(a \,.\, b) \,.\, \neg(a \,.\, c))/d$ (6.84; 6.85; 6.851)

This is a form of the distributive law, expressed in terms of conjunction and negation; for it yields immediately:

(6.87)

$a \,.\, (\neg(\neg b \,.\, \neg c))/\neg(\neg(a \,.\, b) \,.\, \neg(a \,.\, c))$ (6.86; 6.41; 6.42; 6.421)

(6.88) $\neg(\neg(a \,.\, b) \,.\, \neg(a \,.\, c))/a \,.\, (\neg(\neg c \,.\, \neg b))$ (the same)

We also have

(6.881) $a/\neg(\neg a \,.\, \neg b)$ (6.84, $c/\overline{\neg(\neg a \,.\, \neg b)}$)

(6.882) $b/\neg(\neg a \,.\, \neg b)$ (the same)

We now combine disjunction with conjunction

(6.883) $a \,.\, b/a \,.\, (b \vee c)$ (6.41; 6.42; 6.51; $2.11_{1,1}$; $2.11_{2,1}$)

(6.884) $a \,.\, c/a \,.\, (b \vee c)$ (similar, using 6.52)

(6.89) $(a \,.\, b) \vee (a \,.\, c)/a \,.\, (b \vee c)$ (6.5; 6.883; 6.884)

In order to obtain the distributive law in the converse direction, we have to make use of negation also:

(6.891) $a \vee b/\neg(\neg a \,.\, \neg b)$ (6.5; 6.881; 6.882)

(6.892) $\neg(\neg a \,.\, \neg b)/a \vee b$ (6.84; 6.51; 6.52)

(6.893) $\neg(\neg(a \,.\, b) \,.\, \neg(a \,.\, c))/(a \,.\, b) \vee (a \,.\, c)$ (6.892)

(6.894) $a \,.\, (b \vee c)/a \,.\, (\neg(\neg b \,.\, \neg c))$ (6.891; 6.47)

leaving now negation aside, we have:

25 (6.90) $a\,.\,(b \vee c)/(a\,.\,b) \vee (a\,.\,c)$ (6.894; 6.893) |

(6.901) $a\,.\,b/d \;\&\; a\,.\,c/d \;\leftrightarrow\; (a\,.\,b) \vee (a\,.\,c)/d$ (6.5)

(6.902) $a\,.\,b/d \;\&\; a\,.\,c/d \;\leftrightarrow\; a\,.\,(b \vee c)/d$ (6.90; 6.901; $2.11_{1,1}$)

now we can leave conjunction aside too:

(6.91) $a, b/d \;\&\; a, c/d \;\leftrightarrow\; a, b \vee c/d$ (6.902; 8.41; 8.42; 2.91; 8.421; 2.92)

This is the same as rule 6.5+.

7. Completeness of the Various Systems

The proof of completeness for the system 6.1 to 6.5 is now trivial.
We have

(7.1) $$a/b \rightarrow c/a \supset b,$$

since $a/b \rightarrow c\,.\,a/b$ (2.14_1 2.13_2), from which 7.1 follows by 6.2. Rule 7.1 asserts that, if a/b, then $a \supset b$ follows from any premise whatsoever. (Thus $a \supset b$ will be in this case a tautology, and 7.1 is a mixed rule of derivation and demonstration; see section 5.)

With the help of 7.1, we can easily show of all the axioms of the propositional calculus of Hilbert and Bernays that they follow, in our system, from any premise whatsoever. If any of these axioms, therefore, is added to any premise or set of premises, then we cannot derive any conclusion from this set which we would have been unable to derive without adding the axiom. Accordingly, we may use all these axioms as "suppressed premises", i.e., without explicitly stating them. Since we also have the *modus ponens* at our disposal, we can derive from any set of premises every conclusion which we might derive from it, with the help of the *modus ponens*, from the axioms of the propositional calculus.

The rules of derivation in section 6 which correspond in view of 7.1 to the Hilbert Bernays axioms are: I, 1)–3) of Hilbert and Bernays correspond to 6.21, 6.22, 6.23; II, 1)–3) to 6.41, 6.42, and 6.743; III, 1)–3) to 6.51, 6.52, and 6.753; IV, 1)–3) to 6.731, 6.732, and 6.733; and V, 1)–3) correspond to our rules 6.71, 6.102, 6.103.

Furthermore, rule 6.720 corresponds in the same manner to the first formula
26 on p. 70 of ⟨Hilbert and Bernays, 1934⟩; i.e., | in our notation, to the formula "$((a \supset b) \supset a) \supset a$"; and it is known that, if this formula is added to the axioms I,1) and 3), all logically true formulae containing the conditional only can be derived.

From this, our contentions stated in section 5 follow; for the systems there described are (considering 2.6 which permits the omission of the premises $a_1, \ldots, a_n$) obviously stronger than the corresponding systems in section 6. (5.2X, we recall, corresponds to 6.72).

In order to prove our contentions stated in section 4, we only have to prove that 4.11^{+} and 4.12^{+}, in the presence of the other rules, are sufficient for proving Hilbert and Bernays' axioms V, 1) to 3). We shall confine ourselves to showing that they are, in the presence of the other rules, equivalent respectively to the formulae

$$\vdash (\neg a \supset a) \supset a \tag{7.11}$$

$$\vdash \neg a \supset (a \supset b) \tag{7.12}$$

which are known, in the presence of the other axioms, to be equivalent to the axioms in question.

In order to prove 7.11, we need, from the other rules,

(7.111) $\quad a/b \supset a \quad$ (6.21)

(7.112) $\quad b, b \supset a/a \quad$ (6.20)

and therefore

(7.113) $\quad b/(b \supset a) \supset a \quad$ (7.112; 6.1)

We obtain, by substitution

(7.114) $\quad a/(\neg a \supset a) \supset a \quad$ $(7.111, b/\overline{\neg a \supset a})$

(7.115) $\quad \neg a/(\neg a \supset a) \supset a \quad$ $(7.113, b/\overline{\neg a})$

From 7.114 and 7.115 follows 7.11 immediately, by 4.11^{+}.

In order to prove 7.12 we need

(7.121) $\quad \urcorner\, a \,.\, \neg a \to a \,.\, \neg a/b \quad$ $(4.0\, \urcorner)$

together with the following rules which we likewise obtain by substitution:

(7.122) $\quad a \,.\, \neg a/a \;\&\; a \,.\, \neg a/\neg a \quad$ (6.41; 6.42)

(7.1221) $\quad a \,.\, \neg a/b \to \neg a, a/b \quad$ $(6.4; 2.13_2)$

(7.1222) $\quad \neg a, a/b \to \neg a/a \supset b \quad$ (6.2)

(7.123) $\quad a \,.\, \neg a/b \to\, \vdash \neg a \supset (a \supset b) \quad$ $(7.1221; 7.1222; 7.1; 4.0 \vdash)$

from 7.121, 7.122, 4.12^{+} and 7.123 follows 7.12.

This concludes the proofs of our contentions concerning completeness.

Endnote

Meeting in Zürich in December 1946, the authors found that, starting from very different questions, they had constructed, independently, very similar theories which they decided to publish conjointly. The main difference in their approach was that P. Bernays re-formulated Tarski's theory of 1930 (see section 3) in order to obtain a general theory of inference operating with statements rather than with classes of statements. The results thus obtained are elaborated in sections 2, 3, and 4. K. R. Popper who had not noticed the connection between his results and Tarski's paper, had developed results elaborated in sections 2, 5, 6 and 7. His starting point was the problem of a purely derivational logic discussed in section 5 (a problem in which P. Bernays had not interested himself) and he saw in the "purification" of derivational logic described there the distinctive difference between his approach and, for example, G. Gentzen's in some respects similar "Untersuchungen über das logische Schliessen", Mathematische Zeitschrift 39 (1935)[g].

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[g] Gentzen (1935a,b).

Chapter 15
A General Theory of Inference

Karl R. Popper

Abstract This is an unpublished article from Popper's Nachlass.

Editorial notes: The source manuscript is from KPS Box 12, Folder 10. It is marked "*A General Theory of Inference* BY K.R. Popper", and consists of 54 handwritten pages, numbered 1 to 52, and an additional sheet numbered 39A and 39B. There is a mark on page 39 with an instruction where to insert pages 39A/B. In footnote 2 Popper mentions that he has not yet published anything concerning the problem of characterising valid inferences in general apart from "Why are the Calculuses of Logic and Arithmetic Applicable to Reality" (Popper, 1946c). This places the manuscript in the second half of 1946, probably before the meeting with Bernays in December 1946. The article could also be seen as a precursor to "Logic without Assumptions" (Popper, 1947b; this volume, Chapter 2) where Popper also discusses different notions of translation in the context of Tarski's concept of logical consequence.

1. Distinction Between General and Special Theories of Inference

1 Since Aristotle, logicians have been concerned, practically without exception, with the problem of the various special theories of inference, as opposed to those of a general theory of inference. They developed rules of inference for special languages (or calculi), such as the language of categorical propositions, for which Aristotle constructed a set of rules of inference, the so-called valid moods of the syllogism; or the more developed language of categorical propositions, usually called "calculus of classes", for which Boole and his successors developed an exhaustive theory of inference; or the language of hypothetical and disjunctive propositions, for which the Stoics constructed the set of rules called "modus ponendo ponens", "modus tollendo tollens", etc; or the more developed form of this language, usually called "calculus of propositions", for which Frege and Russell gave an exhaustive theory of inference.

Most of the contemporary work in logic is confined to the closer investigation of
2 these | languages and their rules of inference, or to the construction of new languages (or calculi), or to the investigation of the relations between these calculi.

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Trends in Logic 58, https://doi.org/10.1007/978-3-030-94926-6_15

The fundamental problem of a general theory of inference is altogether different. It is not the problem of finding valid rules of inference for some language or other, but the problem of characterising valid inferences in general, for example, by giving a general and adequate definition of the term “valid inference”.

If this can be done, then many problems might be solved which otherwise must remain insoluble. The most important of these problems is that of proving the validity of those rules of inference which are assumed as fundamental or axiomatic by the various special theories of inference.

I shall briefly comment on the fundamental nature of this problem.

3 The special theories of inference cannot do more than analyse | intuitive inference. They can give a number of general rules corresponding to some of the inferences in the language in question which we intuitively recognized as valid. They can reduce these rules to a small number of rules which can be accepted as valid on intuitive grounds, and which may be described as the *axiomatic rules of inference* for the language in question. They can combine these in such a way that rules emerge which are too complicated to be judged by intuition but can, nevertheless, be shown to be valid, if only we assume the axiomatic rules to be valid. Beyond this, the special theories cannot go.

But the general theory might go further. It might be able of being applied to the various systems of axiomatic rules, and to establish that they are indeed valid rules of inference. In this way, the various special theories of inference would be reduced to applications of the general theory of inference. In establishing the validity of the various axiomatic rules, the general theory would further reduce the intuitive element in logic. But this is obviously the task of logic – to give definite rules to which we may refer instead of having to appeal to logical intuition, i.e., to the intuitive conviction that a certain inference is valid.

4 | It must be admitted, of course, that the general characterisation or definition of a valid inference may retain intuitive elements. But these elements may be of a very different character from that “logical intuition”, as we have called it, which logic attempts to replace by rules. Besides, the adequacy of any definition may be discussed in various ways, for example, with respect to the capacity of the definition to establish,
5 without any further | recourse to logical intuition, those rules of inference which have been long accepted as valid.

2. Contributions to the Problem

The first logician who, to my knowledge, conceived the idea of a general theory of inference was Carnap who devoted part of his book *Logical Syntax of Language*[a] (1934 and 1937) to what he called “General Syntax”. Carnap himself is now convinced (owing to Tarski’s criticism) that this attempt was unsuccessful (although it raised many important problems). A second attempt was made by Tarski, in his paper *On*

[a] Carnap (1934a, 1937).

6 *the Concept of Logical Consequence*[1]. Further | contributions were made by Carnap in his book *Introduction to Semantics*[b] (1942; see especially the reference, on p. vii, to the distinction between logical and descriptive signs). – Since 1937 I made use of Tarski's ideas in my lecture courses on logic[2]; more especially, I attempted (a) to apply Tarski's general concept of logical inference to the problem of establishing the validity of special rules of inference, (b) to analyse the distinction between logical and descriptive signs and the dependence of the general concept of inference on this distinction, (c) to analyse the relations between the "meaning" of logical signs and
7 the "meaning" of logical inference. I have convinced myself, in these lectures, | that a consideration of the practical value of inference is a useful heuristic starting point for the discussion, and that, from this starting point, we can, in stages, proceed to Tarski's definition; and further, to a statement and to a criticism of this definition, and to a re-statement of what I consider the fundamental problem of General Logic. This will be my procedure in the next three sections (section 3 & 4). In section 5 I shall give some alternative formulations of Tarski's definition, preparatory to a solution of the problem which will be sketched in section 6.

3. Steps towards Tarski's Definition of Logical Consequence

8 | It is an empirical fact that most of us "know how"[3] to draw inferences. Apparently, we learn this very early, and without being aware of it. This may suggest to us that drawing inferences – or "putting two and two together", as this activity is called colloquially – is of some practical value to us. Why? I suggest, because

(1) Valid inferences are such that, if the premises are all true statements of facts, the conclusion must be true as well.

In other words, if we have reliable information, and "know how" to draw inferences, we can rely on the secondary information thus obtained. For example, we may know from George that he sailed by the "Aquitania" and then read in the paper that the "Aquitania" arrived and that all on board are well. Putting two and two together, we

[1] ⟨Tarski, 1936b⟩, *Ueber den Begriff der logischen Folgerung*, published in *Actes du Congrès internationale de philosophie scientifique, fasc. VII.* (Paris, 1936). It may be noted that this important paper is missing in Z. Jordan's ⟨1945⟩ Bibliography in *Polish Science and Learning*, No. 6 (Oxford 1945).

[2] I have, however, nothing published on this problem, except some remarks in my paper ⟨Popper, 1946c⟩ "Why are the ⟨Calculuses⟩ of Logic and Arithmetic Applicable to Reality?" in the *Proceedings of the Aristotelian Society* (Supplementary Volume, 1946).

[3] G. Ryle has drawn attention to the fact that we often "know how" to do a certain thing, as opposed to the explicit knowledge, the "knowing that" a certain thing can be, or must be, done in a certain way. Cp. his Presidential Address to the Aristotelian Society (⟨Ryle, 1945⟩), and my above mentioned paper.

[b] Carnap (1942).

shall draw the conclusion "George arrived and is well"; and provided our original information was true, we may rely on the conclusion to be a true statement of fact.

We take (1) as the first step towards a definition of valid inference. It leads
9 immediately to a | formulation (2) which has been used, in some form or other, by many logicians since Aristotle:

(2) An alleged inference is certainly invalid if the premises are all true and the conclusion false.

We can express (1) and (2) also by saying (with Aristotle):

(3) If an inference is valid, then the second of the following four possible combinations cannot occur: (1) The premises are all true, and the conclusion is true; (2) the premises are all true and the conclusion is false; (3) the premises are not all true and the conclusion is true; (4) the premises are not all true, and the conclusion is false. Combinations (1), (3), and (4) can occur, that is to say, only the second combination is incompatible with validity.

All this would be of little value if we could not add

(4) The validity of an inference can be known without information about the truth or falsity of the premises and the conclusion.

10 That is to say, we need not have any | direct information concerning the conclusion: information about the truth of the premises and knowledge concerning the validity of the inferences is enough, and can be obtained without consideration of the conclusion. On the other hand, we shall say that

(5) The property of transmitting unfailingly the truth of the premises (*if* they are true) to the conclusion is, practically and theoretically, the most important characteristic of a valid inference.

If we consider (5), it is clear that such a term as "unfailingly" or "necessarily" is unsatisfactory. It can be replaced, however, by reference to what we may call the "logical form" of an inference (a term which, in its turn, needs further analysis):

(6) An inference is valid if its logical form is such that no other inference *of the same logical form* can have true premises and a false conclusion.

11 | If we have an inference under consideration, not knowing whether it is valid, then we can try to construct an inference *of the same logical form*, but with true premises and a false conclusion. Such an (obviously invalid) inference with true premises and a false conclusion we call a "counter example" of the inference in question. (If the original inference has true premises and a false conclusion, then it is its own counter-example.) Using this term counter-example, we can now say:

(7) An inference is valid if and only if no counter example of it exists.

This is a reasonably satisfactory general definition, ⟨and⟩ although it will have to be amended in many ways, it can be used for proving the invalidity (and even
12 the validity) of various special rules of | inference. It is based on the technical

term "counter-example" which, in turn, is based on the concepts "true", "false" (or "non-true"), and "logical form".

Of these two concepts we shall take the concept "true" for granted (in the sense of the correspondence theory, i.e., of the phrase: "A statement *s* is true if and only if it corresponds to the facts"). We can do this because of the famous analysis of this concept by Tarski; only this analysis made a general theory of inference possible. Nevertheless, we shall show, later, that we can eliminate this concept, if we use, with Carnap and Tarski, the name "Semantics" for a theory which makes use of the term "truth" (and related terms, such as "*X* is the name of *y*" or "*X* designates *y*"), and if
13 we use, with Carnap, the name "Syntax" | for a theory which considers the logical form of expressions without using terms characteristic of semantics, then we can say that we shall first construct a semantical concept of valid inference, and later a syntactical one. (Tarski's concept of inference belongs to Semantics.)

The second concept mentioned above, that of the "logical form" of an inference, can be replaced by that of a "logical skeleton" (of a statement, or of a class of statements, or of an argument or inference). This term, in its turn, is based on the classification of the signs of the language under consideration into two classes: the *logical signs* or, as I prefer to call them, the *formative signs* on the one hand, and the *descriptive signs* on the other.

14 | This distinction is one which all logicians since Aristotle have made; but it was first explicitly discussed by Carnap (in his "Logical Syntax"). Logical or, as we shall say, *formative* signs are, for example, Aristotle's affirmative and negative copulas, "is" or "are", and "is not" or "are not"; further the words "all" and "some". Other examples are the words "if . . . then . . .", "or", "neither . . . nor . . .", "and", "it is not the case that . . .", etc. Descriptive signs are, for example, the words "cow", "justice", "the 1st of November 1946", etc. The problem whether we can give a more general explanation of this distinction rather than an explanation based on examples will be discussed below.

15 | Once the distinction between logical and descriptive signs is given, it is easy to explain the term "logical skeleton". We obtain the logical skeleton of a sentence, or a set of sentences, or an argument, by deleting all descriptive constants (and only these), taking care, however, to indicate *repetitions* of the descriptive constants. (This can be best achieved by the use of variables: we put variables in the empty places left by the elimination of the descriptive constants, and use the same variable wherever the same descriptive term has been eliminated, and otherwise different variables.)

16 | Considering this explanation, our definition (7) is only a shorter way of saying:

(8) An inference is valid if, and only if, an inference with the same logical skeleton, and with true premises and a false conclusion, does not exist.

4. Tarski's Definition of Logical Consequence

We are now prepared to take the last step which leads us to Tarski's final concept of logical consequence or of an inference.

A definition such as (7) or (8) is, as Tarski points out, not entirely satisfactory;
17 although its intentions are correct, it does not completely answer these | intentions, if we accept it as a definition for inference *in general*, i.e., inference in any language. The simple reason is that some languages may not possess those descriptive terms which are needed for establishing a counter example. Take an invalid inference such as

All men are mortal.
All Greeks are mortal.

All Greeks are men.

In order to establish a counter example, we clearly need, at the very least, either (a) one other term, say "*t*" ("dogs" for example), which satisfies the following condition:
18 "All *t* are mortal" is true, "All *t* are men" is false, | or else (b) one other term, say "*u*" ("plants", for example), which satisfies the conditions: "All *u* are mortal" is true and "All Greeks are *u*" is false. Now it is easy to construct a language which does not contain such terms; for example, we can take Aristotle's formative signs and all the rules of Aristotelian logic, and add to these the terms "men", "mortal", and "Greeks"; or we could add even the terms "Greeks", "Persians", "Egyptians", etc., and the name of any property shared by all these. In both these languages, no counter-example
19 exists to show the invalidity of the inference | mentioned. Accordingly, the inference would be valid in the sense of our definition (7) and (8), which is certainly against our intentions.

In order to avoid this unwanted consequence of definitions of the type (7) or (8), Tarski introduces the idea of a *model of a logical skeleton*. (The idea is taken from the postulational technique: we may say, for example, that light-rays (on Earth) and Fadenkreuze ⟨crosshairs⟩ are a model of, or satisfy, the postulate systems of Euclidean
20 Geometry, if we put light-rays in place | of what Euclid meant by "straight lines", and Fadenkreuze ⟨crosshairs⟩ for whatever Euclid meant by "points".) By a model, Tarski means a set of things, properties, relations etc. (*not* their names); and he says that a model satisfies a logical skeleton, or that a model is a model *of* a logical skeleton, if and only if, the skeleton becomes true after substitution of appropriate names of these things (which names are to be added, if necessary, to the vocabulary of the
21 language in question). In | this sense, the set consisting of the class of all Greeks and the class of all mortals (of the classes themselves, and not of their names) satisfies, or is a model of, the skeleton "All *x* are *y*", or the set consisting of Socrates, the class of teachers (of somebody) and Alcibiades (of the men themselves and the relation and
22 not of their names) satisfies, or is a model of, | the logical skeleton "*a* is one of the *R* of *b*". The idea that a model *satisfies* (or *fulfils*) a skeleton, or it is a model *of* a skeleton, is clearly very closely related to the idea of truth.

With the help of this idea of a model, we can now formulate Tarski's definition of logical consequence or of valid inference:

(9) An inference is valid if every model satisfying the skeleton of the premises is a model satisfying the skeleton of the conclusion.

5. Criticism of Tarski's Definition. Present State of the Problem

23 | Tarski himself has given an excellent criticism of his definition. He points out that the definition is dependent upon the distinction between formative (or logical) and descriptive signs, and that we cannot be sure whether this distinction is not somewhat arbitrary. Surely, the examples given above are innocent enough. But if we realize that we are inclined to take such mathematical concepts as ">" ("greater than") or "=" ("equals") as formative while we would hardly doubt that "taller than" or "of
24 equal height" are descriptive, then we may | begin to doubt whether the distinction is not somewhat arbitrary. But if this is so, then there may be a number of different classifications of the signs of a language into formative and descriptive possible, and to each of these would correspond a different concept of valid inference.

Tarski's ultimate attitude, accordingly, is rather sceptical. He seems to think that
25 the element of arbitrariness in the | distinction between formative and descriptive signs cannot be eliminated, and that the concept of inference, since it depends on this distinction, has a similarly arbitrary character.

The situation can be also described in this way. All logic, and especially the general theory of inference, attempts to replace our logical intuition by definite rules. We now
26 find that what we have | achieved is only this: we have replaced the logical intuition of the validity of inferences by the intuition of the (formative or descriptive) character of the signs of the language in question.

I believe that this is, indeed, an achievement. It is an achievement because in many cases – say, in Aristotle's or Boole's or Frege's and Russell's systems – the distinction between formative and descriptive signs is fairly obvious, and accordingly Tarski's definition can be applied with considerable success. But that we have not really solved the problem we started to solve seems obvious enough.

27 | Carnap seems to take a more optimistic view than Tarski. He points out, very forcefully, the devastating consequences of Tarski's scepticism. One of these consequences is that we could no longer maintain the distinction between logically true and factually true statements; this distinction which has been widely used by philosophers ("analytic" or "tautological" versus "synthetic" propositions), Carnap considers (rightly, without doubt) as "indispensable for the logical analysis of science"[4].
28 Yet | Carnap admits[5] that "no satisfactory precise definition . . . is known" of formative (or descriptive) signs.

From what has been said, it seems clear that the problem (a) of defining valid inference without assuming a classification of the signs of the languages under consideration into formative and descriptive and/or (b) of giving a satisfactory definition of formative and descriptive signs, is perhaps the most urgent and fundamental unsolved problem of general logic.

29 | I may say here that I was inclined for a long time to consider these problems as *insoluble*. One of the reasons was, *undoubtedly*, the influence of Tarski's authority, and the fact that there is, *undoubtedly*, something naïve in the belief that the signs

[4] Carnap ⟨1942⟩, Introduction to Semantics, p. vii.

[5] *Ibid*.

of all languages must be capable of a neat division into two parts. Tarski's criticism of this naïveté is in any case a major logico-philosophical discovery, whether or not
30 this division can be in some way or other justified; and | it seems difficult, in the face of such a discovery, to uphold the main tenets of the view criticized, without laying oneself open to the charge of dogmatic and wishful thinking.

In spite of all that I am now inclined to consider that Carnap's optimism is justified, although in a somewhat modified and restricted way, and that the fundamental problem of general logic is solvable to a very considerable extent. In what follows I shall give an outline of what I believe is a way to a solution.

6. Reformulation of Tarski's Definition

31 | As a first step towards a solution, I shall re-state Tarski's definition in a slightly different form, viz., by replacing the idea of a model by the practically equivalent idea of a form-preserving interpretation.

Let us call the language system for which we wish to give a general definition of valid inference the language-system S_1. Since we intend to give a general definition, we must not assume anything about this language, apart from the fact that it is a language in the logical sense: we assume that it is not only an expressive but also a
32 descriptive language, | that is to say, capable of descriptions which are true or false. Such descriptions are *statements* or classes of statements. (We shall use lower case italics from the beginning of the alphabet as (variable) names for statements, and upper case italics as names of classes of statements. "$a \in A$" should be read "The statement a is an element of the statements class A".) We thus assume that S_1 is a meaningful language, that we understand it as we do, say, English.

We further assume that we have other languages S_2, S_3, etc., about which we
33 make the same assumption. We further assume that all the statements of | S_1 can be translated into the statements of these other languages (but not necessarily *vice versa*: the other languages may be richer than S_1, i.e., contain statements which cannot be translated into S_1).

Besides the normal translations of S_1 into the other languages we shall speak of *interpretations* – of S_1 in S_2, or in S_3, etc. An interpretation is a kind of mock-translation, or a generalized translation. It is based on some set of rules which correlate to certain signs, or sets of signs of S_1, some signs, or sets of signs, in another language, say in S_2. All such rules of interpretation must satisfy the following general principles:
34 (1) The | rules must be such that to every sentence of S_1, a sentence in S_2 is correlated. (2) Within the limits drawn by (1), certain signs (or groups of signs, i.e. phrases) of S_1 may be singled out as "meaningless" in this interpretation, that is to say, they are not correlated to any signs in S_2. (3) All other signs, or a group of signs (phrases) are to be correlated to signs, or groups of signs, in S_2.

It is clear from this description that an interpretation may correlate true statements to false statements, and *vice versa*. We may, for example, interpret the word "All" of
35 S_1, wherever it occurs, by a word of S_2 that means | "some", or "most", or "30%";

or we may treat it as "meaningless", i.e., as redundant. And we can interpret all descriptive terms in whatever way we like. It is clear, furthermore, that if a series of statements which constitutes a valid inference in S_1 is interpreted in this way in S_2, then the correlated series of statements in S_2 will as a rule no longer constitute a valid inference. (This is obvious from the possibility of interpreting "all" as "some".)

We now introduce the concept of an interpretation which *preserves* a certain sign or a class of signs.

(10)[c] If in an interpretation a sign or class of signs is always normally translated,
36 i.e., taken in its proper meaning, then | we say of an interpretation that it preserves this group of signs.

Now we shall again assume, as in Tarski's definition, that the signs of S_1 are classified into two groups, the formative and descriptive signs; and we call an interpretation which preserves all the formative signs a "form-preserving interpretation". With the help of this concept we can restate Tarski's definition of inference in this way:

(11) An inference in S_1 is valid if and only if the following holds: if, in some form-preserving interpretation, all the premises are true, then the conclusion is also true.

37 | Since we here speak of all interpretations – not, perhaps, of interpretations confined to the particular language S_2 which may happen to be as poor as S_1 – the same generality is achieved as in Tarski's definition (as opposed to the definitions (7) or (8)). For if a model exists which satisfies the conclusion but not the premises, then a language can be constructed which contains the appropriate names, and in this language, accordingly, a form-preserving interpretation of S_1 can be given in which all the premises of the inference in question are true and the conclusion ⟨is⟩ false.

We can, if we like, call such a form-preserving interpretation a counter-example
38 (of the alleged inference); and if we | re-define our term counter-example, then we can also define a valid inference by a formula identical with (7), only that the term "counter-example" now means "a form-preserving interpretation that correlates to all premises true sentences and to the conclusion a false sentence". We thus get

(12) An inference is valid if no counter example exists.

Tarski's criticism of his concept of inference remains, of course, completely in force, with all its consequences, for the concept of a form-preserving interpretation
39 is in precisely the same way dependent upon the | division of the signs of S_1 into formative and descriptive signs, as was the case in the original distinction.

39A | However, certain minor advantages arise. We can, for example, give with the help of our method a simple and convincing definition of those statements which are logically true:

(13) A statement a of S_1 is logically true if and only if all form-preserving interpretations of a are true. (The corresponding definition with the help of the idea of a "model" would be: "A statement a of S_1 is logically true if its logical skeleton is

[c] Number added.

satisfied by all models." But this would not do – the models may not fit. We would have to say "by all fitting models", and explain in general what a fitting model is, – not a very easy task.)

39B | Since Carnap rightly emphasises the link between our problem and that of a general characterisation of logically true statements of a language, it is of some advantage if we can show this link clearly. The obvious link between (11) and (13) and the fact that they equally use the idea of form-preserving interpretations, does not need to be further stressed.

39cont. | We now proceed to construct a further alternative and equivalent definition. Let us assume that "I_k", or "I_l", or "I_m", etc., are names of certain form-conservative interpretations. (We do not assume that the set of these interpretations is denumerable; and in general, it will not be denumerable.) In each of these form-conservative interpretations, a certain class of statements of S_1 will be correlated only with *true*
40 statements of the language in which the statements of S are interpreted. | Let us denote the class of all those statements of S_1 which are, in the interpretation I_k, correlated with true statements, by the sign "M_k". It is clear that, although all statements of S_1 which belong to M_k are true in the interpretation I_k, they will not be, in general, true in the language S_1, or in other interpretations, except those statements which are logically true. These will be true in every form-conservative interpretation, that is to say, they will be in every of the classes M_k, M_l, M_m, etc, which are correlated with classes of true statements in the interpretation I_k, I_l, I_m, etc.; in other words, the logically true statements will be those which belong to all these classes.

I shall call the classes M_k, M_l, etc., "fundamental classes". The class of all
41 fundamental classes will be denoted by "*Fd*". Thus "$M_k \in Fd$" | means "M_k is a fundamental class". From what has been said it is clear that the class *Fd* is a class whose elements are the various classes of all those statements of S_1 which are true in the various form-preserving interpretations. We now can define

(14) The class of logically true statements of S_1 is the product of the class of fundamental classes.

or

(15) The statement a is logically true (or in signs: $a \in Lt$) if and only if a is an element of every fundamental class; or in signs (I use the notation of Principia Mathematica) $a \in Lt \equiv M \in Fd \supset a \in M$.

Similarly, we can define again the valid inferences in S_1:

(16) An inference in S_1 with the premises A and the conclusion b is valid (in signs, $A \rightarrow b$) if and only if the following hold: if all the premises in A belong to some fundamental class, then the conclusion b belongs to the same fundamental class; in signs: $A \rightarrow b \equiv M \in Fd \supset (A \subset M \supset b \in M)$.

42 | All these definitions, just as in Tarski's, are semantical (they make use of the concept of truth), and they all are, like Tarski's, dependent on that crucial distinction between formative and descriptive signs.

7. Relativization of the Concept

The solution of our fundamental problem does not, I believe, lie in the more or less
dogmatic assertion that there are signs in every language which are (as it were) "by
43 nature" | formative. It lies, rather, in admitting the relativity of Tarski's concept of
inference, as stressed in Tarski's sceptical criticism of his definition, and in facing
even the most unpleasant and radical consequences of the situation thus created. This
opens the way to constructing a theory which, I believe, can satisfy those who, like
44 Carnap, find it necessary to | demand an absolute distinction between formative and
descriptive signs.

We shall start with the frank admission that the signs of most languages, and
surely of all naturally grown languages, cannot be neatly divided into formative and
descriptive signs, and there may be even languages which do not contain any separate
formative signs. (This should be obvious enough, considering that in practically all
naturally grown languages, and even in many languages constructed by logicians,
45 there are statements constructed of descriptive signs only; take, for | example, the
statement "I won" in English, or even a compound of three component statements like
"*veni, vidi, vici*" in Latin. On the other hand, there are certainly languages, constructed
by logicians – for example, that of Principia Mathematica – which do not contain any
descriptive signs.

In view of this situation, and of our aim to treat the problem in a completely general
way, we shall give up the assumption that we have, as it were, inside information
46 about the nature and classification of the signs of the languages under | consideration;
and we shall, later on, develop a method which does determine the formative signs
"from without", as it were, or by the way in which they affect the general properties of
the language. (This is a very vague way of stating our programme, but it will become
more definite in what follows.)

Having given up the idea that we know how to classify the signs of a language
S_1 into formative and descriptive signs, we shall now start with the (provisional)
assumption that any classification is as good as any other, or, in other words, that we
47 can classify | the signs of S_1 in as many ways as we like into formative and descriptive
signs. We can start by making first a division in which all signs are descriptive and
none formative; we can make a second division by picking out, quite arbitrarily,
one sign and classify it as formative; a third division may pick out another sign
as formative; a fourth division may pick out perhaps two or three; and so on, until
we obtain, ultimately, a division in which all signs are put into the class labelled
48 | "formative signs"[6]. We shall denote the first division – the one in which the class of
formative signs is empty – by the name "D_0", and the last by the name "D_z"; and
unspecified divisions will be indicated by "D_m", "D_n", "D_p", "D_q", etc.

Now to each of these divisions – say the division D_n – there will correspond a set
49 of form-conservative interpretations; form-conservative in the sense that | they are

[6] In general, the number n of different signs will be finite, and therefore the number of different classifications will be also finite, viz. 2^n. But our considerations are not dependent on the assumption that the number of different signs is finite, or the set of different classifications denumerable.

conservative with respect to the class of signs labelled in this division as "formative". Accordingly, to each division D_n there will correspond a class Fd of fundamental classes M_k (where M_k is the class of all statements of S_1 which become true in the interpretation I_k). In order to indicate that the class Fd corresponds to a certain division D_n, we shall now replace "*Fd*" by "Fd_n". Of a class M which is an element of Fd_n we shall say that it is an n-fundamental class. (We observe at once that, since
50 the interpretations corresponding to D_z are | simply the normal translations, Fd_z can have only *one* element M, viz., the class of statements which are true in S_1. On the other hand, the elements of Fd_0 are *all* the various statement classes of S_1.[7])

With the help of the concept "Fd_n", we can now restate our definitions (15) and (16) in a way which makes their relativity, i.e., their dependence on an arbitrary division D_n, explicit:

(17) The statement a is logically true on the basis of the division D_n (or in signs,
51 $a \epsilon Lt_n$) if and | only if a is an element of every class M which is n-fundamental; or in signs: $a \epsilon Lt_n \equiv M \epsilon Fd_n \supset a \epsilon M$.

(18) An inference with the premises A and the conclusion b is valid on the basis of the division D_n (in signs $A \underset{n}{\rightarrow} b$) if and only if the following holds: if all the premises in A belong to some n-fundamental class, then the conclusion b belongs to the same n-fundamental class; in signs: $A \underset{n}{\rightarrow} b \equiv M \epsilon Fd_n \supset (A \subset M \supset b \epsilon M)$.

This explicit recognition of the relativity of Tarski's concept of inference may now be used for a solution of our fundamental problem.

8. A Method of an Absolute Characterization of Formative Signs

52 | On the basis of our explicitly relative concept of inference, we shall now give a general characterization of signs of a language S_1 which we shall call "absolute formative signs". (They must not be confused with Carnap's "absolute concepts".) *They are those signs whose meaning or function in D_n of S_1 can be characterized completely on the basis of our relative concept of inference.*

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[7] That is to say, if the number of statements in S_1 is infinite, Fd_0 is a non-denumerable set.

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Chapter 16
On the Logic of Negation

Karl R. Popper

Abstract This is an unpublished article from Popper's Nachlass.

Editorial notes: The source typescript is from KPS Box 14, Folder 8. It has 8 pages, numbered 1 to 8. There are numbered references but a list of references is missing. We resolved most of the references in editorial footnotes. The text makes reference to two previously published papers by Popper on the same subject. If we exclude "Are Contradictions Embracing?" (Popper, 1943) as a possible candidate, then the typescript seems to have been written in 1947 after "Logic without Assumptions" (Popper, 1947b) and "New Foundations for Logic" (Popper, 1947c) had been published, and before the publication of any of Popper's other articles. The term "minimum calculus" refers to Johansson's (1937) "Minimalkalkül".

1 The object of this paper is to investigate the mutual relationships between various ideas of negation – the classical idea, the intuitionistic idea, two weaker forms, and, ultimately, the "modal" idea of impossibility. Our results depend upon the employment of a metalogical method which allows us to give formalized explicit definitions of the various signs of negation, as well as of the other formative signs of a language.

Our method is entirely metalinguistic; that is to say, we never quote a statement or even a sign from the object language under consideration. The method has been explained in some detail, although in an informal way, in (6) and (7).[a] An outline of a formalization of this method is given in § 1. In § 2 the main results for classical logic are summarized. In § 3, we proceed to the discussion of our topic.

§ 1

Our Universe of Discourse are certain *expressions* of some language L, viz., the statements and statement functions of L and the variables of L. We denote the class of statements (statement functions included) of L by S, the class of variables by V.

[a] Probably Popper (1947b) and Popper (1947c).

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Trends in Logic 58, https://doi.org/10.1007/978-3-030-94926-6_16

We shall discuss these entities in a formalized metalanguage which is, essentially, the language explained by Hilbert Bernays (5)[b], but without the use of the sign of negation. More precisely, we shall assume Hilbert-Bernays axiom-groups I to IV (5, vol 1, p. 66)[c], with the alteration that we use for equivalence the sign "$\leftrightarrow$". In addition
2 we shall use the axioms and rules of inference for quantification, as | developed in (4)[d], (5)[e] or, for example, (9)[f]. We use, furthermore, the two termed predicate "=", as introduced in (5, vol. 1, p. 165)[g], and the sign "$\neq$", without, however, assuming about the latter more than that

$$(x = y \;\&\; x \neq y) \rightarrow A;$$

that is to say, we do not assume "$\neq$" to be defined by "=" and negation.

As variables we use "a"; "b", etc. "a_1"; "a_2" etc; "x"; "y"; "z"; "n"; "w"; "x_1"; etc., that is to say, lower case italics from the beginning and the end of the alphabet, with or without numerical subscripts. (For subscripts, we shall use numerical conditions such as "$1 \leq i \leq n$" etc.) These variables are variable names of the expressions of L, that is to say, the values of the variables are expressions (esp. sentences, or variables, of L).

Our undefined predicates are (apart from "=" and "$\neq$"): "S"; "V"; and "D". "S" and "V" have been explained above; they are one-termed predicates. Instead of "$S(a)$" etc. we shall also write $a \in S$ etc. Our most important term is the n-termed predicate "D".

$$\text{“}D(a_0, a_1, \ldots, a_n)\text{”}$$

asserts that a_0 is deducible from the premises $a_1, a_2, \ldots, a_n$. For various reasons, I prefer to speak of an n-termed predicate "D" rather than of an infinite series of one-termed, two-termed, etc., predicates "D_1"; "D_2"; etc. This, however, is a matter of taste, and those who prefer this second mode of speaking, in the meta-metalanguage, about the predicate or predicates "D", can easily re-interpret our assertions in accordance with their preferences.

We do not employ variable predicates in our metalanguage, but only constant predicates definable in terms of "S"; "V"; and "D".

3 | Although our variables "a"; "x"; etc. are intended to be of one kind, we shall, for the sake of convenience, use "a"; "b"; etc. as variable names of statement and statement functions, "x"; "y" etc. as variable names of variables. More precisely, all our axioms and theorems are understood to be preceded by the suppressed condition:

(1.1) $$\text{“}a, b, \ldots, a_1, \ldots, d_n \in S \;\&\; x, y, \ldots, x_1, \ldots, w_m \in V \rightarrow \ldots\text{”}$$

which assures the appropriate range for the variables. We further assume that S and V have no member in common, i.e. we postulate:

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[b] Hilbert and Bernays (1934, 1939).

[c] Hilbert and Bernays (1934).

[d] Hilbert and Ackermann (1928)?

[e] Hilbert and Bernays (1939).

[f] This reference could not be resolved. Maybe Quine (1940)?

[g] Hilbert and Bernays (1934).

(1.2) $$S\,.\,V = \bigwedge.$$[h]

Apart from our simple variables $a, b \in S$, we shall also use complex variable names for statements and statement functions, viz. (A) variable compound names, such as "$a \wedge b$"; "$a > b$"; etc., (B) variable names for the result of the substitution of a variable for another in a statement (function), such as "$a\binom{x}{y}$", which denotes the result of substituting x for y in the statement (function) a; and (C) the result of the universal or existential quantification, say, of a with regard to x, which will be written "Axa" and "Exa" respectively. All these complex variable names of statements may be substituted for any of the variables "$a, b, \ldots \in S$"; or more precisely, we have to place, wherever necessary, before our axioms and theorems the suppressed condition:

(1.3) $$a, b, \ldots, a\binom{x}{y}, \ldots, Axa, \ldots \in S \rightarrow \ldots$$

So much about our language.

§ 2

The axioms or primitive rules of inference which determine the meaning of "D" are:

4 (2.1) $$D(a_i, a_1, \ldots, a_n) \qquad (1 \leq i \leq n)$$ |

(2.2) $$(D(b_1, a_1, \ldots, a_n) \;\&\; \ldots \;\&\; D(b_m, a_1, \ldots, a_n)) \rightarrow (D(c, b_1, \ldots, b_m) \rightarrow D(c, a_1, \ldots, a_n)).$$

The first may be called the generalized rule of the reflexivity and the second the generalized rule of the transitivity of the deducibility. These two principles suffice as a basis for the theory of statement composition (including the theory of modalities). For the logic of functions, a number of primitive rules must be added which serve to determine the meaning of "$a\binom{y}{x}$". They can be more easily formulated if we first introduce the two termed predicate "DD";

"$DD(a, b)$"

asserts that a and b are mutually deducible from each other:

Definition:

(2.3) $$DD(a, b) \leftrightarrow D(a, b) \;\&\; D(b, a).$$

The primitive rules for "$a\binom{y}{x}$" can now be formulated in this way:

(2.41) $$DD(a, b) \rightarrow DD\left(a\binom{y}{x}, b\binom{y}{x}\right)$$

[h] Popper here writes $\bigwedge$ for the empty set.

$$DD(a, a\tbinom{x}{x}) \tag{2.42}$$

$$DD(a\tbinom{y}{x}, (a\tbinom{y}{x})\tbinom{y}{z}) \qquad (x \neq y) \tag{2.43}$$

$$DD((a\tbinom{y}{x})\tbinom{x}{z}, (a\tbinom{y}{z})\tbinom{z}{w}) \tag{2.44}$$

$$DD((a\tbinom{y}{x})\tbinom{z}{x}, (a\tbinom{z}{x})\tbinom{y}{x}) \tag{2.45}$$

$$DD((a\tbinom{y}{x})\tbinom{w}{u}, (a\tbinom{w}{u})\tbinom{y}{x}) \qquad (x \neq w \neq y \neq u) \tag{2.46}$$

These are all the primitive rules needed as a basis for the (modal as well as ordinary) logic of the functions of first order.

In what follows, we shall introduce as a graphic abbreviation of the expression

$$\text{“}D(a_0, a_1, \ldots, a_n)\text{”}$$

the expression

$$\text{“}a_1, \ldots, a_n/a_0\text{”.}$$

5 | Similarly, we shall abbreviate the expression

$$\text{“}DD(a, b)\text{”}$$

by writing instead

$$\text{“}a//b\text{”.}$$

This is a deviation from the Hilbert-Bernays method of writing, but it saves space, and it has the advantage that it makes our meta-linguistic calculus more readily comparable with the customary object-linguistic one. (For example, “a/b” corresponds somewhat to “the statement designated by a strictly implies the statement designated by b”; and “$a//b$” corresponds similarly to strict equivalence.)

Now with the help of “//”, we can give explicit definitions for our complex variables of kind (A) and (C). The definitions for the compound variable names of statements of classical logic are (read for “$a//b \wedge c$” perhaps “a has the logical force of the conjunction of b and of c”, etc.):

Definition of the Conjunction:

$$a//b \wedge c \ \leftrightarrow\ (d)(a/d \ \leftrightarrow\ b, c/d) \tag{2.51}$$

Definition of the Hypothetical:

$$a//b > c \ \leftrightarrow\ (d)(d/a \ \leftrightarrow\ b, d/c) \tag{2.52}$$

Definition of the Disjunction:

$$a//b \vee c \ \leftrightarrow\ (d)(a/d \ \leftrightarrow\ b/d \ \&\ c/d) \tag{2.53}$$

Definition of the Classical Negation:

$$a//\neg b \ \leftrightarrow\ (c)(a, b/c \ \&\ (a, c/b \rightarrow c/b)) \tag{2.54}$$

Definition of the Alternative Denial:[i]
(2.55)
$$a/\!/b \curlywedge c \;\leftrightarrow\; (d)((d/b \;\&\; d/c \;\&\; b, c/d) \;\leftrightarrow\; (r)(a, d/r \;\&\; (a, c/d \rightarrow c/d))).$$

The last definition is perhaps a little complicated, but we must remember that this one explicit definition replaces both the strong primitive rule of inference and the
6 complicated primitive proposition | of Nicod. (We must not overlook that Nicod, just as Russell and Whitehead, make tacit use of our primitive rules 2.1 and 2.2, and where they deal with functions, also of 2.31 and 2.36.) Definition (2.55) alone suffices for a basis of the classical theory of statement composition, and so do, for example, 2.1 and 2.4 together. (Cp. also my papers (6) and (7).[j])

Some general remarks should be added here about the correspondence between our metalinguistic calculus and the usual objectlinguistic calculi such as the one of Russell and Whitehead.

If we denote

“a is demonstrable”

by the sign

“$\vdash a$”

which corresponds closely to the assertion sign of Frege and Russell, then we can define this symbol as follows:

$$\vdash a \;\leftrightarrow\; (b)b/a \tag{2.61}$$

In the “D”-notation, we might introduce for “$\vdash a$” a one-termed predicate “$D(a)$” by defining:

$$D(a) \;\leftrightarrow\; (b)D(a, b) \tag{2.62}$$

Our various assertions about the sufficiency of our primitive rules and definitions can now be formulated more precisely:

Our rules and definitions in the sense that, whenever “proportion” such as, for example,

“$p \supset p$”

is assertable in the classical system, the demonstrability of the corresponding statement, for example, the formula

“$\vdash a > a$”

is derivable in our metalinguistic system, from our primitive rules and definitions.

7 | A trivial example will explain the situation. By substitution, we obtain from 2.52 and 2.1

$$a/b > c \;\leftrightarrow\; a, b/c \tag{2.71}$$

[i] In the typescript, Popper uses a symbol resembling ↓, which he uses for joint denial in Popper (1947c), for example. We have replaced it here by the symbol $\curlywedge$, which Popper uses elsewhere.

[j] Probably Popper (1947b) and Popper (1947c).

and with the help of either 2.51 or 2.54, we can also obtain the analogous but stronger rule

$$(2.72) \qquad a, b/c > d \leftrightarrow a, b, c/d.$$

Now from 2.71, we obtain, by $c/\overline{b}$ ("$c/\overline{b}$" means "substituting b for c"; this meta-metalinguistic description must be distinguished from the metalinguistic "$a\binom{y}{x}$"), together with 2.1

$$(2.73) \qquad a/b > b$$

which can be written

$$(2.731) \qquad (a)a/b > b$$

and therefore by $a/\overline{b}$, $b/\overline{a}$ and 6.1:

$$(2.732) \qquad \vdash a > a.$$

The general situation may be summed up as follows: The usual object linguistic calculi make use of rules of inference *and* primitive propositions similar to our 2.732. We use *only* rules of inference, including definitions in terms of rules of inference. From these it is possible, with the help of (6.1), to obtain formulae such as 2.732 which correspond to the usual axioms ⟨or⟩ theorems.

§ 3

We now proceed to our real object, the comparison of various alternative ideas of negation. Our main interest is the intuitionistic idea of negation, which will be simply called "intuitionistic negation", and denoted by "${}^{i}a$"; the second, which will be discussed at a later stage, will be called "intuitionistic or modal impossibility", or briefly, "impossibility", and denoted by "$I\,a$". Both concepts seem to me to conform
8 with intuitionistic ideas; this can be | formally established for the first; but I believe that the second concept really comes nearer to the intended meaning.

Apart from the intuitionistic negation "${}^{i}a$", and the classical negation which will be alternatively denoted by "$\neg a$" or "${}^{c}a$", we shall also discuss the negation of Johansson's so-called, but not properly so-called, "minimum calculus" (cp. (5, vol. II, p. 449))[k], which will be denoted by "${}^{m}a$", and the negation of an even more reduced calculus, denoted by "${}^{r}a$".

We shall show that of these negations only "${}^{i}a$" and "$I\,a$" can be explicitly defined, apart from "${}^{c}a$"; that "$I\,a$" can occur in every system in which the others occur without interfering with them; that "${}^{r}a$" and "${}^{m}a$", just because they cannot be explicitly defined, can occur in the same language system with either "${}^{i}a$" or "${}^{c}a$";

[k] Hilbert and Bernays (1939).

and that, just because they can be both explicitly defined, "^{i}a" and "^{c}a", if they occur in the same language system, can be proved to be equivalent – in such a way, namely, that "^{i}a" is absorbed, or assimilated in the presence of "^{c}a", by the latter.

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Chapter 17
A Note on the Classical Conditional

Karl R. Popper

Abstract This is an unpublished article from Popper's Nachlass.

Editorial notes: There are two typescripts entitled "A Note on the Classical Conditional", both in KPS Box 39, Folder 20. Typescript 1 has 8 pages of text plus 4 pages containing 15 footnotes. The pages are numbered 1 to 12. Typescript 2 has 5 pages numbered 1 to 5. The text contains four footnote marks, but there is no accompanying sheet of footnotes. The material in Box 39, Folder 19 shows that the typescripts must have been written in January to May 1952 and that they were presented to some colleagues. In May 1952 Popper sent a version to Joseph H. Woodger for a review; Woodger's response can be found in KPS Box 39, Folder 19. However, this version seems to be different from both typescripts in KPS Box 39, Folder 20, since Woodger's comments do not match with the line numbers of these two typescripts. The logical symbols $\wedge$, $>$, ϵ, $\leftrightarrow$ and $\rightarrow$ were not available on the typewriter. For $\wedge$, ϵ and $>$ the typescript shows empty spaces, and for $\leftrightarrow$ and $\rightarrow$ it shows a horizontal line: —. Whereas in Typescript 1 all the logical signs were filled in by hand, in Typescript 2 this was done only at the beginning, and we thus had to reconstruct the logical signs by relying on the surrounding text and the logical meaning of the formulas. In most cases this was unproblematic. In Typescript 1, footnote 14 contains an erroneous repetition: "we can say that in the presence of one may perhaps say that, in the presence of". We corrected this to "one may perhaps say that, in the presence of". In Typescript 2, rule (5.2) and its explanation appears twice; we have deleted one occurrence.

A Note on the Classical Conditional (Typescript 1)

1

1 There are some logicians – outstanding among them is Brouwer and his school, the "Intuitionists" – who accept neither the classical truth-table characterization of the conditional (or hypothetical, or "if-then" statement; misleadingly also called "material implication") nor that of negation. Intuitionists have offered very serious arguments to support their objections, and they may well be right. But there are others who object to the classical truth-table characterization of "if-then" statements (perhaps because

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it seems to them to disagree with the usage of ordinary language) while accepting the rule[1] (implicit in the truth-table of classical negation) that if a statement a is false, its negation is true. They are, I believe, mistaken.

It is the purpose of this note to show that, in the presence of classical negation (and perhaps[2] also of conjunction), the truth-table of the classical conditional can be shown to be valid, provided two rules (2.1 and 2.2, below) concerning the conditional are granted to be in order – rules which, to my knowledge, have never been seriously challenged.

The argument amounts to the contention that the so-called "paradoxes of material implication" are no paradoxes at all but misunderstandings (re-inforced, if not caused, by the misapplied expression "implies" or "materially implies") and that those who object to the classical conditional while accepting classical negation have to say more precisely whether they object to rule 2.1, and why.

2

There are two rules which appear to be acceptable to the most sensitive analysts of the conditional, (1) the so-called *deduction-theorem* and (2) the *modus ponendo ponens* (or principle of detachment). It is the contention of this note that (in the presence of two so far unchallenged rules involving conjunction, viz. the principles of contraction[3] and exportation) these two rules suffice for the derivation of the truth-table characterization of the classical conditional from that of classical negation.

In order to elucidate my argument, I shall make use of certain abbreviations[4]. In what follows, the letters "a", "b", "c", "d", . . . "x", "y" . . . etc. serve as variables whose values are statements (i.e., as variable for which names of statements may be substituted; only *closed* statements will be considered). The sign "$a > b$" serves as the name of the conditional with the antecedent a and the consequent b, and "$\neg a$" as the name of the negation of a. The assertion that b is logically derivable or deducible from a will be expressed by writing a/b, and the assertion that c is logically deducible
2 from the two | premises a and b by writing $a, b/c$, etc. The assertion "the statement a is logically demonstrable (or logically true)" will be expressed by $\vdash a$.

Using these abbreviations, we can now state the so-called[5] "deduction theorem" as follows.

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[1] Compare *FT,2*, in section 6, below.

[2] As will be shown below, the rules 4.1 and 4.2 involving conjunction are only introduced in my argument in order to derive 3.1 from 2.1. If no objections are raised against 3.1, then the reference to conjunction can be avoided altogether. See also note 14, below.

[3] So called because it allows us to contract *two* premises into *one*, viz. their conjunction. Cp. 4.1 below and my article ⟨Popper, 1947c⟩, p. 233, rule (3.4).

[4] Most of the abbreviations used here have been also used by me in the article ⟨Popper, 1947c⟩ referred to in the preceding footnote.

[5] The name was introduced in ⟨Hilbert and Bernays, 1934⟩, p. 155. The provisos there made are clearly unnecessary here since we are concerned here with closed statements only (as opposed to open statement functions). Moreover, the provisos turn out to be unnecessary in any case, if we keep

(2.1) $$\textit{If } a/b \textit{ then } \vdash a > b$$

in words, "If b is logically deducible from a, then the conditional statement $a > b$, with a as antecedent and b as consequent, is logically true (or demonstrable)."

Similarly, the *modus ponens*, commonly expressed in a notation like the following

$$\frac{\begin{array}{c} a \\ a > b \end{array}}{b}$$

can now be written in the form

(2.2) $$a, a > b / b.$$

It may be remarked that incidentally, a corollary of the *modus ponens* is the following

(2.21) $$\textit{If } \vdash a > b \textit{ then } a/b,$$

owing to the fact[6] that a premise, if logically true (or demonstrable) may be omitted from an inference without impairing its validity. Although 2.21 is a little weaker than 2.2, it can replace 2.2 in our argument (in the presence of 4.1 and 4.2). I mention this fact here without proof, and I shall base our considerations on 2.2 rather than on 2.21. But I should like to mention that 2.2 and 2.1 can be combined into

(2.3) $$\vdash a > b \textit{ if and only if } a/b,$$

which too is sometimes called "deduction theorem" and which is not only valid for the ordinary conditional but which, I believe, expresses fully the tendencies underlying the idea of "strict implication".[7]

in mind the difference between "a/b" and "If $\vdash a$ then $\vdash b$"; cp. my remarks on p. 232 of *New Foundations for Logic* ⟨Popper, 1947c⟩, referred to in footnote 3 above.

[6] This fact (cp. rule (8.2) on p. 232 of my above mentioned article ⟨Popper, 1947c⟩) can be easily proved with the help of of the transitivity principle and the formal definition of demonstrability. But although 2.21 is used below in 4.5, neither 2.21 nor 4.5 form an essential part of our argument; both are referred to only incidentally, and need therefore not be proved here.

[7] If "Na" is the name of the statement asserting that what is stated by a is logically necessary, then we have (cp. ⟨Carnap, 1946⟩, p. 36, C1-2)

(·) $$\vdash Na \textit{ if and only if } \vdash a$$

This, together with 2.3, yields

(··) $$\vdash Na > b \textit{ if and only if } a/b$$

where "$Na > b$" is the name of the strict implication with the antecedent a and the consequent b. We see from this that 2.3 remains valid even if we interpret $a > b$ as a strict implication rather than as an ordinary If-then statement. See also note 14 below.

3

In order to achieve our aim, i.e. in order to show that the truth-table characterization can be obtained from that of classical negation, we need, as mentioned, in addition to 2.1 and 2.2, two rules (to be numbered 4.1 and 4.2) involving conjunction. The role of these two rules 4.1 and 4.2 in our argument is, however, restricted to the derivation, from 2.1 and 2.2, of a rule 3.1 which does not involve conjunction, viz. of the following slightly strengthened form of 2.1, the deduction theorem:

(3.1) *If* $a, b/c$ *then* $a/b > c$

in words: "If c can be deduced from the premises a and b, then $b > c$ can be deduced
3 from the single premise a." (Example: If "Socrates is | mortal" can be deduced from the two premises "All men are mortal" and "Socrates is a man", then the conditional "If Socrates is a man then Socrates is mortal" can be deduced from "All men are mortal".)

Rules 3.1 and 2.2 are sufficient for our purpose. It may be mentioned, however, that for reasons of symmetry, 2.2 may be replaced by the equivalent rule (which is the converse of 3.1):

(3.2) *If* $a/b > c$ *then* $a, b/c$

This rule can be obtained from 2.2 as follows. We first have

(3.21) *If* $x, b/c$ *then, if* a/x *then* $a, b/c$

This rule is one of the forms of the *principle of transitivity of deducibility*; it is demonstrable[8]. It asserts that, if a/x, i.e., if a is logically stronger than x (or at least as

[8] For other forms of the transitivity principle cp. pp. 197ff. of my above quoted article ⟨Popper, 1947c⟩. Rule 3.21 is not only valid, but it can be proved to be *absolutely valid*, i.e. *valid irrespective of the logical form of the statements involved.* For the concept of absolute validity, cp. my paper *Logic without Assumptions* ⟨Popper, 1947b⟩, esp. p. 277. (The definition, in note 12 on this page, of the absolute validity of a *rule* of inference covers unconditional rules of inference, such as "a/a" or "$a, b/a$" (cp. the next note and section 7). In order to cover conditional rules of inference (with one or more antecedents), such as 3.21, the following obvious definition should be added to this note. "A conditional rule of inference is absolutely valid if, and only if, every statement preserving counter-example of the consequent of the rule is a statement preserving counter example of (at least one of) its antecedent(s)." It then becomes quite clear that (as is indicated the first italicized phrase on p. 285 of *Logic without Assumptions* ⟨Popper, 1947b⟩) an inference, or a rule of inference, is absolutely valid if, and only if, it is valid *irrespective of the logical form of the statements involved.* Of course, every inference or rule of inference which is absolutely valid can be shown to be valid.

I may perhaps point out that the criticism ⟨McKinsey, 1948⟩ of "*Logic without Assumptions*" ⟨Popper, 1947b⟩ and of my paper ⟨Popper, 1947c⟩ quoted above is invalid as it stands (I do not, of course, wish to assert that no valid criticism of some of the points of these papers can be made). It is based, apart from other misunderstandings, on an interpretation of my notation "$a, b/c$" or "$a_1, \ldots, a_n/b$" which is contrary to my explicit and repeated explanation (see for example (2) on p. 261 and p. 277 of "Logic without Assumptions" ⟨Popper, 1947b⟩, pp. 194f. of my article ⟨Popper, 1947c⟩, and the explanation given in the present paper) that "$a, b/c$" is simply an abbreviation of strong as x), then we can replace, in an inference $x, b/c$, the premise x by a, without impairing the validity of the inference. From 3.21 we obtain, by substituting "$b > c$" for "x"

(3.211) *If* $b > c, b/c$ *then, if* $a/b > c$ *then* $a, b/c$

On the other hand, the *modus ponens*, 2.2 can be written

(3.22) $b > c, b/c$

But 3.211 and 3.22 yield 3.2.

Since I mentioned that 3.2 and 2.2 are equivalent, I may indicate that 2.2 can be, in its turn, obtained from 3.2 by substituting in 3.2 "$b > c$" for "a" and combining the result with

(3.231) $b > c/b > c$

which is obtained by substitution from the "absolutely valid rule"[9]

(3.23) a/a

Rules 3.1 and 3.2 can be combined into one, by writing

(3.3) $a/b > c$ *if and only if* $a, b/c$

Either 3.3, i.e. 3.1 and 3.2, or 3.1 and 2.2, suffice for our purpose.

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the (metalinguistic) statement or rule "c can be deduced or derived from the two premises a and b" (or "c follows from these premises"). It simply states that there is an inference, and must be therefore clearly distinguished from the assertion that a certain inference is *valid*. "By an inference, *valid or invalid*, we shall understand ... a number of statements ... of which one is marked out for a conclusion and the others for premises (for example, by writing the conclusion last, below a horizontal line, etc.)", I wrote on p. 261 of "Logic without Assumptions" ⟨Popper, 1947b⟩. (Italics added now.) The question whether such an inference is *valid* (which is the same as the question whether the statement which states that there is an inference is *true*) is a question belonging to the semantics of the metalanguage in which the inference is stated, and to which the sign "/" belongs. (This is stated explicitly in note 18 on p. 280 of "Logic without Assumptions" ⟨Popper, 1947b⟩.) It is therefore hardly adequate to say of my notation, as the reviewer does, "it is not clear, however, whether this notation is intended to indicate arbitrary inferences or is to be restricted to absolutely valid inferences". (Note also that in the article ⟨Popper, 1947c⟩ which introduces the same notation absolute validity plays no role whatever.) But with the realization that my concepts "valid" and "absolutely valid" are to be clearly distinguished from "/", the reviewers criticism collapses. My definiendum "valid" and my defining terms "absolutely valid" (or "absolutely valid rules") and "inferential definition" all belong to the meta-metalanguage (as pointed out in several places, e.g. in note 18 on p. 280), and the dilemma which the reviewer finds, owing to his belief that "/" is one of the defining terms, never arises.

[9] The rules "a/a" and "$a, b/a$" or "$b, a/b$" (cp. 3.23 and 7.2; 6.11 and 7.1; and 5.11) are, as mentioned in the preceding note, absolutely valid, and this fact can be easily demonstrated. Those who, like Professor G. E. Moore, feel uneasy about "a/a" and doubt whether we can deduce a statement from itself, are simply mistaken. The reasons underlying their misgivings are discussed in section 7, below.

4

Those who accept 2.1 and 2.2 but are doubtful about 3.1 may be perhaps persuaded into accepting 3.1 once they have satisfied themselves that 3.1 can be obtained from 2.1 and 2.2 with the help of the following two rules of involving conjunction (we use "$a \wedge b$" as the name of the conjunction of a and b). The first is the extremely trivial
4 *principle of contraction* (of contracting two premises into one): |

(4.1) $$\textit{If } a, b/c \textit{ then } a \wedge b/c$$

in words, if c can be deduced from the two premises a and b then it can also be deduced from the single premise which is the conjunction of a and b.

The second is the well-known *rule of exportation*:

(4.2) $$\textit{If } \vdash (a \wedge b) > c \textit{ then } \vdash a > (b > c)$$

Combining 4.1 with 2.1 (after substituting in 2.1 "$a \wedge b$" for "a" and "c" for "b") we obtain

(4.3) $$\textit{If } a, b/c \textit{ then } \vdash (a \wedge b) > c$$

and further, with the help of 4.2,

(4.4) $$\textit{If } a, b/c \textit{ then } \vdash a > (b > c)$$

From 2.1 (a consequence of the *modus ponens*, as mentioned above) we obtain, by substituting "$b > c$" for "b"

(4.5) $$\textit{If } /a > (b > c) \textit{ then } a/b > c$$

Combining 4.4 and 4.5, we obtain 3.1.

5

We now consider the following consequence of 3.1

(5.1) $$b/a > b$$

is far from self-evident, but which nevertheless can be obtained by substituting, in 3.1, "a" for "b" and "b" for "a" and for "c", and by combining the result with

(5.11) $$b, a/b,$$

i.e. one of the demonstrably valid[10] rules of inference.

[10] Cp. the preceding footnote, and the discussion in section 7, below.

We shall show, in this section, that the first and the third lines of the classical truth-table of the conditional are a consequence of 5.1, and the second line a consequence of the *modus ponens* 2.2.

The *truth-table characterization of the classical conditional* (for short, *TFTT*) is as follows

(*TFTT*)

a	b	$a > b$	lines
T	T	T	1
T	F	F	2
F	T	T	3
F	F	T	4

This table has to be read, each line, from the left to the right, in the way indicated by the following example

Line 3 of (*TFTT*) reads "If a is false and b is true, then $a > b$ is true."

Using obvious abbreviations, we can write:

(*TFTT*,3) *If* $a \in F$ *and* $b \in T$ *then* $b > c \in T$.

5 | In order to obtain (*TFTT*,1) and (*TFTT*,3) from 5.1, we merely consider the principle that, *if an inference is valid and the premise(s) are true, the conclusion must be true*.

Assume $b \in T$; it is clear that, in view of this principle and 5.1, $a > b \in T$, whether $a \in T$ or $a \in F$. Thus we have indeed

(*TFTT*,1) *If* $a \in T$ *and* $b \in T$ *then* $b > c \in T$.

(*TFTT*,3) *If* $a \in F$ *and* $b \in T$ *then* $b > c \in T$.

Next, we consider the *modus ponens* 2.2, in the form

(5.2) $$a, b > c / b$$

Assume $b \in F$. Then, clearly, at least one of the two premises, either a or $a > b$, must be false too (since if both were true, b would have to be true). Accordingly, if $a \in T$, we obtain $a > b \in F$, i.e.

(*TFTT*,2) *If* $a \in T$ *and* $b \in F$ *then* $b > c \in F$.

We find that 5.1 which is a consequence of 3.1 and the *modus ponens* 2.2 allow us to establish lines 1 to 3 of the truth-table (*TFTT*).

It may be remarked that all the argumentation and the results obtained so far allow of an *intuitionistically valid* interpretation[11].

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[11] The three first lines of (*TFTT*) become intuitionistically valid if we interpret "$a \in T$" as "a is logically necessary" (or "demonstrable"), and "$a \in F$" as the intuitionistic negation of "$a \in T$", i.e. as "it is impossible that a is necessary" or in other words, as "a is not necessary" (or "non-

6

In order to obtain line 4 of (*TFTT*), we shall make use of the *truth-table characteristic of classical negation* (for short, *FT*).

(*FT*)

a	$\neg a$	lines
T	F	1
F	T	2

This allows us to assert

(6.01) $$\textit{If } a, b/c \textit{ then } c, b/\neg a$$

a principle much used in the classical theory of "indirect reduction".[12] We can establish 6.1 by considering that, if "$a, b/c$" is valid and the conclusion c false, either a or b must be false, so that $\neg a$ follows from $\neg c$ and b.

Substituting in 6.01 "a" for "c", and "c" for "b", we obtain

(6.02) $$\textit{If } a, c/a \textit{ then } \neg a, a/\neg c$$

If with this we combine

(6.11) $$a, c/a$$

which, of course, is the same rule as 5.11, then we get

(6.12) $$\neg a, a/\neg c$$

and, by 3.1 (and appropriate substitution in 3.1)

(6.13) $$\neg a/a > \neg c$$

6 Here we can further substitute "$\neg b$" for "c"; we obtain |

(6.14) $$\neg a/a > \neg\neg b$$

All this is intuitionistically valid. An intuitionistically invalid argument allows us to replace, in view of the truth-table (*FT*), "$\neg\neg b$" by "b"; the result which corresponds to 5.1, viz.

(6.1) $$\neg a/a > b$$

demonstrable"). Under this interpretation, the last line of (*TFTT*) is clearly incorrect, since even on the classical interpretation, not every if-then statement whose antecedent and consequent are both non-necessary is true (to say nothing of necessarily true).

[12] It allows us to say that, if a syllogism $a, b/c$ is valid, the syllogism whose premises consist of the negation or contradictory of the original conclusion and one of the original premises, and whose conclusion is the contradictory of the other original premise, must be valid too.

happens nevertheless to be again intuitionistically valid. To this result we now apply line 2 (which is intuitionistically invalid) of the truth-table (*FT*)

(*FT,2*) $\quad$ *If* $a \in F$ *then* $\neg a \in T$

Assume $a \in F$; then, by (*FT,2*), $\neg a \in T$, and in view of 6.1, $a > b \in T$. Thus we obtain irrespective of the truth or falsity of b,

(6.2) $\quad$ *If* $a \in F$ *then* $a > b \in T$

But this establishes lines 3 and 4 of (*TFTT*).

We have thus completed the derivation of (*TFTT*). Our argument, up and inclusive 6.1, allows of an intuitionistically valid interpretation. But 6.2, based on the truth-table (*FT*) for classical negation and with it, line 4 of (*TFTT*), are intuitionistically invalid.

7

It is a fact that 2.1, 2.2, 4.1, and 4.2 are intuitively convincing to some philosophers to whom even the first three lines of (*TFTT*) are highly suspect; but we have shown that these three lines depend upon 3.1 and 2.2, and therefore on 2.1, 2.2, 4.1, and 4.2. It is also a fact that some of these philosophers accept the truth-table of negation (*FT*), but are highly suspicious of line 4 of (*TFTT*). If we trace back our steps, we shall find, I believe, that line 2 of (*TFTT*) is usually felt to be ⟨as⟩ unobjectionable as the *modus ponens* from which we derived it; it is lines 1, 3, and 4 of (*TFTT*) which are felt to be objectionable, and so are 5.1 and 6.1, from which lines 1, 3, and 4 can be derived.

Let us therefore consider the derivation of these two objectionable rules, 5.1 and 6.1. Both their derivations depend upon 6.11 which may be rewritten

(7.1) $\quad a, b/a$

It seems to me that it is this rule which causes the trouble. It is a rule which, like the rule 4, i.e.

(7.2) $\quad a/a$

is never used in the ordinary every day reasoning. We do not ordinarily deduce "Socrates is mortal" from the two premises "Socrates is mortal" and "Peter is stingy";
7 nor do we ordinarily deduce "Socrates is mortal" | from the single premise "Socrates
is mortal". Nevertheless, such deductions are undoubtedly valid[13], since they will unfailingly lead to a true conclusion whenever the premise(s) is (or are all) true. The

[13] They are even valid irrespective of the logical form of the statements involved, i.e. absolutely valid; and this means that every (statement preserving) interpretation which renders all the premises true must also render the conclusion true. Thus truth is "necessarily" transmitted from the premises to the conclusion – which means that the conclusion can be validly derived from the premises. Cp. notes 9 and 10 above.

reason why these inferences are intuitively unconvincing is, simply, due to the fact that they are not ordinarily used; not because of their unreliability, but rather because of their excessive triviality. They are never needed, except, of course, in such technical arguments as those which we are carrying out here. But although they are intuitively unsatisfactory, they are unavoidable consequences of intuitively satisfactory rules. For example, 7.2 is an unavoidable consequence of the transitivity principle in the form

$$\text{(7.3)} \qquad \textit{If } a/b \textit{ and } b/c \textit{ then } a/c$$

together with the ordinarily accepted rules (of which the second, however, is intuitionistically invalid)

$$\text{(7.4)} \qquad a/\neg\neg a \textit{ and } \neg\neg a/a$$

as one sees at once by substituting "$\neg\neg a$" for "b" and "a" for "c" in 7.3.

The fact that 7.2 unavoidable follows from 7.3 and 7.4 shows that in these matters we cannot rely on intuition based upon ordinary usage; the simple reason being that intuition based on ordinary usage becomes unreliable in all those borderline cases for which there is no ordinary usage (for example because of their triviality). I call these borderline cases "zero-cases", because the question whether or not a can be validly deduced from a is, in the ordinary usage of the word "deduced" as queer and as unsatisfactory as the question whether or not zero is a number ("I have a number of books" – "How many?" – "Zero".) What I wished to suggest in this note is that, owing to the connection between (*TFTT*) and the zero-case rules 7.1 and 7.2, (*TFTT*) itself is of the zero-case character and that, therefore, intuitive objections to lines 1, 3, and 4 of (*TFTT*) (except these which are based upon a rejection of *FT*) may be dismissed, as long as they are not substantiated by an intuitive criticism of 2.1, 4.1, and 4.2[14].

---

[14] If we consider the following three concepts (a) strict implication, characterized in note 7 above, (b) the intuitionistic conditional (called by Hilbert-Bernays, "positive identical implication") and (c) the classical conditional, then one may perhaps say that, in the presence of the *modus ponens* 2.2, (a) corresponds to 2.1 in a similar way in which (b) corresponds to 3.1 and (c) to 8.01.

Indeed, 4.2 is characteristic of the difference between 2.1 and 3.1⟨, and⟩ is not valid for strict implication (its converse is, however, valid); nor is the deduction theorem in the form 3.1 valid for strict implication. (This was shown by Ruth C. Barcan ⟨Barcan, 1946, p. 117⟩, with the help of a matrix given by ⟨Parry, 1934⟩. Ruth Barcan does not, however, mention the validity of the deduction theorem for *one* premise (or one "hypothesis"), i.e. of 2.1. Were 3.1 (or 4.2) true of a strict implication, then this strict implication would be the same as the intuitionist conditional, since all positive identical formulae involving the conditional can be obtained from 3.3 alone (i.e. from 3.1 and 2.2, using, in addition, only absolutely valid rules, i.e. rules involving no reference to logical form). Similarly, *FT,2* is invalid for the intuitionist conditional, and so is 8.01, 8.11 and 8.22 (because the idea of a generalized inference involves a kind of "or" which, because of its connection with the law of the Excluded Middle, is intuitionistically unacceptable).

8

This concludes my main argument. By way of an appendix, I wish to state briefly a further method of obtaining the truth-table *TFTT* from one simple rule of inference.

The first method is based upon a generalization of the idea of inference. We make use of a notation which allows us to write *more than one conclusion* on the right hand
8 side of "/"; we may confine ourselves to *two* conclusions, and write |

$$(\cdot) \qquad a/b, c$$

or perhaps with two premises

$$(\cdot\cdot) \qquad a, b/c, d$$

in order to express the idea that these statements have such a logical form that, whenever all the premises are true, *at least one* of the conclusions (i.e. of the statements on the right of "/") must be true too. We can read (··) *roughly* "*c or d* is deducible from the two premises, *a* and *b*".

Making use of this notation, we can now formulate three rules which are, as it were, generalizations of 3.1, 3.2, and 3.3, viz.:

(8.01) *If* $a, b/c, d$ *then* $a/b > c, d$

(8.02) *If* $a/b > c, d$ *then* $a, b/c, d$

(8.03) $a/b > c, d$ *if and only if* $a, b/c, d$

I shall show that from 8.01 we can obtain lines 1, 3 and 4 of (*TFTT*), and from 8.02 (as from 3.2) line 2, so that we can obtain the whole of (*TFTT*) from 8.03.[15]

By an argument exactly analogous to the one developed in section 5, we obtain (using the absolutely valid rule "$b, a/b, d$")

$$(8.1) \qquad b/a > b, d$$

and, assuming $d \in F$, we obtain as before (*TFTT,1*) and (*TFTT,3*).

In order to obtain line 4 from 8.01, we substitute in 8.01 "*b*" for "*c*"; "*c*" for "*a*"; and "*a*" for "*b*" and "*d*". The result, together with the (absolutely valid rule "$c, a/b, a$" yields

$$(8.11) \qquad c/a > b, a$$

Assuming $c \in T$, we obtain lines 3 and 4 of (*TFTT*).

Line 2 may be obtained by deriving from 8.02 the following generalization of the *modus ponens* (the derivation is exactly analogous to the one of 3.22 from 3.2)

$$(8.22) \qquad b > c, b/c, d$$

Assuming $d \in F$, we obtain line 2 of (*TFTT*).

[15] For the idea of absolute validity see footnotes 8, 9, and 13 above.

A Note on the Classical Conditional (Typescript 2)

1

1 The conditional or hypothetical statement, or "If-then"-statement, has given rise to a great deal of discussion among logicians, from ancient to very recent times. This note does not attempt to argue in favour of any one solution, but it attempts to clear up some of the issues involved by showing that those who wish to reject the *truth-table characterization of the classical conditional* (i.e. that characterisation which has been associated with the unfortunate name "material implication") are unavoidably committed to rejecting things which they do not, as a rule, wish to reject.

2

The following rule (R) appears to be acceptable to the most sensitive analysts of the conditional:

The conditional with the antecedent a and the consequent b is logically true (or tautological; or demonstrable; or analytic; etc.) if and only if b is deducible from a (or derivable from a; or logically implied or entailed by a).

If[1, a] we abbreviate "if and only if" by "$\leftrightarrow$"; if we write "a/b" for "the statement b is derivable from the statement a"; if we write "$\vdash a$" for "a is demonstrable" (or "a is analytic" etc.); and if we use "$a > b$" to denote the conditional with the antecedent a and the consequent b, then the above rule (R) can be written:

(R) $$\vdash a > b \;\leftrightarrow\; a/b$$

I believe that few object to (R). The main object of this note is to analyse the conditions under which (R) can be shown to be equivalent to the well-known *truth table of the classical conditional* (to be denoted by "TTC") to which so many philosophers have objected.

(TTC)

a	b	$a > b$	lines
T	T	T	1
T	F	F	2
F	T	T	3
F	F	T	4

Here, "T" means "true" (whether logically or factually true) "F" means "false" (again logically or factually), and each line of the table has to be read from the left to

[a] Footnotes 1-4 are missing in Typescript 2. In section 5 below it is stated that a/a (5.1) has been established in footnote 1, and that from $a/b > a$ and the end of footnote 1 the following is obtained: $a \epsilon T \to b > a \epsilon T$ (5.7).

the right; line 1, for example: "If a is true and b is true, than $a > b$ is true." This last assertion shall be abbreviated by writing:

$$\text{“}(a \in T \;\&\; b \in T) \rightarrow a > b \in T\text{”}$$

3

We first show that in the presence of certain rules concerning *conjunction* which, to my knowledge, have never been challenged, (R) is equivalent to another (and somewhat more powerful) rule (R^+), viz. to the rule

$$(\mathrm{R}^+) \qquad a/b > c \;\leftrightarrow\; a, b/c$$

Here "$a, b/c$" abbreviates the assertion that c can be validly deduced from the two premises, a and b. Accordingly, whenever $a, b/c$, then, if a is true and b is true, c must also be true; and whenever c is false, then at least one of the two premises must be false.

4

2 | The rules concerning conjunction which are necessary and sufficient for establishing the equivalence of (R) and (R^+) are the following two rules, 4.1 and 4.2.

$$(4.1) \qquad \vdash (a \wedge b) > c \;\leftrightarrow\; \vdash a > (b > c)$$

Here $a \wedge b$ denotes the conjunction of a and b. 4.1 is the well-known rule of exportation and importation. It happens, in its turn, to be a consequence of (R^+) and the following extremely trivial rule:

$$(4.2) \qquad a \wedge b/c \;\leftrightarrow\; a, b/c$$

in words "c is derivable from the one premise which is the conjunction of a and b if and only if it is derivable from the two premises, a and b." The triviality of this is admitted; but triviality of a rule is no reason to suspect its correctness.

The derivation of (R^+) from (R) with the help of 4.1 and 4.2 is easy. We obtain from (R) by substituting "$a \wedge b$" for "a" and "c" for "b":

$$(4.3) \qquad \vdash (a \wedge b) > c \;\leftrightarrow\; a \wedge b/c$$

Transforming the left hand side of 4.3 with the help of 4.1 and the right hand side with the help of 4.2 we obtain

$$(4.4) \qquad \vdash a > (b > c) \;\leftrightarrow\; a, b/c$$

We obtain, again, from (R) by substituting $b > c$ for b:

(4.5) $$\vdash a > (b > c) \leftrightarrow a/b > c$$

Transforming the left hand side of 4.4 with the help of 4.5, we obtain (R^+).

5

We shall now show that (R^+) entails the first three lines of the truth table of the classical conditional (TTC). We shall use for this purpose the following general rules 5.1 to 5.4:

(5.1) $$a/a$$

this has been established in footnote 1.

(5.2) $$a, b/c \leftrightarrow b, a/c$$

i.e., the order of the premises does not affect the validity of a derivation.

(5.3) $$a/c \rightarrow a, b/c$$

i.e. strengthening of the premises does not make a valid derivation invalid.

(5.4) $$a, b/a \text{ and } b, a/a$$

This can be obtained from 5.1 to 5.3. We now obtain from (R^+) by substituting "c" for "a":

(5.5) $$a/b > a \leftrightarrow a, b/a$$

This yields, together with 5.4

(5.6) $$a/b > a$$

3 | From this, and the end of footnote 1, we obtain

(5.7) $$a \epsilon T \rightarrow b > a \epsilon T$$

Result: We have established lines 1 and 3 of the *truth table of the classical conditional.*

6*[b]

We now obtain from (R^+), by substituting "$b > c$" for "a":

(6.1*) $$b > c / b > c \leftrightarrow b > c, b / c$$

Applying to this 5.1, we obtain the following assertion of the modus ponens:

(6.2*) $$b > c, b / c$$

Now assume $b \in T$, $c \in F$, i.e. the case of line 2 of TTC. The conclusion being false, one of the premises must be false, and since b is true, $b > c$ must be false.
Result: we have established line 2 of the truth table of the classical conditional.

It may be remarked that the results so far established hold for intuitionist logic; more precisely under the assumption that "$a \in T$" is interpreted as "the necessity of a is true" and "$a \in F$" as "the necessity of a is false" while $\neg a \in T$, i.e. the truth of the negation of a is to be interpreted as that of the impossibility of a.

6

In order to derive the fourth and last line of the *truth table of the classical conditional*, we shall introduce $\neg a$, i.e. the negation of a. For $\neg a$ we have the following rule[2]

(6.1) $$a, b / c \leftrightarrow a, \neg c / \neg b$$

This yields, together with (R^+), the following principle (used in "indirect reduction")

(6.2) $$a / b > c \leftrightarrow a, \neg c / \neg b$$

and, substituting "$\neg b$" for "a"

(6.3) $$\neg b / b > c \leftrightarrow \neg b, \neg c / \neg b$$

The right hand side of this is true by 5.4, and we obtain

(6.4) $$\neg b / b > c$$

a rule which happens to be intuitionistically valid, and which we may have obtained from assumptions which are weaker than 6.2. Only 6.4 and 6.5 are needed for the result to be established.

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[2] ⟨Footnote missing⟩

[b] In Popper's typescript both this and the next section are numbered 6. We name this section 6* and use rule numbers (6.1*) and (6.2*) instead of the original numbers (6.1) and (6.2), respectively. Further references to rules 6.1 etc. all refer to the rules given in the next section.

In addition to 6.4 we need the following rule which is *part* (i.e. line 2) of the *truth table of classical negation*:

$$(6.5) \qquad b \in F \leftrightarrow \neg b \in T,$$

in words, if b is false, non-b is true. This rule, in spite of its apparent triviality, is crucial for obtaining line 4 of the *truth table of the classical conditional*. It is
4 intuitionistically invalid. (The | reason is that from the falsity of the necessity of b, i.e. from its non-necessity, we cannot conclude to the truth of its impossibility). Assuming 6.5, we obtain from 6.4 immediately:

$$(6.6) \qquad b \in F \leftrightarrow b > c \in T.$$

Result: we have established line 4 (and 3) of the *truth table of the classical conditional*.

7

General results. It is to be noted that some of the so-called "paradoxes of the classical conditional" are contained in the (intuitionistically valid) lines 1 and 3 of its truth table TTC. Those who dislike these paradoxes are bound to reject (R^+), and, if they assert (R), also at least one of the rules for conjunction 4.3 or 4.4.

Those who object to line 4 of the truth table are bound to reject at least either 6.4 or 6.5. The rejection of either 5.1, 5.2, or 5.3 which, of course, would do the trick, does not appear to me an alternative to be seriously entertained, in spite of the fact that it has been entertained by some philosophers; for these rules can be easily shown not only to be valid rules of inference, but even to be "absolutely valid".[3]

It appears to me that (R), 4.3 and 4.4; 6.1 and 6.5 are in accordance with "ordinary language", whatever may be meant by this phrase. This would establish that, in spite of all appearance to the contrary, the truth table interprets the meaning of "if-then" statements of "ordinary language", provided such a language is used consistently.

A kind of ad-hominem argument tending into the same direction is this. The *modus ponens*, i.e. 6.2, is fairly generally accepted (and so is line 2 of the truth table TTC, which is immediately obtainable from the *modus ponens*). Now it can be shown that (R^+) characterizes the conditional as the logically *weakest of all components which satisfies the* modus ponens, or as a statement which follows from every compound statement that satisfies the *modus ponens*. But the usual doubts about the conditional are often expressed in the form: "This argument of yours may be valid, perhaps, if your conditional is the classical one (or is identical with material implication), but it is not valid if we assume another meaning of the conditional, implying that this other meaning is a *logically weaker* one." But this is certainly mistaken, as long as the *modus ponens* is considered a valid argument.

I may briefly show why (R^+) has the alleged force. Assume $f(b, c)$ to be a

[3] ⟨Footnote missing⟩

compound (not necessarily a truth function) of b and c. We say that $f(b, c)$ satisfies the *modus ponens* if and only if

$$b, f(b, c)/c.$$

It is clear that $b > c$ satisfies the *modus ponens* in the sense defined; and it is also clear that every statement a which is stronger than $b > c$, i.e. every statement a for which $a/b > c$ hold⟨s⟩, must satisfy the *modus ponens* in the sense defined; for if the *modus ponens* is valid, i.e. if

$$b > c, b/c$$

and if $a/b > c$, then $a, b/c$ must also be valid (because we have strengthened | the premises of the *modus ponens*; "stronger" is here always short for "stronger or at least equal in strength").

In the light of this consideration, we can characterise $f(b, c)$ as the weakest statement satisfying the *modus ponens* by stating (a) that every statement a which satisfies the *modus ponens* is at least as strong as, or stronger than, $f(b, c)$, i.e. by

$$(\cdot) \qquad a, b/c \rightarrow a/f(b, c)$$

and by stating that every statement a which is stronger than $f(b, c)$, i.e. by

$$(\cdot\cdot) \qquad a/f(b, c) \rightarrow a, b/c$$

But $(\cdot)$ and $(\cdot\cdot)$ together constitute a rule identical with (R^+), but for the substitution of "$b > c$" for $f(b, c)$. Accordingly, (R^+) characterizes the classical conditional as the weakest of those compounds of b and c which satisfy the *modus ponens*.

8

A remark on Intuitionism may be made in conclusion. While contending that the truth table of the classical conditional does not, upon closer inspection, conflict with the usages of an "ordinary language", the tendencies and the consistently developed usages of an ordinary language, I am very ready to admit with the Intuitionists (Brouwer, Heyting) that "ordinary language" usages involve us into difficulties when problems of infinities are involved, and that we may have to sacrifice classical negation, with its characteristic truth table, and especially ⟨6.5⟩ (and the law of the excluded middle). In such an intuitionistic logic, however, (R^+) remains valid, and with it the first three lines (properly interpreted) of the truth table of the classical conditional.[4]

[4] ⟨Footnote missing⟩

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Chapter 18
Three Notes on Derivation and Demonstration

Karl R. Popper

Abstract These are three unpublished notes from Popper's Nachlass.

Editorial notes: The first note (§ 18.1) is a manuscript entitled "Derivation and Demonstration in Propositional and Functional Logic" from KPS Box 36, Folder 20. The second note (§ 18.2) is an untitled manuscript, also from KPS Box 36, Folder 20, to which we give the title "The Distinction Between Derivation and Demonstration". The third note (§ 18.3) is a typescript entitled "The Propositional and Functional Logic of Derivation and of Demonstration" from KPS Box 36, Folder 21. These notes were probably written in 1946 or 1947 since some of their content can also be found in Popper (1947c, § 8). They may represent a first draft of a planned paper on derivation and demonstration that never materialized. In a letter to John C. Eccles of 10 November 1946 Popper (1946b) writes: "[. . .] at present I am writing a paper on 'Derivation & Demonstration'. It is a really important paper, strange as it may sound that there should be anything important in this field." (communicated to us by Brian Boyd). We use the symbol $\Longleftrightarrow$ instead of Popper's $\leftrightarrows$, and $\Longrightarrow$ instead of his $\rightrightarrows$. In the first note footnote 1 is missing, and in the third note all footnotes are missing; we indicate this by writing "⟨Footnote missing⟩".

18.1 Derivation and Demonstration in Propositional and Functional Logic

1 1. The distinction between derivation and proof has been recently emphasized by Carnap[1]. The concepts of derivation and of demonstration (or absolute logical proof) as analysed in the present paper will be found to be only partly in agreement with Carnap's, although they realize at least some of his intentions, and vindicate a considerable number of his formulations. Yet it will be shown that, if contradictions are to be avoided, at least in the field of functional logic, a sharper distinction between rules of derivation and (conditional) rules of proof must be made than that which has been made by Carnap.

[1] ⟨Footnote missing⟩

D. Binder et al. (eds.), *The Logical Writings of Karl Popper*,
Trends in Logic 58, https://doi.org/10.1007/978-3-030-94926-6_18

2 | 2. All symbols used in this paper belong to the metalanguage (or higher languages). It is therefore unnecessary to use Gothic letters. Using lower case italics from the beginning of the alphabet as variable names of statements or formulae of the object language, we shall write "$a \rightarrow b$" for "from a follows b" (or "b is derivable or deducible from a"), and "$a \mathbin{\&} b \rightarrow b$" for "from the conjunction of a and b follows b", etc., and "$\mapsto a$" for "a is proved" (or "a is demonstrable" or "a is logically true" or "a is L-true"). The words "if … then …" (of the metalanguage) will be often replaced by "$\ldots \implies \ldots$"; "if and only if" by "$\iff$". The distinction between a rule of derivation and a conditional rule of proof (or of demonstration) referred to
3 above, will then | be the distinction between an expression like

$$a \mathbin{\&} b \rightarrow b \tag{1}$$

– i.e., "from the conjunction of a and of b follows b" – and one of the form

$$\mapsto a \mathbin{\&} b \implies \mapsto b \tag{2}$$

– i.e., "if a & b are proved, then b is proved".

3. Rules of derivation and rules of demonstration are closely related. Indeed, we shall later prove (t. . .) that

$$a \rightarrow b \implies (\mapsto a \implies \mapsto b) \tag{3}$$

Nevertheless, the two kinds of rules must be clearly distinguished, for we shall also
4 prove that the converse is not true, or in other words that |

$$a \rightarrow b \iff (\mapsto a \implies \mapsto b) \tag{4}$$

is a false statement, in the sense that if (4) is added to the normal rules of a system of functional logic (for example, as formulated in Hilbert-Ackermann), the system becomes contradictory.

4. Yet the distinction between these two rules seems to have been overlooked by most writers, with the result that their systems do not conform with their intentions. As they stand, they are either contradictory (e.g. the functional calculus of Carnap's *Formalization of Logic*[a]), or they cannot be interpreted as a formalization of the logic
5 of derivation (e.g. the system | of Hilbert-Ackermann), or they do not make it clear which of these two possibilities they chose.

This is due to the fact that in any system of propositional and functional logic, the rule (or rules) of substitution must have the status of a rule (or rules) of demonstration, while the rule of implication (modus ponendo ponens, or "Schlußregel"[b]) must have
6 the status of a rule of derivation. But I do not know any system in which these | two rules are given different status. (In Carnap's functional calculus, a rule of substitution[2]

[2] Formalization of Logic, p. 137, rule 10 ⟨Carnap (1943).⟩

[a] Carnap (1943).

[b] inference rule

is formulated as a rule of derivation; in Hilbert-Ackermann's propositional[3] and functional[4] logic, it is made quite clear that all rules are, in our sense, rules of demonstration; and Hilbert Bernays[5] give, in both propositional and functional logic, the two kinds of rules clearly equal status, suggesting that they are rules of derivation, although they are not as explicit as the other authors mentioned.)

7 5. The claim that the usual | procedure has the very undesirable results indicated will be substantiated in the paper. These results can easily enough be avoided; one or two re-formulations of the rules in question would, indeed, be sufficient, (these will be given). Yet it appears to me that a more radical re-formulation is desirable if we are to avoid the feeling of uncertainty and bewilderment which must arise from the discovery that these much discussed and most carefully formulated systems are
8 unsatisfactory. What is | needed is an approach which not only makes it obvious that these mistakes have been made but, moreover, makes it clear why they have been made, and how similar mistakes can be avoided.

6. All the systems in question go back to *Principia Mathematica (PM)*[c]. *PM* was written before the distinction between object language and meta language was made, and it is well known that this has had its unfortunate evil effects – for example, Russell's remark that a conditional "⟨if . . . then . . .⟩" expresses "from . . . follows . . ."
9 | (and, in connection with it, his use of the name "implication" for the conditional). At present, Russell's symbolism, excluding the assertion symbol, is usually interpreted as constituting an object language; the assertion symbol is taken to belong to the meta-language and to mean something like our $\mapsto$, followed by quotation marks; and his principle of inference (modus ponendo ponens) is taken to belong to the meta
10 language. This | interpretation is, undoubtedly, reasonably satisfactory. But it forces us to take the following view of logic (say, of propositional logic):

Logic is (a) the theory of deduction and derivation, (b) the system of L-true (logically demonstrable, or "asserted") propositions. If we look at it mainly as (b), then we obtain (a) as a by-product. For each L-true conditional yields, together with the *modus ponendo ponens*, a valid rule of derivation, and the infinite system of
11 L-true conditionals, therefore, | yields the infinite system of valid rules of derivation. And these are obtained from *one* rule of derivation – the *modus ponendo ponens* – together with a few "primitive propositions". (It will be seen here that if the modus ponendo ponens is not interpreted as a rule of derivation but only – for this is a weaker interpretation – a conditional rule of proof, i.e., applicable only to proved premises, then no rules of derivations will be forthcoming.)

7. But if (a) is our aim – and for the logician, rather the more important of the two aims –, then it is not clear why we should reach it by the roundabout way *via* (b). Why should we not develop the system of valid rules of derivations in a more direct
12 fashion, that is to say, independently of the development of the | L-true propositions (including those which are "primitive")?

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[3] Theoretische Logik, p. 23 ⟨Hilbert and Ackermann (1928).⟩

[4] ibid., p. 56ff.

[5] Grundlagen der Mathematik, p. 63 ⟨Hilbert and Bernays (1934).⟩

[c] Whitehead and Russell (1925–1927).

Some logicians, under the influence of the present tradition, believe that this is impossible, and that any system of the propositional logic must contain "primitive propositions" or "axioms" or "postulates", and that rules of derivation alone would not be sufficient. This view has, obviously, arisen from the discussion of the "unavoidable dualism" which is involved in the fact that propositional logic cannot work with
13 primitive propositions alone – that it needs, | besides, some "rules of inference". But although it certainly cannot do without rules of derivation, it can do without primitive propositions. This has been shown first by G. Gentzen, in his *Untersuchungen über das logische Schließen*[d], and he has also shown that a small number of primitive rules of inference are as sufficient for the construction of the whole infinite system as the usual systems.

14 8. The system to be developed in this paper is | distinguished from Gentzen's "Calculus of natural inference" mainly by the fact that Gentzen's calculus does not attempt to keep the two tasks, (a) and (b), clearly apart, – a prerequisite for making their mutual relations clear – but rather develops rules of proof with incidental rules of derivation.

As opposed to this, I shall develop (a) a pure calculus of derivation, in which no proposition is provable, I shall ⟨b⟩ show that, if a definition of proof is added to this
15 calculus, all L-true | propositions can be shown to be provable, without any further assumption of primitive propositions or axioms.

18.2 ⟨The Distinction Between Derivation and Demonstration⟩

1 In this paper, an attempt is made to present a new solution to one of the oldest and, without doubt, one of the most fundamental problems of logic.

The distinction between derivation and demonstration has played a certain role in the history of logic, from Aristotle down to contemporary mathematical logic. To Aristotle, syllogisms are of interest mainly as instruments of demonstration. But Aristotle is quite clear that not every syllogism *is* a demonstration. There are valid syllogisms, he insists, with contingent, or even with false premises, and these are not demonstrations. A demonstration, to Aristotle, is a syllogism with necessary premises.

We are thus led to distinguish between, on the one side, inference which – although valid – is not demonstrative, and on the other side demonstrative inference; or, as we shall say, between *derivation*, and *demonstration* (or *proof*).

The relation between derivation and demonstration has been interpreted, ever since Aristotle, as one between genus and species; that is to say, demonstration has always been interpreted as a *kind of derivation*. Demonstration was considered to be a derivation from particularly important and reliable premises, – from definitions,

[d] Gentzen (1935a,b).

or from axioms, or from primitive propositions, etc.[1] The present paper attempts to
2 show that this view is | misleading, and that it prevents us from a clear understanding of derivation as well as demonstration; that is to say, from an understanding of logic.

According to the theory here to be developed, demonstration is a procedure of which derivation is always a part – the most conspicuous part – but never the whole; a demonstration always contains a derivation (it often contains more than one derivation), but it never is a derivation. The fact that derivations constitute the
3 most conspicuous part of every demonstration explains why | demonstration has been so long considered to consist merely as a special kind of derivation, namely, of a derivation from premises which are considered to be particularly secure; and our final theory will clearly show the extent to which this view is quite justified, even though it cannot be considered in itself as a satisfactory theory of demonstration.

4 | In order to make clear at once what I mean when I say that a demonstration consists of derivations, or refers to derivations, or contains derivations as parts, I shall give examples of some rules of demonstration, and their relation to the rules of derivation (such as, for example, the syllogistic rules of inference).

Let us assume that a conclusion, which we shall call by the name "*B*", can be inferred (with the help, say, of syllogisms) from some premise, or premises, which we shall call by the name "*A*". We then say "From *A* follows *B*" or, in a short-hand notation,

$$\frac{A}{B}$$

5 | Now let us assume that the negation of the statement B – which we denote by the name "B'" – also follows from the same premise or premises A. This, of course, can happen only if A contains a contradiction, that is to say, if A is logically false. We can say:

(1) If $\frac{A}{B}$ and $\frac{A}{B'}$, then A is logically false, or refuted.

We now assume that A can be negated. (If A consists, for example of two premises, C and D, say, then A', i.e., the negation of A will amount to the assertion that either C or D or both are false, i.e., to the assertion that C' or D' or both of these are true.)

6 | In view of (1), we can now say:

(2) If $\frac{A}{B}$ and $\frac{A}{B'}$, then A' is demonstrated.

The conditional statement (1) may be described as a *rule of refutation*, the conditional statement (2) as a *rule of demonstration*.

The antecedent of either of these conditionals, i.e., of (1) or of (2), refers to *derivations*; it has the form "If ... is a valid inference", and it refers, of course, implicitly to a certain body of rules of valid inferences (such as the syllogisms). The

[1] This holds also good for Carnap's theory of the relationship between derivation and proof; see Carnap. (expand this note). Introduction to Semantics, p. 167 ⟨Carnap (1942)⟩.

7 consequent | of these conditionals state that a certain statement (or a conjunction of statements, or a disjunction of statements) is refuted or proved.

We shall make use of a simple short-hand method of writing such conditional *rules of demonstration*; we shall divide the antecedent and the consequent by a horizontal double line, and we shall write "$\vdash X$" for "X is demonstrated". Thus (2) becomes

$$(2^*) \qquad \frac{\dfrac{A}{B} \quad \dfrac{A}{B'}}{\vdash A'}$$

8 | If we look at (2^*) then we can clearly discern the difference between a rule of derivation and a rule of proof. A rule of derivation such as Barbara may be, for example, written thus:

$$(3) \qquad \begin{array}{c} ZaY \\ \underline{XaZ} \\ XaY \end{array} \qquad \text{or} \qquad \frac{ZaY \quad XaZ}{XaY}$$

Another example would be the following very obvious rule of derivation by which we can introduce the conjunction of the statement A and the statement B; we shall use for such a conjunction the name "$A \ \& \ B$", and can write:

$$(\&_i)\ \frac{A \quad B}{A \ \& \ B} \qquad (\&_e)\ \frac{A \ \& \ B}{A} \qquad (\&_{e'})\ \frac{A \ \& \ B}{B}$$

9 The first of these rules states that | from the two premises A, B, we can deduce the conclusion $A \ \& \ B$, and the second[e]

18.3 The Propositional and Functional Logic of Derivation and of Demonstration

1 *Informal Introduction.*

1. The principal purpose of this paper is to show that the propositional and also the functional calculus can be obtained in a system which does not assume any primitive propositions, axioms, or axiom schemata but only "genuine" rules of derivation. By a "genuine rule of derivation" I mean either an unconditional rule such as "from the premise a together with the premise b, the conclusion a can be derived", for which we shall write, using "$\rightarrow$" for "from ... follows (or is derivable) ..."

$$(1) \qquad a, b \rightarrow a,$$

or a conditional rule such as "If from the premise a, the conclusion b is derivable, then, from the premise non-b, the conclusion non-a is derivable"; this we shall write

[e] The manuscript ends abruptly here.

(using, in the metalanguage, "... $\implies$ ..." as an abbreviation for "if ... then ...".

$$a \to b \implies {\sim}b \to {\sim}a \tag{2}$$

We also consider as "genuine" rules of derivation certain slightly more complicated conditional rules such as the rule expressing that derivability (i.e., $\to$) is a transitive relation:

$$a \to b \implies (b \to c \implies a \to c) \tag{3}$$

and rules which refer to derivation rules of the kind (1), (2), and (3).

We shall not make use of absolute derivation rules such as those used by Carnap[1], which state that certain statements or formulae are derivable without premises (or "directly derivable from the zero class of premises"); we do not consider these as "genuine" rules of derivation, but rather as a different way of formulating axiom schemata, and it is our aim to avoid all axioms, however formulated: this is precisely why we confine ourselves to "genuine" *assuming* derivation rules, as described.

2. Starting with a number of primitive (or assumed) rules of derivation, we shall show that our calculus corresponds in so far to the usual calculi that all and only those
2 conclusions can be obtained | from any given set of premises which are obtainable with the help of the customary calculi of propositions and functions which employ axioms or make use of axiom schemata, etc. But although all these derivations can be carried out, none of the well-known logically true formulae (or of the asserted propositions, or L-true sentences) can be proved in the system, although these L-true statements or formulae have a peculiar status in the system.

3. This, however, is merely due to the fact that our system, as a complete system of derivation rules, does not need to make use in any sense of L-true formulae or statements. If, in spite of this fact, we add to our system an appropriate definition of "demonstrable formula" (or of "L-true statement" or of "asserted proposition"), then we can show, again without the use of axioms, that the customary system of L-true statements coincides with the system of statements which are "demonstrable" in the sense of this definition.

4. We thus operate with two connected, but precisely distinct systems – the system of derivation rules, and the system of demonstrable statements. Or in other words, we make a clear distinction between the *logic of derivation* and the *logic of demonstration* (or proof), and we develop the former independently *from* the latter.

This is the main point in which our system differs from earlier systems: Carnap[2], for example, who recently emphasized a distinction between derivation and proof, and who uses these terms, in intention, in the same sense in which they are used here, constructs systems in which practically no derivations can be carried out without recourse to proved sentences, or to proof schematas (viz., to his axiom schematas formulated in terms of "directly derivable from the zero class of sentences"). Gentzen[3],

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[1] ⟨Footnote missing⟩

[2] ⟨Footnote missing⟩

[3] ⟨Footnote missing; Gentzen (1935a,b).⟩

on the other hand, operates without any axioms or axiom schematas; but he, too, does not keep the system of derivations distinct from that of demonstration; on the contrary, he operates, without any clear distinction between them, with rules which, in our system, are clearly distinguished into *rules of derivation* and *rules of demonstration*.

3 5. But this distinction between rules of derivation and rules of | demonstration is, indeed, fundamental. It will be shown that every system which neglects this distinction is vitiated by this neglect. *It either cannot be applied to derivations, or it is contradictory.* For example, the system of Hilbert and Ackermann[4] will be shown to be only apparently applicable to the problem of derivation of conclusions from some given premises; as it stands, it is merely a logic of demonstrations. The very similar system of Hilbert and Bernays[5], on the other hand, and similarly Carnap's functional calculus[6], will be shown to be contradictory.

This result will be established in the paper; but it will be to some advantage to explain it to some extent at once.

I distinguish between a (simple) rule of derivation, such as (1), and a (conditional) rule of demonstration. Writing "$\mapsto a$" for "a is demonstrable"[7], and using "$a \;\&\; b$" as the (variable) name of the conjunction of the statements a and b, we can write, for example,

$$\mapsto a \;\&\; b \implies \;\mapsto a, \tag{4}$$

that is to say, if $a \;\&\; b$ is demonstrable, then a is demonstrable. This conditional assertion of the metatheory must be clearly distinguished from the following unconditional assertion of the metatheory

$$a \;\&\; b \rightarrow a, \tag{5}$$

that is to say, from $a \;\&\; b$ follows a. (4) is a (conditional) rule of demonstration; (5) a rule of derivation.

These two types are closely connected: we shall establish that, generally,

$$a \rightarrow b \implies (\mapsto a \implies \;\mapsto b) \tag{6}$$

that is to say, that if a derivation such as (5) holds, then the corresponding demonstration rule must hold also. It is easily seen that (6) must be a consequence of any definition of demonstrability which is adequate, for (6) is obviously equivalent to

$$\mapsto a \implies (a \rightarrow b \implies \;\mapsto b) \tag{7}$$

that is to say, whatever is derivable from a demonstrable statement must be demonstrable. I suppose that (6) is the reason why derivation rules such as (5) and demonstration rules such as (4) have not usually been distinguished. But the failure to distinguish

[4] ⟨Footnote missing; Hilbert and Ackermann (1928).⟩

[5] ⟨Footnote missing; Hilbert and Bernays (1934, 1939).⟩

[6] ⟨Footnote missing; Carnap (1943).⟩

[7] ⟨Footnote missing⟩

4 them is fatal. For it can be easily shown that (8), i.e., the converse of (6), is not | true; and since

$$(8) \qquad (\mapsto a \implies \mapsto b) \implies a \rightarrow b$$

is false, derivation rules and the corresponding demonstration rules are not, in general, equivalent; a derivation rule is stronger than the corresponding demonstration rule.

That (8) is false can be easily seen by assuming that a is, say, a satisfiable but not demonstrable formula of the functional calculus; for example, a may be the formula

$$(9) \qquad \text{“}\varphi(x) \supset (y)\varphi(y)\text{”}$$

in Russell's notation; this formula is generally satisfied in a universe of no more than one element, and therefore not demonstrable. On the other hand, since it is satisfiable, no contradictory formula can be derived from it. If we choose a contradictory formula and name it “b”, then the antecedent of (8) is true, since “$\mapsto a$” is false; at the same time “$a \rightarrow b$” is false; thus (8) cannot, in general, be true.

We thus have shown that, if “derivable” and “demonstrable” are adequately defined, derivation rules and the corresponding demonstration rules are not, in general, equivalent. Now it is a result of this paper that all rules of inference pertaining to the substitution of free variables must be (conditional) rules of demonstration, otherwise, the identity of all elements represented by free variables becomes demonstrable. On the other hand, every system which contains only demonstration rules clearly is inadequate for the derivation of conclusions from premises which have not been demonstrated. In this way, every system which treats its rules of substitution as being of equal status to its other rules of inference (and this is done by all systems I know of) is inadequate – either it does not contain the logic of derivation or (if it contains a rule of substitution for propositional or functional variables) it is contradictory or (if it contains a rule of substitution for argument-variables) it is satisfiable only in a universe containing at most one element and therefore contradictory if it contains any assertion such as “$0 \neq 1$”, or any assertion to the effect that “$x = y$” is not universally true.

It must be admitted that the system of Hilbert-Ackermann can be very easily
5 amended. All that is needed is, (writing “$a \supset b$” for | the conditional (or material implication) of a and b) to add the derivation rule[8] that, for all closed a and b,

$$(10) \qquad \mapsto a \supset b \implies a \rightarrow b$$

But the alterations needed to rectify the systems of Hilbert-Bernays and Carnap are not so simple (they will be discussed below). Accordingly, a feeling of uneasiness must remain even after the necessary alterations have been made. If an elementary logical system such as that of Hilbert-Bernays which has been used extensively for a dozen years contains an unsuspected contradiction (not a paradox, but simply one or two mistakes), and if another system (Hilbert-Ackermann) apparently practically

[8] ⟨Footnote missing⟩

identical, contains, although not a contradiction, but another, more or less opposite mistake, then, I think, it may be of some advantage not merely to repair it but to choose, instead, a method of formalization in which this type of mistake is not likely to occur; that is to say, a method which, from the very start, distinguishes the logic of derivation from that of demonstration.

6. But there are more direct arguments in favour of this method: the present method, even in so far as it is valid, fails, I believe, to exhibit some very important logical facts (if I may say so). I mean the fact that every “natural” logical proof, although making use of derivations, is not a derivation but something different from the derivation which forms part of it.

Let us take as an example an indirect proof. If we can validly derive, from some statement a, a statement b, and if we can validly derive, from the same statement a the negation of b (we write it “$\sim b$”), then, we argue, a is refuted (we write “$\mapsfrom a$” for “a is refuted”), and, accordingly, $\sim a$ proved. Writing $\frac{a}{b}$ for $a \to b$, and $=\!=$ for $\Longrightarrow$, we obtain the following couple of schemata (a “refutation schema” and a “demonstration schema”):

$$(11) \qquad \begin{array}{c} \dfrac{a}{b} \quad \dfrac{a}{\sim b} \\ \hline\hline \mapsfrom a \end{array}; \qquad\qquad \begin{array}{c} \mapsfrom a \\ \hline\hline \mapsto \sim a \end{array};$$

6 | which can be combined to

$$(12) \qquad \begin{array}{c} \dfrac{a}{b} \quad \dfrac{a}{\sim b} \\ \hline\hline \mapsto \sim a \end{array}$$

This is clearly not a derivation schema, but it contains a reference to derivation; it says that, if certain derivations can be validly made, then a certain statement is proved.

From (12) we obtain, substituting a for b (we write “$\binom{b}{a}$”), together with

$$(13) \qquad \frac{a}{a}$$

the proof schema

$$(14) \qquad \begin{array}{c} \dfrac{a}{\sim a} \\ \hline\hline \mapsto \sim a \end{array}$$

If we make use of the derivation schemata

$$(15) \qquad \begin{array}{c} a \\ \hline b \\ \hline\hline \sim b \\ \hline \sim a \end{array} \qquad \text{and by } \binom{a}{\sim a}, \qquad \begin{array}{c} \sim a \\ \hline b \\ \hline\hline \sim b \\ \hline \sim\sim a \end{array}$$

then we obtain, from (12), $\left(\begin{array}{cc} a & b \\ \sim b, & \sim a \end{array}\right)$,

(16)
$$\begin{array}{c} \dfrac{a}{b} \qquad \dfrac{\sim a}{b} \\ \hline\hline \mapsto \sim\sim b \end{array}$$

which, being derivable from (12) is the intuitionistically valid schema of the direct proof (or proof by excluded middle), and from this we get, provided we use the derivation rule of double negation in the form $\sim\sim a \rightarrow a$, the classical schema of the direct proof.

(17)
$$\begin{array}{c} \dfrac{a}{b} \qquad \dfrac{\sim a}{b} \\ \hline\hline \mapsto b \end{array}$$

In all these examples we can clearly distinguish between the use of derivation rules and of demonstration rules. But the customary method of treating these matters
7 by means of asserted propositions | such as

(18)
$$\mapsto (a \supset \sim a) \supset \sim a$$

– which ⟨corresponds⟩ to schema (14) – although in itself a perfectly valid method, seems to me to hide the fact that a proof must always refer to derivation rules (and perhaps to rules of demonstration), but does not need to proceed from proved or asserted propositions. Indeed, (18) may correspond to proof schemata in different ways. We can obtain from it and the rule of detachment (modus ponendo ponens)

(19)
$$\mapsto a \supset \sim a \implies \mapsto \sim a$$

which is a conditional rule of demonstration, and does not refer to derivations. From (19) we may obtain (with the help of the "deduction theorem" $a \rightarrow b \implies a \supset b$) the rule

(20)
$$a \rightarrow \sim a \implies \mapsto \sim a,$$

which is the same as (14); but the "deduction theorem" is, as a rule, not capable of being formulated in the logical systems in question – it is, at best, a theorem about these systems; in other words, not part of their theory of proof, but a statement about their theory of proof. Besides, the deduction theory is only valid for systems in which a clear distinction is made between a rule of derivation and the corresponding conditional rule of demonstration – a distinction which of course cannot be formulated within these systems. Now it seems to me that, although valid, this method is lacking in clarity as far as the relations between derivation and demonstration is concerned.

On the other hand, our examples show that only if we have a reasonably complete set of rules of derivations (as opposed to rules of demonstration) to refer to, can we hope to make practical use of our derivation schemata; in other words I suggest that it is advisable to treat the theory of proof as logically secondary to the theory of derivation, and to build up the latter independently of the former.

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Chapter 19
Lecture Notes on Logic (1939–1941)

Karl R. Popper

Abstract These are unpublished lecture notes from Popper's Nachlass, which he used in his lectures on logic at Canterbury University College in Christchurch from 1939 to 1941.

Editorial notes: During his time as a Lecturer and Senior Lecturer in Philosophy at Canterbury University College, Christchurch, New Zealand (1937–1945), Popper taught logic regularly. The annual Canterbury University College Calendars[a] of that period list logic courses for different levels (Philosophy I, II and III), and also mention the textbooks used: in 1937 Wolf (1930) (Philosophy I and II), Joseph (1916) and Cohen and Nagel (1934) (Philosophy III), in 1938 Wolf (1930) (Philosophy I and II), Cohen and Nagel (1934) (Philosophy III), from 1939 until 1945 Chapman and Henle (1933) (Philosophy I and II), Stebbing (1930) (Philosophy III). From the textbooks used, the overview of the lecture notes and Popper's remarks in § 19 it can be concluded that "logic" did not exclusively mean deductive logic, but covered also inductive logic and scientific method.

We rely on three typescripts, which we call TS1, TS2 and TS3, from KPS Box 366, Folder 19. TS1 is the earliest; it has 25 numbered pages without a cover page or date. TS2 and TS3 are very similar; they are subsequent versions of TS1 with additions and small alterations. TS2 has a cover page which contains the information "Canterbury University College. Logic. Lecture Notes, 1939. (Philosophy I or II.)". TS3 has the same cover page except for the year, which here is 1941. Both TS2 and TS3 contain one page which on first sight appears to be a table of contents. Since its structure does not correspond to the structure of the chapters which follow, we think it should be read as a systematic description of the different subdivisions of what is commonly called "logic". This systematic overview is marked with the year 1939 in TS2, and with the year 1940 in TS3.

The text reproduced here is based on TS2 and TS3. The first main difference between TS1 on the one hand and TS2 and TS3 on the other is that TS2 and TS3 contain three additional sections at the end of Chapter I (i.e., sections 31–32). Secondly, the entirety of Chapter III, on descriptive and logical constants, is absent from in TS1.

A section beginning with "30.⟨TS2, TS3⟩" indicates that section 30 is contained only in TS2 and TS3. A section beginning with "33.⟨TS2, TS3; 30. in TS1⟩" indicates that the section is contained in all typescripts, but has the number 30 in TS1 and the number 33 in TS2 and TS3. Up to page 17, TS1 corresponds precisely to TS2 and TS3. Starting from page 18 of the typescripts we indicate only the original page numbers of TS2 and TS3; this is also indicated in the margins. The footnotes were marked with asterisks in the original typescript; we have replaced them with numbered footnotes.

[a] Cf. Popper and Prior in New Zealand, Karl Popper: Correspondence and official documents, accessed 1 September 2021.

D. Binder et al. (eds.), *The Logical Writings of Karl Popper*,
Trends in Logic 58, https://doi.org/10.1007/978-3-030-94926-6_19

⟨Overview⟩

Philosophy I or II – Logic.
1939.[b]

I. Formal, or Deductive Logic.
II. Inductive Logic, or Methodology of Science.
III. The Nature of Logic.

I. – Formal or Deductive Logic, is subdivided as follows:–

1. General Introduction to the Problems of Formal Logic.
2. The Logic of Terms.
   A. Theory of the Syllogism.
      (a) The Logic of Categorical Statements.
      (b) The Logic of Hypothetical and Disjunctive Statements.
   B. Theory of Definition.
   C. The Doctrine of the Fallacies.

II. – Inductive Logic is subdivided as follows:–

1. Introduction to the Problems of Inductive Logic.
2. The Logic of Inductive Generalisation.
   A. The Empirical Basis of Science.
      (a) Observation and Generalisation – Experiment and General Law (Hypothesis, Theory).
   B. Scientific Laws in the Making.
      (a) Formulating of Hypotheses.
      (b) Testing of Hypotheses.
   C. The Problem of the Validity of Induction.
      (a) Verification of Laws.
      (b) Probability of Laws.
      (c) Uniformity of Nature.
      (d) Causation.

III. – The Nature of Logic, is subdivided as follows:–

A. Logic as the Theory of Thinking.
B. Logic as the Theory of any Object Whatsoever.
C. Logic as the Theory of Scientific Theories.

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[b] "1940." in TS3.

1 Introduction

I. Preliminary Remarks.

1. The study of Logic is for the most part rather difficult for the beginner for two very different reasons.

Firstly, logic is much more abstract than, for instance, any of the natural sciences as it does not deal with facts or happenings in nature, or with things but with our methods and ways of *describing* such facts or happenings in nature. Clearly, then, such an abstract subject is not easy to grasp.

The other reason which renders logic rather difficult to study is that very many, if not most of the books about the subject are rather obscurely written. Not only is the subject an abstract one – the books about logic are often written in such a way that anybody who is used to reading books critically is handicapped by the lack of clarity and precision, so that he is often doubtful whether he understands correctly what the author intends to express. I should like to quote in this connection the text-book of logic[c] by A. Wolf, who says, in his Preface, "Strange though it may appear, text-books of logic are among the chief sinners against the canons of logical method." And we may well substitute here, for the words "logical method", simply the words "clear and correct formulation."

2. The situation mentioned, namely, the obscurity of many logical books, is due mainly, not so much to the abstractness of the subject as to the rather strange fact that the logicians and even some excellent logicians, who have contributed a great deal to the development of the subject, were *not clear about the nature of their subject.*

To make this point comprehensible we shall consider some examples of other subjects. It is quite easy, for instance, to imagine that somebody may be an excellent mathematician, or an excellent physicist or biologist, yet unable to give a clear answer to questions like the following: "What is the nature of mathematics (or biology, and so on)?"; "What is the nature of mathematical (or biological) objects?"; or, for instance, "What is number?". In most cases the mathematician will know better how to operate with numbers than to answer such abstract questions, and it is therefore not so very strange as it might at first sight appear, that the logicians were, until very recently, in a very similar position: they knew – at least some of them – how to tackle certain logical problems, but were quite lost with regard to the question of the nature of what they were actually doing. Certainly, they tried very hard to give an interpretation of what they had been doing, but those very attempts were precisely the cause of most of the obscurities in their writings.

3. It will be fairly clear that nobody could possibly know what the reason was for the obscurity of logical writings before a really satisfactory answer had been found to the question of the nature of logic. The development of new methods which, after all, led to such an answer was originated by the English logician George Boole (and his successors De Morgan and Jevons). But neither Boole nor Russell and Whitehead

[c] Wolf (1930).

(in their “Principia Mathematica”[d], undoubtedly one of the most important books on Logic ever written) succeeded themselves in giving a satisfactory answer to the question of the nature of logic; it was not before the last five or ten years, as the work of those authors was further developed by others, that such an answer has been given.

Since this development is so very recent, one naturally cannot hope that the answer
2 will soon be generally accepted. | As a matter of fact, one should expect the process of absorption of the new theory to take many years, probably some decades.

I am, therefore, in this elementary treatment, not going to trouble the reader very much with the sometimes technically complicated results of this modern movement, but I shall attempt to make some use of those results – a rather negative one: I shall use them mainly in order to *avoid* the difficulties mentioned above, namely, the obscurities connected with the question of the nature of logic or, as may be said, the question concerning the interpretation of the doings of the logician.

II. What Do We Call Logic?

4. Most text-books of logic start with a *definition* of *logic*, or with an explanation of the *object* or of the *scope* of logic. My opinion about this point is as follows: it is certainly very useful for a student of the subject to have a fair idea about what he is going to study. Such an idea will enable him to understand his reading better. I need not say much about the advantages, because they are fairly obvious, but I do not think that the student needs more than a fairly general idea; for if I should give more than this, for instance, a rather subtle definition of logic or a full theory of its aims and problems, then, I fear, this would have the opposite effect: the reader who obviously cannot be aware of what the reasons for my subtleties would be, would not be able to appreciate them. Not knowing what kinds of difficulties and what controversies underlie these subtleties, he would be unable to see their implications, and would probably feel confused rather than enlightened.

I think, therefore, that the following way will be the best: to give the reader, in this Introduction, first a certain idea of the interests of a logician, mainly with the help of some examples, and after this to give a general outline of the main questions with which this present treatment will deal.[1]

5. With this aim in view it will be convenient to adopt from the very beginning a rough division of the field of logic into two main parts, which we may call, according to tradition:

A. *Deductive Logic*, or *formal Logic*, or *pure Logic* (or, using a more recent terminology: *the logical analysis of the language of science*).

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[1] In the last part of the present treatment – when the reader may have obtained some knowledge of the problems of logic, and of the answers given – I shall try to deal more fully with the question concerning the nature of Logic.

[d] Whitehead and Russell (1925–1927).

B. *Inductive Logic*, or *Logic of scientific method; or Logic as applied to scientific investigations* (or, *the logical analysis of the methods of testing scientific statements*).

In what way these two parts are connected and co-ordinated we shall see later. At present we shall simply remember that there are two such main parts and, without attempting to connect them, we shall try to gain some *idea* about each.

3 III. What Do We Call Deductive or Formal Logic?

6. The centre of our interest within the realm of the part of logic which we shall call Deductive or Formal Logic is the concept of *logical consequence*.

I will try to explain this concept with the help of examples. (I am not going to give a strict definition.) In these examples I will make use not only of the expression: "is a logical consequence of", but also of some exactly equivalent expressions such as "follows logically from", "can be inferred from", "can be logically deduced from", and similar phrases.[2]

Example 1. If we have the two sentences: "Mr. Jones is taller than Mr. Smith", and "Mr. Roberts is taller than Mr. Jones", then the sentence, "Mr. Roberts is taller than Mr. Smith", *is a logical consequence of* the two previous sentences.

Example 2. If we have the two statements: "My copy of the 'Principia Mathematica' is either blue or black", and "My copy of the 'Principia Mathematica' is not black", then we are entitled to make the *logical inference:* "My copy of the 'Principia Mathematica' is blue".

Example 3. From the three statements: "All sons of mortal beings are mortal"; "Edward VII is a mortal being"; and "George V is a son of Edward VII", we are entitled to *deduce logically* the statement: "George V is a mortal being."

Example 4. The statement: "In room No. 48 is a table" is a *logical consequence* of the statement "In room No. 48 there are two tables and eight chairs", but the latter statement is *not* a *logical consequence* of the former; whilst each of the following two statements: "Mr. Jones is taller than Mr. Smith", and "Mr. Smith is less tall than Mr. Jones", *follows logically* from the other.

7. Now a few words about these examples. Everybody will be able to see that there is a certain kind of *relationship* between the statements, from which some other statements *follow logically* (or can be *logically deduced*, or *logically inferred*). This *relationship*, which has such very great significance for logic, is sometimes called *logical deducibility*, or *derivability*. These words are a little clumsy but no doubt they express fairly clearly what is meant.

[2] We are not going to adopt here a distinction between the words "sentence", "statement", "proposition". Some problems which arise from such distinctions will be discussed later.

Using the term *deducibility* we can say, instead of "The statement A follows from the statement B", for example, "The relationship of logical *deducibility* holds between the statements A and B."

Our examples show that it may very well be the case that the statement A follows from the statement B, but that the statement B does *not* follow from the statement A. We therefore have to be careful in using the term "*deducibility*"; the relationship of deducibility is, to a certain extent, similar to, say, the relationship of brotherhood as exemplified as follows: If A is a brother of B, B need not be a brother of A, because B might be the sister of A, and therefore the relation of brotherhood holds sometimes from A to B and not from B to A. Similarly the relation of deducibility. If we are, for a moment, puzzled as to whether we shall say that the relation of deducibility holds
4 between A and B or between B | and A, we remember that we have to take A and B in such an order that the statement "A is deducible from B" would hold.

8. It is clear that, in ordinary life, we make great use of the relationship of deducibility. There is no doubt about it that, obtaining information as in Example 1 (Mr. Jones is taller than Mr. Smith, and Mr. Roberts is taller than Mr. Jones), we usually make at once, *more or less intuitively*, an inference – we know at once that Mr. Roberts would be taller than Mr. Smith, without thinking very much about it, and especially without having studied logic or making use of any logical rules of inferences. In drawing such inferences in ordinary life we are relying on certain feelings or intuitions rather than on clearly formulated analysis. Such a feeling is sometimes called "logical intuition", and there is no doubt that we would hardly be able to make use of any kind of rational knowledge in ordinary life if we did not have the faculty of "logical intuition"; to have such logical intuitions is clearly even more important for a scientist than it is for anybody else. Every scientist, every philosopher, and certainly every logician has very often to draw inferences, and he could never go very far if he had to think a great deal about every inference he had to draw, or if he had to elaborate and analyse every inference in full.

But although the logician, like everybody else, makes use of this logical intuition, he is not very much interested in the fact that such logical intuition exists, or in the question how it works. (That is, however, rather interesting for the psychologist.) His task is not so well characterised by saying that he has to *make* inferences, to *draw* consequences – he makes inferences just as any other scientist does – although one of his main interests is *to analyse* inference, *to analyse* the relationship of deducibility.

To make this point a little clearer: the relationship of deducibility is an object of investigation for the logician in a similar way as, for instance, the relationship of mutual attraction between certain material objects, is an object of study for the physicist; and the logician would be interested in this relationship, so to speak, even if there was nothing like logical intuition. Let us imagine we did not have logical intuition – or rather that we had it in a much less degree than we actually do: then we would still have to make inferences for purposes of ordinary life, and (being unable to rely so much on our intuition) we would have to analyse in a rather awkward way every inference so as to see whether or not the relation of deducibility holds in the case in question. If such were the case, the study of logic would be very important from the standpoint of practical usefulness, because such an analysis would fall into the scope of logic.

9. I have made these brief remarks about logical intuition just in order to make it clear with what we are *not* going to deal.

Coming back now to the question of what the logician *is* going to deal with, we shall consider for a moment the analogy to which I referred when I said that the logician is dealing with the relationship of deducibility between statements in a manner more or less similar to that adopted by the physicist when he tries to investigate certain relationships between physical bodies.

There are certain similarities and certain dissimilarities between those two cases. First I shall mention some similarities:

10. The physicist may, for example, study physical relationships between, say, two
5 pieces of iron, – this magnetic needle | here, for instance, and this key. He is investigating these relations using different means – in fact he will use all the means he can think of: he will make observations, he will try to make certain alterations in objects, and he will, certainly, make use of his brain. All the time he will be mainly interested in the physical bodies and their relationships, and as long as he is *making use* of the different means indicated he will not worry, as a rule, about those means themselves (although certainly those means could be made an object of some other investigation); and he will not remain at this stage of the investigation, namely, just making statements about those two special pieces of metal – he will try to find some *general laws* about the relationship, say, of all pieces of metal of a certain similar kind.

Similarly the logician: he may, for instance, begin with the investigation of the properties of the relationship of certain special statements like the examples I have given above in number 6. In doing so he may make use of different means. He may, for instance, make certain alterations. He may substitute the name "Mr. Brown" for that of "Mr. Smith" wherever it occurs, and in doing so he will observe that the relationship of deducibility still holds between the statements thus altered – a fact which may seem to be quite obvious and even trivial, but which is, nevertheless, very well worth noticing, as it has to do with some fundamental properties of the relationship of deducibility, which could be expressed in the words "this relationship remains unaffected by variations of certain kinds". (Later – in section 28 – we shall have to discuss this point more fully.) In making these investigations the logician will certainly have to use his brain, as the physicist does, but, just as in the case of the physicist, he need not be interested in this fact (in which, however, the psychologist may take a deep interest); hence he will not mix up the problems or objects of his investigations with those mental processes occuring in his thoughts.[3] Like the physicist, he will not be satisfied

[3] Some people find special difficulties in the fact that the logician, whilst investigating inferences, has to *make* inferences; they maintain that the logician (for that reason) has to take a special interest in his own intuitively working brain, or, that the object of logic is, like the object of a certain part of psychology, the investigation of our thoughts, of certain *mental processes* of inference. The comparison with the physicist which I have made above is partly intended to show that we need not worry about such problems; but they will be discussed in a chapter on the nature of logic at the end of this treatment.

with remaining at the stage of investigating merely special objects as in the examples given, but will try to develop certain general rules or laws about deducibility, which laws will perhaps look something like the following:– "If we have one or two or three statements of such and such form then we will be justified in deducing one or two or three statements of such and such (similar or different) form." (One of the most important general rules of this kind will be considered presently. Other more special rules will form the *main point* of our treatment of Formal Logic, namely the famous *Theory of the Syllogism*.)

Now we shall have to turn to the dissimilarities between the interest of the logician and that of the physicist.

11. In the first place, it is rather striking how very much more abstract is the interest of the logician compared with that of the physicist. I need not say much about this point;
6 but it may be of comfort for the novice to know that | logic is not much more abstract than, for instance, grammar (apart from the fact that grammar, which deals, like logic, with formulations, sentences, statements, is not very much concerned with the mutual *relationships* between these objects). We can, roughly, characterise the higher degree of abstractness of logic compared with physics in the following way: the physicist aims at investigating physical objects and formulating *statements* about those objects, whilst the objects in which the logician is interested are statements (formulated, for instance, by the physicist), so that he aims at making statements about those objects – therefore:– at making *statements about statements*. This situation (which will be more closely inspected in the last part of the present treatment) has been sometimes formulated with the help of fairly appropriate slogans such as: "Logic is the science of the sciences", or, "Logic is the theory of the theories".

12. In the second place I want to draw attention to a very simple but at the same time slightly technical point which has often been overlooked, although it is of some significance: the physicist is not tempted to put his very *objects* of investigation on the paper on which he is writing, using them instead of their names. Certainly, it may occur sometimes, perhaps in botany, that a scientist will stick on the paper certain objects about which he is speaking, for instance, certain leaves; but in writing *about* these objects he will not want to put an oak leaf on his paper every time instead of the words "oak leaf". The object put on the paper, therefore, is not used as its own *name*, but merely serves as an illustration. With the logician or the grammarian the situation seems to be slightly different, because he will have no difficulty in writing down the sentence which is the object of his investigation, thus using the object as a name for itself (like a man, who, when writing about matches, would place a match on his paper every time instead of the word "match"). This possibility may perhaps appear to be rather convenient, but actually, – it has turned out to be a source of confusion and lack of strictness, if the object and its names were used without clear distinction. The easiest way to avoid such confusion is to distinguish between the use of a word or of a statement in the ordinary way in which it is a name of a certain object – e.g. of Mr. Smith – and its use as a name for itself, for instance, as the name "Mr. Smith," by putting it, as we have just done and as is customary, in *inverted commas*.

To give some examples: The last two words in the foregoing paragraph form a *name* of a certain object – of the object we call inverted commas. They are not themselves this object (and we do not put them, therefore, into inverted commas). But if we want to speak about the *name* of this object – i.e. about the *words* “inverted commas”, then it is quite natural to put that name, as we have just done, in inverted commas.

What I have just said may appear to some people at first sight terribly complicated and perhaps rather pedantic, but the situation is as follows:

As already stated it is quite customary to make use of the inverted commas in the way indicated; but this custom is very often not strictly observed – especially in logical or grammatical texts, where one would have naturally, to use inverted commas so frequently that the author may feel tempted to spare them. The omission often may do no harm, because it may be fairly obvious what is meant; but sometimes it does. So we are faced with the situation that this custom is observed in ordinary texts, where it is convenient but would not lead to bad consequences if we did not observe it, whilst it is not always used in logical texts, where it is not convenient because we would have to use it too often, but where its avoidance sometimes leads to very bad
7 results. | We could therefore formulate our rule like this: Do not become tired of using inverted commas. And that is all.

Coming back to our comparison between the logician and the physicist, we see that the second point discussed, the dissimilarity (I am not going to discuss further similarities, although there may be more), consists just in the mere possibility of the logician’s putting down the objects of his investigations with black ink on white paper, which is usually not possible for the physicist (who, even if he writes about *ink*, – its chemical structure for instance – would never use his object, i.e. *ink itself*, e.g. a spot of *ink*, instead of the *word* “ink”). Now when the logician uses his symbols according to the ordinary custom, i.e.: when he makes use of inverted commas, then his situation again becomes rather similar to that of the physicist: like the physicist, he then does not use his objects as names for themselves, but he uses *names* (or *symbols*) to denote his objects, and under them often such kinds of names, which consist of *inverted commas plus* this object. These types of names may be called “inverted comma names”. They are, however, not the only type of name used in logic. If we speak about one of the statements which we have used before as examples, for instance the statement, “Mr. Smith is taller than Mr. Jones”, then we can put it, as we have just done, in inverted commas (thereby indicating that we do not want to state something about the objects Mr. Smith and Mr. Jones – neither to *state* that Mr. Smith is taller than Mr. Jones nor, for example, to discuss the question whether Mr. Smith is taller than Mr. Jones). Another way of speaking about this statement would be to refer to the statement which was used as the first example in section 6 (thus using the words “first example in section six” as a *name* of the statement).

Probably the explanation and examples given are quite sufficient, but I think it would be advisable to read them at least twice. The second reading will show that the thing is simpler than it seems to be.

IV. The Rule of the Transmission of Truth (The First Rule About Inference).

13. We will now after all go a little deeper into the matter itself and examine a little more carefully the examples indicated to illustrate the relation of deducibility.

Turn back again to our first example. Imagine it was not *true* that Mr. Jones is really taller than Mr. Smith. Then, obviously we could not be certain that the inferred statement, "Mr. Roberts is taller than Mr. Smith", is *true* – even if the second of the two sentences, from which we inferred the third, were true. But, certainly, it might very well be the case that the first statement "Mr. Jones is taller than Mr. Smith", is false and that the inferred statement, "Mr. Roberts is taller than Mr Smith", is nevertheless true; for Mr. Roberts (but not Mr. Jones) may be taller than Mr. Smith.

To analyse these relations in a more general way we will introduce the following technical terms:–

Such statements or sentences from which we can deduce logically certain other statements we shall call *premises*, and those statements which are deducible from these premises we shall call *conclusions*, or, sometimes, *consequences*, or simply the inferred or deduced statements. It has to be noted that we cannot simply say of a certain statement – for instance, "Mr. Roberts is taller than Mr. Smith" – that it *is*
8 a conclusion, without referring to certain premises from | which it can be deduced; because with regard to some other statements it may be, perhaps, not a conclusion, but one of the premises; and, vice versa, we cannot simply say that a certain statement is a premise (or that certain statements are premises) – without referring to some other statements which can be deduced from them; because it may very well be the case that the former statements are conclusions – with regard to some different statements. It is customary to express this situation with the help of a phrase like: "the concepts 'premises' and 'conclusions' are correlative: they cannot be applied to certain statements without referring to certain other statements."

Making use of the terms "premises" and "conclusions", we now can formulate in a more general way the remarks which we made with reference to our first example.

14. If a statement is a logical consequence of certain premises, then it may happen that one of the premises is false (or even, that all of them are false) – but the consequence may be true all the same; in other words, *in the process of logical deduction FALSITY of the premises is NOT always transmitted to their logical conclusion.* If we have seen this fact and tried with the help of our examples to understand it, to see that it really may happen sometimes, then we are, I hope, prepared fully to appreciate the significance and even importance of another logical rule, which we may formulate as follows:–

If the premises are *all true* then any statement which can be logically deduced from them – i.e., any of their consequences – must be true as well: in other words, *in the process of logical deduction, the TRUTH of the premises is ALWAYS transmitted to their logical conclusion.*[4]

[4] Such properties as are always transmitted by a certain process, P, from one link of the process to another are sometimes called "hereditary with regard to the process ***P***", or, briefly, "hereditary".

In order to show the significance of this result I want to draw the reader's attention to the fact that every use made of inference, – in daily life just as in science, whether we make the inferences consciously or just intuitively, – depends solely on its transmission of the truth just formulated by the *law of the transmission of the truth*, (or, as we may say with regard to the preceding foot-note, the law of the inheritance of truth.) For when do we use inference? We use it when we have certain information which does not exactly tell us at once some things we want to know, but which nevertheless involves the answer to those questions. We therefore use inference to get certain additional or secondary information if certain primary information is given, and we want to be able to rely on that secondary information in the same way as we can rely on our primary information. Therefore, if we were not sure that the relationship of deducibility or the process of logical deduction would invariably, in all cases, transmit the truth of our primary information to the secondary, we could never make any practical use of logical deduction.

9 *Validity of Inferences and Truth of Propositions.*

15. Certainly all this holds only if the statements which we consider to be conclusions are actually logical consequences of the premises; in other words if we are not mistaken in assuming that the relationship of deducibility really holds between the statements under consideration. If we have made a mistake with regard to this question we naturally cannot say anything about the truth or falsity of the alleged conclusions even if we are sure about the truth or falsity of the – equally alleged – premises. (I say "equally alleged" because according to the above-mentioned fact that the terms "premises" and "conclusions" are correlated they would not be premises if the conclusions are not conclusions.)

If somebody erroneously supposes that the relationship of deducibility holds between certain statements and if he therefore "deduces" certain statements from other statements from which they actually never could be deduced, then it is customary to say that his inference or deduction is *not valid*. To be quite correct we should say that he has not made any inference at all, that he was merely claiming to have made an inference, and that his claim was not justified. Strictly speaking, every inference, every deduction, must be valid if it is an inference or deduction at all. (To say of an inference that it is not valid is, if we want to be quite strict, similar to saying of a circle that it is not round.) But nevertheless we will follow the customary usage and speak about valid and invalid inferences in such a way that if we say, "such and such an inference *is valid*", we will remember that this is only short for "such and such a supposed inference *is* actually an inference, – the relationship of deducibility *does hold* between the statements under consideration." Similarly, if we say, "Such and such an inference is *invalid*", we will remember that this will only be short for, "such and such an alleged or supposed inference is no inference at all, – the relationship of deducibility *does not* hold between the statements under consideration."

Using this expression, which is in its suggestiveness rather helpful, we should say that truth is a hereditary property with regard to the process of logical deduction (or: with regard to the relationship of deducibility) whilst falsity is not always hereditary with regard to this process (or relationship).

16. Having made this point clear we can now turn to an analysis of the relationship between the *truth of the statement under consideration and the validity of the inference*.

A. *If the inference is valid then –*

(a) If the premises are all true the conclusion is always true.
(b) If at least one of the premises is false then the conclusion may be either false or true.

B. *If the inference is invalid then –*

(a) If the alleged premises are true the alleged conclusions may be either true or false.
(b) If the alleged premises are false the alleged conclusion may be either true or false.

In other words, if the inference is valid we can be sure of only one of the possible relationships of truth and falsity, namely, that the truth of the premises will be transmitted to the conclusions; and if the inference is invalid we do not know anything about the possible relationships of truth and falsity at all.

We can now tackle the whole thing from the other side and say –

A. *If the supposed premises and the supposed conclusions are all true statements then –*
The alleged inference may be really a valid inference, but it need not be – we cannot be sure of it.

B. *If the supposed premises are all true statements and the supposed conclusion is false then –*
The alleged inference is no inference at all (because the truth has not been transmitted).

C. *If (one or more of) the supposed premises are false statements and the supposed conclusions are true statements then –*
The supposed inference may be valid but it need not be – we cannot be sure of it.

D. *If (one or more of) the supposed premises is false, and the supposed conclusions are false too, then –*
The supposed inference may be valid but it need not be – we cannot be sure of it.

In other words, only in one of the possible cases, i.e., – when the supposed premises are true and the supposed conclusion is false, we can be sure of something: namely, that there is certainly *no* relationship of deducibility between those supposed premises and supposed conclusions – the alleged inference is *invalid*. In all other cases the inference may be either valid or invalid.

It would be *very well worth while for the reader* to find examples illustrating each of the points enumerated above, or at least to use our examples from Chapter ⟨III⟩[e] for this purpose.

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[e] The typescripts leave a blank space where the number of a chapter has to be inserted.

V. The Second Rule About Inference (Converse of the First).

17.[f] The foregoing analysis enables us to draw a rather important conclusion about inference.[5] Suppose we can be sure that a certain inference is valid. Let us suppose, furthermore, that we are not certain whether or not our premises are true. Now we may look at a certain conclusion and, comparing it with the facts, we may discover that the conclusion is false.

It is clear from the analysis of the foregoing chapter that in such a case *at least one of the premises must be false too*. To see this, we have only to consider the following points:– Let us suppose provisionally that all of the premises are true. Then, as we have said that the inference is valid, the conclusion would have to be true as well. But the conclusion is false, therefore the supposition we have provisionally made cannot be correct: if one of the conclusions is false, at least one of the premises must be false as well.

Consider our old example 1.

The inference is valid, the relationship of deducibility actually holds between the premises and the conclusion. Now we may be in a position to compare Mr. Roberts with Mr. Smith and we may find that he is less tall than Mr. Smith, i.e., we may find that our conclusion is false. Such a discovery would justify us in saying that at least
11 one of the premises | must be false as well, because, obviously, it cannot be true that (as we have now discovered) Mr. Roberts is less tall than Mr. Smith – but (according to our premises) taller than Mr. Jones, who, again, is taller than Mr. Smith.

We can therefore add to our first rule of inference, – namely to the rule that the truth of the premises (if they are true) is transmitted by the process of deduction or inference – a second rule, which is something like its converse:–

If the inference is valid, the falsity of any of the conclusions (*if* any of them should be false) *is always re-transmitted to at least one of the premises*.

This rule does not help us to find out which one of the premises is false.

VI. Some Remarks About the Concept of Truth.

18. In the two foregoing chapters I have very often made use of the word "truth". I did so without explaining what I meant by it and I think I was justified in doing so because I am using this word exactly in the usual sense: *we call a statement "true" if it corresponds with the facts*; we will say, e.g., that the statement, "Mr. Roberts is taller than Mr. Smith" is true, if and only if, Mr. Roberts is taller than Mr. Smith. In looking at what I have just said the reader may notice that I have put the statement

[5] As I have explained above (in section 10) we need not worry about the fact that we are, in logic, drawing inferences about inferences; we never can avoid making use of the logical part of our mind in any scientific investigation.

[f] This chapter was named "The Second Rule Of Inference (Converse of the First)." in TS1, and has been renamed to "The Second Rule About Inference (Converse of the First)." in TS2 and TS3.

which is said to be true in inverted commas. I did so for conformity with what was said in 12, for if we say that the statement is true then the object about which we are speaking is this statement (and not the fact which the statement describes). If we say that the statement, "Mr. Roberts is taller than Mr. Smith" is true, then we maintain something *about* this statement – namely, that it corresponds with the fact – but we are not immediately speaking about the fact itself, namely about Mr. Roberts being taller than Mr. Smith; for if we wanted to say something immediately about this fact, we would simply say that Mr. Roberts is taller than Mr. Smith.

Truth, therefore, is a property of statements, namely the property of their corresponding with the facts which they describe. We shall always use the word "truth" in this sense, which, certainly, agrees well with the ordinary everyday usage of the word "truth" (although the ordinary usage is not always quite as strict as the usage which will have to be observed by us).

I wish to add the remark that there has been a long controversy about the concept of truth, but I am not going to deal with this controversy in this treatment.

VII. Truth in the Sciences and in Logic.

19. It is the task of every scientist to do his best to put forward statements which are *true*. He must not attempt to maintain a statement or a theory which is found to be false, even though it may have been first offered by himself.

This does not imply that this task forbids him to offer any statement the truth of which is not completely established. On the contrary, there is no doubt about the fact that science often makes its progress with the help of mistakes, with the help of theories which later turn out to be false. To forbid the scientist to offer theories the truth of which is not completely established would mean the strangling of science and its development, because as a rule this condition cannot be fulfilled, and therefore the
12 scientist would not be permitted | to say anything.

(We could compare the scientist and the statements or theories advanced by him with a mother and her children. She will do her best to see that they are always neat and she will wash them when she finds that they have become dirty – rather than maintain that they are clean when they are not – but she cannot guarantee their future cleanliness.)

Keeping in mind the limitations mentioned, we can well agree with the dictum that *the task of the scientist is the search for truth* – the search for statements or theories which are in accordance with the facts he is investigating.

Clearly the realm of facts in which a scientist is interested varies with the different sciences. The geographer is, for instance, interested in facts which are, as a rule, very different from the facts in which the student of chemistry is interested.

20. All that we have said can be applied not only to the natural sciences but also to logic. The search for truth is the task of the logician too – but, certainly, only in the realm of (rather abstract) facts with which he is concerned.

He therefore won't be interested in the question whether or not Mr. Brown is taller than Mr. Smith, or, in other words, whether or not the statement, "Mr. Brown is taller than Mr. Smith" is true, but he will be concerned with the question whether or not the statement mentioned *follows logically* from the two other statements given in our first example – in other words, whether or not the statement, "The third sentence mentioned in Example 1 is a logical consequence of the two others", is true.

Thus, the logician is interested in logical relations, like deducibility, between statements as statements – false ones just as much as true ones. He, therefore, does not worry about the truth of the statements which (together with their relations) are the *objects* of his investigation, but he does worry about the truth of those statements which he maintains *about* his objects – *about* the statements under investigation, and the relationships between them.

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13 Chapter I. What Do We Call a "Rule of Inference"?

21. We may now be far enough advanced to formulate a rather general view with regard to the nature of what we call logical inference, or the drawing of logical consequences.

Looking back on our examples we can first say the following: drawing a logical consequence presupposes the constructing of a sentence or statement – the conclusion – out of one or more given statements – the premises. This construction, certainly, has to make use of the *material supplied by the premises*. I shall try to explain this phrase a little more fully. Image that our premises do not refer at all, say, to Mr. Brown – that they refer to Mr. Smith and Mr. Jones only. Then, obviously, it would not be possible to draw such a conclusion from these premises which would give us some valuable information about Mr. Brown! Similarly, if we speak in our premises, say about the height only of Mr. Smith and Mr. Jones, then as a rule, we shall not be able to deduce from these premises any valuable information about, say, the place where these people live, or their occupation or education etc.

22. That is what I had in mind when I said that the conclusion has to be constructed out of the material supplied by the premises: it has to be constructed with the help of the terms – the nouns (or names), adjectives, and perhaps some other words – which occur in the premises.

Certainly the analysis just given is not sufficient: we have not yet said *in what way* the conclusion has to be constructed out of the material of the premises. It is plain that we can easily construct some statements if we like, out of the material of some premises, which are *not* conclusions of those premises. To give a very simple example: we may construct out of the material of the statement, "Mr. Jones is taller than Mr. Smith" the statement, "Mr. Smith is taller than Mr. Jones", which latter is clearly not a logical consequence of the former.

23. It will now be understandable that a logical conclusion is a statement which is constructed out of the material of the premises – but *according to more or less specific*

rules of construction; and we may now say that it is the main business of what we usually call "formal logic" to develop these rules. I have said some time previously that the business of formal logic is to analyse the relationship of deducibility. Now, we can explain better what was meant by the term "analyse": it is meant that logic has to give such *rules of construction* which enable us to see whether or not the relationship of deducibility does hold between certain given statements. Usually one calls those rules which are needed to construct the conclusion out of the material supplied by the premises the "*rules of inference*".

(The famous rules of the so-called "syllogism" which are the centre of the traditional treatment of formal logic, and which will play a central role in our present study, are just one attempt to give a complete list of the rules of inference.)

At present, while we are dealing not so much with the solution of the problems of logic, but rather with an introduction to the nature of formal logic, we will not attempt to develop any of the rules of inference.

24. With the help of what we have said in Nos. 13 to 17 we now can consider (not a
14 rule of inference itself but) | something *about* those rules of inference: the rules of inference will have to be of such a kind that in employing them we will be able to construct only such conclusions as will be true always if the premises are true; and such as will be false only if at least one of the premises is false. In other words, the rules of inference have to fulfil the condition that all inferences which may possibly be drawn according to those rules, transmit the truth from the premises to the conclusion, and therefore retransmit (as we have explained above in No. 17) the falsity from the conclusion to at least one of the premises.

This follows obviously from our previous analyses, but what I am going to say now does not follow from them – it goes beyond what we have said hitherto.

It is justifiable to call any rule of constructing some statements out of the material of some other statements a *rule of inference* if this rule fulfills the conditions mentioned above, i.e.: if it leads from true statements only to such statements which are true as well, and therefore: if it leads to a statement which is false, then at least one of the statements which supply the material (at least one of the premises) must have been false too.

If we make the suggested decision to call every rule fulfilling those conditions a *rule of inference* then we can give here something like a definition[6] of the terms (a) "rule of inference" and with the help of it, definitions of what we call (b) logical consequence, and (c) of the relationship of deducibility, – thus repeating and summing up more or less all that we have said hitherto.

(a) A rule of inference is such a rule of constructing a statement – the so-called conclusion – out of the material supplied by some other statement – the so-called premises – as fulfills the condition that the truth of the premises will always be transmitted to the conclusion drawn in accordance with the rule in question (and that therefore the falsity of the conclusion will be re-transmitted to at least one of the premises).

[6] We shall have to deal later in full with the question of what we do call a definition.

(b) Drawing a logical consequence means to construct a logical conclusion according to some rule of inference.
(c) The relationship of deducibility holds between a statement – the so-called "conclusion"[g] – and some other statements – the so-called premises – if the former statement is constructed according to some rule of inference out of the material supplied by the latter statements.

Why is formal logic called "formal"?

25. So far, when we mentioned the expression "formal logic" we just used it as a name for a certain kind of interest or, if you like, for a certain science. I have tried to give the reader some idea of this science with the help of examples and by other means, but I left the name itself, "formal logic", unanalysed. Actually it is not important to know why something has a certain name, for instance why England is called England, or why biology is called biology. Such questions may be of interest to somebody who wants to study the history of the language; but as a rule the understanding of the name would not contribute much towards the understanding of the object to which this name refers.

What I want to explain in the present chapter is therefore not so much the linguistic side of the name "formal logic" (although it may be mentioned that the word "logic" comes from the Greek word "logos" which means, first, word, then, knowledge, science, study and such things). It is rather a certain significant feature of the subject formal logic about which I want to say something, namely, what one often calls its *formal character*.

26. The word "formal" is used by different philosophers in very different ways, and some controversy is due to misunderstandings originating in ambiguous ways of using this word. I certainly cannot attempt to enter into this discussion here, and therefore I do not want to give a definition of the word "formal" (because possibly such a definition might mislead a student who came across some remarks about the same thing written by somebody else who might use the word with a different meaning). But I want to explain something about its use. This I shall do with the help of some examples and by similar means.

One usually contrasts the *formal* view-point with the *material* one. This is done in different subjects – not only in logic but, for instance, in legal administration. It will perhaps be convenient to begin with a few remarks about the usage in the latter subject.

We will illustrate our problem by considering two judges, both, perhaps, excellent in their way, but with a very different attitude towards both the law and the human beings involved.

The one may proceed as the famous statue of Justice – blindfolded, with scales in hand. The judge is applying the letter of the law correctly without considering the human material with which he is dealing. His own exclusive interest is to classify the case in the right way – to find the right pigeon-hole for it, so to speak – and then to apply the law. This is a formal viewpoint, the judge being a formalist.

[g] TS1 has "consequence" instead of "conclusion".

The other tries to do his best in being just but at the same time tries to understand and consider the persons and their relations in their individuality. He understands his task as to help everybody as much as he can (which does not mean that he is necessarily more lenient than the other type). He applies law not because application of the law is his last aim but because it is a means in his attempt to help adequately all persons concerned. He therefore does not try to put every case in a pigeon-hole, but he will be impressed rather by the diversity of the different cases and by the fate of the individuals involved. We may call this viewpoint a material one as opposed to the formal one, because of his interest in the material – in this case human beings – with which he is dealing.

For the formal viewpoint as opposed to the material, it is characteristic that the attitude (of the judge) would not be altered if different individuals were involved in the same type of case.

27. Now let us return to logic. Referring to our Example 1 in section 6: we may have before us, say, three real men, namely Mr. Jones, Mr. Smith and Mr. Roberts; and we may compare them first in order to find out whether Mr. Jones is taller than Mr. Smith and whether Mr. Roberts is taller than Mr. Jones; and if we make some inference in order to obtain some information about the question whether Mr. Roberts is taller than Mr. Smith, – all the time concentrating our interest on those three particular individuals and their relative height.

16 If we proceed in this way then we could say that our interest, our view-point, is a material one.

But we may proceed in a different way. We may look at our statements of Example 1 without being interested in these individual beings, Mr. Jones, Mr. Smith and Mr. Roberts at all. We may not even be interested in the question whether those three ever existed at all – and whether it is true *if* they existed that Mr. Jones is taller than Mr. Smith etc . . . Our interest may be concentrated exclusively on the fact that, *given* such information as is supplied by the two premises of Example 1, we are entitled to deduce the consequences there given. And, most significant of all these points, we shall know that even if we actually find that Mr. Jones is not taller than Mr. Smith (in which case those people obviously do exist), then this information would not have any effect on the point in which we are interested – for we are not interested in these persons, but only in the relationship between certain given statements.

The viewpoint thus described is the one characteristic of the feature of logic to which the word “formal” refers.

28. In No. 10 I mentioned the fact that in substituting the name “Mr. Brown” for that of Mr. Smith *wherever it occurs* in a certain set of statements the relationship of deducibility – if such a relationship holds between the statements in question – would not be affected.

To express the same thing in more general terms: we may take any example of a logical reference we like – and therefore indeed any of the examples 1, 2, 4 and No. 6 –, and we may replace any one term (or any two or any three and so on, . . . different terms) wherever it occurs in the example under conisderation by any other term which fits, so to speak, into its place, and the relationship of deducibility will hold between the new statements thus constructed. I have just used the words "which fits into its place" and I wish to illustrate these words by some examples.

We may, as I have mentioned, replace the name "Mr. Smith" by the name "Mr. Brown". In the same way any other name of a person would fit into the place where the name of the person stood before – we can therefore just as well replace, for instance, the name "Mr. Jones" or the name "Mr. Roberts" by the name "Mr. Brown", and, clearly, by the name "Mr. Hammersmith". But not only can we alter the names – we can easily alter the term "taller" wherever it occurs in Example 1 to the term "less tall" or, perhaps, "richer" or "wiser", and the relationship of deducibility would not be altered. However, should we try to replace the term "Mr. Jones" by the term "wiser", or perhaps "stouter", then such a replacement *would not fit* at all – the series of words so constructed would not make a correct, meaningful sentence at all.[7,h]

29. What we have just discussed, the possibility of replacing certain terms in the sentences under consideration by some other (fit, or as we may say, appropriate) terms, without affecting the relationship of deducibility between the statements, may be regarded as another attempt to characterise the "formal" side of logic. This
17 characterisation | is, perhaps, a little more strict than what we said previously about this point, because it explains the matter without talking about such rather vague things as our "interest". (The reader will remember that the previous explanation of the word "formal" was more or less based on the distinction between our interest in some people or some things or some facts and our interest in the relationship of deducibility between certain statements.) It is, I hope, clear, that it is the same point, namely, the formal character of logic, which I tried to explain with the help of the new formulation; the possibility of replacing the terms by some other terms is obviously closely connected with the fact that it is irrelevant to the specific logical view-point, to which special subjects we actually refer in our statements.

It has to be noticed that not only is the relationship of deducibility – if such a relationship should hold between the statements under consideration – not affected by any appropriate alteration of the terms, but also if such a relationship does *not* hold between the statements under consideration, the appropriate alteration of a term *wherever it occurs* would never transform such a set of statements into a set of statements between which the relationship of deducibility would hold.[i]

30.⟨TS2, TS3⟩ Let us now see how our original examples, e.g., the one about Mr. Smith comply with what we have just said about formal inferences. We have seen that the

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[7] In pursuing this point we would find that all those terms which fit into a certain place in a statement will generally *all* fit into a certain place in another statement, if *one* of them fits into this place; and *none* of them would fit if *one* did not. The class of all terms which fit into one and the same place in a statement is sometimes called a "semantic" or "syntactic category", sometimes a "logical type." The examples given should make it clear that there must be at least *more than one* different logical type (or semantic ⟨category⟩).

[h] The footnote ends abruptly in the typescripts.

[i] Chapter I ends in TS1 with the end of section 29. TS2 and TS3 contain the additional sections 30–32.

particular name “Mr. Smith” is not significant in our deduction; we could replace it just as well by the name “Mr. Field”, or “Mr. *X*”, or simply by “*X*”.

What we have to infer now is: *do such words as “taller” (or “taller than”) have any significance in our deduction?*

Our original example was:

> (Premises:)
> Mr. Jones is taller than Mr. Smith;
> Mr. Smith is taller than Mr. Roberts;
> (Conclusion:)
> Mr. Jones is taller than Mr. Roberts.

It is clear that we can replace in this example the word “taller” by very many other words without influencing, or rather, without damaging the relationship of deducibility. Such words by which we can replace “taller”, are, for instance, “smaller” or “richer”, “poorer”, and so on. And we can equally well replace the two words “taller than” by words like “equally tall” or perhaps, by “is living in the north of”, or “is living in the neighbourhood of”, or, “is a relative of”. (The last example will be of considerable interest in what follows.)

The examples given may suggest that the words “taller than” play a role analogous to the role played by the proper names; it may be supposed, namely, that we can replace the words “taller than” by *any expression which fits* into the place held by these words, or as we have said, by any appropriate expression. But that is not so. We can give what may be called *counter-examples*, i.e. examples of words which fit the place held by the words “taller than”, at the same time destroying the relationship of deducibility. Such counter-examples would be, for instance, “is living next to” (although it is *nearly* the same as “is living in the neighbourhood of”) or, “is the father of” (although it is very near to the example “is a relative of”).

18(TS2, TS3) If we replace the words “taller than” by either of the two counter-examples just mentioned, then it is obvious that, from the premises thus formed, the conclusion would not follow any more;

In other words:

Even if the following two sentences were true:

> “Mr. Jones is living next to Mr. Smith”, and,
> “Mr. Smith is living next to Mr. Roberts”,

the following sentence need not be true:

> “Mr. Jones is living next to Mr. Roberts”.

Similarly, even if the following two sentences were true:

> “Peter is the father of Paul”, and,
> “Paul is the father of Jack”,

the following sentence would be, obviously not true:

> “Peter is the father of Jack”.

In other words, if we substitute such words as “is living next to”, or, “is the father of”, then the truth of the sentences which have been premises is not any more transmitted to the sentence which has been the conclusion. Thus the relationship of deducibility breaks down.

31.⟨TS2, TS3⟩ We call such relations as “taller than”, or, “to the left of”, and so on – in general, such relations that, if the relation holds between A and B and between B and C, then it holds also between A and C – we call such relations “*transitive relations*”. All relations which are transitive can obviously be, in our example, substituted for the relation “taller than” without damaging the relationship of deducibility; and only transitive relations can be substituted.

Keeping this in mind we can say that the two premises:

> “Mr. Jones is taller than Mr. Smith”, and,
> “Mr. Smith is taller than Mr. Roberts”,

are by themselves *not* sufficient for the deduction of the conclusion:

> “Mr. Jones is taller than Mr. Roberts”.

They are sufficient *only if* we know, or if we take into account that, “taller than” is a transitive relation. If we do not know this, or if we do not consider it, then the premises would be insufficient for drawing the conclusion.

Now, it is obviously our task in making formal deductions to formulate explicitly *all* premises which we have in our minds as far as they are needed for the drawing of the conclusion. Thus, we can say that our two premises in the above example are really insufficient; in order to be able to draw correctly purely formal inferences, we have to add a third premise, which expresses that “taller than” is transitive. Therefore, the really correct formal deduction would be the following:

From the three premises,

> “Mr. Jones is taller than Mr. Smith”,
> “Mr Smith is taller than Mr. Roberts”, and
> “‘taller than’ is a transitive relation”,

we can deduce the sentence: “Mr. Jones is taller than Mr. Roberts”.

19 (TS2, TS3) 32.⟨TS2, TS3⟩ If we consider this last-rectified-example, then we shall see that in this new example (embodying a new third premise) we can now really *replace the words “taller than” by any expression whatsoever, provided that it fits into the place.* Our former counter-examples are not any more counter-examples now, because they would most obviously render this third premise false; and we know that we are *not* permitted to say that the deducibility does not hold between some alleged premises and some alleged conclusion, on the grounds that the conclusion is false, if one of the premises was false too. Besides, we could say that if the relation, e.g. “is the father of”, *were* transitive (which is not the case) then obviously the conclusion would hold.

Generally speaking, we can say that an inference is correct only if formulated in such a way that all these replacable, descriptive, terms can *actually be replaced* by any fitting term whatsoever, without damaging the relationship of deducibility.

Chapter II. The Use of Symbols and Variables in Logic.

33.⟨TS2, TS3; 30. in TS1⟩ Even in daily life we are sometimes in a situation which has some similarities with the situation described in the last sections. I suppose you will sometimes have received an advertising circular addressed "To the Householder" or "To a Garden-Lover". Let us consider for a moment the somewhat subtle logical difference between such an address and an address like "To Subscribers of 'The Press'".

If we write an address like the latter – "To Subscribers of 'The Press'", or, "To all who are Interested in Music", or "To all Concerned" – then we intend to address, so to speak, a whole crowd; we are not addressing single persons, single individuals, but rather the public. Thus, our intention would not be met if we should substitute for "To all Concerned" simply the different proper names of different men who are concerned.

On the other hand the address "To the Householder" on some advertising pamphlets is intended to stand there because we either don't know or don't care to put the different single proper names – of Mr. Harris, Mr. Jameson, etc. – on the different envelopes. Thus the address "To the Householder" has the following function: it has to indicate something like an empty space which has to be filled up with his own name by everybody who receives the letter. (Of course nobody will actually fill it up but everybody understands how it is meant and that it is intended as a letter which might have been addressed to him personally.)

We can speak of such an address as "To the Householder" as a *variable name*. There are not very many occasions in ordinary life when we use such variables. We sometimes speak of "Mr. *X*" or perhaps of "Mr. What's His Name" or "Mr. So-and-So" 20(TS2, TS3) or perhaps of "Whosoever it may be", and we use | such phrases not quite consistently; we use them sometimes instead of a name because we have forgotten the name, but sometimes – and that is the kind of use in which we are interested here – we use them with the clear intention of leaving the question open as to which special proper name has to be substituted for the phrase used as a variable name.

There are even fewer occasions in ordinary life when we use variables for expressions other than the proper names. We sometimes say "You can't eat your cake and have it.". Here the words "eat", "cake" and "have" are metaphors. They have a definite meaning – everybody knows what a cake is and what eating is, but the metaphor is used to cover many possible variations – it is used with the intention that we may replace the words "cake" and "eat" by other terms, for instance by the corresponding terms in the expression "You can't have your bath and be dry" and any other expression indicating that you cannot perform mutually exclusive actions; in so far as we intend that the words "cake" and "eat" should play, actually, the role of certain variables for which everybody may substitute those terms which apply to the special situation in which he is interested.

We can sum up what we have said about *variables in ordinary language*: they are sometimes used, though not very often, and mostly with the help of phrases like metaphors which, as a rule, do not indicate perfectly unambiguously and clearly our intention to use a variable. The only phrase of our language used as a variable of which I am aware at the moment is the phrase "So-and-So" (for instance in the sense of "Mr. So-and-So"); like the use of the letter "*X*" in the expression "Mr. *X*". This would clearly indicate that a variable actually does not belong to ordinary language; it is already an adoption from the scientific language of mathematics or perhaps even of logic.

34.⟨TS2, TS3; 31. in TS1⟩ Let us now go back to logic and let us consider again our Example 1 of section 6. We said in section 28 that it is irrelevant for the logical relationships between statements whether we substitute the name "Mr. Brown" for the name "Mr. Smith", wherever it occurs. Clearly that does not depend on our choice of the name, "Mr. Brown": we could substitute any other name we like so long as we consider strictly that (a) we have to substitue one and the same name every time wherever the name "Mr. Smith" occurs, and (b) that we must not use one of the other names already used in the statements under consideration, like Mr. Jones or Mr. Roberts.

What we have just said can be expressed in the following way: the logical relationship between the statements under consideration would still be the same if we should use a *variable name* instead of the name, "Mr. Smith", and if we agreed at the same time that any special name – or any constant name, as we may say (the word "constant" is generally used as the opposite of the word "variable") – might be substituted for the variable. If we do this our Example 1 would assume the following form:

Example 1, a. If we have the two sentences, "Mr. Jones is taller than Mr. *X*", and "Mr. Roberts is taller than Mr. Jones", then the sentence "Mr. Roberts is taller than Mr. *X*" is a logical consequence of the two previous sentences – and will remain so if we replace the variable "*X*" wherever it occurs in the three sentences by one and the same arbitrarily chosen proper name.

If we look at this Example 1, a it suggests itself that it was rather casually that we did replace only the name "Smith" by the symbol "*X*", which indicates a variable.
21(TS2, TS3) Obviously we could just as well have chosen any one of the | two other names to replace it by a variable name. Or, we can replace two names, or even all three names, by variables. If we adopt the latter course all constant proper names would disappear.

If we want to proceed in this way we have to be careful about one special point: we cannot use the letter "*X*" to replace more than one of the three names in our examples. We would not express our Example 1 adequately if we should, for instance, write that from the two sentences, "Mr. *X* is taller than Mr. *X*", and "Mr. *X* is taller than Mr. *X*", the sentence "Mr. *X* is taller than Mr. *X*" can be deduced. (Replacing in such a way different proper names by one and the same variable symbol we obviously alter the meaning of our example; although it is still true that the third of the statements just mentioned would follow from any of the two foregoing statements and therefore from both.)

Thus, if we want to express much the same idea as we did with the help of Example 1 and 1, a, but replacing the constant names by variables, then we have to make use of different variables for the different names and to write something like this:–

Example 1, b. If we have the two sentences, "Mr. X is taller than Mr. Y", and "Mr. Z is taller than Mr. X", then the sentence "Mr. Z is taller than Mr. Y" will be a logical consequence of the two previous sentences – and will remain so if we replace the variable "X" wherever it occurs in the three sentences by one and the same arbitrarily chosen constant proper name, and if we replace it in a similar way with the two other variables. (We have only to be careful to see that we have to replace in all three statements a certain definite variable by one and the same constant proper name.)

If we look at the new Example 3 then we see that the use of different variables is more or less inevitable if we want to express adequately with the help of variables what we expressed in Example 1 with the help of constant names. In ordinary life we hardly ever (if at all) have occasion to use more than one variable, and it is therefore understandable that ordinary language simply does not possess the means of expressing different variables. It is already a highly artificial language in which such variables as X, Y and Z (and we can easily introduce others) are used; it may be that it is to a certain extent the use of variables which renders logic so artificial and abstract to the beginner.

35.⟨TS2, TS3; 32. in TS1⟩ In logic not only such variables are used as may be replaced by constant *names of persons or things*, but some other variables as well, for instance we may use, sometimes, variables which can be replaced by the *name of a whole sentence* (as a rule by the name of any section whatsoever). Consider, for instance, the following example, which expresses a very important logical rule (a rule which we may call the rule of transitiveness of the relationship of deducibility):

Example R. If the statement A is a logical consequence of the statement B, and if the statement C is a logical consequence of the statement A, then the statement C is always a logical consequence of the statement B as well.

In this Example A, B and C are obviously used as variable names of sentences.

Lastly, I wish to mention that we sometimes make use in logic of variables which do not represent names of sentences, but the sentences themselves. Consider, for instance, the following example:

See section 12.

22(TS2, TS3) *Example S.* A statement of the form "If p then p", will always be a true statement whatever the sentences we may substitute for "p", provided, of course that we substitute one and the same sentence both times. (For instance, the statement, "If it rains tomorrow then it rains tomorrow" will quite obviously be a true statement whatever the weather may be tomorrow, and the same will hold, for instance, if we substitute both times the sentence "I like logic".)

I want to draw the attention of the reader to the fact that in the foregoing example the letter "p" was used as a variable for a sentence itself and not for a name of a sentence, whilst in Example R the variables were used as variables for names of sentences: in Example R we were *referring to our speaking about* certain sentences, and that can be done only with the help of names, as was explained in No. 12 (just as we have to use a name for the table, – for instance the word “table” – and not the table itself, if we want to talk about the table). As opposed to this consider Example S. In a statement like “If I like logic then I like logic” we do not talk about, we do not refer to, this statement, “I like logic”: what we do is simply to state something in such a way that the sentence “I like logic” occurs as a part of our statement without being in any way the object about which our statement is stating something. (The statement “*If* I like logic *then* I like logic” is in this respect similar to a statement like “I like logic *and* I like psychology”: it is a statement composed of other statements, and not a statement *about* those other statements.

36.⟨TS2, TS3; 33. in TS1⟩ To conclude about the rule of variables in logic, the formal character of logic which we tried to describe before has such an effect on logic that all logical considerations which hold with regard to some special constants as a rule hold with regard to some other constants as well. In expressing such logical considerations we have, therefore, often to make use of variables – thus indicating that our considerations are not dependent on special constant values. The symbols used to indicate variables are highly artificial symbols because ordinary language does not supply us with the necessary means of expressing variables. We generally use letters like “*A*”, “*B*”, . . .; “*X*”, “*Y*”, . . .; “*p*”, “*q*”, and so on to indicate different kinds of variables.

Roughly speaking one could say that the more we develop our logical system and the more the system is generalised, the more we shall have to make use of the symbols which indicate variables. If the use is highly developed then one usually speaks about “symbolic logic”, but the use of artificial symbols in logic is a matter of degree – even traditional logic (“Aristotelian logic”) has to make use of some symbolism in indicating variables.

23(TS2, TS3)

Chapter III. Descriptive and Logical Constants. The Logical Form of Statements and Arguments.

37.⟨TS2, TS3⟩[j] Names like “Mr. Smith”, “Mr. Brown” and so on, have been characterized as constant names, or constants – as opposed to variables – for which constants can be substituted. We have seen that the meaning of these constants is not relevant to the problem of deducibility.

Words playing a very similar role are, e.g. “table”, “chair”, “cow”, etc. For it is clear that in the example:

[j] The chapter “Descriptive and Logical Constants. The Logical Form of Statements and Arguments.” is only contained in TS2 and TS3, where it forms Chapter III.

(Premises:)
All men are mortal.
All Frenchmen are men.
(Conclusion:)
All Frenchmen are mortal.

we could without disturbing the relationship of deducibility, replace e.g., the word "men" (wherever it occurs) by the word "cows" (and, if you like, "Frenchmen" by "French cows"); and similarly, we could replace, without disturbing the relationship of deducibility, the word "mortal" by the word "immortal" or, if you like, "thirty" by any other word which fits the place.

Thus, such words, the meaning of which has no influence on deducibility, can be called "descriptive constants", and the variables, which may take their place, and for which descriptive constants can be substituted, can be called "descriptive variables".

38.⟨TS2, TS3⟩ A role which is very different indeed is played by words like "is", "are", "not", "all", "some", "or", "and", "if . . . then".

If we consider our last example (in 37), then we see at once: although we can replace "mortal" by "immortal", wherever it occurs, it would destroy the relationship of deducibility if we were to replace "all" by "some", wherever it occurs; and this, although the word "some" obviously, fits the place excellently. Similarly, the relationship of deducibility would break down if we were to replace "are" wherever it occurs, by the words "are not", although these words would also fit very well. The words "is", "are", "not", "all", "some", "or", "and", "if . . . then", which play a role not very different from the role played by descriptive constants and variables, shall be called "logical constants".

39.⟨TS2, TS3⟩ But what is the character of words like "taller than", "richer than", "father of", and so on? These words are *names of relations* and they are, as such, certainly of a kind different from "Mr. Brown", or, "table" or, "cow". (By the way – even the words "Mr. Brown" and "Peter", are not exactly the same kind as the words "table" and "cow"; nevertheless, both kinds are descriptive constants.)

That the names of relations are of a kind different from, e.g., the proper names, can be seen by the fact that they *do not fit* into the places where proper names do, and vice versa.

Nevertheless, we shall classify "richer than", "father of", with the *descriptive constants*; for it is, as we have seen in sections 31–33, possible, to replace such words
24(TS2, TS3) by words of an entirely different meaning without disturbing the deducibility. | On the other hand, the term "*transitive*" (and some other similar terms, describing certain properties of relations) has to be classified with the *logical constants*. Other logical constants would be the words "true" and "false". Words which can be defined with the help of logical constants alone, are always logical constants.

40.⟨TS2, TS3⟩ If we replace in a statement all the ⟨descriptive⟩ constants by dots, then what remains can be called the scaffolding, or framework of the sentence; e.g., the framework of the sentence,

"All men are mortal",

would be,

"All . . . are . . ."

Thus, the scaffolding or framework of a sentence is given when (a) the logical constants are given, (b) their order, and, (c) the empty places into which the descriptive constants or variables may be fitted.

That the order of the logical constants is of significance may be seen from the examples,

"All . . . are not . . ."

and,

"Not all . . . are . . .",

which represent different frameworks.

We can now explain the term "logical form of a sentence": the logical form of a sentence is given when (a) its scaffolding is given, (b) the *kind* of the descriptive constants or variables which might be fitted into the framework, and, (c) an indication which of the descriptive constants or variables, if any, occur more often than once, and where they do re-occur.

For instance, the logical form of

"Peter is taller than Paul",

is given by,

". . . is xxx - - -"

and the explanation that ". . ." and "- - -" indicate places into which *proper names* may be fitted, whilst "xxx" indicates the place where the name of a relation (between individuals) may be fitted.

Similarly, the logical form of the statement:

"All men are men",

would be given by,

"All . . . are . . .",

together with the explanation, that in both places indicated by ". . .", the same descriptive (class-) name has to be substituted.

Similarly, the logical form of a set of premises (together with a conclusion), i.e. the logical form of an argument, is given when the form of its different statements is given, together with an indication of the repetitions of the descriptive terms throughout the whole argument; e.g.:

(Premises:)
All . . . are - - -
All xxx are . . .
(Conclusion:)
All xxx are - - -

where the repetition of "...", "- - -", "xxx" indicates a repetition of descriptive terms, can be taken as the description of the logical form of an argument.

25(TS2, TS3) 41.⟨TS2, TS3⟩ The writing of "..." or of "- - -", together with an explanation, is not a very practical way of explaining or describing the logical form of a sentence or an argument; as a matter of fact, the best way is using variables instead of the dots and dashes.

Thus the logical form of the sentence:

"All men are men",

can be easily indicated by writing:

"All *Y* are *Y*"

or,

"All *X* are *X*".

We have only to be careful to explain that we are using this for characterising a logical form, and we have to indicate, in some way or other, which kind of descriptive terms are represented by the variables used.

Similarly, we can describe the logical form of

"Paul is taller than Max",

by saying that its logical form is:

"*X R Y*",

when we explain that we use "*X*" and "*Y*" and "*Z*" as variables representing proper names, and that we use "*R*", "*S*", "*T*" as variables representing relations (between individuals).

Thus, it is just another use of variables if we use them for describing or representing the logical form of a statement.

Chapter IV. Statements Which Are Formally True. Analytic and Synthetic Sentences.

42.⟨TS2, TS3; 34. in TS1⟩[k] In example *S*, No. 35[l], we were dealing with a statement of a kind which we can call "*formally true*": this statement "If I like logic then I like logic" is true independently of all matters of fact – it can be decided to be true even by somebody who does not know me and who does not know what logic is. We can

[k] In TS1 this chapter has the number III, and the last part of the title reads "Analytic and Synthetic Statements" instead.

[l] TS1 correctly refers to section 32.

discover its truth, so to speak, without having to discover anything about the facts – just by considering its logical (or, if you like, grammatical) *form*.

26(TS2, TS3) If we use, as before, the letter "*p*" as such a variable as indicates that any whole sentence may be substituted for it (thus "*p*" can be called a "variable sentence"), then we can say that any statement of the form "If *p* then *p*" (where "*p*" may be replaced by any sentence whatsoever) is a formally true statement.

Similarly we can easily construct statements which are, so to speak, *formally false*, i.e., statements the falsity of which can be found out by considering their logical or grammatical form only and without taking into consideration any matters of fact. In order to construct such a formally false statement we may employ a very simple device: we need only take a formally true statement and deny it. Thus, the statement "If I like logic then I don't like logic" would be a formally false statement[m].

It is important to see how very different the character of such formally true or formally false statements is compared with ordinary statements, as for instance, "I like logic".

The question whether or not an ordinary statement is true depends on the facts to which the statement refers (cf. No. 18): if somebody wants to know whether his statement is true then an inspection of the statement itself will never help him to find out; he is bound to ask me or to observe my behaviour or to take other steps of that kind which may give him some information about the facts.

43.⟨TS2, TS3; 35. in TS1⟩ In ordinary life we nearly always use only such kinds of statements the truth or falsity of which can be made out only by inspecting the facts to which they refer. One often calls such statements "*Synthetic statements*"[8] – as opposed to the "analytic statements"[8] – the truth or falsity of which can be made out by analysing the logical and grammatical form of the statement itself, without any reference to facts.

As I have said, almost every ordinary statement is a *synthetic* statement. Examples would be very easy to find: "Mr. Jones is taller than Mr. Smith", or "There is a table in room 48", or "It will be raining tomorrow" are obviously all synthetic. A synthetic statement supplies us with some more or less valuable information about matters of fact. The facts described need not always be entirely concrete facts. They may sometimes be more abstract. A statement like "The third statement in quotation marks of Example 1 in section 6 of the present treatise is a logical consequence of the two other statements in quotation marks". This statement is itself a synthetic statement because merely by analysing its logical form alone without considering the objects to which it refers, i.e., the statements in Example 1, we would never be able to find out whether or not it is true.

As opposed to this, *analytic* statements do not supply us with any valuable information about matters of fact. If somebody tells me that it will rain tomorrow supposing it rains tomorrow, then I am none the wiser for that – I have not obtained

[8] The use of this kind of terminology is due to the German philosopher Kant.

[m] The statement is, of course, contradictory, but it is not the negation of the tautology considered above.

any information about tomorrow's weather. (I hope it is clear that the logical situation with regard to the statement "It will rain tomorrow supposing it rains tomorrow" is the same as the situation with regard to the statement "If it rains tomorrow then it rains tomorrow", which, obviously, is an instance of an analytic statement of the form "If *p* then *p*" which we discussed above.)

27(TS2, TS3) It is now obvious why analytic statements are not used in ordinary life: we speak in ordinary life with the aim of supplying information about matters of fact, and analytic statements do not do this. However, it is not only in ordinary life that we use almost exclusively synthetic statements: all those sciences which are sometimes called natural sciences, and history, and most of the social sciences aim at supplying us with information about the facts of nature, or about the facts of the history of mankind or of social institutions, and are, therefore, concerned almost exclusively with synthetic statements.

Only logic (and perhaps some other branches of philosophy) and mathematicians make use to any extent of analytic statements. For us at the present moment it is of great importance to be perfectly clear about the distinction between those two classes of statement and about the fact that any statement whatsoever must be either synthetic or analytic.

Analytic statements can be distinguished into such as are formally true and such as are formally false. The true analytic statements are sometimes called "positive analytic statements", or simply "analytic statements", in the narrower sense (sometimes they are also called "tautologies"), whilst the false analytic statements are sometimes called "negative analytic statements", or, more often, "self-contradictions", or simply "contradictions".

44.⟨TS2, TS3; 36. in TS1⟩ These terms can easily be remembered in the following way: "Analytic" statements are those the truth or falsity of which can be found out by logical *analysis* alone, without making use of any kind of experience of facts. The term "tautology"[9], which literally means the saying of the same thing twice over in different words, and which is commonly applied to statements like "A table is a table" (which obviously would be another example of a positive analytic statement), is meant to suggest that no formally true, or positive analytic statement is able to give us more information than a statement like "A table is a table"; and lastly, the term, "self-contradiction", or, more shortly, "contradiction", is used to indicate that the statement witnesses against itself – like, perhaps, the statement "A table is not a table".

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[9] The use of this term and the term "contradiction" in approximately the sense adopted here is due to the German philosopher, Schopenhauer, and to Russell's pupil, Wittgenstein.

Chapter V. Some Famous Analytic Statements – The So-Called "Laws of Thought".

45.⟨TS2, TS3; 37. in TS1⟩[n] Although formally true statements or analytic statements in the narrower sense have no importance for ordinary communications, we have said that they play a certain role in logic. Some of them became famous, not so much because of their special significance, but rather because of the name which was given to them: they were called the laws of thought, in my opinion a name which is very 28(TS2, TS3) apt to lead to confusion and misunderstanding; but I will not discuss the | question why they were so-called before the last chapter of this treatment[o], which deals with the problem of the nature of logic, and I here ask the reader just to accept the fact that they are named "the laws of thought" and not to attempt to include any special reasons with the use of this name.

The first of the formally true statements to which I shall refer is generally called the *Law of Identity*. To make it clear may we refer to the example given: we said before that the statement "A table is a table" is formally true, or analytic (or a tautology). Using variables we can say that generally speaking *every statement of the form "X is X", where any term (or any noun rather) may be substituted for the variable "X", is analytic*.

The traditional way of formulating this fact would be simple:

The Law of Identity: X is X.

In order to explain the two other so-called laws of thought – one usually speaks of them as "The Law of Contradiction" and "The Law of the Excluded Middle" – we have to add some new symbols the symbolism with the help of which we are indicating variables.

46.⟨TS2, TS3; 38. in TS1⟩ If we have a certain statement, say, "It will rain tomorrow", then the statement "It will not rain tomorrow" is called a *negation* of the former statement. It is easy to construct examples of statements and of their negations, for instance: the negation of the statement "Mr. Jones is taller than Mr. Smith" would be the statement "Mr. Jones is not taller than Mr. Smith"; and the negation of the statement, "Five times five equals twenty-five" would be the statement, "Five times five does not equal twenty-five".

It is easy to see that if a statement is true then its negation will always be false and vice versa: if a statement is false then its negation is true. This rather important law holds equally for statements which are formally true, or analytic, and formally false, or contradictory on the one hand and such statements as are synthetic and therefore true, or false, according as they correspond with the facts or do not. (Analogous to the expression "formally true" we speak in the case of synthetic statements often of their "material truth" or "material falsity", indicating by this means that the truth or falsity depends on their accordance with the *matters* described.)

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[n] This is Chapter IV in TS1.

[o] The typescripts do not contain any further chapters.

It is very convenient indeed to make use of the following symbolism in order to indicate that a certain variable statement is the negation of some other variable statement: If we choose to denote a variable statement by "p" then we shall denote its negation by the symbol "$\overline{p}$", i.e., we shall use the same letter but with a horizontal stroke over it. It is customary to read the symbol for the negation of p, i.e., the symbol "$\overline{p}$" as "non-p".[10]

In a way similar to that in which we symbolised the negation of a variable sentence, we can symbolise a variable *name* of a sentence which is the negation of some other sentence, for which we have chosen some variable name. If, for instance, we have chosen for one sentence the variable name "A", then the name of the negation of that
29(TS2, TS3) sentence can be symbolised by "$\overline{A}$", which may, again, be read as "non-A". | With the help of this demonstration we can, for instance, formulate the rule mentioned in the paragraph before the last; we can express it as follows: If A is true then $\overline{A}$ is false, and if $\overline{A}$ is true, A is false – whatever name of a statement may be substituted for A.

Lastly, we can introduce an analogous symbolism for such variable terms as describe certain properties. If we have, for instance, the statement "This piece of metal is round", then we can form the following statement which is something rather similar to a negation but not quite the same: "This piece of metal is something which is not-round", where "not-round" may be looked at as forming *one-term*, namely, a term which is opposed in a certain way to the term "round". The relationship between the terms "round" and "not-round" has to be understood in the following way: we shall say that a thing has the property of being not-round in any case in which it does not have the property of being round. In a similar way we can speak of something as being not-black if it has some other colour but not the black colour; or as being a not-animal or a not-horse or a not-stone if it is something, whatever it may be, but not an animal, not a horse, or not a stone, respectively.

It is customary to call the property of being a not-table complementary, contradictory or negative to the property of being a table, and one speaks accordingly of a complementary property, contradicting or negative of some other property, and of the complementary term of some other term.

If we use symbols like, say, "P", "Q", "R", "S", to denote terms of which the complementary term can be constructed, then we can in the same way as that we introduced above denote those complementary terms with the help of the symbols "$\overline{P}$", "$\overline{Q}$" ,"$\overline{R}$", "$\overline{S}$".

47.⟨TS2, TS3; 39. in TS1⟩ Now we can return to the third so-called "law of thought". We shall first consider what one usually calls the "*Law of Contradiction*". We have already had an example of self-contradiction – at the end of section ⟨44⟩[p] – namely, the statement, "A table is not a table". It is obvious that any statement of the form "R is $\overline{R}$", where we may substitute any term like "a house", "a stone" and so on for the variable symbol "R" and correspondingly the term "a not-house", or "a not stone"

[10] There are many different ways of symbolising the negation in use, for instance, the symbols "$-p$" or "$\sim p$". We will use here exclusively the symbol described in the text above.

[p] This reference should refer to section 44 in TS2 and TS3. All three typescripts refer to section 36, which is correct only for TS1.

and so on for the term "$\overline{R}$", will be a self-contradictory statement, i.e., formally false. Thus we may formulate the Law of Contradiction in its *first form*: Any statement of the form "R is $\overline{R}$" where we may substitute for R any term of which a complementary term can be constructed, is formally false.

The second form of the Law of Contradiction can be formulated with the help of sentential variables as follows: Any statement of the form "p and $\overline{p}$", where we may substitute for the variable "p" any sentence whatsoever, (and correspondingly the negation of this sentence for the symbol "$\overline{p}$") is a formally false statement, i.e., contradictory.

Examples of such statements can easily be found; the statement, "Mr Jones is taller than Mr. Smith and Mr. Jones is not taller than Mr. Smith", or "I like logic and I do not like logic", or "Three times two equals six and three times two does not equal six" – and just as well "Three times two equals seven and three times two does not equal seven"; all of these may be taken as examples of such statements as are formally false according to the Law of Contradiction.

There are some other forms of the Law of Contradiction but I think that these two may suffice.

30(TS2, TS3) 48.⟨TS2, TS3; 40. in TS1⟩ The third of the so-called "Laws of Thought" is named "The Law of Excluded Middle". I will give first some examples.

The statement, "That piece of metal is either something round or something not-round", or the statement "This animal is either a horse or a not-horse", is obviously formally true, i.e., analytic. This can be generalised with the help of the first form of the Law of the Excluded Middle: Any statement of the form "X is P or $\overline{P}$", where any term may be substituted for the variable "X" and any such term the complement of which can be constructed may be substituted for the variable "P", is a formally true statement, i.e., a tautology.

As to the second form of the Law of the Excluded Middle, we can start with the following example: The statement "It will rain tomorrow or it will not rain tomorrow" is obviously formally true and so is the statement "I like logic or I don't like logic." Generally speaking we may say this, giving the second form of the Law of the Excluded Middle: Any statement of the form "p or $\overline{p}$", where we may substitute for "p" any sentence whatsoever, is a formally true statement.

49.⟨TS2, TS3; 41. in TS1⟩ The so-called Law of Contradiction and the so-called Law of the Excluded Middle have to do with the conjunctions "and" and "or" respectively. That makes it easy to remember them.

The traditional formulation of these two laws is something like this:

> Law of Contradiction: Nothing can be both P and $\overline{P}$.
>
> Law of the Excluded Middle: Everything must be either P or $\overline{P}$ – there is no third possibility.

(The last remark explains the name "Law of the Excluded Middle"; that there is no third possibility may be formulated as "It is excluded that something may have a property which, so to speak, lies between P and $\overline{P}$".)

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Chapter 20
The Origins of Modern Logic

Karl R. Popper

Abstract These are unpublished lecture notes from Popper's Nachlass.

Editorial notes: The source typescript is from KPS Box 366, Folder 19. The text refers to Popper in the third person, which suggests that these are lecture notes that were prepared by one of Popper's students. There is a reference to Popper's lecture notes on logic of 1939–1941 (cf. this volume, Chapter 19), which indicates that he gave these lectures during the same period or shortly afterwards. The page numbers skip number 9, but no page seems to be missing. The typescript uses the notation $\overline{p}$ for the negation of p.

1 Modern logic has had a recent and rapid growth and represents a complete break away from the traditional or Aristotelian logic. We do not mean by this that no relationships can be established between the two logics, on the contrary the Aristotelian logic of syllogism is to be regarded as a small, and not very well formulated portion of the whole field of modern logic, but that the new logic owes only little of its development to the older logic.

The first steps towards a new logic came from the work of mathematicians such as Frege and Peano who used deductive logical methods in working out the foundations of Arithmetic, and Cantor who in his work on infinity introduced the notion of classes or aggregates.

From another angle Boole and Peirce contributed to the rise of the new logic by using mathematical symbolism and methods to state and work out the principles of logic.

Russell's work in Principia Mathematica[a] consists largely in bringing together the work of all these men.

These sources of modern logic emphasize its main differences from traditional logic:

1. The traditional logic had no contact with mathematics. Now whatever view one takes of the nature of mathematics, it is apparent that it is the field of study in which really deductive logical methods are used with most effect. Mathematics makes more use of deductive logical methods than any other science. Exponents of

[a] Whitehead and Russell (1925–1927).

D. Binder et al. (eds.), *The Logical Writings of Karl Popper*,
Trends in Logic 58, https://doi.org/10.1007/978-3-030-94926-6_20

the traditional logic however confined themselves to the examination of the logic of everyday discourse and were thus divorced from logical practice and unable to understand the character of deductive systems by which a vast superstructure is built upon a few initial postulates.

2. The traditional logic showed little capacity for development and expansion while the new logic derived from the rich source of mathematics, has developed and
2 expanded with great rapidity. |

3. Modern logic realizes the significance and importance of the paradoxes, which the traditional logic failed to do. Epimenides' paradox for example is not even formulated within Aristotelian formalism. It was soon realized that the concept of classes, which was employed in the interpretation of the Boolean Algebra, led to the same paradoxes as those which involved the concept of predicate.

Modern logic unlike the old logic has set itself the task of eliminating the paradoxes, realizing that until this is done there can be no adequate system of deductive logic. If in a system there is a contradiction then in that system one can prove any sentence whatsoever (and therefore its negation), and from contradictory premises follows any statement whatever and its negative. A deductive system which did not *both* permit something and exclude something else would be useless.

To show this: –[b]

The following rules of inference are valid

$$\text{(a)}\quad \begin{array}{l} p \\ \underline{\text{not } p \text{ or } q \text{ or both}} \\ \therefore\ q \end{array} \qquad \left.\right] \qquad \begin{array}{l} p \\ \underline{\overline{p} \vee q} \\ \therefore\ q \end{array}$$

$$\text{(b)}\quad \begin{array}{l} \underline{\overline{p}\qquad\qquad\qquad\quad} \\ \text{not } p \text{ or } q \text{ or both} \end{array} \qquad \left.\right] \qquad \begin{array}{l} \underline{\overline{p}\qquad\quad} \\ \therefore\ \overline{p} \vee q \end{array}$$

To prove that from the two premises p, $\overline{p}$ follows any proposition whatever:

$$\begin{array}{ll} p & \\ \underline{\overline{p}\qquad\quad} & \text{rule (b) (where } q \text{ may be any proposition)} \\ \underline{\therefore\ \overline{p} \vee q} & \text{rule (a) (using premise } p\text{)} \\ \therefore\ q & \end{array}$$

3 | Thus modern logic is distinguished from the traditional logic by its contact with mathematics, its richness of material, its capacity to develop, and by the serious consideration which it gives to the paradoxes. These rather than the superficial characteristic of using a symbolism resembling that of mathematics are the fundamental features of the new logic.

[b] In the following paragraph the principle of *ex contradictione quodlibet sequitur* (that from p and its negation $\overline{p}$ follows any proposition q) is demonstrated using a form of disjunctive syllogism (rule (a)) and disjunction introduction (rule (b)). The rules are given in semi-symbolic and symbolic notation. We have deleted some duplications, corrected typographical errors, and have rendered the proof (with some simplifications) in symbolic instead of semi-symbolic notation to improve readability.

Note: Paradox of the classes of all classes which do not contain themselves as an element

1. A class is *normal* if it does not contain itself as an *element*
2. The class of all normal classes
3. Is this class (2) itself normal or not?

(a) *Assume* it is *normal*
(then it does not contain itself as an element)
but it is the class of *all* normal classes therefore if it is normal it must itself be an element of that class of *all* normal classes and therefore of itself
therefore it is *abnormal*

(b) *Assume* it is *abnormal*
then it must contain itself as an element
But it contains only such classes as elements which are normal
therefore it must be *normal*

1. *Modus Tollens*

$$\begin{array}{l} p > q \\ \underline{\overline{q}\qquad} \\ \therefore\ \overline{p} \end{array}$$

2. *Reductio ad absurdum*

from p follows q
now q is impossible or absurd (for some reason)
———
$\therefore$ p is impossible too
$\therefore$ $\overline{p}$

3. Special case of the reductive *ad absurdum* – *the indirect proof*

from p follows $\overline{p}$
therefore
from p follows p . $\overline{p}$ which is absurd
———
$\therefore$ $\overline{p}$

4. *Paradox*
Z is a paradoxical statement if from Z follows $\overline{Z}$ *and* from $\overline{Z}$ follows Z

4 | Logic like all studies investigates certain types of objects. These objects are linguistic, consisting of statements which are often premises or conclusions. Logic is mainly concerned with the analysis of the relationships existing between premises and conclusions so that its task may be stated broadly as the investigation of the conditions of *inference*.

In order to refer to objects we commonly use *names*. Now in other subjects of investigation people are not likely to use the thing itself in place of its name. Thus the botanist though he may use actual plants as illustrations is never likely to insert a specimen in place of the name of a plant when he is writing about his subject. The logician however who has groups of words for his object of study is often tempted to put down the words themselves instead of the name for them. The easiest way to make the distinction between words as ordinarily used and words as names in a statement about linguistic entities is by the use of inverted commas. Here are two sentences for logical comparison:

"Tom is dark. Mary is fair." Now when we write *about these sentences* we must show that we are referring to these sentences and not to the situations represented by the sentences. Thus we should write: "Tom is dark" is compatible with "Mary is fair." If the inverted commas were omitted then it would not be apparent that we were referring to the sentences themselves.

This is a mistake that is frequently made in logical textbooks and one that is liable to lead to serious confusion.

—

Our consideration of logic as concerned with the kind of relationships which hold between linguistic expressions is valid whether we prefer to talk about thoughts, judgements, propositions, statements or sentences because for logical analysis these must be linguistically expressed.

We cannot pass *logical* judgements on a man's thoughts or actions until he or someone else expresses them in sentences or some such linguistic form. Indeed logical relationships *as such* only occur between linguistic forms and not between the objects represented by those forms. For example "*A* follows from *B*" is a statement
5 about | statements made with the help of the name *A* and the name *B*.

A may stand for the statement "Auckland is to the north of Wellington and Wellington is to the north of Christchurch" and *B* may stand for the statement, that, "Auckland is to the north of Christchurch". We cannot say that the *fact* that Auckland is to the north of Christchurch *follows from* the *fact* that Auckland is to the north of Wellington and Wellington is to the north of Christchurch. It is only of the statements that we can say that one follows from the other. The relationship "follows from" *may be* based on some relationship between the facts referred to by the statements, but if it says something about these facts it is only in a secondhand way.

That logical relationships differ from factual relationships is clearly seen from the logical relationship of contradiction. "Jones is six feet tall" contradicts "Jones is less than six feet tall" but it is only the statements which contradict each other; there ⟨do⟩ not exist two lots of fact about Jones in contradiction with each other. Although the contradiction of statements may correspond to the exclusiveness of facts we have to recognize that logic is concerned with one and not directly with the other.

In this connection it is important for us to make clear the distinction between *compound* statements and *meta* statements. Both contain statements within statements but in a different way. Thus "Humpty Dumpty sat on a wall and Humpty Dumpty had a great fall" is a compound statement about fact containing two distinct factual statements. On the other hand the sentence "'Humpty Dumpty sat on a wall' is compatible with the statement 'Humpty Dumpty had a great fall'" while it contains the same sentences (but in inverted commas) treats them in an entirely different way. In the meta statement it is the sentences as such which are referred to and not the facts they express. In the first case the words in the two sentences are used as statements of fact while in the second case, when put into inverted commas, they are the *names of these statements.*

On this basis we may distinguish between an object and a meta language. The object language is (usually) the language of everyday discourse or the language of the sciences, and it is the object of our interest – that which we are going to analyse.
6 But to refer to it we must use a language and this we call the meta language. | About the meta language we *need* not talk at all, we just talk in it. The two may coincide (e.g. they may be English) but they may not. In one context only one language should be used; there should be no mixture. This does not mean that we cannot refer to the other language but when we do so we must use some such device as inverted commas or preface it by the phrase "the statement which says that," thus in effect translating it into the meta language, just as we do for example when referring to a French word in the English language, e.g. the French word "garçon" has the same meaning as the English word "boy".

In "Principia Mathematica"[c] Russell has not kept strictly to the meta language. His formula $p \supset q$ does not express the logical relationship of deducibility i.e. $p \supset q$ is not equivalent to q follows from p, instead its equivalent is $\overline{p} \vee q$.

The following are important meta-linguistic terms referring to relationships which hold between statements:

"*A* contradicts *B*"
"*A* and *B* are compatible"
"*A* follows from *B*"
"*A* is analytic"
"*A* is synthetic"
"*A* is self-contradictory"
"*A* is consistent"
"*A* is true"
"*A* is a generalization of *B*"
"*A* is a negation of *B*"

7 Note on the Concept of TRUTH

(a) If truth is a property of certain statements and implies a relationship to facts then the meta language must not be confined to speaking only or exclusively about linguistic objects otherwise the concept of truth could not occur within the meta language.
(b) The definition truth means correspondence between the sentence and the fact does not cover analytical true sentences. However, the definition can be extended in such a way that this difficulty disappears.

(A propositional function is a sentence which contains variables. Names which can be substituted for these variables are said to *satisfy* the propositional function

[c] Whitehead and Russell (1925–1927).

while the fact or object represented by the name is said to *fulfil* the propositional function. A proposition may be regarded as a propositional function with no variables in which case it may be regarded as being fulfilled by the null class of facts).

We can extend our definition by saying that a true proposition or statement is one which is fulfilled by certain facts. Analytic statements would then be propositions that were fulfilled by any facts whatever.

Remarks about Truth

A technical question: property and class.

Truth is a property or a class. In saying a sentence is true we are saying that it has a certain property or belongs to a class of sentences. We are thus talking *about* a *sentence* and therefore statements about truth belong to the *meta language*.

If "*A*", "*B*", "*C*" are names of sentences then I can write symbolically "$A \in$ Tr" ("*A* is true") where "$\in$" means "is an element of", and "Tr" is the name of the class of true sentences.

Note: A sentence of the form "Socrates $\in$ man" must always be constructed so that on the left of the symbol "$\in$" there is a *name* of an *element* and on the right there is a *name* of a *class*.

8 | Therefore, if I am stating that a certain proposition belongs to a certain class (or has a certain property) then I can express this only in a statement where the name of a proposition occurs (e.g. on the left) the⟨n⟩ some symbol which symbolizes the individual class relationship and then (e.g. on the right) the name of a class.

For the proposition itself is the element of the class not what is named by it.

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If we have the concept of truth at our disposal in the meta language then we can define with the help of this concept most of the *logical constants* of our language – words like these: "and", "are", "is", "if", "or", "some".

Logical constants of the Aristotelian syllogism e.g. such words as "all", "some" and the copula. We distinguish between *logical constants* and *descriptive constants* like "table", "chair", "Socrates", and between *variables* (which themselves can be either logical or descriptive; usually variables representing descriptive constants are employed.)

Definition of "and" (as used between *sentences*) with the help of the concept of truth.

A sentence of the form "*p* and *q*" or symbolically "$p \,.\, q$", where "*p*" and "*q*" may be replaced by any sentence of the language under consideration, *is true if* and *only if* both of the constituent sentences (represented by "*p*" and "*q*" are true).

Definition of "or" "*p* or *q*" (symbolically "$p \vee q$") is true if and only if at least one of its constituents is true.

To define Conjunction and Disjunction

A Conjunction is a sentence composed of two sentences in such a way that the whole sentence is true if and only if both of its constituents are true.

A Disjunction is a sentence composed of two sentences in such a way that the whole sentence is true if at least one of its constituents is true.

The words "conjunction" and "disjunction" obviously are terms of the meta language on the same level as "true".

The words "and" and "or" or "." and "∨" are words of | the object language, they do not themselves describe anything, they only express a certain way of composition of sentences. Which way can be expressed in meta language with the help of constants.

Truth Tables

"p"	"$\sim p$"
T	F
F	T

Constituent sentences		Whole sentences		
		Conjunction	Disjunction	Implication
"p"	"q"	"$p \,.\, q$"	"$p \vee q$"	"$p > q$"
T	T	T	T	T
T	F	F	T	F
F	T	F	T	T
F	F	F	F	T

Thus they show us in what way we have to make use of the meta language in constructing or analysing an object language.

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The class of formally true or analytical sentences will be named by the symbol "An". Thus "The sentence A is analytic" can be symbolized by "$A \in$ An". Now we have in the meta language a theorem An $\subset$ Tr ($\subset$ subclass relation). If a sentence "A" follows from a sentence "B" then the sentence which is the implication with the antecedent B and the consequent A must be analytic and vice versa.

Thus if "p" and "q" represent sentences of the object language, the sentence of the meta language "'$p > q$'$\in$ An" expresses that the second of these sentences of the object language i.e. a sentence "q", follows from the first "p".

Examples showing the relationships set out in the truth table:
Conjunction "$p \,.\, q$" e.g. Tom is dark and Mary is fair. Unless it is true that both, "Tom is dark" and "Mary is fair" the statement as a whole is a false one.
Disjunction "$p \vee q$" e.g. Jones has a horse or Jones has a car. This sentence may be true as a whole if ⟨one or both sentences are true.⟩[d]

[d] The sentence ends abruptly in the typescript.

11 Criticism of W. E. Johnson

Johnson[e] distinguishes between primary and secondary propositions. He says that there may even be tertiary propositions. He defines “a secondary proposition is one which predicates some character of a primary proposition”[f].

Examining this definition we see that if a secondary proposition predicates some character of a primary proposition then it must refer to the primary proposition with the help of some sort of description or name. For, if a certain statement, whether it is secondary or primary, some character of some object, then it must refer to the object with the help of a name; e.g. ⟨if⟩ I want to predicate of my fountain pen some characteristic – for instance that it is black – then I have to use a sentence or proposition like “My fountain pen is black”.

If we look at this proposition then we see that neither my fountain pen nor the characteristic in question occurs in it, but *words* or *names* referring to my fountain pen and to the characteristic. Namely, the names “fountain pen” and “black”.

In the same way if a secondary proposition predicates something of a primary proposition then the primary proposition cannot occur itself in it but a name of it must occur in the secondary proposition.

Johnson writes “taking *p* to stand for any proposition we may construct such secondary propositions as: *p* is true, *p* is false, *p* is certainly true, ...”

Let us criticise this sentence of Johnson’s. In saying “taking *p* to stand for any proposition” he is indicating that he is using the letter “*p*” not as a name (variable name) referring to, but as a symbol standing for, or representing some proposition itself, but in that case we cannot say that “*p* is true” is a correct example of a secondary proposition. It is not correct to say “It is now raining is true” but rather “‘It is now raining’ is true” or even clearer “The proposition ‘It is now raining’ is true”.

12 The Consequence Relation and Analytic Implication

In what follows we will make use of the following symbols: “*p*”, “*q*” and “*r*” *represent* certain statements like “It is raining” (they represent not they denote or they name i.e. “*p*” is not used as a name for e.g. “It is raining” but the letter “*p*” is used instead of some series of letters like “It is raining”). We say “*p*” is a variable representing or standing for sentences.

We use symbols like “*A*”, “*B*”, “*C*” as *names* of sentences and we use symbols like “*P*”, “*Q*”, “*R*” as *variables* representing or standing for not sentences but *names of sentences*.

We want to express that a certain sentence *B* follows from another sentence, say *A*. Every statement like “*B* follows from *A*” or “from *A* follows *B*” must be a sentence

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[e] William Ernest Johnson (1858–1931), British philosopher and logician who wrote the three-volume *Logic* (1921–24).

[f] The quote is from Johnson (1921, p. 50)

of the meta language because this sentence states that a relationship between two propositions holds (similar to a sentence which states a property, say truth, of a proposition or say that a proposition is analytic.)

Now we want to show that if the proposition named by "*A*" is, for instance, "It is raining now" and the proposition named by "*B*" is, for instance, "It is sometimes raining" then instead of saying "From *A* follows *B*" we can say "'It is raining now implies it is sometimes raining' is analytic". Now, instead of writing "It is raining" we will write "*a*" and instead of writing "It sometimes rains" we will write "*b*" (i.e. the small letters "*a*", "*b*" and "*c*" shall represent constant propositions – certain givens i.e. they shall be just shortenings of these propositions).
Now what we want to show is: the sentences

$$\text{“From } A \text{ follows } B\text{”} \quad \text{and} \quad \text{“‘}a > b\text{’} \in \text{An”}$$

express the same thing.

It has to be noted that both these sentences are sentences of the meta language the one making use of the relationship "follows" the other of the predicate "analytic" which are both in terms of the meta language.

We can use a third way to express it: if we introduce the name "$A \rightarrow B$" as a *name* for the proposition "$a > b$" then we can write "$A \rightarrow B \in$ An". In other words, we maintain that if implication of the object language is analytic i.e. is formally true
13 then the | two components of this implication stand in such a relation that the second follows from the first.

If an implication of the object language is not analytic i.e. not formally true but for instance true (not formally) or false (either formally or not formally) then the second of the components does not follow from the first. In other words, the symbol of implication (">" or "If . . . then") does not express the relationship of deducibility or consequence, it cannot because it is the symbol of the object language.

To show that the two sentences

$$\text{“From } A \text{ follows } B\text{”} \quad \text{and} \quad \text{“}A \rightarrow B \in \text{An”}$$

express the same thing. We have seen in the lecture notes[g] that there are two characteristics of the consequence relation

(1) Transmission of truth
(2) This transmission must be based on the *formal* structure of the sentence involved.

We may apply this to the sentence "From *A* follows *B*". This is a sentence in the meta language expressing the fact that sentence *B* is deducible from sentence *A* (i.e. the truth of *A* guarantees the truth of *B*). This inference is formal because even if *A* and *B* were both false the inference would not be rendered invalid, and also the inference is made solely from an examination of the structure of the sentences *A* and *B*.

We may express these two requirements of a valid inference of the form "*A* follows from *B*" by using "$A \rightarrow B$" to indicate the relationship of sentence *A* guaranteeing the truth of sentence *B*. And we may indicate the formal nature of this inference

[g] Cf. this volume, Chapter 19, § IV.

relation by stating that this statement belongs to the class of analytical statements. "From A follows B" would then be fully expressed by the statement "$A \rightarrow B \in$ An".

14 Remarks on the Theory of Deducibility

What we have already said serves only to make clear in what way to speak about deducibility.

Now we will proceed to questions like – under what conditions is a certain sentence of the object language, say Q, deducible from another sentence of the object language, say P. The rules which answer this question can be called Rules of Inference:

1. (The Principle of Inference) Q is deducible from P, if P is a conjunction, the one component of which is some proposition, say R, whilst the other component of the conjunction consists of an implication with the implicant R and the implicate Q.

In symbols:

Q is deducible from P if P has the form "$R \;\&\; (R \rightarrow Q)$" or "$(R \rightarrow Q) \;\&\; R$".

Where we use the symbol & in order to form the *name* of a conjunctino as we have done with the symbol "→" in order to form the name of an implication. Therefore we can say

$$\begin{array}{l} R \\ \underline{R \rightarrow Q} \\ \therefore\; Q \end{array} \qquad \text{or} \qquad \begin{array}{l} \underline{R \;\&\; (R \rightarrow Q)} \\ \therefore\; Q \end{array}$$

That shall be used as some symbolism belonging to the meta language and expressing the principle of inference.

Other rules of inference

Rule 2 From any statement whatsoever can be deduced the same statement

$$\begin{array}{l} \underline{P} \\ \therefore\; P \end{array}$$

Rule 3 From any given statement whatsoever can be deduced any disjunction of which the given statement is one of the components

$$\begin{array}{l} \underline{P\qquad} \\ \therefore\; P \vee Q \end{array}$$

Rule 4 From any conjunction whatsoever can be deduced either of the components

$$\begin{array}{l} \underline{P \;\&\; Q} \\ \therefore\; Q \end{array}$$

Rule 5 From any sentence whatsoever can be deduced any implication of which
15 the given sentence is the implicate |

$$\begin{array}{l} \underline{P\qquad\quad} \\ \therefore\; Q \rightarrow P \end{array}$$

Rule 6 From any given sentence can be deduced an implication of which the negation of the given sentence is the implicant

$$\frac{P}{\therefore\ \overline{P} \rightarrow Q}$$

Rule 7 From an implication of the form $P \rightarrow \overline{P}$ can be deduced

$$\frac{P \rightarrow \overline{P}}{\therefore\ \overline{P}}$$

Rule 8 From any implication with an analytic implicant can be deduced a sentence identical with the implicate

$$\begin{array}{l} P \rightarrow Q \\ \underline{P \in \text{An}} \\ \therefore\ Q \end{array}$$

Rule 9 From any implication with a contradictory implicate can be deduced the negation of the implicans.

$$\begin{array}{l} P \rightarrow Q \\ \underline{Q \in \text{Con}} \\ \therefore\ \overline{P} \end{array}$$

(1)[h] $\vdash :: p : p \,.\supset .\, q : \supset : .q$

(2) $\vdash : p \,.\supset .\, p$

(3) $\vdash :.\, p \supset : p \vee q$

(4) $\vdash :.\, p \,.\, q \,.\supset : q$

(5) $\vdash :.\, p \,.\supset : q \,.\supset .\, p$

(6) $\vdash :.\, p \,.\supset : \overline{p} \,.\supset .\, q$

(7) $\vdash :.\, p \,.\supset .\, \overline{p} \supset : \overline{p}$

(8)
$$\begin{array}{l} \vdash p \supset q \\ \underline{\vdash p \text{ (analytic)}} \\ \therefore \vdash q \end{array}$$

(9)
$$\begin{array}{l} \vdash p \supset q \\ \underline{\vdash q \text{ (contradictory)}} \\ \therefore \vdash \overline{p} \end{array}$$

[h] The typescript uses "+", which we have replaced by "⊢". In (8) we have added "(analytic)", corresponding to the restriction given in Rule 8. We have added the rule in (9), which is missing in the typescript.

16 # The Concept of Truth

Ramsey and Johnson maintain that the Concept of Truth is redundant. We do not agree with this, but fully agree with the evidence they offer for their opinion. They have misinterpreted their own evidence. What is this evidence? Both emphasize that the statement "It is true that it rains" conveys exactly as much, not more or less than the statement "It rains".

From our standpoint truth is a property of a statement and Johnson and Ramsay's evidence can be termed a criterion for the right and correct definition of this property – namely:

We can say that the word "truth" or rather the predicate "Tr" is properly defined if it will always be that "$X \in$ Tr" is analytically equivalent with the proposition *named* by X (and expressed in the meta language). For instance if English is the meta language and German the object language, and if the English translation of the German sentence "Es regnet" is "It rains" then the following sentence (of the meta language)

"Es regnet $\in$ Tr" or

"The German sentence which consists of the two words 'es' and 'regnet' is true" must be analytically equivalent with the following sentence of the meta language, "It rains". This can be shortly expressed in the following way: $X \in \mathrm{Tr} \equiv S$
Where X is the metalinguistic name of some sentence of the object language, and where S is the meta linguistic translation of that sentence.

Now, in case the object language and meta language coincide or rather where the object language is a certain part of the meta language (for instance where the object language is the part of English which is not covered by linguistic or logical entities) "X" would be a name of a sentence of the object language and S would be simply that sentence itself (which would in this connection i.e. in a metalinguistic connection belong to the meta language) e.g. "'It rains' is true" must be equivalent with "It rains".

Tarski said that the criterion for a correct definition of truth was that from a correct definition of truth all statements of the described form "$X \in \mathrm{Tr} \equiv S$" must be deducible.

17 | We must distinguish between the criterion for a definition and the definition itself.

When we are required to give a definition we must know what precisely is the *task* which has been set for us. The demand for a definition of say, time or truth remains utterly vague if there is not given

(a) a clear indication as to the terms admissible as definitions
(b) a criterion with the help of which we can decide whether or not the task to define "truth" or "time" is successfully carried out.

Tarski succeeded in giving such a criterion, having done so it was comparatively easy to do the task set. These considerations lead to a more general statement of procedure, namely, do not try to solve a problem before you have stated it. The difficulty presented by many philosophical problems is very often due to the fact that the problem has not been stated in a clear and precise fashion. Often we become aware of problems in a vague way without being able to give a precise account of what is involved. Until the problem is correctly formulated there can be no hope of solving it.

The Logic of Thought and the Logic of Linguistic Form

We may contrast two opposing attitudes towards the study of logic. W. E. Johnson in his "Logic"[i] defines his subject as follows: "Logic is most comprehensively and least controversially defined as the analysis and criticism of thought", and again he says, "Adopting as we do the general view that no logical treatment is finally sound which does not take account of the mental attitude in thought, it follows that the fundamental terms 'true' and 'false' can only derive their meaning from the point of view of criticising a certain possible mental attitude."

The opposing point of view is represented by R. Carnap in his "Logical Syntax of Language" where he says, "The development of logic during the past ten years has shown clearly that it can only be studied with any degree of accuracy when it is based, not on judgments (thoughts, or the contents of thoughts) but rather on linguistic expressions, of which sentences are the most important because only for them is it possible to lay down sharply defined rules"[j].

18 | Of these two methods the latter has been by far the more useful and fruitful for the study of logic, but it may be that it is not so far from achieving the aims of the former method as it may appear.

The study of the purely linguistic form of sentences may reveal some very important facts about "thoughts" and "Judgments" which would not have been discovered by the direct method of treating logic as concerned with mental attitudes. To use a metaphor. The economist who stays at home and makes a comprehensive study of the statistics concerning the economic condition of a certain remote country may have a far better conception of the conditions prevailing in that country than a person who has been there and relied merely on his own observations. The indirect method may be the more fruitful one.

19 | Logic has been regarded from various points of view as being the study of judgments, thoughts, propositions or sentences.

The most extreme views are held by those who on the one side regard logic as the study of thoughts, and by those on the other side who regard logic as the study of sentences.

On the one hand thought need not be formulated at all and on the other hand the sentence may be regarded as just a series of symbols, like black marks on white paper, or else a collection of sounds.

Those who emphasise the fact that they are only interested in a logic of *meaningful* sentences and who identify this with a logic of thoughts we may call psychologists (logical psychologism).

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[i] Johnson (1921).

[j] Carnap (1937, § 1).

The opposing wing who emphasise their interest in black marks on white paper we may call the logical *formalists*.

Now between the extremes of logic as a study of unformulated thought and logic as a study of sentences there are many intermediate stages, like the one expressed in the definition, “A judgement is a linguistically formulated thought.”

Before we proceed to a logical analysis of the matter we will first give some terminological analysis which although it is rather problematic, will help us to see how the different times can be differentiated. This analysis goes back to that used by the Stoics.

1. We can distinguish between three principle entitites involved in an ordinary judgment as when someone says “It is raining.” There is an entity – a certain mental act which we can call a mental attitude of assertion taking place in the head of this person (to express it rather crudely).

The next entity is a certain part of the world or a certain spatial-temporal aggregate in which there are falling drops of water i.e. in which it is raining. (We assume that the judgement is true).

The third entity is a series of symbols which may be either spoken or written or
20 perhaps even only thought of namely, the | symbols “It”, “is”, and “raining” which go to make up the sentence “It is raining.”

Let us name these three entitites as follows:

(i) active assertion
(ii) objective basis
(iii) the sentence

In addition to these three there is a further entity namely the *feature* of the objective basis which is *designated* by the sentence. This is not the whole objective basis itself. We will call it the *fact* designated by the sentence. However, if we do not look at it from the side of the objective basis, but from the side of the sentence, we could say that it is what is *meant* by the sentence, or that it is the objective content of the sentence.

21 | The logicians who maintain that they are only interested in the meaning, or what is meant by sentences, or in the content of the sentence have never succeeded either in:

(a) saying what they mean by this, i.e. in saying *how* to distinguish this content from

    (i) the asserting act
    (ii) the sentence
    (iii) the objective basis

    i.e. some of them emphasized that the content is not the psychological attitude but rather its object, and all of them emphasized that it is not the sentence.

(b) or in showing the significance of their emphasis i.e. in showing what difference it does make to logical theory or (especially) in showing its advantage for logical theory. It is clear that only in contrasting their results with the results of a purely formal analysis, could the significance of their emphasis be shown. This has never been done because of the universal agreement of the insignificance of the verbal formal investigations.

Dr. Popper's view is that hardly any progress has been made since the Stoics to analyse the meaning of meaning (c.f. Ogden and Richards quotation of Gomperz[k]) and especially (b) (see above) was entirely overlooked. Dr. Popper maintains further that the purely formal analysis has opened the way for deeper penetration into all logical problems – especially is this the case with the theory of meta logic.

The development of this theory which is based upon the formal analysis of sentences has exposed many simple though serious confusions which may arise in treating logic as a study of thoughts. Of these we have already noted the confusion of the proposition with its name, and the mistaken conclusion that the concept of truth is redundant. Thus, the formal treatment of logic has cleared up issues which the treatment of logic as thought clouded over.

22 | 2. Most logicians (until recently) have assumed that a sentence consists of black marks on white paper or something of that sort – and that is all. Its meaning or content or anything of that kind must be found in something outside – something mental (or factual) for instance.

They did not see that apart from the factors mentioned, a language is more than a mass of symbols – that it involves a certain system of rules which declare how to use such symbols. These rules may in part be non formal, like for instance the rules for using the different descriptive terms of a language or the rules of the use of words like "rain" or "apple", and so on. Anyone learning a language discovers such rules by finding out in what kind of situation he has to use the words "rain" or "apple".

These rules refer to certain situations of a practical kind, that is, they refer to something outside of the language and correspond therefore to the old idea of meaning as referring to something outside the sentence.

But the formal analysis has shown that the problem of the meaning of the descriptive constants is of comparatively small significance compared with the problem of the meaning of such words (constants) like "is" ("is an element of", "has the property", "is a part of", "is a sub-class of"), "and", "if . . . then".

The meaning of such words is of the greatest importance – their analysis shows that loosely speaking they make a language *to be* a language, and their analysis shows that the meaning of such words can be found by analysing their *rules of use* which turn out to be entirely *formal* rules i.e. rules which have only to do with the handling of symbols and which do not refer in any way to something outside of the language. These rules are of the following type: ". . . $\in$ . . ." forms a sentence if on the left of "$\in$" and on the right of "$\in$" there are two different kinds of symbols; thus: If "Socrates $\in$ man" is a sentence, "man $\in$ Socrates" *cannot* be a sentence.

23 | The problem of meaning can be approached by two radically different methods.

The traditional approach is by way of psychology, that is by relating the word or sentence to someone's field of experience, placing it in its psychological background.

The opposing method is to treat it in a purely formal linguistic manner; the meaning of a word or sentence being the place it occupies in a linguistic context and the purely formal rules which govern its use in that context.

Carnap asserts that the whole problem of meaning can be solved by the second

[k] Cf. Ogden and Richards (1923).

method. Dr. Popper thinks that this is going further than the evidence warrants but that nevertheless the purely formal analysis does go a long way towards solving the problem. He thinks that it has been justified by its results and that it has certainly been a more successful approach than the psychological one.

Tarski, who also developed the formal side upon which Carnap places such emphasis was particularly concerned with showing the relations between the purely formal structure and the context of a sentence. While Carnap developed a language which deals only with matters of syntax, Tarski developed a language capable of referring to the *object* as well as to meta linguistic entities. Thus his language is able to show the connection between the two. Tarski divides the subject of meta-logic into two parts:

1. The part dealing with the purely formal structure of language, and
2. The portion which treats of the relation between this formal structure and the context of the sentence. This he calls semantic.

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24 | One may distinguish between a more radical and a less radical attitude towards the role of logical analysis and the nature of philosophical problems.

Wittgenstein first took the standpoint that philosophical problems were nothing but linguistic confusions and that when these were analysed the problems were not solved but just disappeared. Wittgenstein thought that philosophical problems were of the following type. Instead of saying that one's watch was going and has now stopped one might say that the "go" of the watch had gone and might then proceed to ask, "where has the 'go' gone?" This example would be analogous for instance, with the philosophical problem of mind and body.

This radical attitude was also the one adopted by Carnap in his "Logical Syntax of Language"[1]. He said that all the problems of philosophy could be reduced to those of syntax, that is to questions concerning the rules of the use of language – problems which could not be solved in these terms were mere linguistic confusions. Thus all that remained of philosophy was the study of the formal structure of sentences.

Dr. Popper adopts a less radical attitude. He thinks that while the purely formal study of the structure of language is undoubtedly of great importance and clears up many philosphical difficulties yet residual problems may remain which are not purely a matter of linguistic structure. This attitude was confirmed by the work of Tarski who showed that the problem of truth could only be dealt with satisfactorily by getting beyond the formal structure of the sentence to its relations with the object.

One of the main tasks of analysis is to reformulate the problems of philosophy. This may be done partly by the use of more accurate terminology. For example, for the term "knowledge" should be substituted "scientific statements", for "sources of knowledge" should be substituted "method of testing scientific statements", for Kant's "limits of knowledge" should be substituted "characterisation of the method of science." Such
25 a change of terminology is necessary when discussing the philosophy | of science for the term knowledge implies "truth". We cannot, strictly speaking, use the phrase

[1] Carnap (1937).

"scientific knowledge" for if you find that what you professed to know was incorrect then you cannot say that you had knowledge at all. Since no scientific statements can be conclusively proved it is misleading to talk about scientific knowledge.

The Viennese Circle has been concerned with the following main problems:

1. The Nature of Philosophy
2. The Nature of Logic and Mathematics
3. The Nature of Science – especially its empirical basis which involves the problem of induction
4. The Nature of Language
5. The special problem, traditionally known as the body-mind problem, formulated in the typical form of "Is psychology dealing with something different from physics".

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26 | In 1931–32 Carnap, influenced by Neurath, developed a standpoint which he called "Physicalism" (from Neurath's side the intention was to develop something like a modernised materialism, that is connected with the fact that Neurath was interested in Marxism though not an orthodox Marxist).

The philosophical idea was the following: Behaviourism is *methodologically* correct i.e. a psychological statement can only be tested with the help of *observation of the behaviour* in the widest sense of the word. Even the so-called introspective method can be said to be based on observation of the behaviour, because its results must be formulated with the help of sentences, but these are communications i.e. bound up with behaviour such as mouth movements or movements of the hand in writing and so on. The only empirical methods available in psychology are observing what a man does or what a man says and writes.

Carnap expressed this in the following way: All terms of psychology are physical terms or rather must be capable of being reformulated within the language of physics. If I say "Mr. A is excited" then I refer to certain typical reactions.

A statement of that kind is fundamentally not different from a statement like, "Mr. A is ill" or "Mr. A has a broken leg" (the last is obviously a sentence of a physical kind). The thesis of Physicalism is therefore the following: Every scientific theory, if scientific must be able to be formulated in the language of physics. This last formulation indicates already a certain connection with what Dr. Popper calls the Kantian problem – namely the characterisation of science and its limits towards metaphysics; i.e. an attempt to characterise the empirical feature of science. This is here identified with being able of formulation in physical terms. Because all our empirical observations are of temporal and spatial happenings.

27 | Wittgenstein attacks metaphysics from the point of view of the verification of sentences. A sentence is only verifiable when it is possible to analyse it into its constituent atomic propositions. By atomic proposition he means *given sense data* as expressed by such propositions as "The grass is green", "Water is wet" and so on. These atomic propositions or given sense data may be verified immediately by empirical observation. Any sentence therefore which cannot be reduced to these

immediately verifiable atomic propositions cannot itself be verified and hence is meaningless. Such sentences are typically those of metaphysics.

Dr. Popper criticises this standpoint from certain aspects. It is psychologically wrong – there are no *givens*, what we have is always an interpretation – we can either take it as a basis for further interpretation or else analyse it into its components – but however far we analyse we never get beyond an interpretation. Wittgenstein has just reproduced in the dialect of the Viennese Circle the older positivist view namely, that the mind contains ultimate sense data to which science must reduce all knowledge.

Wittgenstein's view has affinities with the Kantian view of the empirically given upon which, according to Kant, the mind worked to produce scientific theories – the empirically given was not itself sufficient for science, it had to be combined with the a priori forms of the mind. Wittgenstein's theory has the further consequence that the generalizations of science must also be classed with the sentences of metaphysics, as meaningless. For they cannot be completely reduced to their constituent atomic propositions and therefore they are not verifiable. This question of verification is bound up with the problem of induction.

We may formulate this problem by indicating the apparent contradiction involved between the following statements:

1. Hume's analysis proved that induction cannot be based on a pure observational basis.
2. The fundamental thesis of every empiricist is that only observation and experiment are *decisive authorities* about all kinds of scientific sentences.
3. The fact that science consists mainly of strictly universal sentences or theories.

28 | 1 and 3 apparently contradict 2, and this represents Kant's view. He held that there must be a non-empirical basis for scientific theories. 2 and 3 contradict ⟨1⟩ and represent the view of Bacon and Mill (who ignored Hume's criticism of induction).

1 and 2 contradict 3, and this is the consequence of Wittgenstein's view.

These contradictions however are only apparent. There would be a real contradiction if in 2 instead of saying that observation and experiment are *decisive authorities*, we said that they were a *complete verification* of scientific sentences.

All three standpoints, however, are correct, for although only experience *decides about* scientific theories there is no *conclusive positive decision*.

By experiment and observation we test theories i.e. we attempt to show that they are false, but we can never succeed in conclusively verifying them.

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29 | In any deductive inference there is no transmission of truth from the conclusion to the premises. But does not this depend upon the precision and completeness with which the conclusion is stated? In the following argument for example,

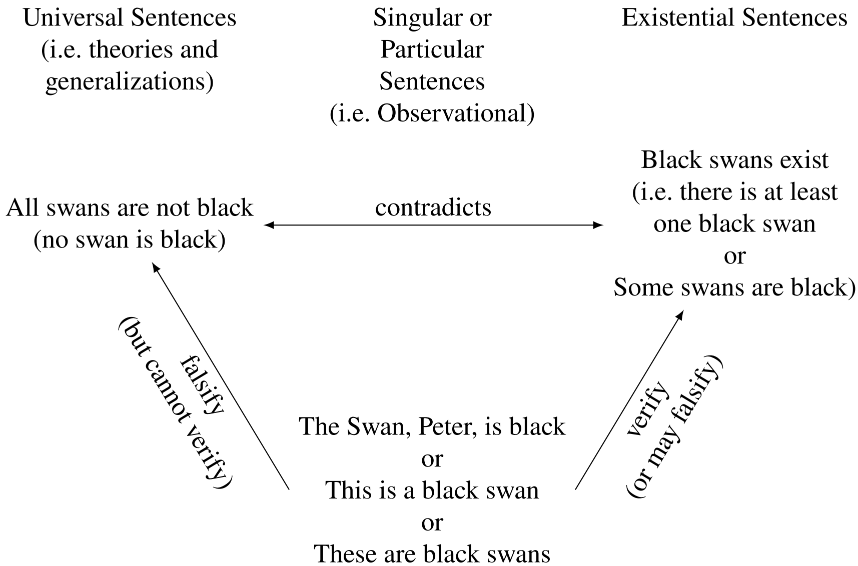
Prussic acid is a deadly poison
A took a dose of Prussic acid
therefore *A* is dead

we cannot argue from the statement "*A* is dead" to the statement "Prussic acid is a deadly poison," or to the statement "*A* took a dose of Prussic acid", for it might be the case that he died a natural death, or was shot or stabbed. We have not, however, stated the conclusion precisely enough. If we said

> Prussic acid is a deadly poison which acts in such and such a way
> *A* took a dose of Prussic acid
> therefore *A* died such and such a death,

we could argue from the conclusion, "*A* died such and such a death" to the premise "*A* took a dose of Prussic acid", with the help of the generalization "Prussic acid is a deadly poison which acts in such and such a way".

Our first conclusion above was "weak", we could not use it in arguing back to the truth of one of the premises, but our second more complete conclusion was "strong" since with the help of a generalization we could do this. Sometimes the conclusion is more than strong, in which case it is in itself sufficient to demonstrate the truth of its premise. In such a case we have two sentences which are mutually deducible – they are said to be equipollent i.e. when $A > B$, and $B > A$ then $A \equiv B$. When it is the case, however, that from a generalization and certain initial conditions we deduce certain consequences which can be expressed as observational sentences or basic propositions, then if the conclusion is true we are unable to deduce from it the truth of the generalization: if it is false, however, we may conclude that the generalization is false. In this connection we may note that there is a symmetry between the pure
30 universal and the pure existential sentence and there is a | symmetry between each of them and a singular or observational sentence. The universal and existential sentences may contradict each other, while the existential sentence may be verified by the observational sentence and the universal may be falsified by it.



2. Can a statement be conclusively falsified any more then it can be conclusively verified? To falsify a theory we must show that it leads to a conclusion which can be

contradicted by observational statements or basic propositions. But no such empirical statements are beyond the possibility of error, therefore it cannot be shown that any theory is certainly false any more than it can be shown that it is certainly true.

It is true that any statement of fact may be false and that therefore in the last analysis we cannot be certain that we have falsified a theory any more than we can be certain that we have verified it. But this is a ground for the uncertainty of falsification and verification alike. We can see, however, that the verification of theories is impossible even before we cast doubts upon the truth of our basic propositions (i.e. observational sentences) and for an entirely different reason.

In any investigation or process of reasoning we must be in by taking something as given – the empiricist regards it as safer and offering less possibility of error to
31 take basic propositions, or observational sentences, as given, in contrast | with the rationalist who assumes certain general laws.

On the empiricist basis we may examine the claims of verifiability and falsifiability. As we have seen above a conclusion (or prediction) which consists solely of basic propositions cannot transmit truth back to its premises. Thus verifiability fails to fulfil the logical requirements necessary for making a deduction.

It might be argued that every basic proposition itself implies a theory, for if doubt is expressed concerning it we proceed to test it on the grounds of some generalization or theory. But this occurs on a different level of argument, we are no longer taking our basic propositions as given but are treating them as objects for investigation. The point is that before an argument can be developed we must take something as given, although on another occasion, or at a later stage, we may cast doubt on, or inquire into the truth and falsity of what we previously took as given. Thus, falsification while sharing with verifiability the uncertainty of basic propositions does not, like the latter, lack logical justification.

3. The problem of Induction[m]

The problem of induction was raised by Hume when he showed that it was impossible to make logically justifiable *positive* decisions concerning theories and hypotheses like those formulated by science.

If we ask, why does science formulate theories and hypotheses? – then the answer is that it does so in order to make predictions.

Then the main question arises – On what grounds does it prefer one theory or hypothesis to another? While, as Hume showed, it cannot make conclusive positive assertions about the truth of its hypotheses it can *test* them by trying to falsify them.

32 | While it cannot arrive at positive conclusions about the truth of a theory – it can come to negative conclusions to the effect that a theory is false since the predictions based on it were not fulfilled.

[m] The number 3 probably refers to the corresponding list item on p. 25 of the typescript.

Thus a solution is given to the problem of induction by characterising the aims and methods of scientific procedure.

The aim of science is to make predictions for which purpose it frames hypotheses – it tests these hypotheses in the course of using them i.e. the very making of predictions constitutes the testing of the hypotheses – if the predictions are untrue the hypotheses are falsified.

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33 | Basic propositions are statements that some characteristic can be observed at a particular time and place. We may call the particular spatial area indicated the “surrounding”. Hence basic propositions predicate a characteristic of a particular “surrounding”.

Universal propositions, on the other hand, do not assert that anything exists e.g. a statement about “all X are Y” is not falsified if in fact there are no X’s, but it is falsified if in fact there exists one or more X’s that are not Y’s. The universal proposition “All X’s are Y’s” does not assert X but it does exclude from existence any X that is not Y.

We may bring out the relationship between the various types of propositions by using the following symbolism. ( ) is the universal operator, or generalisator so that $(X)(X \in \text{swan} \supset X \in \text{white})$ is equivalent to “All swans are white” (X is a variable satisfied by “swan”)

($\exists$) is the existential operator, or particularisator, so that $(\exists X)(X \in \text{swan} \;.\; X \in \text{black})$ is equivalent to “There is at least one swan which is black”. Substituting the propositional function φX for $(X \in \text{swan} \supset X \in \text{white})$

$$\overline{(X)(\varphi X)} \equiv (\exists X)\overline{(\varphi X)}$$

i.e. the contradictory of the universal proposition is equivalent to an existential proposition, and also

$$\overline{(\exists X)(\varphi X)} \equiv (X)\overline{(\varphi X)}$$

i.e. the contradictory of the existential proposition is equivalent to a universal proposition. Substituting ψ for φ such that $\psi X \equiv \overline{\varphi X}$

$$(X)(\psi X) \equiv \overline{(\exists X)(\varphi X)}$$

then if “a” is a particular surrounding[n]

$$\varphi a \rightarrow (\exists X)(\varphi X) \longleftrightarrow \overline{(X)(\psi X)}$$

i.e. a basic proposition implies an existential proposition which is equivalent to the contradictory of a universal proposition.

34 | A basic proposition is concerned only with what can be observed at a certain time and place, hence its contradictory *may* not be a basic proposition itself. While the contradictory of the basic proposition “Here is an elephant,” namely the proposition,

[n] What is meant in the following is: $\varphi a \rightarrow (\exists X)(\varphi X)$, and $(\exists X)(\varphi X) \equiv \overline{(X)(\psi X)}$.

"There is no elephant here" may be regarded as a basic proposition also, there are occasions when inability to detect the presence of something is no guarantee of its absence. Thus, while "There is a needle in this haystack" is a basic proposition, its contradictory "There is no needle in this haystack" is not.

However, there is another and perhaps more important reason for stressing this fact and that is that basic propositions should not be deducible from universal propositions.

We can arrive at basic propositions only by observation, or by deduction, from a universal (general law) *and* another basic proposition (initial condition).

But the contradictories of some basic propositions *follow from* universals and therefore these contradictions cannot be themselves basic propositions.

G = general law I = initial condition F = forecast

$$\begin{array}{ll} G & \text{universal proposition} \\ I & \text{basic proposition} \\ \hline \therefore\ F & \text{basic proposition} \end{array}$$

$$\begin{array}{ll} \overline{G} & \\ \hline \therefore\ I + \overline{F} & \text{basic proposition} \end{array}$$

$$\begin{array}{ll} G & \\ \hline \therefore\ \overline{I + F} & \text{not a basic proposition} \end{array}$$

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35 The Development of the Vienna Circle

The movement originated with the appointment to a Chair of Philosophy at Vienna of men such as Mach and Boltzmann whose training had been in the physical sciences and who were themselves eminent physicists. Their interests were naturally in the methods of science rather than in metaphysical speculation. Schlick who was appointed to this chair in 1922, also had training in the physical sciences and around him developed the Vienna Circle.

Russell's work in logic and the foundations of mathematics was introduced to the circle by mathematicians at Vienna, and interest was also aroused by Russell's attempts to explain the concept of physics as logical constructions, and by his work "Our Knowledge of the External World"°.

Up to this point the Viennese Circle had not developed positivist views, being more inclined towards realism. They were not, however, very interested in such questions

° Russell (1914).

of[p] They had already adopted an anti-metaphysical attitude and were inclined to believe that many philosophical problems were due to confusion of words.

A new stage was reached with the appearance of Wittgenstein's ⟨Tractatus⟩ which greatly influenced the circle. The conception of atomic propositions as statements concerning given sense data, and the notion that any statement not reducible to these elements was meaningless, introduced a positivist attitude, gave a definite ground for the attack upon metaphysics and strengthened the influence of Russell's logical work.

Then came Carnap who had been very much influenced by Russell. He wished to give an account of the whole field of experience in terms of logical constructions. This was termed logical solipsism by which he meant that each person's conception of the world was a logical construction of his own observations.

Carnap thought that he could show how such concepts were built up and give all definitions in these terms with the help of only one non-logical relationship, namely
36 the relationship | expressed by the sentence "This reminds me of that"[q]. From such a relationship Carnap hoped to build up all his definitions. Thus the notion of before and after could be defined by saying, that when *A reminds me of B*, then *B* is before *A* and *A* is after *B*.

Dr. Popper pointed out the fundamental defect of this system namely that if everything that we were acquainted with is a logical construction out of our immediate observations, then we could not make predictions or talk about the future in any way since we had not observed and could not as yet have made a logical construction out of it.

Since logical constructions could not be made prior to the event and could have no reference to the future, it followed that there was no place for scientific laws within this system and the activity of scientific investigation was left unaccounted for.

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37 Criticism of Physicalism and the Unity of Science

Carnap and Neurath developed from Wittgenstein's conception of atomic propositions, the theses of "Physicalism" and the "Unity of Science".

They said that statements with an empirical content such as those of science could only be verified and thus rendered significant by the protocol statements which they implied.

A protocol statement was the statement of the intermediate sense experience of some particular person and as such it could only be verified by that person. Thus while protocols are (according to this view) the ultimate source of verification yet they are not intersubjective, for each person's protocols are private to himself.

Therefore, said Carnap, the protocols must be translated into a universal and intersubjective language, i.e. a language in terms of which all states of affairs can be

[p] Sentence ends abruptly in the typescript.

[q] This theory is developed in Carnap (1928).

expressed. “Such a language”, said Carnap[r], “is the physical language which expresses a quantitatively determined property of a definite position at a definite time.” This introduces the notion of the Unity of Science, for although each science may have its own terminology it must refer to certain physical determinations expressable in the physical language, and the definition of all such terminology must be in relation to these physical determinations.

There are two problems involved in the conception of Physicalism. One could say that sentences like, “I have a toothache” i.e. “at this time, and at a particular spot in my jaw there is a pain”, or for that matter one could say “I feel sad”, i.e. “somewhere in the region defined by my body there is a certain feeling which occurs when I am aware of certain situations.” There is no reason why we should not call the states indicated by these sentences, physical determinations, and we might regard it as something in common to all empirical observations, and all empirical sciences that they deal with qualities or characteristics of a certain spatial-temporal region. Carnap, however, |
38 although he may have this in mind means something further. An observer cannot feel *my* toothache or *my* sorrow, all that he observes about me when I tell him that “I have a toothache” or “I am sad” are certain facial expressions and actions. These Carnap regards as the physical determinations, and the statement of them as the sentences of the physical language. Thus my statement “I have a toothache” and “I am sad” in the physical language can only be translated as “I have such and such an expression on my face”.

Now Carnap regards these latter sentences as *translation* of the former ones – that is they are exactly equivalent. The sentence, “I have a toothache” ≡ the sentence “I have such and such an expression on my face.”

This view, however, is not correct. It may be that when I truthfully assert the psychological sentence A, that what is asserted by the physical sentence B is also always the case, and it may be in some cases (perhaps all) that what is asserted by the physical statement is an essential part of the state of affairs referred to by the psychological statement, but it is certainly not *all* that is meant, or referred to, by the psychological statement, and it is not all that the observer understands by it; e.g. he sympathises not about the aspect of my face but about what is going on in my tooth. Furthermore, I may make psychological statements without being aware of what my overt behaviour looks like, while the overt behaviour corresponding to two different statements like “I have a toothache” and “I am sad” may be exactly the same.

Therefore, it seems false to speak of *translation* into the physical language, or to imply that the physical language expresses precisely the same thing as is expressed, say, in the language of psychology. It may express part of it, or it may express an accompaniment, and it may be possible to infer from a statement of the physical language a certain statement of the psychological language (usually with the aid of some generalisation).

It is very important to note this criticism for Carnap makes use of this equivalence of statements in the following way. Let us imagine that one statement in the material
39 | mode (i.e. non-physical) of speech contradicts some other statement in the same

[r] Carnap (1934b, p. 52f.).

mode. Then let us *translate* them into the physical language. In the physical language they do not contradict each other, hence the equivalent statements in the material mode of speech do not really contradict each other. But if the relationship between the two languages is not one of equivalence then it will not be possible to solve problems in this way.

The whole theory outlined and criticised above is typically an idealist approach to such problems – the attempt is made to guarantee the truth of certain propositions by equating the mind with its objects, in this case protocol sentences with sentences about physical determinations, the former always containing a reference to the observer and the latter being the common object of knowledge.

In criticism of this view Dr. Popper pointed out that the so-called protocol sentences are exceedingly difficult to verify, and that science does not test its theories by reference to statements like "I *perceive* so and so", but by statements on predictions such as "At a certain time and place there is so and so", such sentences unlike the former are not bound up with the experience of a particular person who is inaccessible to other people. On the contrary *anyone* can proceed to test the statements of science by proceeding to see if "so and so" is "there" at "a certain time". Such simple objective statements with which scientific theories are tested Dr. Popper calls "basic propositions".

Carnap acknowledged the validity of this criticism and having failed to provide definitions by means of translations into the physical language and attempted to provide definitions in another way e.g. Green could be defined as the property of the class composed of grass, trees, and so on, naming all the particular green objects with which one is acquainted. Dr. Popper pointed out that here the old problem of induction appeared in that one could never be sure of having exhausted the total enumeration of green objects – hence if one came across an object with which one
40 was not previously acquainted, say a piece of jade, then either it would not | *have* the property green since it did not belong to the class of enumerated objects or if one granted that it was green then the previous definition of green must have been false.

Dr. Popper considers that definitions should be considered as an operation with terms analogous to the deduction of sentences. The following table shows the points
41 of comparison between these two operations. |

	Deduction of Sentences	*Definition of Terms*
1.	All scientific theories are deductive systems	
2.	They start with certain sentences which are just assumed.	They make use of certain terms, or concepts, or ideas which are just assumed.
3.	That is not deduced within the theory	That is not defined within the theory
4.	We can call them *axioms* or postulates or fundamental *hypotheses*	We can call them primitive *ideas* or primitive *concepts* of the theory, or fundamental terms or *universals*

5.	From the axioms or postulates we can deduce a certain body of theory	With the help of the primitive ideas we can define a certain body of *derived* terms
6.	The deduction – every deduction starts with one ore more axioms	Every definition starts with one or more primitive terms
7.	and it presents in such a way that a series of sentences is constructed such that every sentence of the series is either an axiom or immediately deduced from one of the foregoing sentences.	and it proceeds in such a way that a series of terms is constructed such that every term of the series is either a primitive idea or immediately derived from one of the foregoing terms.
	Thus the procedure is based upon some rules which describe certain transformations of sentences as permissible thus defining what is meant by immediately deducible from	Thus the procedure is based upon some rules which describe certain substitutions of terms as permissable, thus defining what is meant by immediately derivable from
8.	Rules of deduction	Rules of definition
9.	Positivism with regard to sentences: – Criterion of verifiability	Positivism with regard to terms: – the terms must be such that they can be constituted.
	Verification means deduction from recognized basic atomic propositions with the help of some logical rules and analytical sentences	Constitution means definition with the help of some terms, based on observation (basic or atomic terms) with the help of certain logical rules and purely logical terms
	BUT	
	hypotheses cannot be verified only particulars can be	Universals cannot be constituted only particular terms of proper names can be.

Thus we can see that the problem is one of universal sentences and universals.

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42 The Nature of Logic and Mathematics

In his works on Logic and the foundations of Mathematics Russell speaks of mathematics as being a part of logic. Strictly, however, in addition to the rules of logic, mathematics requires certain postulates which are not themselves logical. Russell, in fact, uses three such postulates although perhaps only one, the axiom of infinity is necessary.

We can apply all mathematical systems to nature although some are more convenient than others in this respect. Must we in the final analysis decide between various mathematical and logical systems on empirical rather than formal grounds? The evidence as yet is not conclusive but it appears as if we can make our preference on purely formal considerations.

Axiom Systems

Descriptive terms can be replaced by, or interpreted as variables. A set of things which fulfils this system is called a model of the system (cf. Carmichael, The Logic of Discovery, Chapter concerning deductive systems)[s]. A system is called categorical if any two models of it are *isomorphic*, that is if the relations between their terms have the same structure (structural characteristics are symmetry, reflexiveness and so on).

An Axiom system can be said to define or determine its primitive terms in an implicit manner so far as it determines the models in a certain way. It can never characterize them entirely, and only in terms of a structure. Thus it cannot do more in that direction than to determine the structure of the model completely, i.e. to be categorical.

We have to distinguish from categoricalness another sort of perfectness, or completeness.

A system can be called complete if every sentence formulated in terms of the system can be either deduced or refuted with the mere help of the axioms of the system. Arithmetic is categorical but not complete in the second sense, as Gödel has
43 shown. (But it is complete in a third sense, namely it is | possible to show in the meta theory that every sentence of Arithmetic, even a "Gödel" sentence, is either true or false and whether it is true or false – if the axioms of arithmetic are accepted as being true.)

The possibility of characterising systems, not only as consistent or inconsistent but also from the standpoint of their different degrees of completeness (in at least three different senses or dimension) shows that there are certain possibilities formally to distinguish between the merits of different concurring deductive systems.

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[s] Carmichael (1930, Ch. III, cf. also Ch. II).

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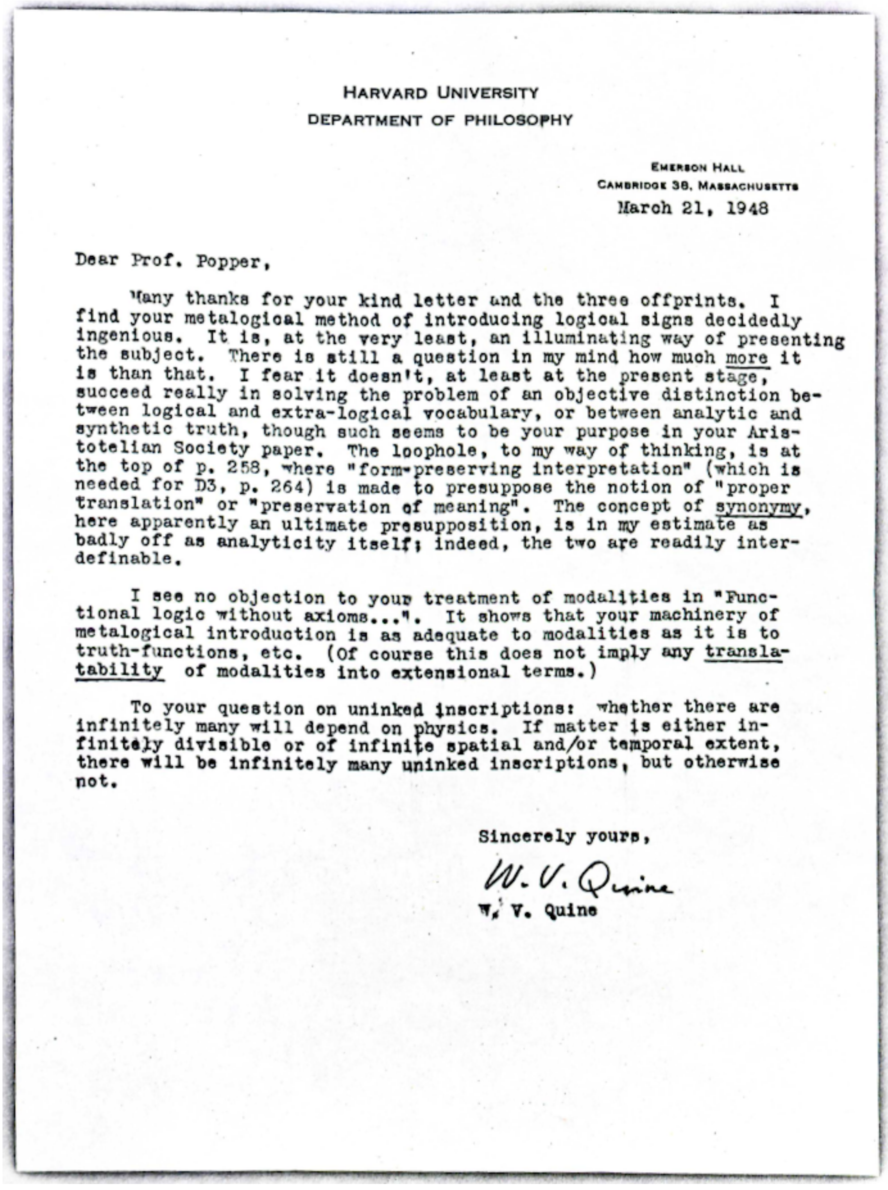
Part III
Correspondence



HARVARD UNIVERSITY
DEPARTMENT OF PHILOSOPHY

EMERSON HALL
CAMBRIDGE 38, MASSACHUSETTS

March 21, 1948

Dear Prof. Popper,

Many thanks for your kind letter and the three offprints. I find your metalogical method of introducing logical signs decidedly ingenious. It is, at the very least, an illuminating way of presenting the subject. There is still a question in my mind how much more it is than that. I fear it doesn't, at least at the present stage, succeed really in solving the problem of an objective distinction between logical and extra-logical vocabulary, or between analytic and synthetic truth, though such seems to be your purpose in your Aristotelian Society paper. The loophole, to my way of thinking, is at the top of p. 258, where "form-preserving interpretation" (which is needed for D3, p. 264) is made to presuppose the notion of "proper translation" or "preservation of meaning". The concept of synonymy, here apparently an ultimate presupposition, is in my estimate as badly off as analyticity itself; indeed, the two are readily interdefinable.

I see no objection to your treatment of modalities in "Functional logic without axioms...". It shows that your machinery of metalogical introduction is as adequate to modalities as it is to truth-functions, etc. (Of course this does not imply any translatability of modalities into extensional terms.)

To your question on uninked inscriptions: whether there are infinitely many will depend on physics. If matter is either infinitely divisible or of infinite spatial and/or temporal extent, there will be infinitely many uninked inscriptions, but otherwise not.

Sincerely yours,

W. V. Quine

W. V. Quine

Letter from Quine to Popper of 21 March 1948 (KPS Box 340, Folder 9).

Chapter 21
Popper's Correspondence with Paul Bernays

Karl R. Popper and Paul Bernays

Abstract Paul Isaak Bernays (1888–1977) was a Swiss logician who worked on the foundations of mathematics and set theory. (More biographical information can be found in Lauener, 1978.) From 1919 until 1933, when his *venia legendi* was revoked by the Nazi regime, he worked at the Mathematical Institute in Göttingen. He subsequently returned to the ETH in Zürich, where he would stay for the rest of his career. During this time he wrote *Grundlagen der Mathematik I, II* (Hilbert and Bernays, 1934, 1939) together with David Hilbert. Popper owned a copy of both volumes, which still exist with many of his own marginal notes in the Karl Popper Collection Klagenfurt. Popper also frequently cites the *Grundlagen* in his articles on logic. The letters reproduced here concern a meeting between Popper and Bernays which took place in April 1947 in Switzerland. At that time, Popper and Bernays pursued a joint publication on logic, a draft of which is reproduced in Chapter 14 of this volume. This joint work was never published, but these letters provide the context that helps explain its genesis. The correspondence furthermore contains a discussion of some of the criticisms raised by the reviewers of Popper's articles.

Editorial notes: The letters are either from the the Karl Popper Collection Klagenfurt or from the Paul Bernays estate, which is housed at the ETH Zürich. The rights to the letters of Paul Bernays belong to Ludwig Bernays.

21.1 Popper to Bernays, 22 December 1946

ETH Zürich: Hs 975 : 3649. 1 sheet of lined yellow paper, handwritten, 2p.

Dec 22nd '46

Dear Professor Bernays,

D. Binder et al. (eds.), *The Logical Writings of Karl Popper*,
Trends in Logic 58, https://doi.org/10.1007/978-3-030-94926-6_21

John Findlay's Formulation of a Goedelian sentence[a] could be reconstructed like this:

We first introduce the technical term "*Quotation name*" (of an expression) – *Anführungsname* – der Name eines Ausdruckes der dadurch gebildet wird dass man den Ausdruck in Anführungszeichen setzt. Dann schreiben wir:

We cannot demonstrate the statement obtained by substituting for the variable in the statement-function "We cannot demonstrate the statement obtained by substituting for the variable in the statement-function x the quotation name of this statement-function" the quotation name of this statement-function.

Oder Deutsch:

2 Wir können den Satz nicht beweisen den man erhält | wenn man für die Variable in der Satzfunktion "Wir können den Satz nicht beweisen den man erhält wenn man für die Variable in der Satzfunktion x den Anführungsnamen dieser Satzfunktion einsetzt" den Anführungsnamen dieser Satzfunktion einsetzt.

Natürlich kann man auch den *Lügner* so formulieren (das scheint Findlay entgangen zu sein):

Der Satz den man erhält wenn man für die Variable in der Satzfunktion "Der Satz den man erhält wenn man für die Variable in der Satzfunktion x den Anführungsnamen dieser Satzfunktion einsetzt, ist falsch" den Anführungsnamen dieser Satzfunktion einsetzt, ist falsch.

Vielen Dank und herzliche Grüße
Ihr K. R. Popper

Liste von geborgten Abhandlungen.[b]

Church: Introduction
Bernays: Logik & Mathematik (MS)
Gentzen, Satzsysteme
Herz: Abh. der Frieschen Schule[c]
üb. Satz-Systeme Math. Ann. 1929
Vortrag, Annalen d. Philos.
Tarski: Fundament. Begr. (Polnische Zeitschr.)
Jaskowski:[d] Annahmenkalkül
Glivenko: 2 Abhandl.

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22 December 1946

Dear Professor Bernays,

John Findlay's Formulation of a Goedelian sentence[a] could be reconstructed like this:

[a] Cf. Findlay (1942) and also KPS Box 36, Folder 1. Findlay had written the article in Otago, New Zealand.

[b] Popper refers most likely to some of the following articles: Church (1944), Bernays (1940), Gentzen (1932), Hertz (1928, 1929a,b, 1935), Tarski (1930b), Jaśkowski (1934), Glivenko (1928, 1929, 1936).

[c] Hertz: Abh. der Fries'schen Schule

[d] Jaśkowski

We first introduce the technical term "*Quotation name*" (of an expression) – *Anführungsname* – the name of an expression which is formed by putting the expression in quotation marks. Then we write:

We cannot demonstrate the statement obtained by substituting for the variable in the statement-function "We cannot demonstrate the statement obtained by substituting for the variable in the statement-function *x* the quotation name of this statement-function" the quotation name of this statement-function.

Or in German:

2 Wir können den Satz nicht beweisen den man erhält | wenn man für die Variable in der Satzfunktion "Wir können den Satz nicht beweisen den man erhält wenn man für die Variable in der Satzfunktion *x* den Anführungsnamen dieser Satzfunktion einsetzt" den Anführungsnamen dieser Satzfunktion einsetzt.

Of course one can also formulate the *liar* in this way (it seems that Findlay has overlooked this):

The statement that one obtains by substituting for the variable in the statement-function "The statement that one obtains by substituting for the variable in the statement-function *x* the quotation name of that statement-function, is false" the quotation name of that statement-function, is false.

Many thanks and warm regards,
Yours, K. R. Popper

List of borrowed articles:[b]
Church: Introduction
Bernays: Logik & Mathematik (MS)
Gentzen, Satzsysteme
Herz: Abh. der Frieschen Schule[c]
üb. Satz-Systeme Math. Ann. 1929
Vortrag, Annalen d. Philos.
Tarski: Fundament. Begr. (Polnische Zeitschr.)
Jaskowski:[d] Annahmenkalkül
Glivenko: 2 Abhandl.

21.2 Popper to Bernays, n.d.

ETH Zürich: Hs 975 : 3652. 1 sheet with letterhead of the LSE, handwritten, 2p.

The London School of Economics and Political Science
(University of London)
Houghton Street, Aldwych, London, W.C.2

Dear Professor Bernays,

Entschuldigen Sie den englischen Anfang – ich bin so gewohnt daran.

Sie erinnern sich wohl an unsere Diskussionen, und an Ihre freundliche (und für mich überaus ehrenvolle) Anregung, eine kleine Arbeit über „Deducibility" gemeinsam zu veröffentlichen. Ich habe inzwischen weiter gearbeitet, hauptsächlich

über „Derivation versus Demonstration (or Proof)“, und habe einige wirklich interessante Ergebnisse erzielt: – ich glaube daß Sie, wenn Sie sie hören werden, zugeben werden, daß die Unterscheidung doch recht wichtig ist. Aber diese Untersuchung setzt die über Deducibility voraus. Ich habe daher in den letzten Tagen an der Arbeit gearbeitet die wir (wenn Sie Ihren Vorschlag aufrecht erhalten)
2 gemeinsam herausgeben sollen; und ich hoffe, daß | ich Sie Ihnen in einigen Tagen zuschicken kann.

Ich komme anfang April wieder in die Schweiz. Zwischen dem 1. und 10. nehme ich an einer Konferenz über politische Philosophie teil[e], würde Sie aber *sehr* gerne unmittelbar nach dem 10. sehen. Sollte Ihnen das unmöglich sein so würde ich versuchen, ein paar Tage früher zu reisen, um Sie zu sehen, aber das wäre mit Schwierigkeiten verbunden. Würden Sie so lieb sein mir postwendend *eine* Zeile, womöglich Luftpost, zu schicken, ob Sie am 11. und/oder 12. April frei sind? Es muß nicht in Zürich sein.

Schönste Grüße,
Ihr
K. R. Popper

The London School of Economics and Political Science
(University of London)
Houghton Street, Aldwych, London, W.C.2

Dear Professor Bernays,

Please excuse the English beginning – I am so used to it.

You probably remember our discussions, and your kind (and for me extremely honouring) suggestion, to publish a small article on “Deducibility” together. I have continued working, mainly on “Derivation versus Demonstration (or Proof)”, and have obtained some really interesting results: – I believe that you will admit, when you hear them, that the distinction is indeed quite important. But this enquiry presupposes that which concerns Deducibility. I have therefore worked in the past days on the article that we (if you uphold your suggestion) shall publish together; and I hope that I can send it to you in a few days.

I will come to Switzerland again at the beginning of April. Between the 1st and the 10th I participate in a conference on political philosophy[e], but I would *really* like to see you immediately after the 10th. Should this be impossible for you, then I would try to come some days earlier to see you, but this would be more difficult. Would you be so kind to send me, by return of post, *one* line, possibly air mail, on whether you are available on the 11th and/or 12th of April? It does not have to be in Zürich.

Warmest regards,
Yours,
K. R. Popper

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[e] Popper participated in the founding and first meeting of the Mont Pelerin Society, which was organized by F. A. Hayek.

21.3 Popper to Bernays, 3 March 1947

ETH Zürich: Hs 975 : 3650. 1 sheet with letterhead of the LSE, handwritten, 2p.

The London School of Economics and Political Science.
(University of London)
Houghton Street, Aldwych, London W.C.2
29/3/47.

Lieber Herr Professor,

Ich fahre morgen früh in die Schweiz, und ich habe die Absicht, diesen Brief in der Schweiz aufzugeben; aber er ist in London geschrieben.

Hier ist der Artikel. Er hat mir ziemlich viel Arbeit gegeben; und, größtenteils, nicht besonders interessante Arbeit – nachprüfen und glätten von Beweisen die ich größtenteils schon lange hatte. Trotzdem glaube ich daß die Arbeit nicht ganz ohne Interesse ist. Das einfachste System der Aussagenlogik das ich kenne ist drin; es besteht aus (α) den allgemeinen Regeln der Transitivität, Reflexivität, etc, der Ableitung (section 2 des Artikels); (β) aus regeln für die negation, z.B. der einen Regel

Wenn $\neg c$ aus a und $\neg b$ ableitbar ist, dann ist b aus a und c ableitbar.

Es genügt, dazu *entweder* (γ) anzunehmen

c ist aus a und b ableitbar wenn, und nur wenn, c aus $a \,.\, b$ ableitbar ist

oder (γ')

$b \supset c$ ist aus a ableitbar wenn, und nur wenn, c aus a und b ableitbar ist.

Dann hat man – mit entsprechenden Definitionen – die ganze Aussagenlogik.

(Ich habe die ganze Zeit Englisch geschrieben – ich mache dauernd Sprachfehler wenn ich deutsch zu schreiben versuche !!)

2 | Titel: Ich schlage vor, den Artikel zu nennen:

On Systems of Rules of Inference.

Der Titel ist nicht sehr gut, aber bisher ist mir kein besserer eingefallen.

Da ich nur ganz kurz in Zürich bleiben kann, wäre es mir lieb wenn Sie den Artikel vorher durchlesen könnten, und, wenn möglich, ein paar Zeilen schreiben würden.

Meine Adresse bis 10. April, Mittag, ist

Hotel Du Parc, Mont Pèlerin sur Vevey.

Ich bin furchtbar müde und überarbeitet – ich habe seit Wochen jede Nacht bis 3 oder 4 Uhr früh gearbeitet; und es wird wohl so weiter gehen im Sommer Semester. Es ist schauderhaft.

Bitte, bestätigen Sie mit einer Zeile den Empfang der Arbeit!

Ich freue mich schon *sehr* darauf Sie zu sehen – so sehr als ich, in meinem gegenwärtigen Zustand der Übermüdung mich überhaupt auf etwas freuen kann!

Sehr herzlich grüßt Sie
Ihr
K. R. Popper

The London School of Economics and Political Science.
(University of London)
Houghton Street, Aldwych, London W.C.2
29 March 1947

Dear Professor,

I will travel to Switzerland tomorrow morning, and I plan to post this letter in Switzerland; but it is written in London.

Here is the article. It has been a lot of work; and, mostly, not very interesting work – checking and smoothing out of proofs which I, for the most part, have had for a long time. Nevertheless I believe that the article is not completely without interest. It contains the simplest system of propositional logic that I know; it consists of (α) the general rules of transitivity, reflexivity, etc., of derivation (section 2 of the article); (β) of rules for negation, e.g. the one rule

If $\neg c$ is derivable from a and $\neg b$, then b is derivable from a and c.

For it is sufficient to suppose *either* (γ)

c is derivable from a and b if, and only if, c is derivable from $a\,.\,b$

or (γ')

$b \supset c$ is derivable from a if, and only if, c is derivable from a and b.

Then one has got – with appropriate definitions – the whole of propositional logic.

(I have written in English the whole time – I constantly make mistakes when I try to write German !!)

Title: I propose to call the article:

On Systems of Rules of Inference.

The title is not very good, but so far I could not think of a better one.

Due to the fact that I can only be in Zürich for a short time, I should be very glad if you could read the article beforehand and, if possible, write a few lines.

My address until 10 April, midday, is

Hotel Du Parc, Mont Pèlerin sur Vevey.

I am terribly tired and overworked – for weeks I have been working every night until 3 or 4 o'clock in the morning; and it will probably continue this way during the summer semester. It is horrible.

Please confirm with a line the receipt of the article!

I am *very* much looking forward to seeing you – as much as I, in my current state of fatigue, am capable of looking forward to anything!

Kindest greetings to you,
Yours,
K. R. Popper

21.4 Bernays to Popper, 12 March 1947

KPS Box 276, Folder 12. Postcard, handwritten, 2p.

12. März 1947.

Lieber Herr Prof. Popper!

Ihr freundlicher Brief wurde mir nachgesandt, da ich mich für ein paar Tage in Braunwald befinde, – das Sie ja bei Ihrem letzten Schweizer Aufenthalt kennen lernten. Um gleich auf Ihre Frage zu antworten, so steht, soviel ich übersehe, nichts dem im Wege, dass wir uns am 11. April in Zürich werden sehen können; auch den
2 12. vormittags werde ich gewiss verfügbar haben. | Dem Empfang Ihres mir in Aussicht gestellten Konzepts sehe ich mit viel Interesse entgegen, – auch im Hinblick auf die event. gemeinsame Publikation.

Im Ausblick darauf, Sie in nicht ganz einem Monat wieder zu sehen, grüsst Sie aufs beste Ihr

P. Bernays

12 March 1947

Dear Prof. Popper!

Your friendly letter has been forwarded to me, because I am staying in Braunwald for some days, – which you got to know during your last stay in Switzerland. Getting back to answering your question, nothing stands, as far as I can see, in the way of us seeing each other on the 11th of April in Zürich; I will certainly also be available in
2 the midmorning of the 12th. | With great interest I am looking forward to the receipt of the concept you promised me, – also with regard to the possible joint publication.

Looking forward to seeing you again in less than a month, sincerely yours,

P. Bernays

21.5 Bernays to Popper, 1 April 1947

KPS Box 276, Folder 12. Postcard, handwritten, 1p.

1.IV.47

Lieber Herr Prof. Popper!

Für heute nur die Bestätigung, dass ich Ihr Manuskript und den begleitenden Brief erhalten habe. Schönen Dank! Ein wenig habe ich mir schon von Ihrer Arbeit angesehen; zu dem Weiteren werde ich wohl erst in ein paar Tagen kommen. Ich werde es jedenfalls noch ein paar Tage vor Ihrer Ankunft einrichten.

Recht erholsame Feiertage wünscht Ihnen mit herzlichem Gruss Ihr

P. Bernays

1 April 1947

Dear Prof. Popper!

For today, just the confirmation that I have received your manuscript and the accompanying letter. Thank you! I have already looked at some of your work;

anything more than that will have to wait some more days. In any case I will manage to do it some days before your arrival.

Wishing you restful holidays and sending best wishes, yours,

P. Bernays

21.6 Popper to Bernays, 19 October 1947

ETH Zürich: Hs 975 : 3651. 1 sheet with letterhead of the LSE, handwritten, 2p.

The London School of Economics and Political Science.
(University of London)
Houghton Street, Aldwych, London W.C.2

19/10/47

Lieber Professor Bernays,

Ich habe so lange nicht geschrieben! Ich war krank und habe einen Auto-Unfall gehabt der mich um ein Haar getötet hätte. Außerdem habe ich sehr hart gearbeitet, und habe dauernd neue Resultate bekommen die ich Ihnen mitteilen wollte – aber es kamen immer weitere dazu, so daß ich nie dazukam.

Das erste wichtige Resultat, das ich etwa eine Woche nachdem ich Sie sah fertig hatte war die Ausdehnung der Methode von

$$a/b \wedge c \leftrightarrow a/b \;\&\; a/c$$

zu quantification:

$a\binom{x}{y}$ ist das resultat der substitution von x für y in a („x“ und „y“ sind variable namen von variablen)

Axb ist das Resultat von universeller quantification
Exb — existentieller —
dann haben wir:

$$a\binom{y}{x}/Ax(b\binom{y}{x}) \leftrightarrow a\binom{y}{x}/b\binom{x}{y}$$
$$Exa\binom{y}{x}/b\binom{y}{x} \leftrightarrow a\binom{x}{y}/b\binom{y}{x}$$

2 | Diese Formeln definieren Universelle und Existenzielle quantification.

Das war das wichtigste Resultat. Aber es gibt eine Menge weitere Resultate.

Ich fand daß ich am besten zunächst Resultate veröffentlichen sollte (da die Beweise lang und kompliziert sind), und da ich eine Gelegenheit dazu hatte, veröffentlichte ich die hauptsächlichen Resultate zunächst in MIND. Ich brachte alle die Fußnoten hinein (und eine oder die andere dazu) die wir in Zürich besprochen hatten. Die Offprints habe ich erst jetzt bekommen. Es sind eine *Menge* Fehler drin. Ich schicke ein Fehlerverzeichnis mit.

Schönen Dank für Ihre Grüße durch Scholz![f]

[f] Cf. this volume, § 31.1, postcard from Scholz to Popper, 17 November 1947.

Sehr herzlich grüßt Sie
Ihr
K.R. Popper

The London School of Economics and Political Science.
(University of London)
Houghton Street, Aldwych, London W.C.2

19 October 1947

Dear Professor Bernays,

I have not written for such a long time! I was sick and had a car accident which almost killed me. Furthermore, I have worked very hard, and have constantly obtained new results that I wanted to communicate to you – but there were constantly more of them, so I never put myself to do it.

The first important result, which I had finished about one week after I saw you, was the extension of the method of

$$a/b \wedge c \;\leftrightarrow\; a/b \;\&\; a/c$$

to quantification:

$a\binom{x}{y}$ is the result of the substitution of x for y in a (x and y are variable names of variables)

Axb is the result of universal quantification

Exb — existential —

then we have:

$$a\tbinom{y}{x}/Ax(b\tbinom{y}{x}) \;\leftrightarrow\; a\tbinom{y}{x}/b\tbinom{x}{y}$$
$$Exa\tbinom{y}{x}/b\tbinom{y}{x} \;\leftrightarrow\; a\tbinom{x}{y}/b\tbinom{y}{x}$$

These formulas define universal and existential quantification.

That was the most important result. But there are a lot of further results.

I found that I should first publish results (due to the fact that the proofs are long and complicated), and because I had an occasion to do it, I published the main results in Mind. I inserted all the footnotes (and some additional ones) that we talked about in Zürich. Only now have I received the offprints. They contain a *lot* of errors. I attach a list of errata.

Many thanks for your greetings through Scholz![f]

Cordially greeting you,
Yours,
K.R. Popper

21.7 Bernays to Popper, 12 May 1948

ETH Zürich: Hs 975 : 3653. 3 sheets without letterhead. 5p. typescript with handwritten insertion of formulas.

Zürich 2, Bodmerstr. 11.
12. Mai 1948.

Lieber Herr Professor Popper,

Schon sehr lange schulde ich Ihnen die Erwiderung auf Ihren Brief, den Sie mir etwa zugleich mit Ihrer Abhandlung „New Foundations for Logic“ sandten. Inzwischen empfing ich zwei weitere Publikationen von Ihnen mit freundlicher Widmung. Haben Sie schönen Dank für alle diese Zusendungen! – Es tat mir sehr leid zu hören, dass es ihnen gesundheitlich nicht gut ging und dass Ihnen ein Auto-Unfall zustiess. Ich hoffe aus Ihrer Mitteilung über Ihre intensive Arbeit entnehmen zu können, dass Ihr Ergehen sich seit damals wesentlich gebessert hat.

Zur Lektüre der drei von Ihnen empfangenen Abhandlungen bin ich erst in diesem Frühjahr gekommen; sie erfolgte grösstenteils in Braunwald (das Sie doch auch kennen) wo ich mich im März einige Tage (wie schon in den vorigen beiden Jahren) aufhielt.

Ihre Abhandlungen haben mich natürlich sehr interessiert. Nur habe ich hinsichtlich der allgemeinen Tendenz der Betrachtungen nicht eine solche Einhelligkeit mit meiner Einstellung vorgefunden, wie ich sie nach unseren hiesigen mündlichen Aussprachen erwartet hätte. Eine Gelegenheit dazu, dass wir diese Dinge mündlich mit eindander diskutieren, wird vielleicht – nach dem, was ich kürzlich von Herrn Gonseth[g] hörte, bald gegeben sein; er erzählte mir, dass Sie für diesen Sommer eine Einladung zu Vorträgen hier in Zürich hätten. Doch wieviel Sie dann hier Zeit haben werden, ist ja immerhin fraglich; jedenfalls möchte ich mich jetzt schon zu einigen Punkten Ihrer Ausführungen äussern.

Meine erste Bemerkung betrifft section 3 Ihrer „New Foundations . . .“. Die hier in den Darlegungen von p. 210–211 behauptete Gleichwertigkeit der „basis II“ mit der „basis I“ scheint mir nicht zuzutreffen. Ich habe mir zum Nachweis folgendes Modell überlegt: Die Beziehung $a_1, \ldots, a_n/b$ werde gedeutet durch die
2 arithmetische Beziehung | zwischen natürlichen Zahlen $a_1 + \ldots + a_n \geq b$; $b \wedge c$ bedeute das Maximum von b und c. Dann sind (Rg), (Te), (Cg) erfüllt, dagegen nicht (Tg), auch nicht (3.4 g), noch auch (3.4).

Sodann kann ich Ihre Methode der expliziten Definition der „compounds“ (p. 218) nicht als ein eigentliches Definitionsverfahren akzeptieren. Sie behaupten, dass solche Definitionen uns erlauben, jeweils das definierte Zeichen ohne Schwierigkeit „from any context“ zu eliminieren. Das dürfte aber kaum zutreffen. Betrachten Sie etwa folgendes Beispiel einer nach dem Schema (D5) gebildeten Definition

$$a//b \triangleright c \;\leftrightarrow\; (b/a \;\&\; a/c)$$

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[g] Ferdinand Gonseth (1890–1975). Mathematician and philosopher who taught at the ETH Zürich from 1929 to 1960.

Aus dieser erhält man, auf dem Weg über $b \triangleright c//b \triangleright c$ als abgeleitete Regeln $b/b \triangleright c$; $b \triangleright c/c$ und aus diesen beiden, mittels (Ts), b/c, sodass das theoretische System sich als *inkonsistent* erweist. Gleichermassen kommt man zur Inkonsistenz mit folgender nach dem Schema (D5) gebildeten (Konjunktion und Disjunktion benutzenden) Definition: $a//b \circ c \;\leftrightarrow\; (b \wedge c/a \;\&\; a/b \vee c)$

Die Einführung eines compound nach dem Schema (D5) ist also überhaupt nicht generell, sondern nur bei besonderer Gestalt des Definiens zulässig; dieses Schema kann also gewiss nicht als eine Form von expliziter Definition gelten.

Abgesehen von dieser besonderen Einführungsform kann man sich klar machen, dass die Einführung der compounds nicht den Charakter einer blossen Begriffserklärung hat, nämlich durch folgende Erwägung: Beschränken wir uns auf die Beziehung $a_1, \ldots, a_n/b$ und die für sie geltenden Axiome (Rg) und (Tg), so ist mit diesen die Deutung vereinbar, wonach $a_1, \ldots, a_n/b$ besagt, dass b mit einer der Aussagen $a_1, \ldots, a_n$ übereinstimmt. Bei Hinzunahme der Konjunktion ist dagegen diese Deutung nicht mehr möglich, wenigstens wenn wir annehmen, dass es mindestens zwei verschiedene Aussagen gibt.

3 | Der gleiche Umstand macht sich auch in Hinsicht auf den Begriff der „absolute validity" geltend, wie Sie ihn in Ihrer Abhandlung „Logic without assumptions" einführen. Nämlich aufgrund Ihrer Definition der absolute validity (p. 277) kann eine inference – wenigstens vor der Einführung von compounds – nur dann absolutely valid sein, wenn sie gemäss der Regel (Rg) erfolgt. Kann sich aber durch die Einführung der compounds etwas daran ändern? Diese Einführung erfolgt ja, Ihrer Anweisung gemäss, mittels des Begriffs der deduktiven Gleichwertigkeit $a//b$, welche zurückgeführt wird auf die deducibility a/b. Es fragt sich nun, ob die deducibility hier im Sinne bloss der valid inferences oder der *absolutely* valid inferences zu verstehen ist. Im ersten Falle liesse sich nicht ersehen, wie für die resultierenden Ableitungsregeln der compounds die absolute validity gefolgert werden kann. Im anderen Falle aber macht überhaupt schon die Anwendung der Definition für die compounds Schwierigkeit: Wird etwa die Konjunktion ihrer logischen Stärke nach durch (D5.1) – aus New Foundations – definiert und seien sonst noch keine compounds eingeführt; b, c seien zwei verschiedene Sätze; welche Bedingung muss dann ein Satz a erfüllen, damit er die logische Stärke der Konjunktion $b \wedge c$ besitzt? Da einerseits jedenfalls die inference von a auf a absolutely valid ist, so muss auch (nach (D5.1)) die inference von b, c auf a absolutely valid sein, also bei jeder statement-preserving interpretation stimmen; das aber ist nur dann der Fall, wenn a mit einer der Aussagen b, c übereinstimmt. Hieraus folgt nun weiter, (da ja jedenfalls die inference von b, c auf c sowie die von b, c auf c absolutely valid ist), dass mindestens eine der inferences von b auf c oder von c auf b absolutely valid sein müsste, während doch die beiden Sätze b, c verschieden sind. Hiernach würde also ein Satz überhaupt nie der Konjunktion zweier verschiedener Sätze deduktiv gleichwertig sein. (Ich hoffe, dass diese Ueberlegung verständlich ausgedrückt ist.)

4 | Nun habe ich mich noch zu Ihrer Kritik der Fassung des All-Schemas, wie sie in den „Grundlagen der Math." gegeben ist, zu äussern. Ich meine die Stelle p. 232–233 Ihrer New Foundations. Sie werden gewiss erwarten, dass ich dazu Stellung nehme;

und ich glaube auch, dass ich den Sachverhalt befriedigend aufklären kann. Der Widerspruch, den Sie, ausgehend von dem von Ihnen beanstandeten Schema $a_{\grave{x}} > b/a_{\grave{x}} > Axb$ herleiten, kommt im Formalismus der „Grundl. der Math.“ deshalb nicht zustande, weil hier die Implikation eine andere Rolle hat als das „hypothetical“ in Ihrem Formalismus. Für die Implikation ist nämlich im Formalismus des Grundlagenbuchs die Regel (4.2) Ihrer New Foundations (vgl. p. 215) nicht allgemein gültig; es gilt zwar $a/b > c \to a, b/c$, aber die Umkehrung, welche auf das „Deduktionstheorem“ hinauskommt, gilt nur unter einschränkenden Bedingungen. Mit diesen Bedingungen hängt die Notwendigkeit der Unterscheidung zwischen „Deduktionsgleichheit“ und „Ueberführbarkeit“ zusammen, wie sie auf S. 49–50 des Grundlagenbuchs (Bd.I) dargelegt ist. Hier wird auch gerade das Beispiel der Formel $A(a) \to (x)A(x)$ herangezogen, die ja der von Ihnen betrachteten Beziehung $a > Axa$ entspricht, und es wird ferner hervorgehoben, dass der festgestellte Unterschied auf der Rolle der freien Variablen beruht. Wenn man in der Weise, wie es im Formalismus des Grundlagenbuchs geschieht, für den Uebergang von Beziehungen der Herleitbarkeit zu Implikations-Beziehungen gewisse Einschränkungen (im Hinblick auf die Rolle der freien Variablen) beachtet, so wird dadurch die Unterscheidung zwischen „rules of proof“ und „rules of derivation“ entbehrlich. Man kann dann z.B. getrost die Einsetzungsregel als eine rule of derivation statuieren.

Beiläufig bemerke ich noch: ich bin wohl nicht im Irrtum, wenn ich annehme, dass in den Formeln p. 227, lines 14, 17, 18 Ihrer New Foundations ein Doppelstrich
5 // anstatt eines einfachen Striches stehen soll. | (Dies scheint mir einerseits aus der nachfolgenden Überlegung hervorzugehen, die sonst ja gar nicht verständlich wäre, und auch aus der Bezugnahme auf p. 233, line 4.)

Schliesslich noch die Frage: Ist es nicht, mit Bezug auf die conditional rules, sachgemässer von einer „antecedent inference“ und einer „consequent inference“ zu sprechen, anstatt, wie es auf p. 202 Ihrer New Foundations geschieht, von einer „antecedent rule“ und einer „consequent rule“? (Meiner Ansicht nach besagt z.B. das Transitivitätsgesetz (Ts), dass, wenn die inferences a/b, b/c beide gültig sind, dann auch die inference a/c gültig ist. Man kann hieraus auch eine Abhängigkeits-Beziehung zwischen Regeln entnehmen; das ist aber dann eine indirekte und spezielle Anwendung des Transitivitätsgesetzes.)

Nun ist dieser Brief recht lang geworden, obwohl ich mich bemühte, mich einigermassen konzentriert zu fassen.

In der Aussicht, Sie eventuell bald hier zu sehen, grüsst Sie herzlich

Ihr

P. Bernays

Ich bitte auch um eine Empfehlung an Ihre Frau.

Zürich 2, Bodmerstr. 11.

12 May 1948

Dear Professor Popper,

For quite some time I have owed you an answer to your letter which you sent me together with your article "New Foundations for Logic". In the meantime I received two further publications from you with a dedication. Many thanks for all these mailings. – I was very sorry to hear about your poor health and that you had a car accident. I hope that I can gather from your notice about your intensive work that your well-being has improved substantially since then.

I managed only this spring to read the three treatises I received from you; I read them mostly in Braunwald (which you also know) where I spent some days in March (as in the previous two years).

Your treatises were naturally of great interest to me. It is just that with regards to the general tendency of the observations I did not find such an unanimity with my own attitudes, as I had expected after our oral discussions which we had here. An occasion to discuss these things in person may soon arise – after what I heard from Mr Gonseth[g]; he told me that you have an invitation for lectures here in Zürich. How much time you will have here is another question; in any case, I would like to comment on some of the points in your remarks right away.

My first remark concerns section 3 of your "New Foundations . . .". The equivalence of "basis II" with the "basis I" which is asserted on p. 210–211 in the presentation does not seem to hold. To prove this I came up with the following model:

The relation $a_1, \ldots, a_n/b$ is to be interpreted through the arithmetical relation between natural numbers $a_1 + \ldots + a_n \geq b$; $b \wedge c$ denotes the maximum of b and c. Then (Rg), (Te), (Cg) are satisfied, but not (Tg), and neither (3.4 g), nor (3.4).

Furthermore I cannot accept your method of explicit definition of the "compounds" (p. 218) as a proper definition procedure. You claim that such definitions allow us to eliminate the defined sign without difficulty "from any context". This can hardly be the case. Consider the following example of a definition constructed according to the schema (D5)

$$a//b \triangleright c \ \leftrightarrow \ (b/a \ \& \ a/c)$$

From this one obtains, by going through $b \triangleright c//b \triangleright c$ the rules $b/b \triangleright c$; $b \triangleright c/c$ as derived rules, and from these two, with (Ts), b/c, so that the theoretical system reveals itself to be *inconsistent*. Equally one arrives at the inconsistency with the following definition, which is constructed according to schema (D5) and which uses conjunction and disjunction: $a//b \circ c \ \leftrightarrow \ (b \wedge c/a \ \& \ a/b \vee c)$

So the introduction of a compound according to the schema (D5) is not at all general, but only admissible for particular forms of the definiens; this schema can certainly not count as a form of explicit definition.

Except for this special form of introduction one can convince oneself that the introduction of the compounds does not have the character of a simple explication of a concept, by means of the following consideration: if we restrict ourselves to the relation $a_1, \ldots, a_n/b$ and the axioms (Rg) and (Tg) which hold for it, then the interpretation according to which $a_1, \ldots, a_n/b$ says that b is identical with one of the propositions $a_1, \ldots, a_n$, is compatible with them. The addition of conjunction makes this interpretation no longer possible, at least if we assume that there are at least two different propositions.

The same circumstance also applies to the concept of "absolute validity", in the way in which you introduce it in your treatise "Logic without assumptions". Due to your definition of absolute validity (p. 277), an inference can only be absolutely valid – at least before the introduction of compounds – if it is made in accordance with the rule (Rg). But can the introduction of the compounds change something about this? This introduction takes places, according to your instruction, with the help of the concept of deductive equality $a//b$ which is reduced to the deducibility a/b. This raises the question whether the deducibilty here has to be understood merely in the sense of the valid inferences or of the *absolutely* valid inferences. In the first case one could not see how the absolute validitiy for the resulting inference rules of the compounds can be derived. But in the other case already the application of the definition for the compounds poses difficulties: if, for example, the conjunction is defined according to its logical strength through (D5.1) – from New Foundations – and if no further compounds have been introduced; let b, c be two different sentences; which condition does a sentence a have to fulfil in order for it to have the logical strength of the conjunction $b \wedge c$? Since, on the one hand, the inference from a to a is absolutely valid, so also (according to (D5.1)) the inference from b, c to a has to be absolutely valid, and thus be valid for every statement-preserving interpretation; but this is only the case when a is identical to one of the propositions b, c. From this it follows, furthermore, (since the inference from b, c to c as well as the inference from b, c to c is absolutely valid), that at least one of the inferences from b to c or from c to b would have to be absolutely valid, while the two sentences b, c are different. According to this no sentence would ever be deductively equal to the conjunction of two different sentences. (I hope that this thought is expressed intelligibly.)

Now I have to comment upon your critique of the formulation of the all-schema, as it is given in the "Grundlagen der Math.". I think of the passage p. 232–233 of your New Foundations. You will certainly expect that I comment upon this; and I also believe that I can elucidate the situation satisfactorily. The contradiction that you derive, starting with the schema $a_{\hat{x}} > b/a_{\hat{x}} > Axb$, which you criticize, does not arise in the formalism of the "Grundl. der Math.", because the implication plays another role here than the "hypothetical" in your formalism. For the implication in the formalism of the Grundlagen book, the rule (4.2) of your New Foundations (cp. p. 215) is not generally valid; one has indeed $a/b > c \rightarrow a, b/c$, but the converse, which amounts to the "deduction theorem", is only valid under restricting circumstances. The necessity of the distinction between "equality of deduction" and "transformation" is related to these restrictions, as is explained on p. 49–50 of the Grundlagen book (vol. 1). The example of the formula $A(a) \rightarrow (x)A(x)$ is used here, which corresponds to the relation $a > Axa$, which you consider, and it is emphasized that the observed difference is due to the role of the free variables. If one observes, as is done in the formalism of the Grundlagen book, certain restrictions (with regard to the role of the free variables) for the transition of the relations of deducibility to the relations of implication, then the distinction between "rules of proof" and "rules of derivation" becomes dispensable. One can then confidently postulate, e.g., the substitution rule as a rule of derivation.

In passing, I note also: I am probably not mistaken if I assume that in the formulas p. 227, lines 14, 17, 18 of your New Foundations there should be a double slanted line // instead of a single slanted line. (This seems to me to follow from the next remark, which otherwise would not be comprehensible, and from the references on p. 233, line 4.)

At last the question: is it not, concerning the conditional rules, more appropriate to speak of an "antecedent inference" and a "consequent inference", instead, as is done on p. 202 of your New Foundations, of an "antecedent rule" and a "consequent rule"? (In my opinion the transitivity law (Ts) says that, if both of the inferences a/b, b/c are valid, then the inference a/c is also valid. From this one can extract a dependency relation between rules; but this is an indirect and special application of the transitivity law.)

Now this letter has turned out to be quite long, despite my efforts to keep myself somewhat focused.

In the prospect of possibly seeing you here soon, cordially,

Yours,
P. Bernays

Please also extend my greetings to your wife.

21.8 Popper to Bernays, 13 June 1948

ETH Zürich: Hs 975 : 3654 Typescript on 2p. of 1 sheet without a letterhead. Attached to this letter is a copy of a letter from Popper to Quine; cf. this volume, § 30.4. Also attached is a list of errata; cf. § 21.8.1.

London School of Economics,
Houghton Street, Aldwych, London W.C.2.

13. Juni 1948.

Lieber Professor Bernays,

vielen, vielen Dank für Ihren Brief! Dass er etwas deprimierend war, ist meine Schuld – man macht nicht gerne Fehler! Aber da ich die Fehler nun einmal gemacht habe, bin ich Ihnen zu grösstem Dank verpflichtet, dass Sie mir so ausführlich und schön darüber geschrieben haben.

Kein Zweifel, Sie haben in allem recht. Bezüglich meiner Basis II habe ich auch nicht ein Wort zu erwidern. Aber bezüglich der anderen Punkte glaube ich doch, dass sich die Sache reparieren lässt. (Damit will ich natürlich nicht zurücknehmen, dass Sie recht haben.)

(1) Seite 2 Ihres Briefes – die *Inkonsistenz*.

Ich habe selbst in meiner „Logic without Assumptions" ein solches Beispiel gegeben (Definition 7.8, p. 284) und ich glaube, dass die Sache anders interpretiert werden kann (wie ich dort andeutete), nämlich so, dass jede Sprache die ein Zeichen enthält das so definiert ist, inkonsistent ist, während die Metasprache (d.h. mein System) es nicht ist; Ihr „$b \triangleright c$“ würde einfach der Name eines paradoxen Satzes sein. Jedoch habe ich immer empfunden, dass hier eine Schwäche vorliegt, und ich habe, glaube ich, einen besseren Ausweg gefunden. (Er is am Ende der beigelegten englischen Berichtigung besprochen.)

(2) Seite 3 Ihres Briefes: Eine ganz ähnliche Kritik schickte mir Quine am 19. April.[h] Die Kritik ist voll berechtigt, aber der Schaden, glaube ich, reparabel. Ich lege einen Durchschlag meines Briefes an Quine bei; die Reparatur ist in der Berichtigung enthalten.

Ich finde jedoch diese Reparatur höchst unelegant. Für Ihre Kritik wäre ich Ihnen ungemein dankbar. Wenn sie nur bald käme! Ich habe wirklich kein Recht zu klagen, aber wenn Sie früher hätten schreiben können, wäre vieles besser geworden. Ich bin hier schrecklich isoliert; und da ich, wie Sie wissen, mehr Philosoph als Logiker bin, bin ich auf Kritik sehr angewiesen. (Ich habe inzwischen selbst 2 Fehler in meiner „Functional Logic“ gefunden, aber sie sind verhältnismässig leicht zu reparieren. Dazu kommen nun noch die Fehler die mit der unhaltbaren „Basis II“ zusammenhängen. Glücklicherweise habe ich inzwischen eine einfachere Konstruktion gemacht, durch die die Basis II ganz überflüssig wird; denn ich brauche für Basis I nur die *eine* einfache Formel:

$$a_1, \ldots, a_n/b \leftrightarrow (c)((c/a_1 \mathbin{\&} \ldots \mathbin{\&} c/a_n) \rightarrow c/b)$$

Dennoch wäre ich froh gewesen, über diesen Fehler früher zu hören.)

(3) Seite 4 Ihres Briefes. Diese Punkte habe ich mir in der Tat inzwischen selbst
2 aufgeklärt . . .. |

(4) Seite 4 Ihres Briefes (Ende): das scheint mir nicht nötig zu sein; natürlich kann man hier überall „//“ für „/“ setzen, aber es ist überflüssig. Denn wenn

$$a/a\binom{x}{y}$$

eine rule of inference ist, dann haben wir auch

$$\mathit{Diff}(x, y)/\mathit{Diff}(y, y) \qquad (x \neq y)$$

oder:

$$\neg \mathit{Idt}(x, y)/\neg \mathit{Idt}(y, y) \qquad (x \neq y)$$

(Wobei „$\mathit{Diff}(x, y)$“ der Name eines Satzes von der Form „$\mathrm{he}_1 \neq \mathrm{he}_2$“ ist.) Da nun $\mathit{Diff}(y, y)$ kontradiktorisch ist, besagt unsere Formel dass $\mathit{Diff}(x, y)$ $(x \neq y)$ auch kontradiktorisch ist, unabhängig von den konstanten Werten die wir für „he_1“ einsetzen; und das kann nur dann der Fall sein, wenn es nur *ein* Individuum gibt.⟨Fußnote: Wir bekommen von $\neg \mathit{Idt}(x, y)/\neg \mathit{Idt}(y, y)$ natürlich $\mathit{Idt}(y, y)/\mathit{Idt}(x, y)$, i.e. dasselbe Resultat.⟩

(5) Seite 5 Ihres Briefes: Auch hier stimme ich vielleicht nicht überein (siehe meinen Brief an Quine, und den Anfang der neuen note 12 in der Berichtigung). Die *metalinguistische* Formel:

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[h] Cf. this volume, § 30.4, letter from Quine to Popper, 19 April 1948.

$$„(a/b \ \& \ b/c) \rightarrow a/c“$$

muss unterschieden werden von der *meta-metalinguistischen* Formel:

„Wenn ‚a/b‘ and ‚b/c‘ gültig sind, dann ist auch ‚a/c‘ gültig.“

In der Tat, nur wenn diese Unterscheidung gemacht wird, ist die ganze Theorie möglich, so weit ich sehen kann. (Es geht hier mit dem Gültigkeitsbegriff wie mit dem Wahrheitsbegriff.)

Ich habe *nichts* von Gonseth gehört. Vielleicht kann ich Sie aber doch Ende September in der Schweiz sehen.

Ich wäre für eine baldige Antwort sehr dankbar, aber ich weiss wohl, dass Sie anderes und wichtigeres zu tun haben.

Nochmals sehr vielen Dank und schöne Grüße von meiner Frau und mir.

Ihr sehr ergebener
K. R. Popper

Nochmals vielen Dank!

P.S. Ich schick Ihnen noch eine Veröffentlichung „On the Theory of Deduction I and II“ – leider wieder mit der unglückseligen „Basis II“. Aber es bleiben doch eine Menge Bemerkungen übrig, nachdem der Abschnitt zwei (über Basis II) weggelassen ist. Ich habe auch eine Anzahl von neuen Resultaten – aber ich glaube nicht, dass ich es je wieder wagen werde etwas zu veröffentlichen (abgesehen vielleicht von einer unendlichen Sequenz von Korrekturnoten zu meinen alten Veröffentlichungen)!

London School of Economics,
Houghton Street, Aldwych, London W.C.2.

13 June 1948

Dear Professor Bernays,

Very, very many thanks for your letter! The fact that it was a bit depressing is entirely my fault – one does not like to make mistakes! But since I have made these mistakes, I owe you my greatest gratitude, since you wrote to me so extensively and nicely about them.

No doubt, you are right in everything. Concerning my Basis II I have nothing in response. But concerning the other points, I believe that the thing can be repaired. (But of course I do not mean to take away the fact that you are right.)

(1) Page 2 of your letter – the *inconsistency*.

I have given, in my "Logic without Assumptions", such an example myself (Definition 7.8, p. 284) and I believe that the situation can be interpreted in another way (as I indicated there), namely, that every language which contains a sign which is defined in that way is inconsistent, while the meta-language (i.e. my system) is not; your "$b \triangleright c$" would just be the name of a paradoxical sentence. But I have always felt that there is a weakness here, and I have, I believe, found a better way out. (It is discussed at the end of the attached English correction.)

(2) Page 3 of your letter: A very similar critique was sent to me by Quine on April 19th.[h] The critique is wholly justified, but the damage is, I think, reparable. I enclose a carbon copy of my letter to Quine; the repair is contained in the correction.

But I find this repair highly inelegant. I would be immensely grateful for your critique. If only it would arrive soon! I really have no right to complain, but if you could have written earlier, much would have been improved. I am terribly isolated here; and due to the fact that I am, as you know, more philosopher than logician, I am very reliant upon critique. (In the meantime I have found 2 mistakes myself in my "Functional Logic", but they are comparably easy to repair. To this one must add the mistakes which are related to the untenable "Basis II". Luckily I have made an easier construction, which makes the Basis II entirely superfluous; because I need for Basis I only the *one* simple formula:

$$a_1, \ldots, a_n/b \leftrightarrow (c)((c/a_1 \,\&\, \ldots \,\&\, c/a_n) \rightarrow c/b)$$

I would have been happy nonetheless to hear earlier about this error.)

(3) Page 4 of your letter. In the meantime I have resolved these points myself.

(4) Page 4 of your letter (end): this does not seem to be necessary to me; of course we can put "//" for "/" everywhere, but it is superfluous. Because if

$$a/a\binom{x}{y}$$

is a rule of inference, then we also have

$$Diff(x, y)/Diff(y, y) \qquad (x \neq y)$$

or:

$$\neg Idt(x, y)/\neg Idt(y, y) \qquad (x \neq y)$$

(Where "$Diff(x, y)$" is the name of a sentence of the form "$\text{he}_1 \neq \text{he}_2$".) Since $Diff(y, y)$ is contradictory, our formula says that $Diff(x, y)$ $(x \neq y)$ also has to be contradictory, independently from the constant values that we substitute for "he_1"; and this can only be the case if there is only *one* individual.⟨Footnote: We get from $\neg Idt(x, y)/\neg Idt(y, y)$ of course $Idt(y, y)/Idt(x, y)$, i.e. the same result.⟩

(5) Page 5 of your letter: I do not quite agree here either (see my letter to Quine, and the beginning of the new note 12 in the correction). The *metalinguistic* formula:

$$\text{“}(a/b \,\&\, b/c) \rightarrow a/c\text{”}$$

has to be distinguished from the *meta-metalinguistic* formula:

"If 'a/b' and 'b/c' are valid, then 'a/c' is also valid."

In fact, only if this distinction is made is the whole theory possible, as far as I can see. (It is with the concept of validity here as with the concept of truth.)

I have heard *nothing* from Gonseth. But maybe I can see you by the end of September in Switzerland.

I would be very grateful for a timely response, but I certainly know that you have other and more important things to do.

Again, very many thanks and best regards from me and my wife.
Your very loyal
K. R. Popper
Again many thanks!

P.S. I send you another publication "On the Theory of Deduction I and II" – regrettably again with the unfortunate "Basis II". But there still remain a lot of remarks, after the section two (on Basis II) is omitted. I have also a number of new results – but I do not believe that I will ever dare again to publish something (except, maybe, an infinite sequence of corrections to my old publications)!

21.8.1 Corrections to Logic without Assumptions

ETH Hs 975 : 3654. Typescript with handwritten corrections, 4p. Sent as an attachment to the letter in § 21.8. Popper sent a very similar list of errata to Quine; cf. this volume, § 30.5.1.

Corrections to "Logic without Assumptions".

I am greatly obliged to Professors W.V. Quine and Paul Bernays for having raised certain objections to my paper. It appears to me that some of these objections (to the concept of a "free property", i.e. to definition (D5) and note 16 of the paper) are completely justified, and that the others, even though they may be met, nevertheless show that some of my formulations were, to say the least, misleading. It seems to me however, that all my conclusions remain unaffected; the necessary alterations, it appears, are all confined to pp. 277, 279, 280. (Besides these important corrections, I wish to draw attention to one particularly misleading *erratum* on p. 292; in line 7 from end, read "object" instead of "meta".) The corrections I wish to make are:

p. 277, shift the phrase "covered by this definition," from line 10 to line 9 (before "and"), and put in its place, in line 10, the following: "i.e., of rules which are *valid independently of the logical form of the statements involved* (cp. the second paragraph of section 7, below),".
Same page: replace footnote 12 by the following new footnote:
"[12]We must be careful not to confound rules of inference which are absolutely valid (and therefore valid) with rules about assertions about absolutely valid inference. For example, a rule like the following holds:

"If, for certain statements *a* and *b*, the assertion that *a* is deducible from *b* is absolutely valid, then, for the same statements the assertion that *b* is deducible from *a* is absolutely valid also."

But the corresponding rule of inference

"If, for certain statements *a* and *b*, *a* is deducible from *b*, then, for the same statements, *b* is deducible from *a*."

is invalid, and therefore not absolutely valid.

In order to define "absolutely valid rule of inference" in such a way that it only covers rules of inference which are *valid irrespective of the formative signs which* *occur in the various statements involved*, we first distinguish atomic and molecular rules. Using the symbolism introduced in the next paragraph of the text, "$a_1, a_2, \ldots, a_n/b$" and "$a_1, a_2, \ldots, a_n/a_1$" are examples of atomic rules (invalid and valid respectively). Examples of molecular rules are:

2 (A) "a/a and b/a" (invalid) |
(B) "a/a and (ab/b and abc/c)" (valid)
(C) "If a/a then a/b" (invalid)
(D) "If a/b and b/c, then a/c" (valid)

(A) and (B) we call simply conjunctive rules, (C) and (D) we call simply conditional rules; we shall also use the term "component rule", "antecedent (rule)", "consequent (rule)", in an obvious way. Note that in our "simply conditional rules", the consequent must be atomic, and the antecedent either atomic or simply conjunctive.

We now can define absolute validity for atomic, simply conjunctive, and simply conditional rules, as follows:

An atomic rule is absolutely valid if and only if no statement-preserving counter-example of it exists (that is to say, if and only if no statement-preserving interpretation of it is a counter-example).

A simply conjunctive rule is absolutely valid if, and only if, all its components are absolutely valid.

A simply conditional rule is absolutely valid if and only if every statement-preserving interpretation which turns its consequent into a counter example also turns its antecedent into a counter-example. (We assume that a counter-example of a component of a conjunctive rule is a counter-example of the whole conjunctive rule; this allows us to deal with a conjunctive antecedent.)

It can be easily seen that these definitions lay down necessary as well as sufficient conditions for the three types of rules under consideration to be absolutely valid. The definitions cover most of the cases in which we are interested. In order to extend it to all intended cases, we define next:

A rule of inference R_0 is called "secondary to the rules of inference $R_1, R_2, \ldots, R_n$" if and only if every inference drawn in observance with R_0 can be drawn in observance of $R_1, R_2, \ldots, R_n$ (without appealing to R_0).

According to this definition, every rule is secondary to itself.

We now can give a general definition of absolutely valid rules as follows:

A rule of inference is absolutely valid if and only if it is secondary to atomic, simply conjunctive, and simply conditional rules which are absolutely valid in the sense defined above."

(This concludes note 12.)

3 | p. 279–280: replace the passage from the word "For" in line 17 on p. 279 to the end of line 19 on p. 280 by the following:

"For it turns out that (D4) is equivalent to the following definition:

(D5) *An inference is absolutely valid if, and only if, the conclusion is one of the premises.*

This definition avoids the term "truth"; but it should be observed that it is of use only because we can show that it guarantees the transmission of truth from the premisses to the conclusion.

As it stands, (D5) is not very helpful; what we need is, rather, the corresponding idea of an absolutely valid *rule* of inference, defined with the help of (D5) or of an equivalent definition.

In order to achieve this end, we can introduce the idea of a *statement-preserving numerical interpretation* of an inference or a rule of inference. This is an interpretation which identifies each of the m different statements (or variables) occurring in the inference (or rule) with one of the first m natural numbers, and which interprets the expression "$a_1, a_2, \ldots, a_n/b$" to mean "one at least of the numbers $a_1, a_2, \ldots, a_n$ is at least equal to the number b." We can, further, define a *statement-preserving (numerical) counter-example* of an inference or rule of inference as a statement-preserving numerical interpretation which is arithmetically false.[16]

We can now replace (D5) by the definition:

(D5′) *An inference (or rule of inference) is absolutely valid if, and only if, no statement preserving numerical counter-example of it exists.*

The search for such a counter-example must, in every particular case, lead after a finite number of steps to a decision concerning the absolute validity of the particular rule in question.[17] (The method is somewhat similar to that of deciding a formula of the propositional calculus by way of truth-tables.) It can be easily shown, for example, that ⟨upon⟩[i] our definition (D5) of absolute validity which avoids any reference to truth, the rules (6.1g) and (6.2g) are absolutely valid, and similarly (6.6), and the rules analogous to it.[18]"

(This concludes the text-correction on pp. 279–280. The remaining corrections are to the notes 16,17 and 18 on these two pages, and a new note, 23a, on p. 284.)

p. 279, note 16: replace this note by the following new note:

"[16]For instance, a counter example of the rule "If a/b then b/a" is "If $2/1$ then $1/2$". – The use of the words "arithmetically false" does not (as it may appear) re-introduce the words "true" or "false" which we intended to eliminate; the words
4 we intended | to eliminate refer to the statements (a_1, a_2, etc.) of the interpretations; those which we retain refer to such arithmetical statements as "1/2", i.e., "1 is at least as great as 2", etc. (In other words, the problem is shifted to the metalanguage.)"

p. 280, note 17: replace this note by the following new note:

"[17]If the number of variables is considerable, the search for a counter-example may, of course, be long. But it is usually quite easy to design methods by which the procedure can be simplified. For example, in order to construct a counter-example of the invalid rule

"(If a/b then c/d) then (if $a, a_1/b$ then $c, a_1/d$)",

we first interpret the last component, "$c, a_1/d$" by "$1, 1/2$". Since this does not lead to a counter-example of the rule, we next choose "$1, 2/3$" instead of "$1, 1/2$"; and this, indeed, leads to the counter-example "(If $1/2$ then $1/3$) then (if $1, 2/2$ then $1, 2/3$)". (This, in turn, can be evaluated by the truth-table method.)"

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[i] Omitted in the version sent to Quine but present in the draft.

p. 280, note 18: Replace the words from “since” in line 4 of the note to “Altogether” in line 11 of the note by the word “But”.

p. 284, line 23, insert a new note sign “23a” after “*necessary*”, and append to the bottom of the page the new note:
“[23a]But we may, if we wish (in order to avoid such undesirable definitions as 7.8) lay down that, for every numerical interpretation of the variables to which the definiendum refers, there exists one and only one true statement preserving numerical interpretation of the whole defining formula which renders this formula true. (“For every *b*” or “For every *c*” etc., has to be interpreted – if we wish to avoid such definitions as 7.8 – by the phrase: “For every number up to a number which exceeds the other numbers used in the interpretation by not more than 1.”)”

21.9 Bernays to Popper, 27 July 1948

ETH Zürich: Hs 975 : 3655 and KPS Box 276, Folder 12. 2 sheets without letterhead. 3p. typescript with handwritten corrections.

Zürich 2, Bodmerstr. 11
27. Juli 1948

Lieber Herr Professor Popper,
Haben Sie vielen Dank für Ihren freundlichen Antwortbrief! Ich hätte ihn gern, Ihrem Wunsch gemäss, schneller beantwortet; aber im letzten Semestermonat fand ich nicht die Sammlung, um mir die Ausführungen Ihres Briefes, sowie der von Ihnen beigefügten zwei Beilagen, genauer zu überdenken. Die Fragen sind ja zum Teil ziemlich subtil. Und ich kann Ihnen auch diesmal nicht zu allem meine Zustimmung erklären.

Zunächst habe ich Bedenken inbezug auf Ihre neue Definition (D5′) der „absolutely valid inference“, wie Sie sie auf p. 3 ihrer „Corrections“ geben. Dass diese Definition nicht adäquat ist, glaube ich Ihnen durch ein Beispiel belegen zu können, wobei ich von dem Umstande Gebrauch mache, dass Sie auch Regeln in Betracht ziehen, bei denen conditional rules als antecedent auftreten, – wie in dem Beispiel, das Sie auf p. 4 Ihrer Corrections (in der neuen Fassung der Fussnote 17) betrachten.

Mein Beispiel ist das folgende:

If $a, b/c$ and (if a/c then t/d) and (if b/c then t/d), then t/d.

Hier ergibt eine einfache Diskussion, dass kein statement-preserving numerical counter-example existiert; andererseits erkennt man leicht, dass die angegebene Schlussregel nicht gültig ist.

Der hier zutage tretende Mangel der betrachteten statement-preserving numerical interpretation scheint mir darin zu bestehen, dass Sie nur denjenigen Fällen der Gültigkeit von inferences $a_1, \ldots, a_n/b$ angepasst ist, in denen schon für mindestens ein einzelnes a_i die inference a_i/b gültig ist.

Was ferner Punkt (4) Ihres Briefes betrifft, so muss ich durchaus auf der
2 Anerkennung der Schlussregel $a/a\binom{x}{y}$, (nota bene | unter der Bedingung, dass x und

y in a und in $a\binom{x}{y}$ *freie* Variablen sind), bestehen. Das ist ja einfach die Einsetzungsregel. Mit Hilfe dieser Regel bekommt man in der Tat $\neg Idt(x, y)/\neg Idt(y, y)$. Aus der Gültigkeit dieser inference kann man aber keineswegs diejenige der inference $Idt(y, y)/Idt(x, y)$ entnehmen. Hier macht sich wiederum der Unterschied geltend, der zwischen der Implikation und Ihrem hypothetical besteht. (Vgl. S. 4 meines vorigen Briefes). Für dieses hypothetical gilt nicht die Regel der Kontraposition, wenn Formeln mit freien Variablen zugelassen werden, weil eine Formel mit freien Variablen im Sinne einer All-Aussage zu interpretieren ist, jedoch die Negation dieser All-Aussage nicht durch die formale Negation jener Formel dargestellt wird. $\neg Idt(x, y)$ ist (im Sinne der Interpretation) nicht das kontradiktorische Gegenteil von $Idt(x, y)$, sondern eine stärkere Aussage.

Bei Punkt (5) Ihres Briefes scheint es mir, dass die Unterscheidung zwischen einer metalinguistischen Formel und einer meta-metalinguistischen Formel, deren Notwendigkeit Sie erwähnen, gerade im Sinne meiner Erwägung liegt. Vielleicht macht folgende Überlegung deutlicher wie ich das meine: Die inferences sind die Gegenstände der derivative logic, die rules of inference sind Sätze dieser Logik. Deuten wir die Formel $(a/b\ \&\ b/c) \rightarrow a/c$ so, dass a/b (und ebenso b/c sowie a/c) eine rule bedeutet, so ist „a/b" nicht der Wortlaut (die Figur) dieser rule, sondern eine Bezeichnung dieser rule. Unsere Formel ist daher gemäss dieser Deutung nicht eine solche der (bereits metalinguistischen) derivative logic sondern der zugehörigen Metatheorie. Fassen wir dagegen „a/b", „b/c", „a/c" als Bezeichnungen von inferences auf, so stellt die Formel einen Satz der derivative logic dar. – Man kann sich den Sachverhalt, wiederum an dem Beispiel des Transitivitätsgesetzes, auch folgendermassen zurecht legen: das deduktive
3 Transitivitätsgesetz betrifft primär nicht | einen Zusammenhang zwischen Schlussregeln sondern schlechtweg zwischen Folge-Beziehungen, in dem einfachen Sinne, wie Sie es ausdrücken „b follows from a"; dabei ist das „follow" als Grundbegriff für die derivative logic zu nehmen und nicht etwa als eine (metatheoretische) Bezugnahme auf eine Regel der derivative logic.

Ich hoffe dass ich hiermit Ihnen deutlich gemacht habe weshalb ich es nicht angemessen finde, bei den conditional rules von einer „antecedent rule" und einer „consequent rule" zu sprechen.

Nun noch zu dem Punkt (1) aus Ihrem Brief wo es sich um die Definitionen von compounds handelt, welche zu Widersprüchen führen. Was ich durch den Hinweis auf die Möglichkeit eines solchen Zustandekommens von Inkonsistenz vor allem zeigen wollte, war dass die betrachtete Definitionsform nicht den Charakter einer eigentlichen (expliziten) Definition hat, dass vielmehr jene Art der Einführung eines compounds der Aufstellung eines Existenz- und Eindeutigkeits-Axioms gleichkommt. Allerdings ist richtig, dass man solche Axiome vom Standpunkt der Metatheorie auf Definitionen von Eigenschaften einer deduktiven Sprache zurückführen kann, in der Weise, wie Sie es auf p. 285/6 Ihrer „Logic without assumptions" tun. Aber diese Möglichkeit ist nur ein Spezialfall der generellen Möglichkeit, Axiomensysteme auf einer höheren Stufe als explizite Definitionen zu behandeln. Auf diese Möglichkeit bin ich in einer Review[j] von einem veröffentlichten Frege-Brief zu sprechen gekommen, von der ich Ihnen einen Durchschlag des Konzeptes beilege. – (Die Veröffentlichung war übrigens durch den üblen Herrn Steck[k] erfolgt, auf dessen Randbemerkungen ich aber auch in der Review nicht eingegangen bin. Ich habe vielmehr nur die Gelegenheit benutzt, mich über den seinerzeit stattgehabten Disput zwischen Hilbert und Frege zu äussern.)

Über die beiden letztbesprochenen Punkte wäre es gut sich mündlich eingehender zu unterhalten. Ich hoffe, dass sich dazu bald Gelegenheit finden werde; aus dem Programm des Amsterdamer Kongresses entnehme ich dass Sie an diesem teilnehmen und dort vortragen werden. So gedenke ich Sie in ein paar Wochen dort zu sehen. Einstweilen grüsst Sie herzlich Ihr

Paul Bernays

Beste Grüsse auch an ihre Frau.

Zürich 2, Bodmerstr. 11

27 July 1948

Dear Professor Popper,

Many thanks for your friendly reply! I would have loved to have answered it sooner, according to your wish; but in the last summer month I did not find the focus to think about the remarks in your letter and the two supplements that you enclosed. The questions are in part quite subtle. And this time again I cannot declare my agreement to everything.

Firstly I am concerned about your new definition (D5′) of "absolutely valid inference", which you give on p. 3 of your "Corrections". I hope to be able to show to you by an example that this definition is not adequate. I make use of the fact that you also consider rules where conditional rules appear as an antecedent, – as in the example which you consider on p. 4 of your corrections (in the new version of footnote 17).

My example is the following:

If $a, b/c$ and (if a/c then t/d) and (if b/c then t/d), then t/d.

A simple discussion shows here that no statement-preserving numerical counter-example exists; on the other hand, one sees easily that the indicated rule of inference is not valid.

The flaw of the statement-preserving numerical interpretation that appears here seems to consist in the fact that it is tailored to these cases of validity of inferences $a_1, \ldots, a_n/b$ where for at least one a_i the inference a_i/b is already valid.

Concerning point (4) of your letter, I must insist on the acceptance of the inference rule $a/a\binom{x}{y}$, (nota bene under the condition that x and y are *free* variables in a and in $a\binom{x}{y}$). That is just the substitution rule. With the help of this rule one obtains in fact $\neg Idt(x, y)/\neg Idt(y, y)$. But from the validity of this inference one

[j] Cf. Bernays (1942).

[k] For Steck's role in Nazi Germany, cf. Menzler-Trott (2001, 2007).

cannot obtain the validity of the inference $Idt(y, y)/Idt(x, y)$. Here the difference between implication and your hypothetical reasserts itself. (Cp. p. 4 of my previous letter). For this hypothetical the rule of contraposition does not hold, if formulas with free variables are admitted, because a formula with free variables has to be interpreted as a universally quantified proposition, but the negation of this universally quantified proposition is not represented through the formal negation of that formula. $\neg Idt(x, y)$ is (in the sense of the interpretation) not the contradictory opposite of $Idt(x, y)$, but a stronger statement.

Concerning point (5) of your letter, it appears to me that the distinction between a metalinguistic formula and a meta-metalinguistic formula, whose necessity you mention, agrees with my considerations. Maybe the following thought makes clearer what I mean: The inferences are the subject matter of derivative logic, the rules of inference are theorems of this logic. If we interpret the formula $(a/b \,\&\, b/c) \rightarrow a/c$ in such a way that a/b (and also b/c and a/c) denotes a rule of inference, then "a/b" is not the literal wording (the figure) of this rule, but a description/name of this rule. Our formula is therefore according to this interpretation not a formula of the (already metalinguistic) derivative logic, but of the corresponding meta-theory. If, on the other hand, we take "a/b", "b/c", "a/c" to be descriptions/names of inferences, then the formula presents a theorem[1] of derivative logic. – One can present oneself the situation, taking the transitivity law as an example again, in the following way: the deductive transitivity law does not primarily concern a relationship between inference rules but between consequence-relations, in the simple sense, as you express it "*b* follows from *a*"; where the "follow" has to be taken as the fundamental concept of derivative logic, and not as a (metatheoretical) reference to a rule of derivative logic.

I hope that I have made clear to you why I do not find it appropriate to speak about an "antecedent rule" and a "consequent rule" in the case of conditional rules.

Now to the point (1) of your letter, which concerns definitions of compounds which lead to contradictions. What I mainly wanted to show by pointing out the possibility of the appearance of inconsistency was that the form of definition that is considered does not have the character of a proper (explicit) definition, but that this form of introduction of a compound is equivalent to the postulation of an existence and a uniqueness axiom. It is in fact true that one can reduce such axioms from the point of view of the meta-theory to definitions of properties of a deductive language, in the way in which you do it on p. 285/6 of your "Logic without assumptions". But this possibility is just a special case of the general possibility to treat axiom systems on a higher level as explicit definitions. I have spoken about this possibility in a review[j] of a published Frege letter; I attach a carbon copy of the concept. – (The publication was done by the nasty Mr. Steck[k], on whose marginal notes I have not commented upon in the review. I have used the occasion to comment upon the dispute between Hilbert and Frege which took place during that time.)

On these last points it would be good to talk more thoroughly in person. I hope that we will soon have the occasion, from the programme of the Amsterdam congress I take it that you will participate and give a presentation there. So I hope to see you there in a few weeks. Meanwhile warm regards, yours,

Paul Bernays

Best greetings also to your wife.

21.10 Bernays to Popper, 28 July 1948

KPS Box 276, Folder 12. Handwritten, 1p.

Lieber Herr Professor Popper,

Bei der Absendung meines gestrigen Briefes an Sie vergass ich, das versprochene review-Konzept beizulegen. Nun schicke ich es Ihnen nachträglich. – Die review[1] befindet sich übrigens im Vol. 7 des Journal of symb. logic, p. 92/93, (June 1942).

Mit nochmaligen herzlichen Grüssen
Ihr
Paul Bernays

Zürich 2, 28.VII.48.
Bodmerstr. 11

Dear Professor Popper,

Posting yesterday's letter to you, I forgot to attach the promised review concept. I am now sending it to you belatedly. – The review[1] is in Vol. 7 of the Journal of symb. logic, p. 92/93, (June 1942).

With further warm regards,
Yours,
Paul Bernays

Zürich 2, 28 July 1948
Bodmerstr. 11

21.11 Popper to Bernays, 31 July 1948

ETH Zürich: Hs 975 : 3656. 1 sheet with letterhead of the LSE, handwritten, 2p.

The London School of Economics and Political Science.
(University of London)
Houghton Street, Aldwych, London W.C.2
31/7/48

Lieber Herr Professor,

Ich habe soeben Ihren lieben Brief bekommen und will nur ganz kurz antworten – das meiste muß ich wohl für Amsterdam lassen.

Ich habe kurz nachdem ich meinen Brief an Sie sandte selbst gefunden dass meine numerical counter examples nicht genügen. Aber ich war so überlastet mit unaufschieblicher Arbeit dass ich nicht schreiben konnte.

[1] Bernays (1942).

Die Hauptsache ist wohl daß die Definitionsmethode möglich ist; wie Sie sagen, sie ist immer möglich. Aber es kommt mir doch so vor als ob das für die Logik bedeutsam ist.

Leider haben Sie das Konzept der review des Frege-Briefes nicht beigelegt. Können Sie es vielleicht nach Amsterdam mitbringen? Vielen Dank!

Ich freue mich schon sehr darauf Sie zu sehen.

Mit den herzlichsten Grüßen,

Ihr
K. R. Popper

The London School of Economics and Political Science.
(University of London)
Houghton Street, Aldwych, London W.C.2

31 July 1948

Dear Professor,

I have just received your nice letter and will answer only very briefly – I have to leave the most of this for Amsterdam.

Shortly after I sent my letter to you I found myself that my numerical counter examples are not sufficient. But I was so overburdened with unpostponable work that I could not write.

The main point is that the method of definition is possible; as you say, it is always possible. But it still seems to me as if this is relevant for logic.

Unfortunately you have not enclosed the review of the Frege letter. Can you maybe bring it with you to Amsterdam? Thanks a lot!

I am already very much looking forward to seeing you.

With warmest regards,

Yours,
K. R. Popper

21.12 Bernays to Gödel, 24 January 1975

ETH Zürich: Hs 975 : 1759. 1 sheet, typescript, 2p. This letter was published in Gödel (2003).

8002 Zürich, 24. Januar 1975
Bodmerstr. 11

Lieber Herr Gödel,

Haben Sie vielen Dank für Ihren Brief vom 12. Januar.

[. . .]

Sie erkundigten sich, ob Karl Popper etwas Wesentliches zur Aufklärung der Grundlagen der Mathematik beigetragen habe. Jedenfalls hat er sich mit Fragen der Logik und der Axiomatik eingehender befasst. Bei der Logik ist sein Bestreben, möglichst Axiome und Grundregeln durch Definitionen zu ersetzen. Die Art dieser Betrachtung finden Sie z. B. in der Abhandlung „Functional Logic without Axioms or Primitive Rules of Inference" ⟨Popper, 1947d⟩ [. . .].⟨Fußnote: Die hier ausgeführte Zurückführung von Axiomen auf Definitionen ist freilich insofern mehr scheinbar, als die Definitionen keine eigentlich expliziten Definitionen sind. Im Abschnitt VIII der genannten Abhandlung bemerkt auch Popper selbst, dass man zur Anwendung seines Systems auf eine Objektsprache zu den Definitionen noch entsprechende Existenzaxiome hinzufügen muss.⟩ In dieser ist auch eine frühere Publikation angegeben. Mit der Axiomatik hat sich Popper speziell für die Wahrscheinlichkeitstheorie abgegeben, für die er verschiedene Axiomensysteme aufgestellt hat, wobei er unter anderem drauf abzielte, ohne die Voraussetzung auszukommen, dass die der Wahrscheinlichkeit unterworfenen Gegenstände einer Boole'schen Algebra genügen, während er andererseits die Theorie der reellen Zahlen voraussetzt. Sie finden dies z. B. im Appendix der Abhandlung „The Propensity Interpretation of Probability" ⟨Popper, 1959c⟩ [. . .]. Eine etwas frühere Abhandlung von Popper über Axiomatisierung der Wahrscheinlichkeitsrechnung ist im gleichen Journal (⟨Popper, 1955c⟩) erschienen. – Kürzlich hat Popper auch eine Abhandlung über ein Axiomensystem der Geometrie publiziert, welches eine Verbesserung eines Axiomensystems von einem Herrn Roehle ist.[m]

[. . .]

Mit herzlichen Grüßen, auch an Ihre Frau,

Ihr Paul Bernays

8002 Zürich, 24 January 1975
Bodmerstr. 11

Dear Mr. Gödel,

Many thanks for your letter of 12 January.

[. . .]

You inquired whether Karl Popper has contributed anything essential to the clarification of the foundations of mathematics. In any case, he has studied questions of logic and axiomatics in greater depth. In logic he has striven to replace axioms and primitive rules by definitions. You find this sort of consideration, e.g., in the paper "Functional Logic without Axioms or Primitive Rules of Inference" ⟨Popper, 1947d⟩ [. . .].⟨Footnote: The reduction of axioms to definitions carried out here is, to be sure, more apparent, insofar as the definitions are not really explicit definitions. In chapter VIII of the aforementioned paper even Popper himself remarks that in the application of his system to an object language corresponding existence axioms still have to be added to the definitions.⟩ In this an earlier publication is also mentioned. As far as axiomatics is concerned, Popper has dealt in particular with probability theory, for which he has set up various axiom systems with which, among other things, he aimed to get along without the assumption that the objects subject to probability constitute a Boolean algebra, while, on the other hand, he assumes the

[m] Popper (1974a).

theory of real numbers. You find this, e.g., in the appendix of the paper "The Propensity Interpretation of Probability" ⟨Popper, 1959c⟩ [. . .]. A somewhat earlier paper by Popper on the axiomatization of probability theory appeared in the same journal (⟨Popper, 1955c⟩). Recently Popper has also published a paper on an axiom system for geometry, which is an improvement of an axiom system of a Mr. Roehle.[n]

[. . .]

With warm regards, also to your wife,

Yours, Paul Bernays

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[n] Popper (1974a).

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Chapter 22
Popper’s Correspondence with Luitzen Egbertus Jan Brouwer

Karl R. Popper and Luitzen E. J. Brouwer

Abstract Luitzen Egbertus Jan Brouwer (1881–1966) was a Dutch mathematician well-known for his work in topology and his philosophy and development of “intuitionism” as a novel form of constructive mathematics. (For further information on Brouwer’s life and work cf. the biography by van Dalen, 2012.) Brouwer presented Popper’s papers “On the Theory of Deduction I, II” (Popper, 1948a,c) and “Functional Logic without Axioms or Primitive Rules of Inference” (Popper, 1947d) to the Royal Netherlands Academy of Sciences. They are reproduced in Chapters 4, 5 and 6 of this volume. In these papers Popper proves the non-definability of various negations weaker than intuitionistic negation. Brouwer reacts very positively to Popper’s articles on logic, in particular to his duality constructions and his novel definition of intuitionistic negation. His high estimation of Popper’s work on logic also shows in a letter that Brouwer wrote to Harold Jeffreys on the occasion of Popper’s application for an academic post.

Editorial notes: The letters from Brouwer to Popper are from KPS Box 280, Folder 8. The letter from Popper to Brouwer is from KPS Box 37, Folder 11. Each letter exists as a handwritten draft and as a typescript. The letters from and to Jeffreys are quoted from van Dalen (2011). They can be found in the Brouwer archive at the Philosophy Department of Utrecht University.

22.1 Brouwer to Popper, 27 August 1947

Typescript with letterhead, 1p.

Prof. Dr. L. E. J. Brouwer
Blaricum, Torenlaan 70
August 27th, 1947

Dear Popper,

D. Binder et al. (eds.), *The Logical Writings of Karl Popper*,
Trends in Logic 58, https://doi.org/10.1007/978-3-030-94926-6_22

Thank you very much for your sending Hayek's beautiful book[a] to me. I think that a wonderfully free spirit must be abroad in your surroundings to allow the germination ⟨of⟩ such works. My next stay in England will last the greater part of October. So I am looking forward to seeing you again within a few weeks. By this same post I send you a reprint bearing upon one of our subjects of conversation in July.

Yours very truly
LEJ Brouwer

22.2 Brouwer to Popper, 25 October 1947

Postcard, handwritten, 1p.

D. K. R. Popper
London School of Economics
Houghton street (Aldwych), London WC2

Cambridge, October 25, 1947

Dear Popper,

Today your paper is presented to the Academy in Amsterdam. The Lost Property Office answered that it regrets that its inquiry had no success. Next Thursday I shall be again in London. Probably I shall look in at Houghton street at 1 o' clock to see if I can have lunch with you there. I hope that you are all right.

With kind regards,
yours,
LEJ Brouwer

22.3 Brouwer to Popper, n.d.

About one third of a letter size sheet, handwritten, 1p.

Dear Popper,

So sorry that I could not manage to catch the train of 10.00 on Saturday! Moreover I became indisposed, and stayed in bed over Sunday. I hope that before long I shall have an occasion of spending more time in London, and th⟨en?⟩ also seeing your home. Please recommend me to Mrs. Popper.

Cordially yours
LEJ Brouwer

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[a] Popper probably refers to Hayek (1944).

22.4 Popper to Brouwer, 18 November 1947

Typescript with handwritten corrections, 2p. There also exists a probably earlier handwritten draft, 5p.

The London School of Economics
Houghton street, Aldwych, London W.C.2

November 18th, 1947.

Dear Professor Brouwer,

I hope you are not disgusted by the sight of a new MS from me. If you think it too much to communicate two papers by my humble self in such quick succession, then, please, don't do it, and let me have the MS back as soon as possible.

The present paper is, if I may say so, the most interesting I have so far written, and mainly devoted to an analysis of intuitionist negation. Among the results are: The superiority of Heyting's calculus over Johansson's (and Kolmogoroff's) is established on purely formal grounds, by proving that Johansson's negation is not definable (because not sufficiently characterized) while Heyting's is definable. Another result is the one mentioned in my last letter to you. A third result is that it is possible to construct a negation (and, indeed, a whole calculus) which is a dual of Heyting's; in this calculus, the negation of *a* can be interpreted as "*a* is not necessary (or uncertain)", which turns out to be a dual of "*a* is impossible". In the dual calculus, the law of the excluded middle is valid, that of contradiction is not. I do not think that this dual calculus is in itself very important, but its existence may remove certain misgivings about the lack of symmetry in the logic of intuitionism. For it now turns out that there is a *formal* symmetry, and that it is merely the comparative irrelevance of the dual kind of negation for the problems of mathematical demonstration which makes intuitionism emphasize impossibility (as opposed to non-necessity).

It is also proved, in my paper, that intuitionist negation and its dual can co-exist without interference, and that a language may contain both without containing classical negation.

The proofs themselves are all intuitionistically valid.

Of course, what I am saying here is nowhere stated in the paper; but it is implicit in the proofs sketched in the paper.

For all these reasons, I very much hope that you may do me and the paper the honour of communicating it, But if you feel that you should rather not, I shall *certainly* not take it amiss.

If it is too long, it can easily be divided into two roughly equal parts, at the end of section III (p. 9). In this case, the first three sections may be published under the title
2 "*On the General Theory of Derivation*" and the remaining sections under the | title "*On the Definitions of Classical and Intuitionist Negation*".

I hope you are well. My cold ist still very bad – I am coughing incessantly.

With many thanks and the kindest regards,
Yours ever,

22.5 Brouwer to Popper, 10 December 1947

Postcard, handwritten, 1p.

D. K. R. Popper
The London School of Economics
Houghton Street, Aldwych, London WC2

Waiting-room of Liverpool Street station, December 10th, 1947.

My dear Popper,

Your duality construction and your new definition of intuitionistic negation have delighted me, and I have presented your last paper on November 29th. As you foresaw, it was stated at the printing office, that your manuscript had to be split up on account of its size. I received a copy of the proof sheet, which I sent back with an indication of the two expanded titles you describe in your last letter. Please return your own proof sheet with the same indication. In my opinion the impression of your paper wins by this division. – I am travelling to Cambridge, but have no time to stop in London this time. Probably I shall leave Cambridge on the 15th (or 16th). I very much hope to stay with you on my return journey, as was agreed upon. Please remember me to Mrs. Popper.

Kindest regards
yours
LEJ Brouwer

22.6 Brouwer to Popper, 4 January 1948

Typescript with letterhead, 1p.

Prof. Dr. L. E. J. Brouwer
Blaricum, Torenlaan 72

January 4, 1948

Dear Popper,

I enquired for your reprints. The matter is that all mathematical communications to the Amsterdam Academy, after appearing in the proceedings, are published a second time in the mathematical periodical “Indagationes Mathematicae”. It is only after this second publication that the reprints are finished off and sent to the authors. Your October reprints will be dispatched within a couple of weeks and, according to your suggestion, in several separate parcels.

As to your November communication, your type $/\!\!-$ (oblique) is lacking in the printing office, but a type $\vdash$ (upright) is available. Consequently the compositor begs your permission to replace the former symbol by the latter. The compositor says that the symbol $/\!\!-$ occurs so often in your November manuscript, that it takes too much time and pains, to manufacture it poorly and defectively by hand one by one in so great number. With regard to your October paper there was no objection against the oblique symbol, because there you used it only a few times. The difference between the oblique and the upright symbol seems very slight to me, so that I hope you can comply with the compositor's wishes.

So long! Remember me to your wife. Prosit 1948 to you both.

Kindest regards
Yours LEJ Brouwer

22.7 Brouwer to Popper, 6 January 1948

Typescript with letterhead, 1p.

Prof. Dr. L. E. J. Brouwer
Blaricum, Torenlaan 70

January 6, 1948

My dear Popper,

The following in addition and correction to my letter of January 4th:

One of these days you will receive, or perhaps you have already received, new proof sheets of your November paper. In these sheets your symbol $/\!\!-$, remaining oblique, has a still unsatisfactory and, moreover, inconstant form. In many places it is not even connected. Nevertheless this is the utmost of adequacy the compositor can attain for the oblique type. If you reject it, the only solution would be to change over to the upright type. But this would probably take some time and delay the publication of your paper by one month. So there would also be a ground to accept the reproduction of your symbol in its present state.

Cordially yours
LEJ Brouwer

22.8 Brouwer to Popper, 19 January 1948

Typescript with letterhead, 1p.

Prof. Dr. L. E. J. Brouwer
Blaricum, Torenlaan 72

January 19, 1948

My dear Popper,

One of these days you will receive your new proof sheets with the upright symbol $\vdash$. It seems to me that at the first appearance of this symbol in your text you should make an insertion referring to the different shape of the symbol denoting the same property in your preceding article P_3[b].

---

[b] Popper (1947d).

If you wish to have an ultimate proof controlled and revised by me, then please send your corrected sheets directly to me.

The more I read and think your paper over, the more I get impressed by its importance.

Cordially yours
LEJ Brouwer

22.9 Brouwer to Jeffreys, 4 May 1948

Letter quoted from van Dalen (2011, p. 2378).

Dear Professor Jeffreys,

You asked my opinion on Dr. K. R. Popper. I consider him one of the leading logicians of the present time, and one of the keenest thinkers in the philosophy of both exact and humanist science.

Faithfully yours
(signed) L.E.J. Brouwer

22.10 Jeffreys to Brouwer, 7 May 1948

Letter quoted from van Dalen (2011, p. 2380).

Dear Brouwer,

I saw the Vice Chancellor yesterday and he said he would like a fuller statement about Popper. Some of the other candidates have sent in several pages of particulars. May I trouble you for some more after all? I am sorry to trouble you. The electors will probably want to know approximate age, what posts he has been in, or principal contributions to knowledge; and of course especially indications of how outstanding the leading ones are. Perhaps 1 to 2 pages.

Looking forward to seeing you again on Sunday!

Yours sincerely
Harold Jeffreys

22.11 Brouwer to Popper, 7 May 1948

Postcard (Gateway & Chapel. King's College. Cambridge), handwritten, 1p.

Cambridge, Gresham Road 3

D. K. R. Popper
The London School of Economics

Aldwych, Houghton Street, London WC2
May 7, 1948

My dear Popper

When your telegram on your reprints arrived in Holland, I was already in Cambridge, so that it reached me with a considerable delay. But *then* I sent a very urgent telegram to the publisher

North Holland Publishing Company
Nieuwezijds Voorburgwal 68, Amsterdam[c]

and I hope that the reprints are in your possession now. If not, then wire also yourself please. Remember me to Mrs. Popper. I hope we shall soon meet.

Yours Brouwer

22.12 Brouwer to Jeffreys, 11 May 1948

Letter quoted from van Dalen (2011, p. 2381f.).

Dear Professor Jeffreys,

You asked for a more detailed exposition of Popper's merits. I am sorry I have not the data to give an elaborate testimonial.

Popper's main contributions to knowledge lie in the following three fields:

(i) Philosophy of natural science, to which belongs his book "Logik der Forschung" which appeared about 1935, and which is just now being translated into English. It is not only synoptic and explanatory, but also in some ways a practical manual for experimental scientists.

(ii) Moral, social and political science, to which belongs his book "The open society and its enemies" (Vol.1: The spell of Plato; Vol.2: The high tide of prophecy: Hegel and Marx) which appeared in 1944, and was recently reprinted; further his series of articles on "the poverty of historicism" which probably led to his appointment at the London School of Economics, and several occasional small papers among which is a very remarkable one on "Utopia and violence" which appeared in the Hibbert Journal of January 1948.

(iii) Mathematical logic, where Popper plays a prominent part in the complete renewal this science is undergoing just now. In particular his papers on derivation and negation which appeared about the end of 1947, I think will be consulted and quoted during a generation.

You also asked for personal data of a formal character. To get these I rang up Popper himself, and here they are:

Born in Vienna, July 28, 1902
Emigrated to England 1936
Senior lecturer in philosophy in New Zealand from 1937 to 1945

[c] Cf. Andriesse (2008).

Reader in logic and scientific method at the London School of Economics since 1945
British subject from 1945

Degrees: Ph.D Vienna 1927
M.A. New Zealand 1938
D.Litt London 1948

Yours faithfully
(signed) L.E.J. Brouwer

22.13 Brouwer to Popper, 29 November 1951

Handwritten, 2p.

Prof. Dr. L. E. J. Brouwer
Cambridge, Gresham Road 3, tel. 54538
(To Mrs. M. Ogilay)
November 29$^{\text{th}}$, 1951

My dear Popper,

Returning next Monday or Tuesday from a five weeks stay in Cambridge I should like to pass one night in London, and if possible seize this possibility to see you. Braithwaite told me that you have removed from East Barnet. I hope your new dwelling is less outlying than the house you left, and is pleasant and comfortable.

In London I should like to put up at the hotel of the Society for visiting scientists, where you took me one day to have lunch with Schrödinger. I should also like to become a member of the Society, and stay in the same hotel during the first half of May 1952, when I shall have to deliver the Shearman lectures.

But I have completely forgotten the address so that I venture to request you, kindly to arrange this matter for me, i.e. to propose me as a member to the Society, to get me
2 a room in the hotel for next | Monday or Tuesday, and to let me know the address and the telephone number of the house, that I may announce day and hour of my arrival in good time there.

Tomorrow I shall have dinner with Von Wright. I wonder whether again a foreigner will be appointed as his successor here.

Thanking you in anticipation, sending hearty greetings to you and your wife, awaiting your answer as soon as possible, and very much looking forward to seeing you, I remain

Yours LEJ Brouwer

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Chapter 23
Popper's Correspondence with Rudolf Carnap

Karl R. Popper and Rudolf Carnap

Abstract Rudolf Carnap (1891–1970) was a German logician and philosopher, an eminent member of the Vienna Circle and one of the main proponents of logical empiricism. The correspondence reproduced here covers the time from 1942 to 1947, when Carnap worked at the University of Chicago and Popper at Canterbury University College in Christchurch. During that time, Carnap published his books *Introduction to Semantics* (Carnap, 1942) and *Formalization of Logic* (Carnap, 1943) and sent copies to Popper in New Zealand. Popper's reading of these books is the starting point for their discussion on logic in this correspondence; Popper sends his detailed notes, remarks and questions to Carnap. Popper later frequently refers to Carnap's work on logic in both his published and unpublished articles. One of his criticisms of one of Carnap's systems, in a footnote of Popper (1947c), ultimately unjustified, is discussed in the last two letters reproduced here.

Editorial notes: The correspondence between Popper and Carnap is from KPS Box 282, Folder 24, and from Princeton University. We omit some letters which are irrelevant for Popper's works on logic. These omitted letters are: Carnap to Popper, 9 February 1946; Popper to Carnap, 25 April 1946; Carnap to Popper, 17 November 1946; Popper to Carnap, 6 January 1947; Popper to Carnap, 1 March 1947; Carnap to Popper, 27 May 1947; Carnap to Popper, 2 January 1950; Carnap to Popper, 17 January 1950.

23.1 Popper to Carnap, 15 October 1942

Typescript with handwritten addition in postscript, 2p. with attached sheet, 2p.

Canterbury University College,
Christchurch, N.Z.
October 15th, 1942

Dear Professor Carnap

D. Binder et al. (eds.), *The Logical Writings of Karl Popper*,
Trends in Logic 58, https://doi.org/10.1007/978-3-030-94926-6_23

Let me first thank you very much indeed for both your book[a] and your letter. I got the book with a three months delay, the letter took only 7 weeks. I have not replied earlier because I wanted to be able to say something about the book, which meant that I had to read it first; and since I was very busy, teaching as well as writing, I was at first only able to glance at it. I finished a first more thorough reading only to-day; so I may make perhaps some remarks on it now. But I must say at once that one reading is, of course, quite insufficient and that I shall go back to it many times.

I shall make a few detailed remarks on a separate sheet. Here are some general impressions.

First of all, may I say that the book appears to me by far the most comprehensive[1] analysis in the field of general logic I know. Especially the analysis of the correspondence between the Absolute concepts, the Semantic concepts (radical as well as L and F) and the Syntactical C concepts is most important, and was in this beautiful and simple form entirely new to me, in spite of the fact that I have been teaching myself on lines closely similar to your § 13 (Logical and Descriptive Signs) to § 16, using especially definitions like your D 16 E 1 and E 2, following Tarski's Folgerung. The way in which Lewis' system appears (p. 93) as an absolute L-system is most satisfactory. (In spite of that, I feel that the semantic and syntactic systems are to be preferred, in practice, to the absolute systems. I cannot help feeling that the absolute systems are not very valuable from the point of view of a logic of science; and I therefore much prefer method 2 to method 1 in § 16; but all that may be due to en extensional upbringing.) I am most keenly looking forward to vol. II, and to the solution of the problem of symmetry you have promised, as well as to the new analysis of the propositional calculus. Indeed, the remarks you make about vol. II are more than thrilling. Many, many thanks again.

You ask me, in your letter, what I am doing. Apart from teaching and continuing with probability (I have a few results here) as well as with confirmation, I have concentrated mainly on practical problems of the methodology of the social sciences.
2 It is a war effort. One fruit of | this is an article *The Poverty of Historicism*, which will appear, I hope, in Mind. Another is a fairly voluminous book which I have just finished. It is an attack on a kind of superstition introduced into the field of social investigation by Plato and further developed by Hegel and Marx. I have written this book because I believe that it contributes to the understanding of fascism and its dangers and throws light on the present crisis. Its title is[b]

False Prophets
Plato — Hegel — Marx.

I am sending the book to some U.S.A. publishers, and I have given your name as a reference, together with the name of G. E. Moore. Of course, I did not imply that you

[1] not C-comprehensive!

[a] Carnap had planned to publish a five-volume series of *Studies in Semantics*, the first volume being Carnap (1942), which is the book referred to here; in the following, "vol. II (of the Studies)" refers to Carnap (1943).

[b] Published as K. R. Popper (1945a). *The Open Society and Its Enemies*. London: Routledge.

knew the book or that you can say anything about its prospective value. All I said is that you know me. I hope that I shall not cause you any inconvenience in referring to you. I have worked on the book most intensively for three years.

Regarding probability, I have simplified my formal system, and I hope I shall find time one of these days to write the thing out in a form in which it can be published. It is very bad to have nobody to discuss these matters with.

Many thanks again and please be good enough to send me vol. II of the Studies when it appears.

Yours
K. R. Popper

P.S. One of my difficulties here is that I cannot obtain the Journal of Unified Science. My attempt to have it in our library failed. I am entirely out of touch since 1938! If you have some back numbers, I would be most grateful. We get the Philosophy of Science, but it has deteriorated deplorably.

The Journal for Symbolic Logic is also not available in Christchurch, but Prof. Findlay gets it in Dunedin, and I can get from time to time a loan of it, which is of course not entirely satisfactory. – I have given Findlay your greetings. K. R. P.

⟨*Separate sheet*⟩

A few remarks on the Introduction to Semantics.

I believe that the most important problems discussed in the book are those of §§ 16, 18–23, and 36.

§ 16. It is possible or even probable that the methods 1a, b may not work out satisfactorily. The following appears prima facie as very likely: Just as in method 2, L-truth and L-implication turn out to be correlative to the chosen interpretation of the concept of "logical sign", so in method 1a, they will turn out as correlative to the choice of "logical necessary in *M*" (and in 2b, to "demonstrable in *M*"); i.e.: it seems likely that method 1 will not overcome the fundamental vagueness of the method 2. And it is therefore questionable whether we are not creating the problem ourselves, and whether we should not simply say that "L-true" is correlative to the intended use of certain signs, as descriptive or logical respectively; so that the real problem would be, rather, to show precisely what use we would call logical or descriptive, and what kind of terms may be affected by this vagueness, and in which way; and what kind of difficulties may arise therefrom.

§ 23. I was most interested in your definition of "Content" as the *complement* of "Range" (and the vice versa method); I have been using this method in my lectures, introducing the term "anti-content" as an alternative for range. What are the objections against defining Content (E-Content, empirical Content) of T_i as the class of L states incompatible with T_i, or as the class of propositions (absolute) L-incompatible with the designatum of T_i? Range (E-Range) could then be defined as the complement of Content. This is what I intended to suggest in my Logik d.F., p. 67, 74, 77.

(By the way: you mention on p. 151 Wittgenstein: "For the assertive power of a sentence consists in its excluding certain states of affairs (Wittgenstein)." But I think you had here not W. in mind, but my book, esp. the last lines of p. 67 and many other passages. This, indeed, was just one of my fundamental ideas, and I do not think I got it from W. I have searched the Tractatus, and I could not find anything like it.)

§ 24. I was most interested in the rules of refutation. (It just shows how one is liable not to go far enough in analysing one's implicit assumptions.)

2 *§ 30.* In order to bring out the relations between comprehensiveness | and inconsistency even more fully, one could define K as C-comprehensive if and only if $\Lambda \underset{C}{\rightarrow} V$.

We would then have:

T 1. Each of the following conditions is a sufficient and necessary condition for K to be C-comprehensive:

a) All T_i in K are C-equivalent.

b) All T_i in K are C-comprehensive.

c) Λ is C-comprehensive.

T 2. If K is C-Comprehensive, then if it contains one T_i which is C-false in K, it is C-inconsistent.

T 3. Every C-inconsistent K is C-comprehensive. Etc., etc.

(Remark: I did not know previous to the receipt of your book that you are using the term "comprehensive", and I have used therefore the term "embracing" instead, in an unimportant Note which appears in the next number of Mind[c].)

§ 36. Are you treating the relations between the problem of § 16 and § 36 in vol. II?

There are many problems in connection with exhaustive calculi; we could, as it were, *convert* the idea of exhaustion, and introduce the idea of "cover": That part of S which is exhausted or "covered" by a certain interpretation I of a K, as the I-K-realm of S. We then could, for instance, speak of the realm of "that logic of S which is formalized or 'covered' by a certain K" or of "that realm of the physics of S which is formalized by a certain theory P", etc.; and we may even obtain "absolute" concepts indirectly referring to such a calculus. E.g. the realm of absolute L-implicates, as "covered" by, or corresponding to, the calculus of propositions; or of F-implicates, as covered by Physics. (The latter realm is particularly interesting. It could be used to describe the realm of physical causal "links".)

I am, of course, most interested in your announcement (p. 224) to treat the problem of formalization in vol. II in detail.

I do not quite understand the remark on p. 224 "For certain semantical systems, L-exhaustive calculi cannot be constructed without using transfinite rules." Certainly; but as it stands, this reads as if it were meant that for all other systems, calculi can be constructed without such rules, and as if therefore for *all* semantical systems L-exhaustive calculi could be constructed. But this cannot be meant, as far as I can see (Goedel!).

[c] K. R. Popper (1943). Are Contradictions Embracing? In: *Mind* 52 (205), pp. 47–50.

23.2 Carnap to Popper, 29 January 1943

Typescript, 2p.

Rudolf Carnap
University of Chicago
Chicago, Illinois.

January 29, 1943.

Dear Dr. Popper,

Thank you for your letter of November 28th and the remarks on my "Semantics". I am glad to see that our views in questions of semantics are very much in agreement. The printing of the second volume has been delayed. It will probably appear in February. Then I shall send you a copy.

The Journal of Unified Science has not appeared for several years. Neurath is trying to find a publisher for it in England (he is in Oxford), but so far without success.

I am looking forward to the book you have written. I hope you will soon find a publisher. If a publisher writes to me I shall be very glad to do what I can to help by recommending it. I hope you are cautious in your criticism of Marx in order not to furnish arguments to those who do not only differ with his views but also reject his goal. A careful separation of what is right and what is wrong in his views is necessary, such as you did in your article on "Dialectics"[d], which I liked very much. Perhaps this article or something similar is now a chapter of the book? If the book has the same careful, objective, critical attitude then it will be very useful.

To your remarks on my book. *§ 16.* At present, I prefer definitions of L-concepts on the basis of L-range (§ 20), with rules of L-range instead of rules of truth. The elements of L-ranges are state descriptions as in § 19E.

To *§ 23.* There are no objections against the indicated definition of content. The L-states incompatible with T_i are the same as the L-states which do not L-imply T_i. Therefore your definition seems to me about the same as my D18-B3 and D23-B1.

I am very sorry that, by error of memory, I referred on p. 151 to Wittgenstein instead of to you. I shall correct it at a suitable place in a later volume.

Strangely enough, I myself used the term "embracing" instead of "comprehensive" in my MS. I changed it because Quine said that "embracing" was quite unsuitable. Probably for peculiar connotations of the term in English.

To your last paragraph, about p. 224. Gödel's result holds only for calculi with finite rules. This fact is often overlooked. My language II in "Syntax", together with the definition of "analytic" taken as transfinite syntactical rules, is an L-exhaustive calculus for a certain system of classical mathematics. I do not know any semantical system for which no L-exhaustive calculus can be constructed.

2 | I did a good deal of work during my present leave on modalities and on probability, also on the principle of the uniformity of nature as a condition for induction. (It seems to me that the task of finding a scientific formulation for this

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[d] K. R. Popper (1940). What Is Dialectic? In: *Mind* 49 (196), pp. 403–426.

principle is not quite as hopeless as we thought.) But in both fields it will be years before anything will be ready for publication.

With best regards for both of you,

Yours,
Carnap

23.3 Popper to Carnap, 31 March 1943

Typescript, 2p.

March 31st, 1943.

Dear Professor Carnap,

Many thanks for your letter of January 29th. Since I wrote you last, I have further read in your Introduction to Semantics, and with great interest. There is no doubt, the progress made by logic during your life-time is immense, and you have a very full share in it.

Concerning my remark to p. 224 which you rightly criticized: I knew of course that Goedel's results do not hold for "true" (inclusive L-true or analytic) but only for "derivable" (in the narrow sense). My mistake was rather that I identified your term "calculus" with "*deductive system*" in the ordinary narrow sense in which it contains only finite rules of deduction. (I was, I now believe, mislead by the introductory note to § 25, p. 155; and your remark on p. 160f. did not sink in. It was a first reading.)

⟨The rest of the letter concerns Carnap's question regarding Popper's criticism of Marx and the publication problems of "The Open Society" (Popper, 1945a), then still called "False Prophets: Plato, Hegel, Marx".⟩

Many thanks and kindest regards.
Yours K. R. P.

23.4 Popper to Carnap, 5 July 1943

Typescript with handwritten postscript, 3p.

Canterbury University College
Christchurch, C.1
New Zealand

July 5th, 1943.

Dear Professor Carnap,

I received the "Formalization of Logic" last week; very many thanks, I found it most interesting, and even thrilling; and this in spite of the fact that the presentation is not easy. The brief summaries at the beginning of each § are certainly a great help: but the difficulty lies mainly in the fact that up to § 15, everything is more or less preparation. The problems treated in §§ 16–32 are intensely interesting and important. I have been telling my students for some years that it appears that the axioms (plus rule of deduction) of PC are a formalization of the truth-tables; but I always had some misgivings, and sometimes I even voiced them in classes. But I had *not the slightest idea* of how to tackle the problem; I could not even formulate it with a sufficient degree of precision. Your simple concepts of normal and non-normal interpretations do not only help to formulate the problem precisely, but also to solve it.

One of the things which surprised me most (in spite of some warnings in vol. I[e]) was that conjunction *and* implication must always have a normal interpretation (in any true interpretation) while this is not the case with negation and disjunction. I must admit that when reading your vol. II[f], I suspected for a time that this situation might be brought about by the peculiarities of the term "true interpretation" as defined in vol. I; but I believe now that this is not the case, and that the root of the matter is indeed the lack of formalization as pointed out by you. (I believe, however, that a brief discussion of this question might have been helpful in § 16; i.e. of the question how much depends on the adoption of your term "true interpretation" as basis for the discussion.)

The thing that is most puzzling is undoubtedly this. In §§ 26 ff. you define a calculus which is, in a certain sense, logically stronger than PC. But it is not richer in theorems. The idea that any "addition" to, or "strengthening" of the calculus must lead to additional theorems is, of course, shown by you to be an unwarranted prejudice. It is this deep-rooted prejudice which has led us all to believe that, since the PC is complete regarding theorems, it is impossible to strengthen it further. But at the end of § 27, one feels the need of an explicit discussion of this problem; for some puzzle remains. What is needed is perhaps a more detailed comparison of the various senses of "completeness" than the one on p. 99.

I was also very thrilled by the sheer beauty, simplicity, and novelty (at least for me) of the method of *involution*. In a way, I feel that you might have emphasized this method more strongly. As *T 23-1* shows, involution can be considered a generalization (extension to sentential classes) of the idea of an implication between sentences, and it can be so considered with exactly the same right as the implication between sentential classes. And it is even a "weaker" generalization, in so far as from
2 "T_i implies T_j" "T_i involves T_j" can be deduced, but not vice versa. | ("T_i involves T_j" does not even L-involve "T_i implies T_j") In spite of this logical weakness, involution turns out, in your treatment, to be in a way more fundamental, in so far as you achieve with involution without junctives as much as with implication plus junctives. It is really a surprisingly neat and elegant method. (But I realize that elegance is a secondary problem.)

Many interesting questions are raised; an obvious question, for example, is this: can the application of your method influence any of the decision problems? And can your method be successfully applied to such "complete" fractions of the PC as, for

[e] Carnap (1942).

[f] Carnap (1943).

instance, the "positive logic", including $((p \supset q) \supset p) \supset p$, or the "positive identical implication formulae" (the latter as indicated in Hilbert Bernays I[g] p. 69)?

If we take, (instead of your table on p. 9) the axioms of the "positive logic"

(a) $p \supset (q \supset p)$; $((p \supset q) \supset p) \supset p$; $(p \supset q) \supset ((q \supset r) \supset (p \supset r))$; see H-B, p. 70

(b) in addition H-B, p. 66, II & IV, leaving the dangerous disjunction aside: does such a weak system permit non-normal interpretations of implication and conjunction? And does the system (a) *alone* permit non-normal interpretations of implication? And what happens if either H-B p. 66, III (disjunction) *or* V (negation) is added, to the exclusion of the other of these two?

Some of these problems seem to be quite simple, thanks to your method; but I had no time so far to think about it, and there are some points which puzzle me. For example it is clear that such a positive logic can be strengthened by your rule of refutation (vol. II, 102), and then, according to your *T20-1*, C-falsity and L-falsity will coincide, while they obviously did not coincide before; for it is possible to give at least two L-true interpretations of the "positive Logic" such that (1) every sentence is translated into an L-true one, or (2) every C-true sentence is translated into an L-true one and all others into true ones. However, as far as I can see, *these interpretations would not be non-normal* in your sense. This seems at first sight to suggest that even the absence of non-normal interpretations does not guarantee full formalization: Where is my mistake? I see, of course, that this argument depends partly upon the use of propositional variables, in so far, namely, as there are simply no L-false sentences in this "positive Logic" if we use axiom schematas (or proceed as you do in vol. II, D2-2) instead of propositional variables; and if propositional variables are discarded (as your method of interpretation demands), then my problem seems to disappear. I suppose that this is all about it; but I am not yet quite sure.

The book is so obviously important that it must, I am convinced, dispel such misunderstandings as that of Langford in his review of vol. I in the Journal of Symbolic Logic[h] (which I just received). I was amazed, and even dismayed about this review. It does not touch any of the fundamental issues opened up in vol. I, such as for example the problem which is pending between you and Tarski (and Bernays) whether the distinction between analytic (or L-true and L-false) and synthetic (or F-true and F-false) is a workable and unambiguously applicable one.

In regard to this problem, I believe with you (vol. I, p. vii) that we need this term in the methodology of science, and that such problems as empiricism cannot be discussed without it. On the other hand, I understand Tarski's scepticism. I may say
3 that I have some ideas which might | reconcile both of you. –

I just mentioned the Journal of Symb. Logic: Oppenheim has been so very kind as to subscribe it for me for a year. Let me thank you very much for having interested yourself in this matter. (In fact, we are so poor that we have not been able to buy a single book for some years; but this is an unimportant point in these days, and I am really a little embarrassed by Oppenheim's generosity.)

[g] Hilbert and Bernays (1934).

[h] Langford (1943).

To come back to Langford's review: I feel sure that he will see the mistake he has made as soon as he reads vol. II. On the other hand, I must say that you have not made it too easy for your readers to find their way through vol. I, and the title "Introduction" may lead to misapprehensions. The real difficulty seems to be that you have perhaps not placed your vol. I with sufficient clarity within a certain historical problem-situation. This is, implicitly, achieved by vol. II.

I am afraid, I have written a long letter. From us, there is nothing new to report. I have an extensive as well as exhaustive lecturing programme. I have courses in Logic; Ethics (two courses); History of Philosophy; Introduction to Philosophy; Political Philosophy; and I have, besides, a W.E.A. course under the title "Philosophy of Nature and Society", and a course for research workers on Scientific Methods (with discussion of their practical research problems, which have led to considerable practical results; a kind of poli-clinical advisery agency for agricultural chemists, etc.). This course (which has just ended) was very interesting and successful. It is comforting to find that philosophy can be of some practical use! My hatred against the empty verbalism and scholasticism of the vast majority of philosophical writings is increasing proportionally with the time I have to devote to the teaching of such matters (of course, I try to do my best in my lectures to expose them), and it sometimes reaches a state of acute nausea. On the other hand, I more and more appreciate the one incomparable writer of our time: Russell. (I exclude, of course, his ⟨I⟩nquiry into Meaning and Truth[i] and a few minor efforts of his.) He is free from the bumptiousness and pretentiousness of the vast majority of philosophers.

I have not heard a word about my book since I wrote you last – in spite of cables etc. It is disconcerting.

Many thanks and kindest regards,

Yours,
K. R. Popper

P.S. Could you send me Tarski's address?

23.5 Popper to Carnap, 28 May 1944

Typescript, 1p.

Canterbury University College
Christchurch, C.1
New Zealand

May 28th, 1944.

Dear Professor Carnap,

I have not heard from you since your kind letter of January 29th, 1943 (to which I answered on March 31st; on July 5th I sent you another letter, about your "Formalization"), and I hope that no letter has been lost.[j]

[i] Russell (1940).

[j] Both letters can be found in *Rudolf Carnap Papers*, University of Pittsburgh.

This is merely a note to tell you that Kegan Paul-Routledges are printing my book in two volumes; the first of which they intend to publish in October, the second in February. Its title will be "The Open Society and Its Enemies" (or perhaps "and Its Antagonists").

A lengthy article – a kind of continuation of my Dialectic-article – is being published in Economica;[k] the first part came out in the May number, the second part is more interesting. I have no reprints so far.

I have written a number of papers on such technical problems as postulate systems for Boolean algebra, but they are rather long and ⟨I⟩ doubt whether anybody would print them.

Yours sincerely,
K. R. Popper

P.S. Could you kindly send me Tarski's address?

23.6 Carnap to Popper, 9 December 1944

This letter is mentioned in Popper's next letter. We have not been able to locate it.

23.7 Popper to Carnap, 11 February 1945

Typescript, 1p.

K.R. Popper,
Canterbury University College
Christchurch, N.Z.

February 11th, 1945

Dear Professor Carnap,

Many thanks for your letter of Dec. 9th, 1944. First about your suggestion concerning the Guggenheim fellowship: if there is any possibility I should of course like to apply. I have heard the name "Guggenheim fellow" before, but I have never taken any notice of it. I should be extremely glad to get anything of this kind, and I should be most grateful if you could tell me how and where to apply; also, if you would tell me of any other such possibility, if one occurs to you. I remember that the Carnegie people were once interested in me (during the Copenhagen Congress) but I never applied for any such thing.

I am particularly grateful for your kind offer that you would write a testimonial "in the highest terms of appraisal" for me, and if you would send me one soon, it may be of great help to me. You may have heard that Findlay resigned his chair here (or

[k] The articles Popper (1944a,b, 1945b) in Hayek's journal Economica were later republished as Popper (1957).

rather in Dunedin) in order to accept a chair in South Africa (Rhodes Univ. College, Grahamstown) and he considered that there may be some chance for me to succeed him; at least, he has done his very best to further my chances, and he has written me an extremely nice testimonial (of which I enclose a copy). A new testimonial from you may help very considerably. The matter of Findlay's vacated chair (the salary is about the same as the sum mentioned by you as the Guggenheim fellowship) may become urgent at any moment. May I ask you if you could send me the new testimonial by air mail? I enclose, apart from Findlay's testimonial another testimonial from here (from Professor Hight, formerly Rector of Canterbury University College) which may give you some idea of my activities here, and your old testimonial, since you may perhaps wish to use it when writing the new one.

I was very glad to hear that your back is much better. I am most anxious to see your papers on probability, and I shall certainly send you a full comment on them, especially since you say that my comments on your "Formalization" were valuable to you.

You mention Tarski's and Quine's views on probability and that they diverge from your view that the L-concepts are indispensible in this field. I am sorry to say that I have never heard or seen Tarski's or Quine's views on this matter. (I know only one publication of Tarski's in which he deals with probability – an old criticism of Reichenbach in Erkenntnis V, 174ff.)

I have some unpublished papers on probability, but they are too long for publication and need some work before they can be used.

My book whose final title is "The Open Society and Its Enemies" seems to be set up. (I have not got any proofs: they are being read in England) but I cannot find out when it will be published. This is depressing: the distance makes me rather helpless. I have, of course, asked the publisher to send you a copy upon publication.

Kindest regards to you and your wife from both of us,

Yours,
K. R. Popper

23.8 Carnap to Popper, 30 May 1945

Typescript, 1p.

Rudolf Carnap
Department of Philosophy
University of Chicago
Chicago, Illinois

May 30, 1945

Dear Dr. Popper

Thank you for your letter of February 11. I am sending you herewith a new testimonial. Since it contains parts of the old one, do not use it in addition to the old one, but instead of it. I hope very much that you would succeed in obtaining

Findlay's vacated chair. Did your new book not yet appear? If I had seen it, I could have referred to it in the testimonial more in detail.

The Guggenheim Foundation wrote to me on my inquiry that their fellowships are available only to people who live in America. I regret this very much because you would have there a very good chance. If you could perhaps come to this country by other means (e.g. by a Rockefeller Fellowship, or by whatever means Findlay used to come here), then you could here apply for a Guggenheim Fellowship; they require only residence here, not citizenship.

I did not mean Tarski's and Quine's views on probability, but on the importance of L-concepts in general. I was referring to your remarks of July 5, 1943, pp. 2–3 that you had an idea how to "reconcile" Tarski's and my views in this point. That would interest me very much.

My papers on probability will appear soon, and then I shall send you reprints. My book on probability is progressing only very slowly, but I hope to work more on it during the summer.

My back is very much improved. I am walking around without cane and have no difficulties in giving my lectures.

The news from Vienna are very interesting, but so far rather scarce. Would you ever consider any plans for returning there, if a position were offered to you?

With best regards, to both of you from both of us,

Yours,
R. Carnap

23.9 Popper to Carnap, 23 June 1945

Typescript, 1p.

Canterbury University College
Christchurch, C.1
New Zealand

June 23rd, 1945.

Dear Professor Carnap

Very many thanks for your kind air letter of May 30th which I got on June 20th, and for the improved version of your testimonial. My book is not yet out, although the publisher's agreement provided for a publication not later than the middle of April. This is the reason why you have not yet got a copy, for I advised the publishers a year ago to send you one upon publication.

I could not use your testimonial for my application for Findlay's chair since applications closed in Dunedin on May 1st and in London (for the same job) on June 1st. But this matter of Findlay's chair is no longer of importance for me, for I am going to London: as I wrote in a surface-mail letter posted early in June, I have been offered a readership in Logic and Scientific Method, tenable at the London School of Economics, and I have, of course, accepted. I am very happy about this development.

I am very glad that your back is better, although I had hoped that the trouble would have entirely disappeared by now. I hope the summer will make a difference. Concerning your question about a possible return to the continent, my answer is: no, never! Is yours different?

Kindest regards,
Yours,

P.S. My address from about October or November will be: The London School of Economics.

23.10 Carnap to Popper, 9 October 1947

Handwritten, 3 numbered pages.

Santa Fe, N.M.,
P.O.B. 1214

Oct. 9, 1947

Dear Popper,

My best thanks for your letter of August & your kind judgement on my book.

I just read your "New Found." in "Mind". It is very interesting & contains a number of new & important results. It is an essential improvement in comparison with my previous attempts of defining the connection in terms of "consequence" (first in "Syntax" § 57, & later in "Formalization"). Among other things, your clear & simple analysis of the three kinds of negation is very valuable.

Your discussion on pp. 232f. is, unfortunately, so short that I am unable to understand it. I should like to understand it, especially since it is the basis of your objections against my rules. You say that the last formula on p. 232 leads to that on
2 top of p. 233. How | does it? Is the restriction $a_{\grave{x}}$ in the former but not in the latter no impediment? Further, you say that $a//a\binom{x}{y}$ violates PF2. How does it? (I say that it violates your interpretation.) This is only a question, not an objection; I assume that your assertions concerning *your* system are correct.

However, I doubt very much whether your assertions (in the footnotes p. 232) of the non-validity of my rules 10 & 11 in D28-2 are correct. Note that rule 11, because of its restriction, does not lead to a proof of "$Px \supset (x)(Px)$" (which would indeed be wrong), although "$(x)(Px)$" is derivable from "Px", in distinction to your system. You have probably made the mistake of inadvertently transforming the interpretation & the rules of your system to mine (+ Hilbert's, etc.) Perhaps you have overlooked the following essential distinction. In my system (& Hilbert's, etc. but perhaps not in
3 Princ. Math., & certainly not in your system), "Px" (as a separate formula) | is interpreted (see, e.g., "Syntax", p. 22, par. 2) as meaning the same as "Py" & as "$(x)(Px)$". Therefore my rules are valid, if you doubt it, please give a counter-example, by using only *my* rules, not yours.

Feigl & Hempel (& his new wife) were here for a few weeks, & we had a very nice time together, with many interesting discussions, mostly on inductive logic.

We shall stay here until Xmas.

With best regards,
yours,
Rudolf Carnap

(Please, let's forget about titles.)

23.11 Popper to Carnap, 24 November 1947

Typescript, 2p.

November 24th, 1947.

Dear Carnap,

I am sending you to-day an offprint of my "Logic without Assumptions", referred to in my "New Foundations" (note 1 on p. 203). Two more papers are on the way; I have been promised the offprints of one of them for next week, and I shall send you a copy at once.

I am overworked (8 hours lecturing a week is too much if one does research – I wish I could get some time off for research, but I don't know how), and really quite exhausted.

You asked me in your last letter for a fuller explanation of my pp. 232f. (of my "New Foundations"). I suppose that it is the misprint on p. 233 ("PF2" should properly read "PF4") which created the difficulty, and that you will have found meanwhile what I meant. Still, here is a fuller explanation.

My contention is this.

Your statement (*Formalization*, p. 136) "that there exists a one-one correlation between the individuals and the natural numbers" indicates that it is your intention to construct a calculus which is *consistent with* my (much weaker) postulate PF4, i.e., with the demand that there exists *more than one individual*.

But with the assumption that there exists more than one individual, each of the following rules of your *Formalization* contradict:

C10	(i.e., D28-2, rule 10, on p. 137)
C11	(i.e., D28-2, rule 11, on p. 138)
C12	(i.e., D28-2, rule 12, on p. 138)
Cb	(i.e., T28-4, case b, on p. 139).

For the proof of this contention, I shall make use of my own formalism. But the proof holds for your formalism as well; for your C-implication satisfies, on the basis of your *Introduction*, p. 64, P14-5; P14-8; and P14-11, all the rules which define my "$\ldots/\ldots$", i.e., the rules which I shall call "generalized reflexivity principle" and "generalized transitivity principle". To the latter, I shall refer as "Tg".

I shall also refer to the following principles ("a^k" is the classical negation of a):

(1.1) $a/b \rightarrow\ \vdash a > b$

(1.2) $a/b \rightarrow (\vdash a \rightarrow\ \vdash b)$

(1.3) $\vdash a > b \leftrightarrow \vdash b^k > a^k$

(1.4) $(a \wedge a^k) \vee b // b$

(1.5) $a // a^{kk}$

(1.6) $(Ax(a^k))^k // Exa$ (cp. your d and e, p. 139)

2 | I begin with C10, which I write

(C10) $a / a\binom{x}{y}$, provided y is not bound in $a\binom{x}{y}$.

We obtain, always assuming that y is not bound in $a\binom{x}{y}$:

(C10.1) $\vdash a > a\binom{x}{y}$ (C10;1.1)

(C10.2) $\vdash (a\binom{x}{y})^k > a^k$ (C10;1;1.3)

(C10.3) $(a\binom{x}{y})^k / a^k$ (C10.2;1.1)

(C10.4) $\vdash (a\binom{x}{y})^k \rightarrow \vdash a^k$ (C10.3;1.2)

Now we take "a" to be the name of an open statement (such as "$x + 1 = y$") which is *satisfiable but not* universally true. We obtain

(C10.5) $\vdash$ "$y + 1 \neq y$" $\rightarrow \vdash$ "$x + 1 \neq y$" (C10.4)

and, substituting further "$x + 1$" for "y" (we *may* confine this to the right hand side, but I shall do it throughout) we obtain

(C10.6) $\vdash$ "$(x + 1) + 1 \neq x + 1$" $\rightarrow \vdash$ "$x + 1 \neq x + 1$" (C10.5)

If there exists only *one* individual, then every statement of the form "$\ldots \neq$ —" is false, and C10.6 is innocuous. But if there are more individuals than one, C10.6 gives rise to

(C10.7) "$x + 1 \neq x + 1$"

which is contradictory.

I now proceed to C11. This may be written:

(C11) $a \vee b / a \vee Axb$, provided x is not free in a.

We obtain, substituting "$Axc \wedge (Axc)^k$" for "a":

(C11.1) $(Axc \wedge (Axc)^k) \vee b / (Axc \wedge (Axc)^k) \vee Axb$ (C11)

(C11.2) b / Axb (C11.1;1.4;Tg.)

(C11.3) $\vdash b > Axb$ (C11.2;1.1)

(C11.4) $\vdash (Axb)^k > b^k$ (C11.3;1.3)

(C11.5) $\vdash (Ax(a^k))^k > (a^k)^k$ (C11.4)

(C11.6) $(Ax(a^k))^k / a^{kk}$ (C11.5;1.1)

(C11.7) Exa/a (C11.6;1.5;1.6;Tg.)

(C11.8) Exa/Axa (C11.7;C11.2;Tg.)

But C11.8 is, clearly, satisfied only if there is not more than *one* individual.

I proceed to rule C12. This may be written

(C12) $a^k \vee b/(Exa)^k \vee b$, provided x is not free in b.

3 Substituting "$Axc \wedge (Axc)^k$" for "b" (as before under C11), we obtain: |

(C12.1) $a^k/(Exa)^k$ (C12)

(C12.2) $b^{kk}/(Ex(b^k))^k$ (C12.1)

(C12.3) b/Axb (C12.2;1.5;1.6;Tg.)

But C12.3 is the same as C11.2, and has the same fatal consequences.

Rule Cb, of course, is also the same as C11.2 and C12.3.

The result of all this is:

(1) Rule Cb can be dropped altogether.

(2) The rules of derivation C10; C11; and C12 must be replaced by the corresponding conditional rules of proof, C′10’; C′11; C′12:

(C′10) $\vdash a \rightarrow\ \vdash a\binom{x}{y}$, provided y is not bound in $a\binom{x}{y}$.

(C′11) $\vdash a \vee b \rightarrow\ \vdash a \vee Axb$, provided x is not free in a.

(C′12) $\vdash a^k \vee b \rightarrow\ \vdash (Exa)^k \vee b$, provided x is not free in b.

The last two rules may be replaced by C″11 and C″12:

(C″11) $a\binom{x}{y}/b \rightarrow a\binom{x}{y}/Axb$, provided $x \neq y$

(C″12) $a/b\binom{x}{y} \rightarrow Exa/b\binom{x}{y}$, provided $x \neq y$.

These two rules, in turn, can be replaced by:

(C‴11) $a\binom{y}{x}/b\binom{x}{y} \rightarrow a\binom{y}{x}/Axb\binom{y}{x}$ $(x \neq y)$

(C‴12) $a\binom{x}{y}/b\binom{y}{x} \rightarrow Exa\binom{y}{x}/b\binom{y}{x}$ $(x \neq y)$

If we replace here "→" by "↔", we obtain the rules which define the quantifiers, and from which, in the presence of the six rules defining "$a\binom{x}{y}$", everything else can be obtained:

$$(\mathrm{C}'''')\ a\tbinom{y}{x}/Axb\tbinom{y}{x} \leftrightarrow a\tbinom{y}{x}/b\tbinom{x}{y} \qquad (x \neq y)$$

$$(\mathrm{C}'''')\ Exa\tbinom{y}{x}/b\tbinom{y}{x} \leftrightarrow a\tbinom{x}{y}/b\tbinom{y}{x} \qquad (x \neq y)$$

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Chapter 24
Popper's Correspondence with Alonzo Church

Karl R. Popper and Alonzo Church

Abstract Alonzo Church (1903–1995) was an American logician and mathematician, well-known for his contributions to the theory of computability. He co-founded the Journal of Symbolic Logic and served as editor for reviews for its first forty-four volumes (cf. Enderton, 1995). Popper writes to him in this capacity in order to have his articles reviewed; he had hoped that Church would review his articles himself, but Church had to decline this suggestion and gave the reviews to Kleene and McKinsey instead (cf. this volume, § 13.12, § 13.13 and § 13.14). Apart from helping to fix the dates of the publication and dissemination of Popper's articles, the correspondence also contains a small addendum to "On the Theory of Deduction I" (Popper, 1948a, Chapter 5 of this volume).

Editorial note: The letters are contained in KPS Box 284, Folder 12.

24.1 Church to Popper, 28 October 1947

1 sheet with letterhead of the Journal of Symbolic Logic. Typescript, 1p.

The Journal of Symbolic Logic
Alonzo Church, Editor
Fine Hall, Princeton, New Jersey
October 28, 1947

Dear Dr. Popper:

I have just received your air mail letter of October sixteenth, enclosing errata of your paper in Mind. The promised reprint of the paper itself has not yet arrived, but will no doubt be along in due course.

I appreciate very much your sending us this reprint and your promise to send us reprints of your further papers in this same direction (in the Proceedings of the Aristotelian Society and elsewhere).

D. Binder et al. (eds.), *The Logical Writings of Karl Popper*,
Trends in Logic 58, https://doi.org/10.1007/978-3-030-94926-6_24

These are papers for which I am especially anxious to find a competent reviewer, and your sending reprints is very helpful because it makes possible a wider choice of possible reviewers. I would like to take the opportunity to make the request that you will send us reprints for review of all your future papers which come within our field.

I am at present taking a year's leave of absence for the purpose of writing a book on mathematical logic, and have rather definitely promised to devote all my time to this from now until next September. In fact, I have as far as possible turned over all the work in connection with the Journal ⟨of Symbolic Logic⟩ to assistants, and I am trying to give up all writing of reviews myself, except possibly for occasional minor papers which can be quickly and briefly reviewed. For this reason I probably cannot follow the suggestion to review your paper in Mind myself. If I find later that I have any sort of comment on it, I will, however, write you about it.

With many thanks, I remain

Very sincerely yours,
Alonzo Church

AC:JC

Dr. K. R. Popper
The London School of Economics
and Political Science
Houghton Street, Aldwych
London, W. C. 2, England

24.2 Church to Popper, 11 December 1947

Postcard. Typescript, 1p.

Alonzo Church
Fine Hall, Princeton University
Princeton, New Jersey

Dr. K. R. Popper
16. Burlington Rise, East Barnet, Herts

December 11, 1947

Dear Dr. Popper,

Thank you for your letter of November twenty-fifth – and the new reprint with typographical corrections, which I am forwarding to the reviewer.

Very sincerely yours,
Alonzo Church

AC:JC

24.3 Popper to Church, 16 February 1948

2 sheets. Handwritten, 3p.

London School of Economics
Houghton Street, Aldwych, London W.C.2
16/2/48

Dear Professor Church,

I am sending you one off-print and two proofs. All three papers have been published, but it takes a long time nowadays before one gets off-prints. The off-prints of the paper "Functional Logic without Axioms" etc. were promised me in November and I got them in February! (The paper was communicated by Brouwer in October.)

I should be glad if you could forward them to the reviewer (provided you have still no time to review these papers yourself, I have not given up all hope). I should be very glad if I could have the proofs back, later, after the off-prints of the two later papers have reached the reviewer: I shall send off-prints as soon as I get them.

Many thanks for your very interesting papers. I was thrilled with them.

Sincerely yours,
K. R. Popper

Before "These characterize" insert:[a]

Conjoint denial and alternative denial also are, in this System, dual to each other

(3.91) $$a \downarrow b \vdash c \;\leftrightarrow\; \vdash a, b, c$$

(3.92) $$a \vdash b \curlywedge c \;\leftrightarrow\; a, b, c \vdash$$

24.4 Church to Popper, 20 February 1948

Postcard, typescript, 1p.

Alonzo Church
Fine Hall, Princeton University, Princeton, New Jersey
Dr. K. R. Popper
The London School of Economics and Political Science
Houghton Street, Aldwych, London, W. C. 2, ENGLAND
February 20, 1948

Dear Dr. Popper:

I have just received your air mail letter of February sixteenth enclosing offprints of your "Functional Logic" and proof of your "On the Theory of Deduction". Many thanks indeed. I will try to arrange that you get back the proofs as requested.

Very sincerely yours,
Alonzo Church

[a] This refers to the paragraph beginning with "These characterizing" in Popper (1948a, p. 182, this volume, p. 177).

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Chapter 25
Popper's Correspondence with Kalman Joseph Cohen

Karl R. Popper and Kalman J. Cohen

Abstract Kalman Joseph Cohen (1931–2010) was an American economist. Before his Ph.D. in economics at the Carnegie Institute of Technology (cf. Reed Magazine, 2011), he completed a B.Litt. in mathematical logic at Oxford University, which was supervised by Popper. In Cohen (1953b) he investigates various deductive systems, in particular sequent calculi, for logics which are dual to intuitionistic logic. Although a precursor to more modern treatments of dual-intuitionistic logics, it has not been recognized or discussed much in the literature (cf. Kapsner, Miller, and Dyckhoff, 2014, and this volume, Chapter 1, § 2). The correspondence is notable for Popper's semantic explanation of dual consequence, that is, of $a/(b_1, \ldots, b_m)$. Popper writes that he intends to publish on this subject, which shows that as late as 1953 he was still working on these topics.

Editorial notes: The correspondence with Cohen is contained in KPS Box 285, Folder 6. We have omitted one undated short note by Cohen, which announces a future letter with more detailed remarks on some unspecified suggestions by Popper.

25.1 Cohen to Popper, 14 February 1953

Typescript, 1p.

The Queen's College. Oxford.
Professor K. R. Popper,
Fallowfield, Manor Road, PENN, Bucks.
14th February 1953.

Dear Professor Popper,

By now I hope that you have fully recovered from your recent illness. Would it be convenient for you if I came to see you at your home on Friday, February 20th? To make the trip worthwhile, I would want to be able to speak with you for a longer time

D. Binder et al. (eds.), *The Logical Writings of Karl Popper*,
Trends in Logic 58, https://doi.org/10.1007/978-3-030-94926-6_25

than I did on my last visit. There is not very much choice in when I come and go, since there is bus or coach service from Oxford to High Wycombe only every two hours. I would like to come Friday on the coach which gets to High Wycombe at 1.14 p.m., which means hat I would get to your house by 2.0 p.m. Then I would have to return on the coach which leaves High Wycombe at 5.39 p.m. Would you please write to me and let me know if these times would fit in with your plans?

I am reading your article "On the Theory of Deduction I and II" of which you gave me a reprint. I shall try also to read the other three articles of yours referred to therein, before I see you on Friday. There are some questions which come to my mind from reading your article.

It would be most helpful if before Friday you could read the article by Jan Łukasiewicz, "On the Intuitionistic Theory of Deduction," *Proceedings Koninklijke Nederlandse Akademie van Wetenschappen, Series A,* vol. 55 (1952), pp. 202–212. I am quite sure that Łukasiewicz's result is not the same as Gödel's old discovery, and that it is not something trivial, but that it is of profound significance.[a]

Yours sincerely,
Kalman Joseph Cohen

25.2 Cohen to Popper, 19 February 1953

Typescript, 1p.

The Queen's College. Oxford.

Professor K. R. Popper,
Fallowfield, Manor Road, Penn, Bucks.

19th February 1953.

Dear Professor Popper,

I am very sorry to learn that you have come down with pneumonia. You say that you are already recovering. I hope that it is a rapid and permanent recovery you are making.

Unless I hear that you shall still not be well enough, I shall come to your house to see you on Friday, February 27th. I shall come on the coach which gets to High Wycombe at 1.14 p.m., and proceed to your home by local bus. It is necessary that I return to Oxford on the coach which leaves High Wycombe at 5.39 p.m.

I have completed, in rough form, the proof that classical restricted predicate calculus and number theory are proper subparts of intuitionistic restricted predicate calculus and number theory. Gödel has shown that this is true for a *weakened interpretation* of classical logic and number theory, but that is very different from showing this for classical logic and number theory in its original, uninterpreted form. If I can complete it in time, I shall send you a complete copy of the proof I've

[a] Cohen published the paper Cohen (1953a) on this observation, which was communicated by Brouwer.

discovered. It is rather lengthy, so it will take me some time to write it up in final, perfect form.

Yours sincerely,
Kalman Joseph Cohen

25.3 Cohen to Popper, 1 March 1953

Typescript, 1p.

The Queen's College. Oxford.

Professor K. R. Popper,
Fallowfield, Manor Road, Penn, Bucks.

1st March 1953.

Dear Professor Popper,

Sorry to hear that your illness is continuing. I hope that you soon will be better, and completely recovered. Would you please let me know if it will be somewhat convenient for me to come see you at your home on Friday, March 6th. If I come on Friday, I would arrive at the previously arranged time: arriving in High Wycombe at 1.14 p.m., and then I must leave High Wycombe on the 5.39 coach.

It may not be possible for me to send you in advance my proof that classical restricted predicate calculus is a subpart of intuitionistic restricted predicate calculus and that classical number theory is a subpart of intuitionistic number theory. This proof is based on Łukasiewicz's result for propositional calculus, and it is an extension of his method. There have been several other things that I have had to do during the past week, so that I have been unable to rewrite this proof in presentable form as yet.

If I can't send this to you in advance, it can wait for us to discuss it at our next meeting, which will be, I hope, on Friday.

Yours sincerely,
K. J. Cohen

25.4 Cohen to Popper, 7 May 1953

Typescript, 1p.

The Queen's College. Oxford.

Professor K. R. Popper,
Fallowfield, Manor Road, Penn, Bucks.

7th May 1953.

Dear Professor Popper,

Since I last wrote to you, I have been developing a formal system of the restricted predicate calculus, named the "dualintuitionistic" restricted predicate calculus, which is precisely dual to the intuitionistic restricted predicate calculus. This system of logic embodies the properties of the "m-negation" which you introduced in your paper "On the theory of Deduction" which appeared in vol. 51 (1948) of *Proc. Kon. Ned. Akad. v. Wet.* It has been necessary for me to introduce the anticonditional as a primitive functor into the dualintuitionistic restricted predicate calculus, since I find that it cannot be defined in terms of the remaining logical functors which I employ. I have found it most convenient to introduce the dualintuitionistic restricted predicate calculus by means of Gentzen-type inferential systems; later I hope to find a Hilbert-type postulational system for the dualintuitionistic restricted predicate calculus. I have been able to prove Gentzen's Hauptsatz (called by Kleene the "normal form theorem" and called by Curry the "elimination theorem") for my dualintuitionistic systems; hence I have established a decision procedure for the dualintuitionistic propositional calculus.

Although you briefly discuss the anticonditional in your above-mentioned paper, I have the impression that this is not the first appearance in the literature of logic. If you could tell me any references to the anticonditional which you know of, you could save me some time and trouble.

Have you had an opportunity yet to read the paper which I sent to you two and one-half weeks ago? If you would like me to come to see you at your home, I could manage to come on any day next week between 12th May and 18th May inclusive. If you do prefer to have me come to see you, rather than your writing to me, please let me know on which of these days it would be most convenient for my coming to your home.

Yours sincerely,
Kalman Joseph Cohen

25.5 Cohen to Popper, 26 May 1953

Typescript with handwritten postscript, 2p.

The Queen's College. Oxford.

Professor K. R. Popper,
Fallowfield, Manor Road, Penn, Bucks.

26th May 1953.

Dear Professor Popper,

At long last I have come to the realization that your system of *derivational logic* is a distinct improvement over the traditional Hilbert-type formalizations of logic. I have been utterly unsuccesful in all my attempts to devise a Hilbert-type formalization of dualintuitionistic logic which will be equivalent to the Gentzen type formalization of dualintuitionistic logic I have developed. I think that Hilbert-type systems are useful only when we are concerned with whether or not certain logical formulas are provable or demonstrable; they do not seem to be suitable when we are interested in knowing whether or not a logical formula is refutable or directly false or something similar.

In my thesis, I use the notation "$\Delta \Vdash \Theta$" to represent what Gentzen calls a "Sequenz" and what Kleene calls a "sequent," where Δ and Θ are each finite sequences of zero or more formulas. I've attempted to set-up Hilbert-type systems equivalent to the Gentzen-type systems in the sense that whenever $\Vdash p$ is provable in the Gentzen-type system, then p is demonstrable in the corresponding Hilbert-type system, and whenever $q \Vdash$ is provable in the Gentzen-type system, then q is refutable in the corresponding Hilbert-type system; and conversely. The best that I am able to do is to devise Hilbert-type systems such that whenever $\Vdash p$ is provable in the Gentzen-type systems, then p is demonstrable in the corresponding Hilbert-type system. The trouble with this is that the Hilbert-type formalizations of classical logic and dualintuitionistic logic are the same, since the class of formulas demonstrable in dualintuitionistic logic is the same as the class of formulas demonstrable in classical logic.

In your paper "On the Theory of Deduction," *Proc. Kon. Ned. Akad. v. Wet.,* vol. 51 (1948),[b] you introduce *relative demonstrability* or *relative refutability*, denoted by

$$(a_1, \ldots, a_n) \Vdash (b_1, \ldots, b_m).$$

I do not think it should be difficult to show that this is the same concept as that
2 formalized in the sequents $\Delta \Vdash \Theta$ | which occur in my Gentzen-type systems. In order to find an interpretation for my Gentzen-type systems which will preserve the distinctness between classical logic and dual-intuitionistic logic, I should like to prove that: $p_1, \ldots, p_m \Vdash q_1, \ldots, q_m$ is provable in my Gentzen-type systems, if and only if, the formulas $q_1, \ldots, q_n$ are complementary relative to the demonstrability of all the statements $p_1, \ldots, p_m$ (in your own terminology).

First I must obtain a concise formalization of your systems of derivational logic. I shall try to do this myself, employing your idea of introducing the primitive constants t (truth) and f (falsehood) in order to eliminate the use of quantification in the metalanguage. It would be helpful if you would send me reprints of the three papers which you refer to as P_1, P_2 and P_3[c] in the above-mentioned paper of yours. Would you be good enough to do this, please? Do you have any more recent publications, embodying the improvements in your systems of derivational logic, about which you have spoken to me? If so, I would appreciate reprints or references to these also.

I do not fully understand how you operate with your "definitions" or "characterizing rules" for the formative or logical signs. What precisely do you mean by characterizing rules, and how are they operationally equivalent to definitions? Your forms of definitions or characterizing rules do not seem to be equivalent to the ordinary notion of definition, in which the definiens and definiendum are mutually replaceable in any context. For example, on page 182 of the above-mentioned paper of yours, you state that (3.81) is a characterizing rule for the conditional $a > b$. But

[b] Popper (1948a,c).

[c] Cf. Popper (1947c), Popper (1947b) and Popper (1947d), respectively.

how does (3.81) enable us to eliminate the occurence of $a > b$ from the statement $a, a > b, c \vdash d$? To my mind, a proper definition of $a > b$ should enable us to eliminate the occurrence of $a > b$ in the above statement. Similar difficulties occur with all your definitions of the formative signs.

I have not spoken to Dr. Waismann or Professor Ryle about the possibility of using my paper, "On the Relation Between the Classical Restricted Predicate Calculus and the Intuitionistic Restricted Predicate Calculus" as an entire B.Litt. thesis. I've decided to include as well my material on dualintuitionistic logic, which I am trying to complete.

Yours sincerely,
K. J. Cohen

Could you please send the reprints as soon as you can conveniently manage?

25.6 Cohen to Popper, 16 June 1953

Typescript, 2p.

The Queen's College. Oxford.
Professor K. R. Popper,
Fallowfield, Manor Road, Penn, Bucks.
16th June 1953.

Dear Professor Popper,

Thank you very much for returning the signed applications to me so promptly. The applications were handed in on time, and now it remains for me to hand in my completed thesis on time.

I have just finished writing and revising the whole of my thesis. Still ahead of me is the big job of typing the thesis in final form, with carbon copies. It should be bound and handed in to the Lit. Hum. Faculty within a week, or ten days at the latest. Since there is so little time, I am afraid that I have to begin right away to type the final copy.

My thesis will consist of two separate papers, *On the Relation between the Classical Restricted Predicate Calculus and the Intuitionistic Restricted Predicate Calculus* is the first paper, and *The Dualintuitionistic Restricted Predicate Calculus* is the second paper. You have already seen the first paper. At your suggestion, I have inserted some additional footnotes mentioning that some of the ideas were suggested to me by you in conversation, and some additional terminological footnotes. However I have not expanded this paper to include discussions of other possible translations which reduce the classical systems to the corresponding intuitionistic systems, nor have I proved that certain additional relations hold between the classical functors and the intuitionistic functors. This would take a lot of additional time, which I do not have at my disposal now.

Unfortunately you have not seen the whole of the second paper, which I have only very recently completed. It has turned out to be fairly lengthy. Its most interesting results are: (i) The class of *formulas* demonstrable in the dualintuitionistic

propositional calculus is identical with the class of formulas demonstrable in the classical propositional calculus. (ii) The class of *sequents* derivable in the dualintuitionistic propositional calculus is a proper subclass of the class of sequents derivable in the classical propositional calculus. (iii) The class of *formulas* refutable in the dualintuitionistic calculus is a proper subclass of the class of formulas
2 refutable in the classical | propositional calculus. (iv) Both the class of formulas demonstrable in and the class of sequents derivable in the dualintuitionistic *restricted predicate* calculus are proper subclasses of the corresponding classes in the classical restricted predicate calculus.

In this second paper, there are several occasions when I make reference to either your published articles or conversations with you. This is so chiefly in terminological points, the anticonditional functor, the "minimum definable negation" functor which is actually the negation functor of dualintuitionistic logic, and the interpretation of sequents occurring in Gentzen-type systems in terms of your concept of "relative demonstrability or relative refutability." I mention that the idea of developing a system of logic dual to the intuitionistic logic was suggested to me by you. I've also had to mention that in reviews of your articles by McKinsey, Kleene, and Curry, errors in your systems of derivational logic have been pointed out.

To establish (iv) above, I was able to prove that the formula

$$(\exists x)p(x) \supset {\sim}(x){\sim}p(x)$$

is not demonstrable in the dualintuitionistic restricted predicate calculus. The rule of inference "If $p(x) \supset r$, and if r is a formula in which x does not occur as a free variable, then $(\exists x)p(x) \supset r$," is *not valid* in the dualintuitionistic restricted predicate calculus.

Do you think that I should come to see you before I hand in my thesis? If there were plenty of time, I of course would want to show you my work before I type it in final form. But unfortunately there is not very much time on my side, and you may not have very much time yourself to read my papers now. If you wish to see me, I could come to your home on Sunday or Monday. But if you don't think that this is necessary, I would prefer not to take the time away from my typing the final copy of the thesis. If I don't see you before I hand in the thesis, I shall send you the extra copy after they have been bound. At least I'll be able to get your judgement on the thesis as a whole before I face the examiners in the oral examination sometime in July.

Yours sincerely,
Kalman Joseph Cohen.

25.7 Popper to Cohen, 28 July 1953

Typescript with handwritten corrections, 2p.

July 28th, 1953.

Dear Mr. Cohen,

The problem of the interpretation of the dual-intuitionist calculus is really quite simple.

I interpret $a_1, \ldots, a_n / b_1, \ldots, b_m$ *normally* (remembering Tarski) to mean: "every model that satisfies *all* the premises $a_1, \ldots, a_n$, also satisfies at least *one* of the conclusions $b_1, \ldots, b_m$." Now in the *dual*-intuitionist calculus we can interpret

$$a / b_1, \ldots, b_m$$

non-normally by

"If there exists at least one model that satisfies the premise a, then there exists also a model that satisfies at least one of the conclusions $b_1, \ldots, b_m$."

(Clearly, in the *intuitionist* calculus, $a_1, \ldots, a_n / b$ can be interpreted by

"If the $a_1, \ldots, a_n$ are all satisfied by every model, i.e. if they are all *allgemeingültig*, then b is also satisfied, i.e., it is also *allgemeingültig*".)

Now this explains everything (e.g. the interpretation of the intuitionist calculus by "necessary" and "impossible") and it makes clear that the following interpretation of the dual-intuitionist calculus holds:

Interpret every single variable "a" to mean "a is possible" (or "a is satisfiable") or more precisely, interpret

$$a / b, c, \ldots$$

to mean "from the possibility of a (or, from the assumption that a is possible), we can deduce that at least one of the $b, c, \ldots$ is possible"; or "provided it is true that a is possible, it must be true, for logical reasons, that at least one of the $b, c, \ldots$ is possible".

2 | Interpret "a^m" to mean "the classical negation of a is satisfiable", i.e. "it is possible that a is false" or "a is possibly false"; "a^x" means "it is possible that a is necessary" or, "'a is necessary (or *allgemeingültig*)' is satisfiable".

Interpret the anticonditional "$a \ngtr b$" so that it is satisfiable if, and only if, a is satisfiable (possible) *and* b is not satisfiable (impossible); interpret "$a \wedge b$" to mean "the conjunction of a and b is satisfiable" or "it is possible that both a and b are true together"⟨Footnote: This also explains why you can no longer adopt the interpretation of a/b to mean "b is deducible if a is adjoined to the axioms"; i.e., why $a \wedge b / c \leftrightarrow a, b / c$ no longer works.⟩; interpret "$a \vee b$" as "the classical disjunction of a and b is satisfiable", i.e., "it is possible that a or b" (which is equivalent to "a is possible or b is possible").

Then the dual-intuitionistic calculus will be satisfied.

With the best wishes,

P.S. I consider publishing these results.

25.8 Popper to Cohen, 3 August 1953

Typescript, 2p.

Kalman Joseph Cohen
The Queen's College, Oxford.
August 3rd, 1953.

Dear Mr. Cohen,

Further to my letter on the interpretation of the intuitionist and dual-intuitionist calculi:

Since we have a rule of substitution for demonstrable formulae, only such formulae may be considered as "satisfiable" in the sense of my letter which do not lead, merely by substitution, to non-satisfiable formulae. That is to say, formulae like

$$\text{“}p\text{”} \qquad \text{“}p \supset q\text{”}$$

must not be considered as "satisfiable". Or, if we use "satisfiable" in the usual sense, then

$$a/b_1, \ldots, b_m$$

of the dual-intuitionist calculus must be interpreted "whatever substitution renders a satisfiable also renders at least one of the $b_1, \ldots, b_m$ satisfiable" (or, in other words, "there does not exist a substitution which renders all the $b_1, \ldots, b_m$ non-satisfiable").

Yours sincerely

P.S. I gather that two examiners have been found for you.

25.9 Cohen to Popper, 22 April 1954

Typescript, 2p.

Department of Mathematics,
Cornell University, Ithaca, New York.
Professor K. R. Popper,
Fallowfield, Manor Road, Penn, Bucks., England.
22nd April 1954.

Dear Professor Popper,

Thank you very much for the trouble you have taken in writing a letter of recommendation to the National Science Foundation. It must have been a very favorable report, for recently I was notified that I have been awarded A National Science Foundation Predoctoral Graduate Fellowship for the coming academic year. This is the best situation I could have for next year, if I stay at Cornell to study mathematics.

This first year at Cornell has been slightly disappointing to me, in that I am not finding advanced mathematics quite as stimulating and interesting as I had expected. While still at Oxford, I was undecided whether to do my graduate work in America in mathematics or in philosophy; either option would have been possible, since my major interest is logic. The considerations which led me to opt for mathematics were its greater practicality and the supposition that maths, if it is to be learned at all, must be learned when one is relatively young. The wisdom of this choice is now being questioned in my own mind. If I continue at Cornell next year, I may try to do less maths and more philosophy or perhaps economics. Mathematics is certainly a fine subject, but I am afraid that it is not the subject where I can be happiest and do my most effective work.

Since I last wrote to you, two other interesting possibilities have arisen. In fact, I learned of them only three days ago, after I had been awarded the National Science Foundation fellowship for next year. The first is a position as Teaching Intern in philosophy of science and general education in science at Wesleyan University (Middletown, Connecticut). This is a special opening made available under a grant to Wesleyan from the Fund for the Advancement of Education of the Ford Foundation. The post would be for one year only, and the work would consist in assisting in two courses: (1) *Philosophy of Science*, a year course which ranges widely in the philosophical aspects of the natural and social sciences, including treatment of classical as well as contemporary philosophers and scientists, and (2) *Physical Science*, a year course for students who have not majored in science, which treats of selected historical and also systematic issues in the physical sciences, and the social relations and functions of science. A friend of mine who is a professor at Wesleyan
2 suggested that I apply, and there is just a chance | that I might be selected from all the applicants for the position. I hope so, for I would certainly profit from and enjoy teaching these courses at Wesleyan next year.

The second possibility is to do graduate study in mathematical economics at Carnegie Institute of Technology (in Pittsburgh, Pennsylvania, where my parents live). I have always been rather interested in the social sciences. (If I hadn't done a research degree at Oxford, I would have read P. P. E.[d]) The uses of mathematics in the social sciences are increasing rapidly, and since I am becoming dissatisfied with pure mathematics, the applications of mathematics in the social sciences is a field to which I might profitably turn. This would both satisfy my inclinations and utilize my knowledge of mathematics. Carnegie Tech's graduate program in mathematical economics dovetails exactly both with my interests and with my previous training, and it seems to be tailor-made for me. It combines work in economics and the other social sciences with advanced mathematical and statistical techniques. You may have already heard of this program, for I believe that one of your students at L.S.E., a Mr. Dennis G. Price is now following it at Carnegie Tech. I've talked with Professor Franco Modigliani (he's also a friend of yours, isn't he?) at Tech about the possibility of my studying there, and although it is now rather late in the year, they might be able to arrange a fellowship or graduate assistantship for me so that I can go there next year.

What I shall do and where I shall be next year is thus still very uncertain. If only I were really happy studying mathematics, there would be no problem, for I would stay at Cornell on the N.S.F. fellowship. But time will tell, and I shall let you know my ultimate decision.

Logic itself is still a study which I enjoy very much. At last I am getting around to

[d] Philosophy, Politics & Economics

rewriting my Oxford B.Litt. thesis, and I shall try to publish its results in one or two articles. A great deal of shortening is required, and I would like to add some additional results to the second paper on the dualintuitionistic restricted predicate calculus. This year I have learned a great deal about many aspects of logic: recursive function theory, Church's systems of lambda-conversion, combinatory logics, effective computability, higher order functional calculi, and axiomatic set theory. Next year Professor Rosser will be back at Cornell, so I should be able to do even more with logic if I remain here.

For this summer I shall be working in the actuarial office of the Indianapolis (Indiana) Life Insurance Company. The work will probably be rather routine, but I have to earn some money somehow and this will enable me to see what the life of an actuary is like.

I hope that both you and your wife have been in good health, and that you have been getting a lot of work done this year. This is the season of the year when England is especially beautiful. But then spring is usually pleasant anywhere.

Cordially yours,
Kalman Joseph Cohen

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Chapter 26
Popper's Correspondence with Henry George Forder

Karl R. Popper and Henry George Forder

Abstract Henry George Forder (1889–1981) was a New Zealand mathematician working mainly on geometry. During Popper's time at Canterbury University College in Christchurch, New Zealand, Forder worked at Auckland University College. (For further biographical information on Forder cf. Butcher, 1985.) From their correspondence it appears that even though they worked in the same country, they mainly communicated by mail and did not meet in person, at least not regularly. Their correspondence is especially significant for understanding the genesis of Popper's novel views on logic. Their conversations revolve mainly around physics, the nature and foundation of mathematics, Boolean algebra and formal logic. They discuss results and systems by Bernays, Brouwer, Carnap, Gentzen, Gödel, Hilbert, Lewis, Russell and Tarski. Forder assists Popper in his research by making articles available to him that he did not have access to in Christchurch. Many of Popper's inferential definitions are inspired by analogous definitions of operations in Boolean algebras, and one can glimpse the germination of this idea in the correspondence between Popper and Forder, for example in the letter of 7 May 1943.

Editorial notes: The letters are contained in KPS Box 296, Folder 15. The handwriting of Forder is sometimes hard to read; we have used $\langle \ldots \rangle$ for problematic passages to indicate a possible reading. The incorrect spelling "Genzen" has been corrected to "Gentzen".

26.1 Forder to Popper, 20 February 1943

Handwritten, 2p.

Auckland University College
(University of New Zealand)
Auckland, C1

Feb 20 43

D. Binder et al. (eds.), *The Logical Writings of Karl Popper*,
Trends in Logic 58, https://doi.org/10.1007/978-3-030-94926-6_26

Dear Dr. Popper

Your protegé Offenberger[a] duly appeared. I ⟨guess I⟩ like him + expect to do profitable work with him. I fear that my lectures will be rather patchy + incoherent for I shall have little time for detailed preparation, and so will fall back on things I know upside down.

It is improbable that I shall ever visit Christchurch. Travelling has never appealed to me and is now more than ever distasteful. But I should certainly like a conversation on foundations. Intellectually I have always been isolated and so view all the problems from a peculiar individual angle – tho in the main Hilbert is my master. He got much deeper than Whitehead + Russell, but unfortunately in *this*[b] work he was a bad expositor.

Do you get the J. of Symbolic Logic? We do not. There ought to be a copy in the country.

2 | Some of the later American developments – Curry – Rosser λconversion + so on seems very repellant + to understand them one would have to do little else. Do you know anything of them?

Again on Gödel's work. Has a *genuine* theorem been found which can been shown to be undemonstrable? ⟨I⟩n some field where this would be unexpected such as integers, where the system of Axioms seems in some sense *complete*.

I fear I must stop. I hear a committee coming up the stairs.

With good wishes
Yours ⟨sincerely?⟩
H.G. Forder

26.2 Popper to Forder, 5 March 1943

Handwritten, 5p.

P.O. BOX 1026
Canterbury University College
Christchurch, C.1
New Zealand

March 5th 1943

Dear Professor Forder,

Many thanks for your letter. I was very pleased to get it. Excuse please my belated reply: I had a very full first week. I hope that Offenberger will live up to expectations.

You ask whether a genuine problem has been shown to be Goedelian. Yes or no: Yes if, as a follower of Hilbert, you recognize such problems as the consistency as genuinely mathematical; no if you have in mind problems of the Fermat and Goldbach type.

---

[a] Forder writes "Offenheimer" or "Oppenheimer". He means John (Hans) Offenberger (1920–1999). A photo with John Offenberger from 1991 exists in the Karl Popper Collection Klagenfurt.

[b] We were not able to resolve this reference.

2 Tarski has shown that if a certain problem t is Goedelian, | i.e. undecidable in the
system P_1, then it is always decidable in a system P_2 (of which P_1 is a part); and he
has even developed a method permitting to construct a proof of ⟨the sentence "t is
true"⟩[c] t or non-t in P_2 if a proof of the undecidability of t in P_1 is given. Thus if
Goldbach's conjecture were shown to be Goedelian, we could use this in order to
prove or disprove Goldbach's conjecture. (Tarski's method, of course, does not
3 permit us to construct a complete system P_2; P_2 will | contain other Goedelian
sentences decidable only in P_3, and so on.) Tarski has applied similar methods to
theorems which are not Goedelian; his method is capable of abbreviating proofs, and
he has also proved theorems concerning transfinite numbers by its help. He also has
given (in 1932) a proof of the consistency of "classical mathematics", using Goedel's
proof that the sentence maintaining this consistency is Goedelian. This proof was
4 later independently | rediscovered by Gentzen who has given it a different emphasis,
and it has become known under Gentzen's name.

The Tarski-Gentzen proof is interpreted by Gentzen as removing any doubt
concerning the consistency of mathematics. Tarski himself is slightly more sceptical;
since the system P_2 in which the proof takes place is logically stronger than P_1
whose consistency is proven, the proof need not prove anything; if P_2 is inconsistent,
anything could be proven. Gentzen's more optimistic opinion rests on the fact that he
has given the proof a form in which it appears to be intuitively sound, in a
5 Brouwerian sense. But this does not really improve | matters much since Goedel has
shown that the apparent caution of the intuitionists is fictitious, and that the
intuitionist system is not only a part of the classical system, but vice versa: it is
possible to express any classical theorem in the intuitionist system, only in a
somewhat altered terminology. (Roughly speaking: for any theorem t of the classical
system there exists in the intuitionistic system a theorem, non-non-t; the theorem that
the impossibility of t is impossible.)

Yours sincerely
K. R. Popper.

26.3 Forder to Popper, 30 April 1943

Handwritten, 4 numbered pages.

Auckland University College
(University of New Zealand)
Auckland, C1

April 30 1943

Dear Dr. Popper

Your letter of March ⟨5⟩[d] was most interesting + I should have followed it up long

[c] Inserted between lines.

[d] Forder wrote "March 4". Popper corrected the date in his letter of March 1943 (cf. § 26.2) from "4th" to "5th", we think, and Forder probably read "4th".

before but for the heavy work here: everlasting meetings and an enormous Stage I + so on.

I was familiar with the work you mentioned except the Tarski paper: could you give me a reference for that? The result that if a question is undecidable in P_1 a construction for its decidability is possible in a wider system is not unexpected: but it could be interesting to see its proof.

I said I was a disciple of Hilbert: But I doubt whether his other followers would regard me as orthodox. I do not accept the *creed* that maths is a meaningless play on symbols, and some do speak as if they accepted this *creed*. I take it to be a useful *method for some purposes*; and as a *method* it constitutes the great advance over Principia. I have little doubt that this was Hilbert's own view: otherwise he would hardly have constructed some of his proofs which Gordan described as "theological".[e]

Some consequences of my attitude are my views on proofs of consistency of arithmetic + on Mengenlehre[f]. I am *sure* arithmetic is selfconsistent – and I am *sure I know* this. Hence Gentzen's proof is to me merely an exercise if it is regarded merely as proving the consistency of arithmetic. It is of course of more value than
2 this | because we may hope that his method can be applied to some problems and because of the way it catalogues demonstration.

On Mengenlehre I should like to be as "naive" as possible. The discussions on MultAx long ago showed that slap-dash was risky + that some modification of the "naive" point of view was essential. But I still look on Axioms of Mengenlehre in the "inhaltlich"[g] way. If we assume MultAx we are delimiting the sets we allow, or consider.

> ⟨*Margin note*⟩
> Is there an English word for "*inhaltlich*"? "Meaningful" is not English and "meaning" is indefinite. What about "*expressive*" or "*purporting*"⟨?⟩

Now comes a puzzle. Is Cantor's continuum hypothesis ($\aleph_1$ = potency[h] of continuum; or $2^{\aleph_0} = \aleph_1$) true? I should like (naive) to regard this as a question of fact concerning things given: The continuum + certain ordered sets. Now Gödel in some recent work which I hope soon to study has shown that $2^{\aleph_0} = \aleph_1$ (and in fact $2^{\aleph_\alpha} = \aleph_{\alpha+1}$) is *consistent* with some such ordinary Ax. of sets (not including MultAx, which is proved also to be consistent with them). How are we to regard this result, looking at things as naively as possible? Assuming (what of course is not yet known) that $2^{\aleph_0} = \aleph_1$ cannot be deduced from the other Axioms, does it mean that if we assume it, we are further delimiting "sets" in a legitimate way?

3 | I don't like this: for our sets may be sets of points in a continuum $0^{\aleph_1}$ and then

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[e] Paul Gordan (1837–1912). For his dictum "This is not mathematics, this is theology!" cf. Noether (1914) and cp. McLarty (2012).

[f] set theory

[g] "contentual"; cf. Forder's following *margin note*.

[h] That is, the power.

once again does it not become a matter of fact, leading as Sierpinski has shown to all sorts of other matters of fact?[i]

Suppose it was shown that[j] Goldbach's conjecture was not inconsistent with Peano's Axs, would anyone be satisfied? In spite of Brouwer I am sure Goldbach is either true or false.

I welcomed Gödel's work on $2^{\aleph_0} = \aleph_1$ because it was an application to a genuine math. question. There is a risk that the new logic may be self⟨-⟩supporting – living to chewing its own tail. I found it to throw light on the proofs of ordinary mathematics.

Consider the theorems:

1. Every odd number is the difference of two squares.
2. Every number is the sum of four squares.
3. Every number except 23 ⟨and⟩ 239 is the sum of eight cubes.[k]

1. is trivial $sum = (n+1)^2 - n^2$
2. *needs* the principle of least number (Fermat's principle of descent)
3. apparently needs analysis

4 | I can see rather vaguely why (1) (2) differ: but the essential difference between (2) (3) – apart from present methods of proof – is much more elusive.

At one time I thought it likely that every genuine theorem (cp. those above) could be shown within the system of natural numbers. Gödel's work has at least thrown doubt on this – and further even "within the system" we apparently must distinguish theorems whose proof needs "sets of sets" from those which need "sets" only – or those which need only the narrow predicate calculus from those which need a wider one.

⟨*Left margin*⟩
Is there a formal logic of deduction, where instead of $p \supset q$ we have "from p we can prove q"? Lewis' *strict implication* + *Brouwer Heyting intuitive logic*[l] do not meet the case. Yet such should exist. Among the "paradoxes" of implication, it is surprising that Principia did not ⟨exclude?⟩ $p \supset q . \vee . q \supset p$.[m]

⟨*Right margin*⟩
Have you seen the recent English edition of Tarski?[n] I reviewed the German for the *Gazette*; and made some criticisms on his use of "equality sign".[o] Is there not room for a book on logic not for philosophers but for mathematicians?

But all this is terribly hazy + I hope the new logic will clear things up.

In geometry things are much clearer: in ordinary projective geometry: the simplest theorems are those which run up into n dimensions at once: the next simplest are those in which a distinction between odd + even dimensions is necessary: the hardest those which refer to a single dimension only, e.g. ⟨. . .⟩ or Hart

[i] Cp. Sierpinski sets.

[j] Forder writes "the".

[k] Dickson (1939).

[l] intuitionistic logic

[m] Cf. *5·13 in Whitehead and Russell (1925–1927).

[n] Tarski (1941).

[o] Forder (1938b).

circles, and it *seems* that these are finally consequences of simple combinatorial facts, e.g. there are three ways in which four ⟨...⟩ can ⟨...⟩.[p]

Yours sincerely
H.G. Forder

26.4 Popper to Forder, 7 May 1943

Typescript, 4 numbered pages.

May 7th, 1943

Dear Professor Forder,

Many thanks for your letter of April 30th. The number of problems you open up is too great for reacting to all of them. I agree with your general philosophical attitude/position in many ways, and I think that what may perhaps be termed as your "realism" in opposition to the orthodox Hilbertian "formalism" is largely justified by the later developments. Important as the purely "formalist" aspect is, it is only one aspect of the matter, and not the most important one. This, I believe, has come out clearly only through Tarski's so-called "Semantics", which considers a calculus (or axiom system) in relation to its "content" or "purport", i.e. to the subject matter formalized by it.

You describe yourself as a disciple of Hilbert, emphasizing the undoubtedly profound advance made by Hilbert beyond Principia Mathematica. I agree with your judgement. I may perhaps describe myself as a disciple of my friend Tarski whose methods, I believe, carry him nearly as far beyond Hilbert (in the direction indicated by you, i.e. towards the "inhaltlich", i.e. "purporting" or "contentual" or "designational" or "denotating" function of a formal calculus) as Hilbert has gone beyond Russell (Whitehead's contribution, I suspect, was not very important; I suspect this in view of what Whitehead himself says on this matter in his dreadful book "Process and Reality"[q]). I think indeed that from Aristotle to Russell, there is one line of development; with Hilbert, a new age begins; but it only begins. With Tarski, a new level is reached. This sounds perhaps slightly extravagant and over-enthusiastic, but I happen to believe that it is a just and well-considered appraisal of the situation.

I must add, however, that although I may describe myself as a disciple of Tarski, I am a bad pupil. Quite a number of the papers he has published are beyond me.

His theorem that every Goedelian sentence can be shown to be true (or false), was first published in his "Der Wahrheitsbegriff in den formalisierten Sprachen"[r]. It permits the transformation of any *proof* of the Goedelian character of a sentence into a proof of the truth (or falsity) of that sentence. But this method seems to yield more.

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[p] Undecipherable. The following combinatorial fact is probably meant: There are three ways to partition four things in pairs.

[q] Whitehead (1929).

[r] Tarski (1935a).

It shows that Goedel's proof is only valid for systems (of a certain degree of wealth) which employ the ordinary methods of proof or deduction. If essentially stronger methods of proof (non-finitist methods) are employed, and especially methods which admit the drawing of conclusions from *infinite* classes of premises, then Goedel's result does not hold.

In view of this situation, I sometimes wonder whether one could not sum up the Goedelian situation as follows:

"If the wealth of the means of expression of a system is not matched by correspondingly powerful methods of deduction, then the system is defective or Goedelian (i.e. contains Goedelian sentences); to every increase in the wealth of the means of expression of a system exists, however, a method of deduction to match, by whose employment the defectiveness can be avoided."

2 I do not know whether Tarski and Goedel would agree with this | formula, but I think it roughly characterizes the situation; and it removes, I think, the somewhat unpleasant taste that the Goedelian situation is liable to produce.

Concerning the "infinitist" methods of deduction, the decisive step was again made by Tarski, in his paper "Ueber den Begriff der logischen Folgerung"[s], read at the Paris Congress in 1936 (a paper in whose rendering in German I helped him); but a certain step in this direction had been made by him before, in 1928 and 1930, and by Carnap in 1932. Tarski's "infinite" concept of a "logische Folgerung" is essentially semantic.

But even apart from Tarski's semantic theory, the situation is very simple indeed. The Goedelian sentences have the form $(x)G(x)$, such that we can prove $G(1)$, $G(2)$, etc. for *any* given number, but not the generalized sentence for *all* numbers. This generalized sentence may be obtained, however, if we admit the infinitist method of deduction which permits us to use the infinite class of premises of the form "$G(n)$" in order to deduce $(x)G(x)$; at the same time, we have of course to employ our meta-mathematical knowledge that every single sentence of that infinite premise-class is deducible. (This can be expressed with the help of the method of arithmetization.)

So much on the Goedel situation.

In regard to the consistency of arithmetics, I feel not quite as sure as you do. Of course, I believe in the consistency of certain parts of arithmetics. But of the consistency of arithmetics including transfinite arithmetics, I would not say, as you do, that I am *sure*: although I do not *expect* new and unsuspected paradoxes to be discovered in this field, I do not think that such a discovery is impossible, in the sense that I am *not* sure that it will never happen. After all, we have suffered some shocks in this respect, and in a certain way, our intuition is unreliable. Even the old paradoxes (Russell; the Liar) have not yet been *solved*, i.e., in a sense which can completely restore our confidence in our logical intuition. Intuitively, "This proposition is false" is a proposition which maintains its own falsehood. Our means of solving these difficulties are *all* "formal", not "inhaltlich" or "designational": i.e. we construct an artificial system that prevents us from calling such a sentence a

[s] Tarski (1936b).

"sentence", and thus from facing unpleasant situations. And our "intuitive" methods of avoiding the problem are analogous: we know that the problem lands us in disaster, and therefore prefer to look away. But this does not enforce my confidence in the impossibility of finding other flaws.

But I am quite willing to concede that I *expect* arithmetics to be consistent, in other words, not yield unpleasant surprises any longer.

My attitude towards Gentzen, under these circumstances, is precisely the same as yours. (A fact that deserves consideration, since our basic attitude here is different.) I also think that it is "merely an exercise", which may be "of value" since it "catalogues demonstrations". (But we cannot get away from the fact that it employs methods which, if arithmetics were inconsistent, would be inconsistent as well. This was pointed out by Tarski in 1932, i.e. before Gentzen; and Tarski indeed outlined Gentzen's proof in his paper of 1932, in the "Mathematische Monatshefte".[t]

Regarding Cantor's continuum hypothesis, or rather $2^{\aleph_0} = \aleph_1$, and Goedel's
3 consistency theorem, there seem to be the following possibilities. |

(1) It may be inconsistent with the other axioms + the multiplicative Axiom.
(2) It may be consistent with the lot, and (2a) dependent from, or (2b) independent from them.

Assuming the latter is the case, it would only mean that the other axioms do not sufficiently characterize the "facts"; i.e., that the "facts" are not to a sufficient degree characterized by them. In other words, there would be two kinds of "facts", a Cantorean and a non-Cantorean kind of fact, which both fulfil the other axioms of set-theory, but of which one does not fulfil the axiom $2^{\aleph_0} = \aleph_1$; or, to put it in a geometrical way, there may be two continua, a Cantorean and a non-Cantorean, which would be perhaps not so very surprising, considering that geometrically-*intuitively* we can hardly say that we can distinguish between density and continuity, in spite of the, as it were, robustness of this distinction! I see from your letter that you "don't like this". But I think the consequences of the one or the other theory may help us one day to make up our mind to say that it was only this Cantorean (or perhaps the other, the non-Cantorean) continuity which corresponds to that thing we meant when we spoke of the "continuum". In this case, I suppose that all "realistic" objections would disappear. Or would they not?

Regarding Goldbach, and against Brouwer, I entirely agree with you; and I think indeed that the Brouwerian nightmare has turned out to be merely verbal. This again is an achievement of Tarski's semantics; and it is quite clear now that there is indeed the alternative that either Goldbach's conjecture is true, or there *exists* a natural number, in the naive sense of the word "exists", which falsifies it; that is to say, it *must* be possible to find such a number (by sufficiently long continued experiments, if not "constructively"), if Goldbach's theorem is false.

Regarding your question "Is there a formal logic of deduction where instead of $p \supset q$ we have 'from p we can prove q'?", my reply is "yes". If you do not make too great demands on the wealth of such a system (regarding the structural analysis of the proposition), we can even interpret the Boolean Algebra as such a system: Interpret

[t] Tarski (1933a). Popper's claim that Tarski's article anticipates Gentzen's proof is highly dubious.

the variables of the Boolean Algebra as variable *names* of sentences; interpret "$a + b$" as the descriptive *name* of the disjunction of the sentences designated by "a" and by "b"; and analogously "$a\ .\ b$" and the negation "$\overline{a}$"; then *every Boolean theorem* of the form "$\ldots \subset$ - - -" can be interpreted as "from ... follows - - -". That is to say, the Boolean sub-class or inclusion symbol "$\supset$" expresses logical implication or deducibility. If we furthermore enrich the Boolean symbolism by defining material implication:

"$a \supset b$" shall designate the same sentence as "$\overline{a} + b$",

then we get the difference between "$\supset$" and "$\subset$", i.e. between material implication and deducibility, very clearly.

We furthermore find that whenever a material implication equals *one* i.e. = 1, then a deducibility holds, i.e.: if $\ldots \supset$ - - - = 1, then

$$\ldots \subset \text{- - -}.$$

This shows that "= 1" means, in this interpretation, "is analytic" or "L-true" i.e. logically true. (If, in the ordinary propos. calculus "$\ldots \supset$ - - -" is analytic, then - - - is deducible from "...".) "= 1" corresponds to Russell's "$\vdash$" i.e. the assertion-sign. Thus every main implication symbol of an asserted proposition of PM[u] can be replaced by "$\subset$".

4 This is a problem in which I have been much interested in connection | with my probability theory. I call this interpretation of the Boolean algebra the "meta-propositional calculus", or the "*calculus of propositional names*". (I interpret the theory of probability as a metrisation of this calculus.)

Your are quite right about (1) $p \supset q .\vee. q \supset p$; it is indeed a "paradox of implication"; this can be seen from the fact that of

$$a \subset b; \quad b \subset a;$$

both may be false. I.e., the analogous Boolean formulae are not disjunctive. On the other hand, the Boolean formula

$$(a \supset b) + (b \supset a) = 1$$

is a theorem and therefore also

$$\text{(2)} \qquad \overline{a \supset b} \subset b \supset a.$$

This means that (2) is the Boolean formula which corresponds ⟨to⟩ (1) in terms of formal implication or deducibility; it tells us that from "$\overline{p \supset q}$" *we may deduce* "$q \supset p$" which is, of course, correct ("$\supset$" always being material implication), and which corresponds indeed to (1).

I have not seen the English edition of Tarski's book: I ordered it but I did not get it. Nor have I seen your review. Could you lend me both? Have you seen Carnap's "Introduction to Semantics"? He sent me a copy. It is an excellent book, but not an

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[u] A. N. Whitehead and B. Russell (1925–1927). *Principia Mathematica*. 2nd ed. Cambridge University Press.

Introduction. Didactically it is not good. But it is a milestone, since it is the first English publication on Tarski's semantics. So far as it deviates from Tarski, I do not feel able to say how valuable it is as an improvement.)

Many thanks for your letter.

Yours sincerely

P.S. Is it really impossible for you to come down to ChCh[v]? I should like so much to discuss some problems with you, – first of all, probability. It would be very nice if you could come in these vacations. Do you know Menger's Theory of Dimensions?

26.5 Forder to Popper, 12 June 1943

Handwritten, 3p.

Auckland University College
(University of New Zealand)
Auckland, C1

12/6/43

Dear Dr. Popper.

Many thanks indeed for your very interesting letter. It is clear that we speak the same language and so can understand each other. I did not make myself clear in one respect. I adopted a "cocksure" attitude about arithmetic – it was *tic* not *tics*. I am confident there can be no contradiction resulting from Peano's Axioms of integers. But I am *not* intuitively sure about the transfinite. Even along the ordinals $\omega, \omega^{\omega}, \omega^{\omega^{\omega}}, \ldots$ I soon loose an immediate intuition and play only with representing symbols.

I was at first surprised at the very high place you give Hilbert in the theory of logic; but I think on consideration there is a strong case for what you say. Between Aristotle + Hilbert I should feel inclined to put Peano as the great pioneer but I have not read Frege and so must hesitate. Tarski's paper in Studia Phil.[w] I have never seen. Such work of his as I have seen is technically very accomplished, but not more than developments of what had already been done.

I don't know whether you could lend me Tarski's paper – *not now* – I am too busy. But if it is less than 200 pages I could read it in a month when I am freer. In borrowing books I prefer as a matter of principle to fix or have fixed a time-limit to which I adhere. Any other course leads to trouble.

On probability I am a barbarian. I only speak of it because your works will be read by people as ignorant as I am.

2 | My view, for what it is worth, is that probability is merely an average. For dice throws this is obvious, for death-probability it is extrapolating an empirical average to the future. The difficulties about infinite sets are only technical. The word

[v] Christchurch

[w] Tarski (1935a).

"probable" in "It is probable the war will end this year" describes a state of mind of the speaker not a fact of the external world. So also for "It is probable the sun will rise tomorrow", "It is probable that Caesar crossed the Rubicon".

Probability in quantum mechanics seems like death probability or perhaps marriage probability (chance or free will). The laws connecting one such probability with another may be only apparent (as I think non quantum laws almost certainly are) and be the logical consequence of concealed structure.

But as I said, on these matters, I speak as the fool speaketh and may change my opinion any time.

⟨*Left margin*⟩
Gödel on Continuum[x] excellent. But ⟨as he?⟩ "confuses types" I had to do mental exercises for several days before I could read him. Your interpretation is I think correct. *If* the "power" of the continuum $\aleph$ is beyond reproach, yet $\aleph_1$ depends on what are "sets" and accordingly the equation $\aleph = \aleph_1$ means nothing from naive point of view. An axiomatic foundation of "sets" is necessary + this ought to be all sorts of shapes.

Tarski's logic. I have not got the English but the German edition. This I will send you under separate cover. I have not seen the new Carnap about Tarski's work. Frankly I did not like Carnap's main book (title forgotten – on language.) It seemed to me he had committed the crime of turning a method (Hilbert's) into a creed. If I have understood what you have said about Tarski, this should now be corrected.

A student of mine, Millar, very good, has joined you at Christchurch. He is interested in Math'l Logic, primarily in maths.

With good wishes.
Yours
H.G. Forder

26.6 Forder to Popper, 29 June 1943

Handwritten, 2p.

Auckland University College
(University of New Zealand)
Auckland, C1

29 June '43

Dear Dr. Popper.

Tarski safely received.

The review is in *Math. Gazette* July 1938[y] + in the same number there is one on *Russell's Principles*,[z] also by me. The first one (on Russell) does seem to give my point of view. The one on Tarski is short. Offprints of these are not sent to me: but I

[x] Gödel (1940).

[y] Forder (1938b).

[z] Forder (1938c).

think there are copies of Math. Gazette in your library or in Saddler's[aa] possession – if not, I will send my copy to you. If you happen to see Watson's article on geometry in July 1938[ab] (the number containing the reviews) you should also see the way I demolished him for good in December 1938[ac].

I ought to have thanked you for your information on Boole algebra + implication: I have studied your letter on that and other points with care. It appears that you have settled the matter from Boole-standpoint. My original question was prompted in another way: – Seeing that in formal logic it appears that $\neg p \vee q \,.\!\equiv\!.\, p \supset q$ is the source of all ill, can we not drop it and add *such axioms as are necessary* + so obtain a theory of deduction exempt from paradox? Among the immense number of sets of
2 Axioms in primitive logic | I thought there would be one at least which satisfies this condition. Yet if it were so why the detour of Lewis + Langford[ad]? I must add that I have never had time to consider the question.

I have asked the library to send you the Gödel Continuum tract[ae] which I am sure you will enjoy.

I am pretty sure I should be interested in your metrising of logic. As soon as the 3-way + n-way logics of the Poles were discovered the application to probability was open – Was it not already discussed in Vienna in the early 30's? *Now* will there not be an analogy between your metrising of ($\vee$ $\wedge$) and the old geometric metrising of descriptive geometry, ⟨introducing?⟩ + × from Desargues + Pascal Th.[af] On this side I can claim knowledge. There is less likely to be an analogy with the deeper question of metrising topology (Urysohn).[ag]

I made an application of the first doctrine to Milne's kinematical relativity about 3 years ago (Quart. Journal Maths Dec. 1939)[ah]. I am out of offprints, but Saddler has one). My interest in the probability would be technical rather than philosophical.

Could you send me Tarski's Wahrheit paper which you were so kind as to put at my disposal. I will keep it a month only from receipt.

If you ever want Herbrand's papers in the Polish journal or the fundamental papers on polish 3-way logic, our library has them.

With good wishes
Yours
H.G. Forder

[aa] William (Bill) Saddler; cf. Everitt (2016).

[ab] Watson (1938).

[ac] Forder (1938a).

[ad] Lewis and Langford (1932).

[ae] Gödel (1940).

[af] Theorem

[ag] Cf. Urysohn's Metrization Theorem.

[ah] Forder (1940).

26.7 Popper to Forder, 21 July 1943

Typescript, 1p.

P.O. BOX 1026
Canterbury University College
Christchurch, C.1
New Zealand

July 21st, 1943.

Dear Professor Forder,

Very many thanks for your letter of June 29th. I am sending Tarski by the same mail.

I have read your reviews of Russell and Tarski. With the first, I fully agree. I found it most interesting and indeed excellent. Regarding the second, your remarks on "=", I was very interested, but not quite satisfied. Similar comments on "=" have been made by Wittgenstein and Waismann. On the other hand, you will have seen that Goedel (Continuum Hypoth., p. 2)[ai] treats $X = Y$ like Tarski. The reason why I am not quite satisfied by your comments is this. Just as you write, in your letter, concerning probability: "My interest in probability would be technical rather than philosophical", I feel that the problems in connection with "=" must be formulated in a technical rather than in a philosophical way. I am just about to finish a paper on Boolean Algebra and extensionality. In this paper, I discuss some problems of "=" in a purely technical manner. I should be extremely grateful if you would go through this paper of mine before I send it to the "Journal".

I have not yet got hold of Prof. Saddler, and therefore not of your article on Milne's kinematic relativity.

Regarding your remarks on Lewis' strict implication, I do not quite understand yet. You call this system a "detour". But your own programme of "adding to the calc. of propos. such axioms as are necessary to obtain a theory of deduction exempt of paradox" seems to me to be precisely the programme which Lewis has realized. (Is the "detour" his introduction of modalities? But this seems to be unavoidable in a theory of deduction as conceived by your programme, since to say that p follows from q is the same as to say that it is logically *necessary* that $p \supset q$, i.e. that $\neg p \vee q$. Thus we get "necessary" and the other modalities.)

Very many thanks for sending Goedel. How long can I have it? I have had two somewhat superficial readings through it, and I enjoyed it. But I have not yet digested the last part, i.e. on Cantor's hypothesis. The other problem, the consistency proof of the axiom of choice is surprisingly simple in its main ideas.

Very many thanks,
Yours,

[ai] Gödel (1940).

26.8 Forder to Popper, 26 July or 2 or 9 August 1943

Handwritten, 2p.

Auckland University College
(University of New Zealand)
Auckland, C1

Monday.

Dear Dr. Popper

I found the Tarski awaiting me today at the college. I am just recovering from a most devastating attack of flu which rendered me completely imbecile. Very many thanks for the ⟨tract?⟩.

I adhere to my criticism of Tarski's truth ⟨work?⟩. But, as its terms show, I was not so presumptuous as to suppose I was telling him anything he did not understand as well as anyone living. It may be in fact that he had pondered the difficulties so much that he hesitated to ⟨print?⟩ them out in a strictly introductory treatise. The sign = when used purportingly seems to be either an alias or a christening as in

(1) Saul = Paul
(2) Senex = the oldest inhabitant of Auckland

Further research might show

J R Robinson = Senex.

On the other point – a formal logic without the "paradoxes", as I said, I have thought little. I don't like Lewis' modalities, but I am not even sure how to ask the question: If we ask for a logic in terms of $\neg\,.\vee.\to$ where $a \to b$ means "from a we can prove b" the question arises what sort of proof is contemplated: is it in terms of the Axioms
2 themselves? If so, we all know what special | care is necessary. I hope I shall be able some day to look into this but the duties here, everlasting meetings, huge classes and unending routine make it very hard to keep all my many interests going.

I should very much like to see your paper on Boolean Algebra when it is ready. I promise not to hold you up by keeping it too long.

Gödel is a library copy + they will remind you when to return it. But it will not be urgently needed here for some time so there is no objection to your keeping it a month or two to enable you to get its main gist.

I shall return your Tarski by the end of August. I think by that time I shall have got from it what I can.

With good wishes
Yours sincerely
H.G. Forder

26.9 Forder to Popper, between 21 July and 11 August 1943

Handwritten, 2p.

Auckland University College
(University of New Zealand)
Auckland, C1

Dear Dr. Popper

Further to my letter.[aj]

The remarks I threw out about my dissatisfaction with Lewis were due to memories of some reading a few years back. I have learned of some notes on the subject + enclose a copy. It is in no sense original research being based on Lewis' Book: a paper of Huntington's + two of Tsung Tao Chen.[ak] What I send is not a mere copy but a digested, *selfcontained* + I think improved version of their work. The numbers of the propositions that come from Lewis are the same as in his book. This facilitates reference, tho' that is quite unnecessary. You probably know the main thesis

I Lewis system is Boole algebra with $p < q$ written for $pq \equiv p$.

⟨*Left margin*⟩
By Lewis I mean *Lewis* + *Langmuir*[al]

Another curious point which I don't fully understand is this

Any two deductions of form $p < q$ are equivalent

We need only show if $p < q$ is a deduction

$$p < q \,.\!\equiv.\, U$$

i.e.

$$pq \equiv p :\equiv: U$$

2 | Now $pq \equiv p$ is a deduction . : We need only show

$$p \equiv p \,.\!\equiv.\, U$$

By 22

$$U \,.\!\equiv.\, pp < p$$
$$\langle\text{or?}\rangle\; U \,.\!\equiv.\, p < p$$

But

$$p < p \,.\!\equiv.\, p = p$$
$$.: U \,.\!\equiv.\, p \equiv p$$

Now the Axioms themselves are deductions and so it seems that Lewis axioms are all equivalent in their own presence.

[aj] Forder refers to the above letter § 26.8.

[ak] Forder means Tang Tsao-Chen. The papers are Tsao-Chen (1936, 1938).

[al] Forder presumably means Lewis and Langford (1932).

This seems to be what Tang Tsao Chen[am] asserts and the above is a concise version of his very tedious argument. He calls the result a paradox. It seems to depend on interpreting (as he says) $p < q$ as from *p we can deduce q*.

I don't feel too happy about it.

Yours
H.G. Forder

26.10 Popper to Forder, 11 August 1943

Typescript, 2p. There is also an incomplete handwritten draft, 3p.

P.O. BOX 1026
Canterbury University College
Christchurch, C.1
New Zealand

August 11th, 1943.

Dear Professor Forder,

Many thanks for your letter. I did not know Tsuny Tao Chen's(?)[an] work, to which you refer; but I do not think that the equivalence of Lewis' axioms in their own presence is alarming.

If we call the *class* of all conclusions which may be drawn from a sentence X the "*content of X*", and if we call its complement the "anticontent of X" or "$a(X)$", then we can construct a class-calculus with these anti-contents, and the following holds: if from X, we can deduce Y, then $a(X) \subset a(Y)$; furthermore, we have such formulae as: $a(X \vee Y) = a(X) + a(Y)$; $a(XY) = a(X)a(Y)$, $a(X \supset Y) = a(\overline{X}) + a(Y) = \overline{a(X)} + a(Y)$, $a(\overline{X}) = \overline{a(X)} = 1 - a(X)$, etc. and furthermore:

If, and only if, $a(X \supset Y) = 1$, then
$a(X) \subset a(Y)$

If we now write instead of "$a(X)$", say, p, and instead of "$\subset$", say, "⥽", then we obtain Lewis' system which, as you say in your letter, is Boolean Algebra with p ⥽ q for $pq = p$,[ao] since referring to this matter in an earlier letter in which I used Boolean Algebra: *The calculus of deductions can be constructed as a Boolean class calculus of anti-contents.*

Now to the problem of the equivalence of the axioms.

In the calculus of anti-contents, $a(X) = 0$ if and only if X is contradictory, from which it follows $a(X) \subset a(Y)$ for any contradiction X and any Y; if further follows that if X is contradictory, $a(\overline{X}) = 1$, i.e. the negation of a contradiction, which we may call "analytic", has an anti-content = 1. If X is analytic, then for any Y,

[am] Tang Tsao-Chen

[an] Tang Tsao-Chen

[ao] Forder writes $p < q$ instead of p ⥽ q.

$$a(Y) \subset a(X),$$

i.e., an analytic sentence follows from any sentence, and thus *all analytic sentences are equivalent.* But the axioms must be analytic; thus they are equivalent. (This is true for *all* logical axiom systems, not only for Lewis'.)

I have a very great request. I urgently need Huntington's paper of 1904 (Transact. American Math. Soc. Vol. 5, pp. 288–309)[ap] because I need his treatment of his second set of postulates, in which *de facto* inclusion is taken as primitive and $a = b$ is introduced (by a postulate) as mutual inclusion. Have you a re-print, or is perhaps the whole volume in your library? I would also like to have, from the same Journal, Huntington's papers in vol. 35 (1933)[aq], A. Church's paper in vol. 27 (1925)[ar], and Bernstein's paper in vol. 17 (1916)[as]. If you have any or all of these, could you send them? The most important is Huntington 1904. I know I am asking a lot of you, but I
2 want to look at | them before finishing my paper on "Extensionality in a Rudimentary Boolean Algebra".

I am extremely grateful for your readiness to look through this paper. In fact, I am worried a great deal about the paper, since it is too long as it stands, considering its very limited importance, and I shall very greatly appreciate your comments on it.

26.11 Forder to Popper, 16 August 1943

Handwritten, 2p.

Auckland University College
(University of New Zealand)
Auckland, C1

Aug 16 '43

Dear Dr. Popper,

Many thanks for your note. Your explanation of what the chinaman[at] described as a "paradox" is complete and removes all the mystery. Roughly speaking as Lewis' system is "equivalent" to Boole and the latter is "complete" all "true" propositions are equivalent.

The term has ended except for a Council meeting this afternoon. Some papers to set and reports to write, and it leaves me very tired and worn out. I am sending you registered Trans AMS 35/1 27/3.[au] These contain a paper of Huntington + one of Church. Our library contains no Trans AMS. In fact when I came here it contained

[ap] Huntington (1904).

[aq] Huntington (1933a,b).

[ar] Church (1925).

[as] Bernstein (1916).

[at] Tang Tsao-Chen

[au] Issues 35.1 and 27.3 of *Transactions of the Mathematical Society*.

no Journals
no collected works
no Books in German (⟨maths?⟩)
1 Book in French
And it had been established 50 years.

2 | I send some notes[av] on Huntington's 2nd set (1904)[aw] which you mention, also Bernstein.[ax] He simplified his set in 1933[ay] + what I send is a ⟨. . .⟩ version of the two papers. Also Huntington has been through my mill.

As to Huntington 35/1. He subsequently showed an Axiom was redundant. I enclose a note on that also.

I lost my fountain pen last year. Today I bought one 37/6.[az] I am working with it. It is rotten.

Yours sincerely
H.G. Forder

P.S. Shall we say *one month* for this loan.

H.G.F.

Have you met my old student Millar now at XChurch. I think you would find him interested.

26.12 Forder to Popper, 1 September 1943

Handwritten, 2p.

Auckland University College
(University of New Zealand)
Auckland, C1

Sept 1 43

Dear Dr. Popper

I am sending along the *Tarski* today with many thanks. I have been through it but I very much doubt that I have got from it anything ⟨like?⟩ all that it contains. I like more symbols. I see the philosophy behind them more easily than when it is explained without them.

A note has reached me that Hilbert is dead: he has done little now for 10 years but he was the leading Math. since Poincaré! I should say Weyl succeeds him with J. Hadamard as second.

Hilbert (with Dedekind) is one of the founders of modern algebra: the movement in Number Th. this century (apart from the "analytic" theory) is due to him entirely.

[av] We do not reproduce these notes which are on Boolean algebra.

[aw] Huntington (1904).

[ax] Benjamin Abram Bernstein (1881–1964).

[ay] Huntington (1933a).

[az] That is, at the price of 37 shillings and 6 pence in New Zealand currency.

Integral equations after Fredholm[ba] are his creation + he gave them new directions. 2 | His work on Dirichlet's principle in Calculus of Variations marked an epoch. In Foundations of Geometry he did not merely catalogue Axioms + Dems[bb] like the Italians but used the method for creative work. Other ⟨items?⟩ like Waring's Theorem, conformal reformulations, Definite forms, Einstein's theory + foundations of physics owe outstanding work to him.

All this apart from his Logic.

I have just reread an algebraical paper of Hilbert's. He does not weave garlands of flowers but shows gigantic rude + remorseless strength as he wrings out his theorems from the most unpromising material. Very unlike Poincaré + Weyl.

Yours
H.G. Forder

26.13 Forder to Popper, 13 September 1943

Handwritten, 1p.

Auckland University College
(University of New Zealand)
Auckland, C1

Sept 13 '43

Dear Dr. Popper

Trans AMS 27/3 safely received. Many thanks. Certainly keep the other for the time you mention.

Yes! You are right about Hilbert + Q.M.[bc] I forgot that side, – but I remember in reading Schrödinger's 1926 pioneer papers ⟨notions?⟩ how he had been influenced by the Hilbert-Courant Math. Physics Vol. I.[bd] In fact Schr. gave the impression that that book put him on the track. Heisenberg's approach was different: it was via Bohr's Korrespondenz Prinzip.[be]

There is some recompense for living in these days: I shall never forget the adventure of following Schr. Heisenberg + Dirac in 1925–1928. It is hard to separate these but perhaps Dirac is the greatest of them all. His thought takes my breath away. Nor should it ever be forgotten that Weyl had completely clear ideas when the Cambridge workers – Dirac excepted – were ⟨just⟩ muddled.

Good wishes
Yours H.G. Forder

[ba] Erik Ivar Fredholm (1866–1927).

[bb] Demonstrations

[bc] Quantum Mechanics

[bd] Courant and Hilbert (1924).

[be] correspondence principle

26.14 Forder to Popper, 21 October 1943

Handwritten, 2p.

Auckland
Oct 21 /43

Dear Dr. Popper

Many thanks for the Trans AMS safely received.

I did not recognize the results you sent me but as you know, a great deal of work has been done recently in "lattices" partially ordered sets. There is still a lot to do – of that I am sure.

My life now is rather full with routine. At present Stage I Examining. In Nov. Stage II. In Dec matric. I do however even in these times try to set apart a period
2 each day for better things: and when you send your paper | I will deal with it as quickly as I can. It may ⟨induce?⟩ me 15 ⟨. . .⟩ results on "lattices" which at present are rather messy.

With good wishes
Yours
H.G. Forder
(Please call me Forder)

26.15 Forder to Popper, 26 October 1943

Handwritten, 1p.

Auckland
Oct 26.

Dear Popper,

Many thanks for your paper. I have already made a ⟨hole?⟩ through it. There will be few, if any, corrections to the English needed.

I do not follow $1 \cdot 22$⟨;?⟩ $\leftrightarrow$ seems not proved. $1 \cdot 21$ a/b c/a gave

$$a < c' + c \quad (\text{not } c + c')$$

If this is just, the matter can be remedied by putting $1 \cdot 27$ earlier.

p. 7 you speak of "our first set". Do you mean "first"?

$1 \cdot 32$ is $1 \cdot 1\beta$ also assumed?

$1 \cdot 51$ should be $a < b' \rightarrow b < a'$.

More later.

I am full ⟨of?⟩ with Examening.

H.G. Forder

26.16 Popper to Forder, 4 November 1943

Typescript, 2p.

P.O. BOX 1026
Canterbury University College
Christchurch, C.1
New Zealand

November 4th, 1943.

Dear Forder,

I am very ashamed that there is some *bad* mistake, inexcusable indeed, in section 1 of my paper. I found it when checking the proofs of consistency. My present *postulate* 1.7^0 *is in order*, but 1.7 (in both the verbal form of p. 4 and the symbolic form on p. 12) is not even fulfilled by

$$\begin{array}{c|cc|c} & 0 & 1 & \\ \hline 0 & 0 & 1 & 1 \\ 1 & 1 & 1 & 0 \end{array}$$

as $a = e = 1$ and $b = c = 0$ shows.

1.7 must read:

1.7. Post. If a is in K then there is a b in K such that for any c, d, e, and f, in K, if $a < e$ and $b < e$ and $c < e$, and if, furthermore $b < f$ and $c < f$ together imply $e < f$, then $d < e$ and $a < c$.

or in symbols:

$$(x_1)(Ex_2)(x_3)(x_4)(x_5)(x_6)\{K(x_1) \to (K(x_2)\ \&\ [\{K(x_3)\ \&\ K(x_4)\ \&\ K(x_5)\& \\ \&\ K(x_6)\ \&\ x_1 < x_5\ \&\ x_2 < x_5\ \&\ x_3 < x_5\ \&\ ((x_2 < x_6\ \&\ x_3 < x_6) \to x_5 < x_6)\} \to \\ \to (x_4 < x_5\ \&\ x_1 < x_3)])\}.$$

I am terribly sorry that by sending you the stuff before having checked the proofs of consistency etc. in the last section (section 5), I may have caused you unnecessary labour; I know how it is if one gets something like this for checking: one does not wish to assume that the author is an ass (even though it is true in this case) and therefore wrecks one's brains in an attempt to understand what is in fact unintelligible.

Of course, pp. 2/3 have to be re-written (and p. 12 as well); also Definition 1.16. I
2 believe, however, that everything is in order now, and I shall | send you the rest in a day or two, hoping and praying that you won't be too disgusted with me, and not give me and my paper up altogether.

26.17 Popper to Forder, 13 November 1943

Typescript, 2p.

P.O. BOX 1026
Canterbury University College
Christchurch, C.1
New Zealand

November 13th, 1943.

Dear Forder,

After many delays, I can send you now the whole thing. In checking the last section (proofs of independence), I found several mistakes which were not difficult to repair, but which had various repercussions throughout sections 3 and 4; so it was necessary to check the lot again and again, and especially the "cross-word-puzzles" of section 5. But I think that everything is now in reasonable order; and I very much hope that there aren't any such shameful mistakes left as the one in connection with the first version of the first set, about which I wrote you an air-mail letter.

In order to make a similar blunder impossible I have now stated the proof of the equivalence of 1.1^0 & 1.17^0 with 1.1 & 1.7 (the latter is newly formulated) in sufficient detail; so I hope at least. 1.7 as it appears now is neither the old one nor the one I sent you in my air mail letter; even the latter had a mistake in it (the operator "x_6" must be confined to a more narrow range); I wrote the letter a bit hastely with the intention to save you further unnecessary trouble with this version. Besides, I have now simplified 1.7, so that only five variables are needed, as in the original (false) version. You will see all that from pp. 3ff. I am sending you the whole thing, including pp. 6 to 11, which are not re-typed, but the same as I sent you originally, only with a few ink-corrections, mainly re-numbering of the theorems.

If I may now say something about the whole paper[bf]: it is all very unfortunate, and I am rather sick of it. It is not quite unimportant, and in some of its detail perhaps as interesting as Huntington's papers; but I feel that it is, all in all, much ado about nothing. And I very much doubt whether anybody will print it.

If you could suggest some cuts, I should be very grateful. In fact, I have tried to compress it as much as I could.

The things which are comparatively the most interesting are, I think, (1) the fact that one can construct Boolean algebra sets that are not clumsy *without either a D-rule or a* $\vee$*-rule* (these are defined on and before p. 18). I *think* at least that this is new. (2) The concurrence set (pp. 37–42 and pp. 44-45). (3) Certain remarks in section 5.

If I now may ask you especially for two favours:

(a) On p. 50 I refer, in the line before I.5, to Huntington's postulates 4.6 & 5.5. Not it is possible that this reference to 5.5 is mistaken. Could you check this? I mean whether my model satisfies Huntington's Fifth Set except 5.5? (I have made no copy of this set, unfortunately.)

(b) On p. 55, Remark 3, I describe a system of Boolean algebra without $a - b + b'$. Have you any idea how this system is related to Boolean "rings"?

I may perhaps tell you that this whole paper developed in this way: some of the
2 sets 3 to 12 are by-products of certain probability | work, and so are parts of

[bf] This refers to one of Popper's papers on Boolean algebra.

section 5. Section 1 (and the second set) was added in the last moment, when I noticed that the other results allowed this application. (This occurred to me only just before I wrote you my letter about my results; the other parts of the paper, including simple independence etc., were long ready by then.)

I feel very strongly how much I am intruding upon your time, and I beg you most earnestly to accept my apologies for the length of the paper and the unnecessary complications. Please, do not be hurried with reading the paper, and do not feel obliged to read it at all if it bores you. And many, many thanks for the trouble you have already taken.

26.18 Forder to Popper, 3rd week of November 1943

Handwritten, 4p. Popper replies to this letter on November 23rd, 1943.

27 Armadale
Armadale Road
Remuera
Auckland SE2

⟨*Left upper corner with pencil:*⟩ Just picked up at college your second part HGF

Dear Popper

a terrific spate of Examining has prevented me from answering you before. I now have a break from this – though college affairs still hold me – until the avalanche of Matric ⟨demands?⟩ early in December.

I was not held up by your error: I took the statement for the time being as read. The word "any" in English is very indefinite.

If x is *any* member of K then $f(x)$ $\qquad (x)x \in K \,.\supset.\, f(x)$

If x is in K and $f(x)$ is true for *any* a in K then . . .

might be[bg]

$$x \in K : (\exists a)\, a \in K \,.\, f(a) :\supset: \ldots$$

or

$$x \in K : (a)a \in K \,.\supset.\, f(a) :\supset: \ldots$$

For this reason I was not sure which you meant.

"Dual *of*" usually: sometimes "dual *to*" might be used.

"The dual *of* Pascal's theorem is . . ."

"The dual theorem of Pascal's Th."

or "⟨The dual theorem⟩ to ⟨Pascal's Th.⟩"

I prefer "*of*".

Huntington Set 3 from my notes. If elements indicated are in class

(1) $a + a = a$

[bg] We add ". . ." for the omitted succedents of the following two implications.

(2) $a + b = b + a$

(3) $(a + b) + c = a + (b + c)$

(4) $(\exists\wedge)\,(a) a + \wedge = a$

(5) $(\exists\vee)\,(a) \vee + a = \vee$

(6) $a, b \in K \,.\!\supset\!.\, a + b \in K.$

(7) If $\wedge$, $\vee$ exist + are unique then
$a \in K \,.\!\supset\!.\, (\exists\overline{a})\, x + a = a \,.\, x + \overline{a} = \overline{a} \,.\!\supset\!.\, x = \wedge \qquad a + \overline{a} = \vee.$

(8) If 1 4 5 7 hold of $a + \overline{b} \neq \overline{b}$ then $(\exists x)\, x \neq \wedge .\, a + x = a \,.\, b + x = b$

(9) at least 2 distinct elements

2 | I have been thru the technical part again after ⟨1.12?⟩ Def. I recommend you put reflexive + transitive law both of which are easily shown there

$$a = a \text{ since } a < a$$

You never use so far as I see

$$f(a) \,.\, a = b \,.\!\supset\!.\, f(b)$$

Indeed it would be unfeasible to prove at this stage: The temptation to use it has to be resisted. I was wrong about "*first*". I did not know you were considering other sets.

⟨*Left margin: twice*⟩ *pq*

$1 \cdot 70\gamma$ How do you prove it? What is $1 \cdot 5$?
I should like $1 \cdot 7'$ before the def $1 \cdot 18$ which as it stands seems harsh – an unconnected hypothesis is ⟨needed?⟩.
$1 \cdot 73$ Is not transitivity of = used?
Maintain put *assert* (Americans often use maintain)

General criticism
I am sure that you will wish me to be quite frank. I myself have derived great benefit from frank criticism of my friends and correspondents.
(1) The work up to p. 11 is quite an advance: I have not seen it elsewhere: and I think it most likely it is new. If I were referee I should recommend acceptance.
(2) But I do not see the point of translating it into H.B.[bh] functional calculus: and I deprecate the change of letters to $x_1, x_2, \ldots$. It is *obvious* that the translation is possible and for that reason alone is hardly worth doing. Moreover you make no use of the translation in the metamaths ⟨discussion⟩. It is right that you should compare
3 your work with | Tarski's reduction: Why not turn his H.B. notation into the earlier much more readable one you first adopt. I think the logical notation should only be used when *either* it makes the work *easier* to read + follow *or* it is basis of a metamathematical discussion. Your paper, you tell me, is long. It will, I feel sure, attract more attention if it is pruned as I suggest.

[bh] Hilbert-Bernays

I looked again at Schrödinger's Cambridge paper.[bi] I have never been able to accept the usual interpretation of Qu. Mechanics + his work seems to be a reductio ad absurdum. Things happen as they happen, perceived or not, observed or not. There is an external world in which ⟨parts?⟩ definite ⟨"definite", not necessarily "causally determined"⟩ things occur. This is my old-fashioned view. The Schr. trouble starts right at the beginning: When a photon is split into two beams it is in both beams until an observation when it jumps into one. Thus the other beam is affected. This is the usual orthodox statement. I myself believe that it is in *one or* the other + which it is in is discovered and not produced by the observation tho it may be produced by the material circumstances. Which beam it is in may be subject only to *probability* laws, but I make no sense of probability in abstracto: in spite of all difficulties I see in probability merely an "average" ⟨=?⟩ "frequency". Anything else leads to mysticism in a wrong ⟨province?⟩.

When your paper is in final form I will check the technical side if you wish: provided that my other duties do not make this impossible.

Yours H.G. Forder [bj]

4 | P.S. we are introducing Dockeray "Pure Maths"[bk] here next year: a good book, but in places stodgy due to the following fact. He is most anxious to be rigorous and hence rightly avoids geometric *intuition* as a constituent of a proof. But he fears to use it in construction of a proof which in its final form would be non-intuitive. I am thinking of sending a note to the "*Math. Gazette*"[bl] on this: but it would need checking for rigour by someone. Is this at all in your line. I mean such things as

$$f(g) \to \omega \,.\supset.\, \log f(g) \to \log \omega$$

under condition.

HG.F

26.19 Popper to Forder, 23 November 1943

Typescript, 4 numbered pages.

P.O. BOX 1026
Canterbury University College
Christchurch, C.1
New Zealand

November 23rd, 1943.

Dear Forder,

[bi] Schrödinger (1935).

[bj] ⟨*Bottom line:*⟩ PTO

[bk] Dockeray (1934).

[bl] There is no corresponding publication in the Mathematical Gazette.

Very many thanks for your letter. Let me first assure you that I should be very happy indeed if I could do any checking for you; provided, always, that I understand the matter sufficiently. I doubt whether this will be the case; but if not, then I shall simply tell you without delay. Your example,

$$f(x) \to \omega \,.\supset.\, \log f(x) \to \log \omega,$$

I did *not* understand, unfortunately, since I have not the slightest idea what the arrow may mean here. But I might perhaps understand it in its context. In any case, I am most willing to do anything within my extremely limited powers.

Regarding the part in your letter on my paper, I was particularly pleased about your readiness to give me your criticism and general impressions. It goes without saying that you were right when you thought that I want you to be frank. If anything, I want you to be quite merciless! I am well aware of the necessity of being blunt in such matters, and of the great advantage of a full mutual agreement on this point. And I should be quite uncomfortable if you had the slightest hesitation of saying what you think; of course, I reserve the right of defending myself, even though it is very unlikely that I shall make use of this right. To be more practical, I am most willing to cut out the symbolic formulations to which you object; but I may just mention why I put them in. One reason is of course Tarski's formulation. But the other was the fact that I found my original formulation of 1.15 and 1.16 nearly impossible to formulate in words. Now that I have simplified these formulations, this reason disappears. I shall gladly accept your final ruling in this matter. I should also be glad to cut out a lot in section 2, which I have re-written at least ten times, in vain attempts to reduce its length. The only success was that my main objective does not come out clearly: I mean the avoidance of the D-rules or γ-rules so far used, as far as I know, by *all* sets of Boolean algebra. (Do *you* know one that does not use it?)[1]

This leads me back to your letter. You suggest that I should prove the reflexive and transitive laws for "=" immediately after 1.14. But unfortunately, I have now shifted
2 all definitions to the beginning. Shall I | shift them back, in order to comply with your suggestion? (My reason for shifting them to the beginning was that I do not agree with Huntington's methods of first proving that a certain expression "exists" and is "unique", and then defining; I prefer to formulate the definitions so that they demand uniqueness and existence.)

Your other comments refer to 1.70γ (= 1.58γ in new numbering) and 1.73 (1.61 new numbering) on p. 9. 1.70γ (= 1.58γ): the reference to "1.5" was a typing mistake; it should have been "1.55", which is, in new numbering, 1.53. Since by 1.14, we get from 1.57 (= 1.69 in old numbering) $(a' + b')' < ab$, it is only necessary to refer to 1.0; the law of transitivity for "=" is thus here not really necessary. However, you are quite right in your demand that I should refer to some such transitivity principle in the reference to 1.73 (= new 1.61). It will be, I think, sufficient if I include a reference to 1.14 and 1.0 in the references to 1.73 (= 1.61). Don't you think so? (The same references should remain in the references to 1.74 & 1.75 (new: 1.62 & 1.63).

[1] This bears on Bernays' reduction of Russell's primitive propositions.

Now regarding the Schrödinger-Einstein-Podolsky-Rosen problem.[bm] I always felt precisely as conservative as you do, and I even published something on this point,[bn] many years ago (which led to lengthy discussions with Heisenberg). I know now that I was wrong in some points, but I was, I think, right in others. My point was that the indeterminacy-formulae must be interpreted statistically (i.e., as probabilities in the sense of relative frequency), since they are derived from equations which must be interpreted in this way. You see, this corresponds exactly to what you say. But later discussions with Heisenberg, and especially with Bohr, made me more conscious of the difficulties. I had shortly before leaving Europe an opportunity to discuss Einstein's and Schrödinger's views with Bohr (or rather, to listen to Bohr's most interesting criticism of these views, for he did, of course, most of the talking). Although I felt much happier with Schrödinger's views, I must admit that Bohr's arguments were very important. He says, in brief (as you may remember from his reply to Einstein-Podolsky-Rosen) that by measuring the one of the two partial systems after their interaction, our own system of reference is interfered with, and it is this interference which reflects upon and blurs the result of any calculation of the second partial system. But although this seems to be correct, it is as such not really satisfactory.

At present, I feel inclined to look for a solution along the following lines. I feel inclined to reject Heisenberg's idea of the *full* duality of "wave picture" and
3 "particle-picture". He contends that nothing can be | gained in giving these two different roles to play, since the indeterminacy affects both equally. I feel inclined to emphasize that all elementary physical "events" are of the character of *particle*-impacts or *particle*-interaction. Or perhaps better: let us drop the particle-picture and speak, more neutrally, of physical "events". Then we find that certain aspects of these events can be predicted precisely, others only statistically (in accordance with the indeterminacy relations), and that fields, and waves of fields (in configuration-space) are statistical fields measuring the frequency of certain types of "events". These frequencies can be measured and are therefore "observables" in Dirac's sense; thus my elementary "events" and Dirac's "observables" do by no means coincide. The best "picture" for individualizing the "events" is an impact etc. of particles. But we must be clear that this is only a picture, a metaphor from the macro-world; that the (blurred) *track* of a particle is, similar to a field, only a picture of the probability of an *event*; and that particles possibly do not exist at all. That is to say, if we split a photon into two beams, it is not necessarily so that "there is a photon" somewhere, either in the one beam or in the other. What we split is, of course, the beam, i.e., the wave-field, i.e., the frequency of the occurrence of an event, i.e., of a photo-electric impact.

All this is rather trivial, but it is neither precisely what Heisenberg says nor what Dirac says (although it hardly contradicts only what the former says). It is, of course, no answer to the problem raised by Einstein and Schrödinger; but I think it might be

[bm] Einstein, Podolsky, and Rosen (1935).

[bn] Popper (1934).

used for ironing out the differences between these and Bohr, roughly along the following lines:

As opposed to the “event”, the particle-track-picture shares with the wave-picture the property that it depicts possibilities of events which may happen under certain circumstances; e.g., if there is something near the path of the particle. As opposed to this, the “event” itself occurs only if at least *two* Quantum mechanical systems interact. Thus the event always *involves* a second system, *for example*, the measuring system; (I say “for example” because I find it important to insist that the measuring system is in no way more “subjective” than any other); and anything interfering with the second system may, of course, modify the probability of an event. In other words,
4 the history of the second system will reflect on the probability of | the event, and therefore on the wave-picture of the event as well as on the particle picture (of the track). But this is *not* due, as Heisenberg contends, to an interaction between observer and observed; this strangely causal-deterministic theory of Heisenberg is, I feel, destroyed by Einstein and Schrödinger. Rather, it is due to the simple fact that every theoretical picture of events must be statistical, i.e., must depict the probability of an event which, of course, is always the interaction between *two* systems; so that the (statistical) *picture* of the one system must be, of course, influenced by changes in the system of reference. I do not know whether I have expressed myself sufficiently clearly, and whether what I say is at all worth mentioning.

With many thanks,

26.20 Popper to Forder, 21 December 1943

Typescript, 2p.

P.O. BOX 1026
Canterbury University College
Christchurch, C.1
New Zealand

December 21st, 1943.

Dear Forder,

Thank you so much for your letter, I am sorry that you are suffering under an attack of matric. It must be too awful for words. I don’t think I could survive it; even in University examination times I always wonder why the suicide statistics does not show a peak at this time of the year. You have my full sympathy, and I certainly do not expect you to reply to this letter.

Many thanks for your corrections and your very valuable suggestions which I all accept; but I may say in self-defence, that the method (which I am trying to exclude) of securing independence by means which you call “fraudulous” has been used by Huntington not only “at the beginning of the century” i.e., in his Third Set[bo], but also

[bo] Huntington (1904, § 3).

in 1933 in his Sixth Set[bp]; furthermore, my point is not so much that such fraudulent methods "ought" not to be adopted, but that one must *redefine independence* in such a way that "ethics" is replaced by definable desiderata.

All this is only self-defence, and I do not hesitate to accept your suggestions. I quite believe that it is hard to see the wood for the trees; for there are, unfortunately, too many trees in the way, but very little wood. I don't like the paper for this reason: it is a large collection of minor results which are at best only moderately exciting. But I need these results (of sections 3 to 5) in connection with my probability theory, which would become too complicated if I had to derive the results there. It is very sad, and I am, as you have guessed, sick of it.

There is a large number of points which I should love to discuss with you, but I realize how difficult it is to do this by letter. One of my points is this: Do you know whether "dense" lattices and Boolean algebras have been investigated? I mean a lattice or Boolean algebra with a postulate of density, e.g.,

POSTULATE Dy: If a, b are in K, then there is a c in K such that

$$(a < c \vee c < b \,.\!\rightarrow\!.\, a < b) \;\&\; (b < a \,.\!\rightarrow\!.\, c < a \;\&\; b < c)$$

On the basis of a dense lattice or Boolean algebra it is of course very easy to build up a continuous lattice or Boolean algebra, by adding a postulate of continuity such as Postulate Cy:

POSTULATE Cy: If, for all (natural numbers) n, the elements $a_1, a_2, \ldots, a_n$, are all in K, and

$$a_n < a_{n+1},$$

2 then there is an element a_∞ in K such that |

$$a_n < a_\infty$$

for all n, and

$$a_\infty < b$$

for any b that satisfies, for all n, the conditions

$$a_n < b.$$

In other words, to every monotonously increasing series of elements of K there is an element a_∞ in K such that

$$\lim_{n=\infty} a_n = a_\infty$$

We can, of course, use a simplified Dedekindian method instead:

POSTULATE Cy*: If A and B are sub-classes of K such that, for any $a_1, b_1 \in K$,

$$a_1 \in A \;\&\; b_1 \in B \rightarrow a_1 \leq b_1 \qquad (26.1)$$

[bp] Huntington (1933b).

then there is an element c in K such that, for any $a_2 \in A$ and $b_2 \in B$,

$$a_2 < c \;\&\; c < b_2 \tag{26.2}$$

I have never seen anything of this sort applied to lattices or Boolean algebras (the nearest can be found in Tarski's "Systemenkalkül", but it is rather different). But it seems very obvious, especially as the basis of a calculus of continuous probabilities.

I strongly suspect (but I have not proved it yet to full satisfaction) that a Boolean algebra without variables of the order of the constant K; i.e., a "lower Boolean algebra", to which Dy is added, becomes *complete*.

26.21 Forder to Popper, 1 March 1944

Handwritten, 4 partly numbered pages.

A.U.C.
1.3.44

Dear Popper

I fear that the reputation I first gained as a good correspondent is irrevocably lost. Your paper interested me but much work and worry has delayed things. Your cross word puzzles have now been checked + I think your work should be presented. But where? It is doubtful whether the Am. Math. Soc. would print it in the Trans.[bq] tho' they have printed ⟨also?⟩ much longer papers on Boole algebra: it is mainly a question of space. Similarly for the Am. Journ.; Annals; + Duke.[br]

The obvious place is *Journal of Symbolic Logic* – I never see that + do not know usual length of papers there.

The war is affecting America. Garrett Birkhoff has deserted lattices + is now
2 | working on Kutta-⟨J⟩oukowski aeroplane formulae. George B. has appealed to maths to keep a balanced sense of values. Others of the logical tribe such as Curry are doing applied Maths. Are the same sane things still so in + you could at least send the paper.

On dense and continuous lattices I know nothing except that this of course turns up in various disguises all over the place. An axiomatic treatment I cannot recall. The recent work on axioms of rational real numbers would be a starting point. I have a feeling that if continuity (Dedekindian) were introduced, the alternatives would be few. But denseness would be worth trying if you mean $a < b \,.\!\supset\!.\, (\exists c)\, a < c < b$.

Are you interested in the technical development of probability: there are some incredible theorems in it: eg.

3 | Let p be number in $0^{\aleph_1}$ and $p_1 p_2 p_3 \ldots$ the binary expansion (this corresponds to heads and tails). Suppose S_n is excess of occurrences of 1 in the first n p_i.

[bq] Transactions of the American Mathematical Society

[br] American Journal of Mathematics; Annals of Mathematics; Duke Mathematical Journal

Of course $\frac{1}{n}S_n \to 0$ for almost all p in $0^{\aleph_1}$ (i.e. the other p form a set of measure 0). (I say "of course", tho it is not too easy to prove.) Successive improvements of theorem gave final result (Khintchine)[bs]

$$\limsup S_n/\sqrt{2n \log\log n} = 1$$

As Hobbes said when he saw the Theorem of Pythagoras "By God, this is impossible".

Philosophers + other commentators of Aristotle are not to be seen at Auckland. The influence of A⟨ristotle⟩ persists in curious ways – he forbade eating snow to mountaineers (what about ice-cream) and fixed 5 as the max. no. of births possible at once for humans.

4 | I am very sorry that Offenberger came down in his Applied Maths III. It was a prerequisite for the MA + he was so interested in the logical side that it is a disappointment for both of us.

This year is going to mix me up with the Postprimary syllabus, I fear. They won't leave me alone. College + University politics are time consuming, but if they are ignored they fall into the hands of men with big hearts + small brains.

I think your ⟨wireless?⟩ talks can make people see, i.a. ⟨this?⟩ importance of Maths. Its standing in this country is a disgrace tho' I must say, it is now respected at Auckland. But talk, chatter + loose thinking have far too much influence here + this new turn of educational policy tends to increase it.

With good wishes
Yours
Forder

26.22 Forder to Popper, maybe 8 or 9 July 1945

Handwritten, 2p. The letter itself is not dated, but there is an envelope with a postage stamp of 8th or 9th of July in KPS Box 296, Folder 15. We conjecture that this letter belongs to that envelope.

Auckland U. C.
Sunday

Dear Popper

Your letter did not reach me till Saturday.

I think the statement[bt] is admirable. I have only one criticism. p. 1 "and were particularly of scientific knowledge". I would rather that was deleted, tho' I do not

[bs] Law of the iterated logarithm; Khintchine (1924).

[bt] That is, "Research and the University, A Statement by a Group of teachers in the University of New Zealand" by R. S. Allan, J. C. Eccles, H. G. Forder, J. Packer, H. N. Parton and K. R. Popper, Caxton Press (Christchurch, N.Z.), July 10, 1945; reprinted in Allan et al. (1991).

insist.[bu] It seems to me that e.g. on interpretation + exposition of ways of life or of thought in antiquity, the middle ages, or in some modern age, is just as valuable and calls for gifts as high as the best needed in scientific work. And there is a risk that in N.Z. any development will be in the Technical direction. For this reason I am very glad of the paragraph: (p. 3) “Beyond providing this framework . . .”

p. 4 “N.Z. has lost opportunities, in recent years, etc.” what “workers” are you thinking of?

I congratulate you on this excellent manifesto.

With good wishes
H.G. Forder

P.S. It was very good of you to enclose the stamps, but I think it is only right I should pay for the the wire. Also let me know my share of the cost of the statement + the distribution.

⟨*Bottom line:*⟩ PTO

2 | P.S. Misunderstanding by the public is of course inevitable. It is no fault of the statement. Many will think that we are suggesting that Stage I students should begin “Research”. (The word is even used in Secondary Schools in N.Z.) We have no Honours Courses in the English sense. Still we are I suppose mainly addressing people like Senate members. But even they will quite likely misunderstand.

If however discussion follows all things can be made plain.

HG.F

26.23 Forder to Popper, 23 July 1945

Handwritten, 1p.

Auckland University College
(University of New Zealand)
Auckland, C1

July 23 1945

Dear Popper.

Received the statement today. Glad you accepted my modification + noticed a consequential one at the bottom of the page 1. Pity we have no English word like “Wissenschaft”. It will please you to know that further consideration of the statement after I sent my wire confirmed my first view that it was very good indeed. I can defend it all without any reservation + I congratulate you on its production.

Copies are being distributed to the Staff + Council + I have had the Press representatives ⟨round?⟩.

Good wishes
Yours
H.G. Forder.

[bu] The deletion suggested by Forder was carried out.

⟨*Left margin*⟩
Please let me know my fraction of the expenses, when the time comes. H.G.F

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Chapter 27
Popper's Correspondence with Harold Jeffreys

Karl R. Popper and Harold Jeffreys

Abstract Harold Jeffreys (1891–1989) was a British scientist who mainly contributed to mathematics, statistics, Bayesian probability, geophysics and astronomy, but also published articles on other subjects. (For further biographical information cf. Cook, 1990.) This letter by Popper to Jeffreys was written in the context of a debate between Jeffreys and Popper on the nature of logic, which took place in the journal Mind between 1940 and 1943. This debate starts with Popper's article "What is Dialectic?" (Popper, 1940), in response to which Jeffreys published the note "Does a Contradiction Entail every Proposition?" (Jeffreys, 1942). Popper announces his response to Jeffrey's article in the letter reproduced here; he will publish it as "Are Contradictions Embracing?" (Popper, 1943; Chapter 8 of this volume.). The letter is also interesting for Popper's remark on the probability interpretation of logical formulas, as well as for his remark on the relationship between formal logic and methodology.

Editorial notes: The letter can be found in KPS Box 313, Folder 9. We have replaced the symbol ~ by ¬.

27.1 Popper to Jeffreys, 26 April 1942

Typescript, 1p.

Christchurch, April 26th, 1942.

Dear Sir,

I have just received *Mind* and have read your note ⟨Jeffreys, 1942⟩ on my Dialectic-paper ⟨Popper, 1940⟩ with great interest. Your doubts induced me to analyse briefly what is really *assumed* if we wish to prove that contradictions are *embracing*, i.e. that any sentence whatever can be deduced from them. I have sent this analysis to the Editor of *Mind*, and enclose a copy of this note. I am grateful for

D. Binder et al. (eds.), *The Logical Writings of Karl Popper*,
Trends in Logic 58, https://doi.org/10.1007/978-3-030-94926-6_27

the opportunity you have created for clarifying this difficulty. May I, in this letter, indicate my attitude to the other points in your note?

When you write: "Carnap is so drastic as to take $\neg p . \supset . p \supset q$ as his first primitive sentence", I cannot agree that this is drastic. Lewis' *System of Strict Implication*[a] certainly does not contain any of the so-called "paradoxes of implication", and I do not doubt that "Strict Implication" fulfils every requirement of "Entailment". Lewis has shown, however, that it is possible to include "Material Implication", i.e. Russell's Implication, in his system, and to deduce, within the system of strict implication, the whole of Russell's system. This shows that the system of Material Implication is perfectly sound, even if judged from the system of entailment, *only that we must not interpret Material Implication as Entailment*, as Russell unfortunately did. But Carnap not only avoids this mistake of Russell's, he also explains at length that his implication corresponds to Material Implication, and that Consequence or Inference or Deducibility (or "Entailment") must not be mixed up with it. Furthermore, he shows at length that Deducibility in the system must be expressed in a very different way (namely "syntactically"). And lastly, he discusses, and admits, the restricted value of other systems like that of Lewis which formalise entailment within the "Object Language", and shows the superiority (greater generality) of a system which introduces the concept Deducibility only in the Syntax-Language or Meta-Language. The reply to your question "What happens if we read . . ." would therefore be: Such a reading is excluded by the very formula which you contest; just as the reading of "and" for "or" (and *vice versa*) is excluded by formulae such as "$(p \,.\, q) \supset p$", and perhaps "$p \supset (p \vee q)$". According to Lewis' investigations (cp. Lewis-Langford[b], p. 139f), only the *asserted* implication-symbol (and this is in the formula "$\neg p . \supset . p \supset q$", of course, the first of the two implication symbols) can be interpreted as Entailment.

Regarding the probability-interpretation of "$\neg p . \supset p \supset q$" I see therefore no difficulty. Why not interpret it "on the data '$\neg p$', the sentence '$p \supset q$' is certain"; i.e. $P(p \supset q \mid \neg p) = 1$. Since "$p \supset q$" is a material implication, i.e. only a shorter way of writing "$\neg p \vee q$", $P(p \supset q \mid \neg p) = 1$ is the same as $P(\neg p \vee q \mid \neg p) = 1$ which is, of course true. But apart from all that: I feel that we should never criticize formal logic from the point of view of some methodology; rather we should criticize any methodology (which of course uses formal logic in every step it takes), and any theory of probability, from the point of view of formal logic.

Many thanks for your interest in my paper.

Yours sincerely,

[a] Lewis (1918, Ch. 5).

[b] Lewis and Langford (1932).

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Chapter 28
Popper's Correspondence with Stephen Cole Kleene

Karl R. Popper and Stephen C. Kleene

Abstract Stephen Cole Kleene (1909–1994) was an American logician, well-known for his fundamental work in various fields of theoretical computer science, such as formal language theory, automata theory and recursion theory. The letter to Kleene reproduced in this chapter shows Popper's continued interest in Boolean algebra. In that letter Popper claims to have found a proof of a theorem in Boolean algebra that contradicts propositions which are widely believed to be true; similar letters were sent to various other researchers. A typescript entitled "The Distributivity of Lattices with Unique Complements" was enclosed, but the alleged proof contains an error. As mentioned in the Preface, Popper's work on Boolean algebra is outside the scope of this edition. This episode, part of a larger critical examination of the logic of quantum mechanics, is discussed more fully in an article by Del Santo (2020).

Editorial note: The letter to Kleene can be found in KPS Box 315, Folder 23.

28.1 Popper to Kleene, 19 April 1968

Typescript, 1p.

Fallowfield, Manor Road, Penn, Buckinghamshire, England
April 9th, 1968

Dear Professor Kleene,

I am sending you a short paper containing what appears to me to be a very short and quite primitive proof that all lattices with unique complements are Boolean algebras.

D. Binder et al. (eds.), *The Logical Writings of Karl Popper*,
Trends in Logic 58, https://doi.org/10.1007/978-3-030-94926-6_28

Unfortunately this clashes head-on with accepted theory – with a theorem due to Dilworth (1945)[a], and endorsed by Birkhoff (Lattice Theory, 2nd edn. 1948, p. 170).[b]

I do not think it would take you more than 10 minutes to look at my proof. If you find time for a word of comment, I should be most grateful.

I have written to Dilworth, Birkhoff and Mac Lane[c].

With apologies for intruding on your time, and with kind regards,

Yours sincerely,
K. R. Popper

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[a] Dilworth (1945).

[b] Birkhoff (1948).

[c] Saunders Mac Lane (1909–2005).

Chapter 29
Popper's Correspondence with William Calvert Kneale

Karl R. Popper and William C. Kneale

Abstract William Calvert Kneale (1906–1990) was a British logician and historian of logic, who is best known for his book *The Development of Logic* (Kneale and Kneale, 1962) on the history of logic, jointly written with his wife Martha Kneale. (For further biographical details cf. Smiley, 1995.) In his letter to Popper, Kneale suggests a generalization of Popper's single-conclusion deducibility relation $a_1, \ldots, a_n/b$ to a multi-conclusion relation $a_1, \ldots, a_n/b_1, \ldots, b_m$, where the formulas $b_1, \ldots, b_m$ are understood conjunctively. This direction would later be pursued in Kneale (1956) and in Kneale and Kneale (1962, Ch. IX). Both of these texts refer to Popper, and Kneale develops Popper's ideas of inferential definitions into a theory where logical constants are defined by double-line rules, that is, rules which can be read in both directions. This makes Kneale one of the few persons who further developed Popper's ideas on the nature of logical constants.

Editorial note: The correspondence is contained in KPS Box 315, Folder 29.

29.1 Kneale to Popper, 5 September 1947

Handwritten, 2p.

11 Holywell
Oxford.
5.9.1947

Dear Popper,

I have been reading your article in *Mind* on *New Foundations for Logic*[a] and want to tell you how much I have enjoyed it. If I may be allowed to say so, it seems to be excellent. I hope you will soon publish a book in which this approach is explained at

[a] Popper (1947c).

D. Binder et al. (eds.), *The Logical Writings of Karl Popper*,
Trends in Logic 58, https://doi.org/10.1007/978-3-030-94926-6_29

greater length with the necessary proofs. It should be very valuable for the teaching⟨?⟩ of logic.

May I make a little suggestion? (Perhaps you have already thought of it and turned it down for some good reason.) Maybe you could introduce the notation "$a, b/c, d$" as an abbreviation for "$a, b/c$ and $a, b/d$"? This would make the statement of your generalized transitivity principle much simpler (Incidentally Base I seems to be so
2 much better than II or any variant | that I should much prefer it for a full exposition: it is not really very complicated). You only need to make clear that the statements whose names appear before a stroke are taken collectively and⟨?⟩ those whose names appear after distributively.

You can then allow yourself to use such expressions as "$a, b//c, d$" with suitable explanations. It is rather convenient to be able to say that two sets of statements (eg two postulate sets) are logically equivalent.

Yours sincerely
William Kneale.

29.2 Popper to Kneale, 30 June 1948

Typescript, 1p.

L.S.E.,
June 30th, 1948.

Dear Kneale,

I am sorry to have let you wait: I had no copy of my little paper, since one is in Ryle's hands (he is publishing it in Mind), while the other was in Ayer's hands till this morning.[b] I should like to hear what you think. Ayer says he is not quite convinced yet.

There was a review[c] in the Journal of Symbolic Logic which

(a) Swallows my New Foundations[d] up by saying it is a re-statement of Gentzen. (But what, for example, about my method of defining the formative signs in terms of deducibility (which is introduced by primitive rules)?)

(b) Says that my only claim to originality is in L. without Assumptions[e], and criticizes this on lines which are based on a misunderstanding (for which I have to take most of the blame); viz: I consider, of course, an absolutely valid rule a rule which is valid without referring to the forms of the statements involved, and not a rule confined to absolutely valid inferences.

Example:

(I) If "a/b" is absolutely valid, then "b/a" is absolutely valid. This (I) is a valid rule about absolutely valid inferences.

(II) If a/b then b/a. This rule II is an *invalid* rule (which does not refer to the forms of the statements involved).

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[b] Perhaps Popper (1949c).

[c] McKinsey (1948).

[d] Popper (1947c).

[e] Popper (1947b).

Thus a valid rule about absolutely valid inferences may not remain valid if formulated for all inferences.

I admit that certain formulations of mine must be re-stated to be really exact; but all this is confined to p. 277, note 12 (which is a silly note) and to pp. 279/280. Everything else can stand as it is.

Following the review there is another[f] discussing in some detail a simplification of Quine's quantification theory. My much more radical simplification of quantification theory (Quine's, Gentzen's or anybody's) in "New Foundations"[g] is not even mentioned.

Kind regards

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[f] Berry (1948).

[g] Popper (1947c).

Chapter 30
Popper's Correspondence with Willard Van Orman Quine

Karl R. Popper and Willard V. O. Quine

Abstract Willard van Orman Quine (1908–2000) was an American logician and philosopher working in the analytical tradition and shaping it in various respects. Quine is well-known for his critical attitude towards modal logic, which makes their conversation about Popper's formalization of modal logic in Popper (1947d) particularly interesting. The letter from Popper to Quine of 6 April 1948 is remarkable for an indication of the demarcation between logic and mathematics. Popper says that he has formulated inferential definitions for cardinal numbers; this would increase the scope of his approach from logic to mathematics. However, this idea has not been pursued in any of Popper's subsequent publications, and we were not able to find definite traces of this idea in his unpublished work either.

Editorial note: The correspondence between Popper and Quine is contained in KPS Box 340, Folder 9 and in the Quine estate housed at the Houghton Library, Harvard University.

30.1 Popper to Quine, 2 February 1948

Houghton Library. Handwritten on paper with letterhead of the LSE, 1p.

17/2/48

Dear Professor Quine,

Very many thanks for your off-prints. They are exciting and admirable. And so is your last paper[a] in the Journal ⟨of Symbolic Logic⟩ on Nominalistic Mathematics.

Question: If we say that a formula exists (in a proof) even if it is not "inked", so to say: do not then infinitely many formulae exist?

I dislike Modal Logic as much as you do. Nevertheless, I give in the enclosed

[a] Goodman and Quine (1947).

D. Binder et al. (eds.), *The Logical Writings of Karl Popper*,
Trends in Logic 58, https://doi.org/10.1007/978-3-030-94926-6_30

paper[b] extensional metalinguistic definitions for the modal functions. I should be extremely grateful to hear whether you object to them.

Yours sincerely,
K. R. Popper.

30.2 Quine to Popper, 21 March 1948

KPS Box 340, Folder 9. Typescript on stationery with letterhead of Harvard University, 1p.

Harvard University, Department of Philosophy
Emerson Hall, Cambridge 38, Massachusetts

March 21, 1948

Dear Prof. Popper,

Many thanks for your kind letter and the three offprints. I find your metalogical method of introducing logical signs decidedly ingenious. It is, at the very least, an illuminating way of presenting the subject. There is still a question in my mind how much *more* it is than that. I fear it doesn't, at least at the present stage, succeed really in solving the problem of an objective distinction between logical and extra-logical vocabulary, or between analytic and synthetic truth, though such seems to be your purpose in your Aristotelian Society paper[c]. The loophole, to my way of thinking, is at the top of p. 258, where "form-preserving interpretation" (which is needed for D3, p. 264) is made to presuppose the notion of "proper translation" or "preservation of meaning". The concept of *synonymy*, here apparently an ultimate presupposition, is in my estimate as badly off as analyticity itself; indeed, the two are readily interdefinable.

I see no objection to your treatment of modalities in "Functional logic without axioms . . ."[d]. It shows that your machinery of metalogical introduction is as adequate to modalities as it is to truth-functions, etc. (Of course this does not imply any *translatability* of modalities into extensional terms.)

To your question on uninked inscriptions: whether there are infinitely many will depend on physics. If matter is either infinitely divisible or of infinite spatial and/or temporal extent, there will be infinitely many uninked inscriptions, but otherwise not.

Sincerely yours,
W. V. Quine

[b] Popper (1947d).

[c] Popper (1947b).

[d] Popper (1947d).

30.3 Popper to Quine, 6 April 1948

KPS Box 340, Folder 9. Typescript, 1 sheet of LSE stationery, 2p.

The London School of Economics and Political Science
(University of London)
Houghton Street, Aldwych, London, W.C.2

April 6th, 1948.

Dear Professor Quine,

So many thanks for your letter of March 21st. I am very grateful for your interest, and most anxious for its continuation. I should like to discuss several of the points you mention, but I realize that I must not take up more of your time than absolutely necessary. I am sorry that I must trouble you with the request to re-consider your criticism of my "Logic without Assumptions"[e], but I shall try to be short.

You say quite rightly that any definition which makes use of the idea of a form-preserving interpretation or of synonymy is as badly off as if it used analyticity itself. All this is unquestionably true. But it does not constitute a criticism of my paper. In fact, I say all this myself on p. 273. Down to the end of page 273, my paper is only introductory; it is, down to this point, hardly more than a re-statement of Tarski's paper (as I have tried to explain). I myself say quite definitely on p. 271f. that the distinction between formative and descriptive signs, and with it the idea of form preserving interpretation, has to be got rid of.

The whole point of my paper is that this is possible; that "form-preserving interpretation" can be reduced to "statement-preserving interpretation". (I do *not* say that the latter term is syntactical, on the contrary, I am aware – cp. note 18 – that my "final definition" on p. 287 – with the same *wording* as D3, but with "form-preserving" defined in terms of "statement-preserving" – is semantical. But this is another problem.)

I should be extremely grateful to you if you could find the time to look at my paper again, especially from p. 274 on. There may be many things which I have overlooked, but I am quite sure that the point you mention is not one of them, and is perfectly in order; moreover, that the point you mention is identical with the problem explained at length in my paper on pp 271 to 273, as the fundamental problem which I wish to solve.

I may mention that I have improved on this paper very considerably, by showing that it is possible to *use*, in the metalanguage, at first only *one* undefined two-termed predicate, "$D(a, b)$", i.e., "a is deducible from b", and *two* formative signs (of the
2 metalanguage) "if-then" and "and". We can | then adopt the following rule:

No formative sign must be used in the metalanguage before it has been established as formative by first formulating (in the metalanguage) a "defining formula" which establishes that the *corresponding sign of any object language* is formative. (A "defining formula" is one which, whenever the universal quantifier is available in the metalanguage, can be shown to be equivalent to an explicit definition.)

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[e] Popper (1947b).

I have also been able to give inferential definitions of (finite) cardinal numbers, addition, etc., thereby showing that the figures and operators of ordinary arithmetic may be claimed as formative signs.

May I say again how extremely grateful I should be for a second look at my paper, and a few lines?

Very sincerely yours

30.4 Quine to Popper, 19 April 1948

Houghton Library. Typescript, 2p.

19 April 1948

Dear Professor Popper,

Thanks for pursuing the matter: I was indeed remiss. I have now gone over your paper again, and find that various of the marginal notes which I had written in it on first reading involved misunderstanding. I fear academic pressure has been too severe, this term, for proper meditation.

I am still not satisfied with your theory, though: and, to whatever extent the following criticisms evince a continuing misunderstanding on my part, I'll appreciate your continued efforts to set me right.

Your concept of "freely interpretable properties" puzzles me. From your definition in note 16 I seem to be able to prove that any property P of statements is a free property provided merely that it holds for some statement a of L_1 and fails for some statement b of L_1. For, consider any partition of statements of L_1 into exclusive classes A and B. Interpretation of all members of A by a, and of all members of B by b, is a statement-preserving interpretation fulfilling the demands of your definition in note 16.

If my reasoning is right, then the definition on your note 16 needn't have been so complex. In my ensuing remarks, however, I am going to turn my back on the concept of freely interpretable properties, and accept "truth" uncritically.

Let us next consider the problem of the meaning of absolute validity of an inference. In the light of D4, it seems demonstrable that absolute validity is *completely* formulated by (6.1g) – to the exclusion of (6.2g), (6.3), and (6.4): The three rules last mentioned are merely incidental observations about absolute validity: the only absolutely valid inferences are inferences of the kind described in (6.1g). Proof: Whenever the conclusion b is not one of the premisses $a_1, a_2, \ldots, a_n$, we can get a statement-preserving counter example simply by interpreting each of $a_1, a_2, \ldots, a_n$ as "0 = 0" and b as "0 = 1".

This being the case, you could have defined absolute validity much more simply than in D4 or D5 – and without appealing either to "truth" or to "free property" or even to "statement-preserving". Viz: an inference is absolutely valid if and only if the conclusion is one of the premisses.

2 Next let us consider how the sign "/" in the | "inferential definitions" is to be viewed. It cannot mean absolute validity of inference (if my foregoing result is right),

for this would make the "inferential definitions" simply false. It cannot mean mere validity of inference, for this is a notion which you are at pains not to presuppose. Are we to take it merely as a notation for stating a rule of inference? But then your "inferential definitions" cease to *define* such concepts as "same logical force as negation of", and become, instead, indistinguishable at bottom from old-fashioned rules of inference governing primitive notations of negation etc. In this event, your definition D6 of validity represents no philosophical advance over the old-fashioned elucidation of validity by enumeration of a complete set of rules of inference relative to a given notation.

I look forward with much interest to your clarification of these points.

Note the enclosed curiosity: your stamps came through uncancelled.

Sincerely yours,

30.5 Popper to Quine, 13 June 1948

KPS Box 36, Folder 23. Handwritten draft. A typescript version (with handwritten corrections) was attached to a letter from Popper to Bernays; cf. this volume, § 21.8. The two versions of this letter differ only with respect to some minor stylistic changes, with the exception of the change indicated in a footnote, and a handwritten remark on the side of p. 2 of the Bernays version, which reads: "To put the matter differently, we can give, in the meta-metalanguage, a (finitist) criterion of truth for all these rules; and this may be used as criterion or definition of validity."

June 13th, 1948.

Dear Professor Quine,

I have been so awfully busy ever since I received your kind letter of April 19th that I simply was physically unable to reply before.

My very best thanks for your letter. All your criticisms are justified, and although I am perturbed at having blundered, I am extremely grateful to get your criticism.

I feel however still reasonably sure that I am, in the main, on the right track. In other words, I think that my construction can easily be repaired. In fact, all that is needed to repair it is to replace the very stupid footnote 12 (in Logic without Assumptions) by another footnote, and to eliminate all reference to "free properties", by re-casting the footnote 16, and (D5).

In other words, I do not think that any of those later pages which contain what I may perhaps call "my results" need to be changed. The main change is needed where I try to define "absolute validity" without the use of truth – a point with which I have dealt rather sketchily in any case; and your main objection, concerning the inadequacy of "absolute validity" for my purpose, can be answered by pointing out that although I have made a most misleading statement in footnote 12, and that I am accordingly deserving of every blame, the main idea is tenable.

The reason why it is tenable is this: I meant by an "*absolutely valid rule of inference*" (as indicated on pp. 282 and 285 (italics)), a rule of inference which is *valid irrespective of the logical form of the statements involved.* Absolute validity in this sense can be defined, and, I believe, does the trick.

An absolutely valid rule of inference, in the sense indicated, is *a fortiori* valid. Accordingly

$$\text{“}a/b \rightarrow b/a\text{”}$$

for instance cannot be an absolutely valid rule of inference in my sense since it is not valid. (I have mislead my readers into believing that it is my intention that such a rule is absolutely valid.)

The distinction which must be made is between rules about assertions about absolute validity and rules of inference which are absolutely valid. For example, the above rule is invalid, but something like

$$\ulcorner a/b \text{ is absolutely valid} \rightarrow b/a \text{ is absolutely valid}\urcorner$$

would, indeed, be true.

In other words, I never intended

$$\text{“}a/b\text{”}$$

to mean “the inference with the premise a and the conclusion b is valid”; I intended it to mean, simply, “b follows from a”; and I asked for a definition of the truth, i.e. the validity (or absolute validity) of such a statement. Thus a rule of inference is absolutely valid if and only if it remains valid after elimination of all references to the logical structure; or in other words, if all statement-preserving interpretations of the rule are valid.

All that is needed, therefore, is to characterize (without reference to the concept of a formative sign) the system of rules which have the property that all statement-preserving interpretations are valid. And this is, surely, a problem which can be solved.

In the correction note which I enclose, the solution is clumsy; this is due (a) to my general clumsiness, (b) to a desperate lack of time (and appalling working conditions).

I do not intend to send this notice to the Editor of the Aristotelian Society at once, and if you could give me your critical comments soon, I should be most grateful.

I have nobody here whom I could ask to discuss my papers and problems with me before publication. This is a terrible draw back. Bernays, the only one with whom I ever discussed anything technical – he is of course an ideal help – is in Zurich, and I have not seen him for 15 months. He wrote shortly after you along very similar lines.[f] You will understand how extremely grateful I was for your letter, and how much I hope that you will write to me again.

Very sincerely yours,
K. R. Popper

[f] In the handwritten draft: “He wrote shortly after you along very similar lines, but he takes even longer, often much longer, to reply to a letter or paper than I do. (I have no right to blame anybody.)”

30.5.1 Corrections to Logic without Assumptions

KPS Box 36, Folder 23. Handwritten, 14p. Popper sent a very similar list of errata to Bernays; cf. this volume, § 21.8.1.

Corrections to "Logic without Assumptions".

Professor W. Quine has very kindly drawn my attention to[g] certain objections against my paper, and Prof. P. Bernays has raised the same objections. It appears to me that some of these objections (against the concept of a "free property", i.e. against definition (D5) and note 16 of the paper) are completely justified, and that the others, even though they may be met, nevertheless show that some of my formulations were, to say the least, conductive of a misunderstanding. It seems to me, however, that all my conclusions remain unaffected; the necessary alterations, it seems to me, are all confined to pp. 277, 279, 280. (Besides these important corrections, I wish to draw attention to one particularly misleading *erratum* on p. 292: in line 7 from end, read "object" instead of "meta".)

Whether my critics will agree with me that their objections are met by the alterations here proposed I do not know.

2 | p. 277, shift the phrase "covered by this definition," from line 10 to line 9 (before "and"), and put in its place, in line 10, the following: "i.e., of rules which are valid independently of the logical form of the statements involved (cp. the second paragraph of section 7, below),"

same page: replace footnote 12 by the following new footnote:

"12 We must be careful not to confound rules of inference which are absolutely valid (and therefore valid) with rules about assertions about absolutely valid
3 inference. For example, | a rule like the following holds:

"If, for certain statements a and b, the assertion that a is deducible from b is absolutely valid, then, for the same statements the assertion that b is deducible from a is absolutely valid also."

But the corresponding rule of inference "If, for certain statements a and b, a is deducible from b, then, for the same statements, b is deducible from a." is invalid, and therefore not absolutely valid.

In order to define "absolutely valid rule of inference" in such a way that it ⟨covers⟩ only rules of inference which are *valid irrespective of the formative signs*
4 *which occur in the* | *various statements involved*, we first distinguish atomic and molecular rules. Using the symbolism introduced in the next paragraph of the text, "$a_1, a_2, \ldots, a_n/b$" and "$a_1, a_2, \ldots, a_n/a_1$" are examples of atomic rules (invalid and valid respectively).

Examples of molecular rules are:

(A) "a/a and b/a" (invalid)

(B) "a/a and b/b and $(a, b, c/c$ or $c/c)$" (valid)

(C) "If a/a then a/b" (invalid)

[g] "drawn my attention to" replaces (?) "informed me of", which is not crossed out.

(D) "If a/b and b/c, then a/c" (valid)

(A) and (B) we call simply conjunctive rules, (C) and (D) we call simply conditional
5 rules. We shall also use the term "component rule", | "antecedent (rule)", "consequent (rule)", in an obvious way. Note that in our "simply conditional rules", the consequent must be atomic, and the antecedent either atomic or simply conjunctive.

We now can define absolute validity for atomic, conjunctive, and conditional rules:

An atomic rule is absolutely valid if and only if no statement-preserving counter-example of it exists, that is to say, if and only if no statement-preserving interpretation of it is a counter-example.

A simply conjunctive rule is absolutely valid if, and only if, all its components are absolutely valid.

A simply conditional rule is absolutely valid if and only if every statement-preserving interpretation which turns its consequent into a counter example also turns its antecedent into a counter-example. (We assume that a counter-example of a component of a conjunctive rule is a counter example of the whole conjunctive rule; this allows us to deal with a conjunctive antecedent.)

6 | It can be easily seen ⟨that⟩ these definitions lay down necessary as well as sufficient conditions for the rules under consideration to be absolutely valid. The definitions cover most of the cases in which we are interested. In order to extend it to all intended cases, we define next:

A rule of inference R_0 is called "secondary to the rules of inference $R_1, R_2, \dots, R_n$" if and only if every inference drawn in observance with R_0 can be drawn in observance of $R_1, R_2, \dots, R_n$ (without appealing to R_0).

7 | According to this definition, every rule is secondary to itself.

We now can give a general definition of absolutely valid rules as follows:

A rule of inference is absolutely valid if and only if it is secondary to atomic, simply conjunctive and simply conditional rules which are absolutely valid in the sense defined above."

(This concludes note 12)

8 | p 279–280 replace the passage from the word "For" in line 17 on p 270 to the end of line 19 on p 280 by the following:

"For it turns out that (D4) is equivalent to the following definition:

(D5) *An inference is absolutely valid if, and only if, the conclusion is one of the premises.*

This definition avoids the term "truth", but it should be observed that it is of any use only because we can show nevertheless that it guarantees the transmission of truth from the premisses to the conclusion.

9 As it stands, (D5) is not very helpful; what we need is, rather, | the corresponding idea of an absolutely valid *rule* of inference defined with the help of (D5) or of an equivalent definition.

10 | In order to achieve this end, we can introduce the idea of a *statement-preserving numerical interpretation* of an inference or a rule of inference. This is an interpretation which identifies each of the m different statements (or variables) occuring in the inference (or rule) with one of the first m natural numbers, and which

interprets the expression "$a_1, a_2, \ldots, a_n/b$" to mean "one at least of the numbers $a_1, a_2, \ldots, a_n$ is at least equal to number b." We can, further, define a *statement-preserving (numerical) counter-example* of an inference or rule of inference as a statement-preserving numerical interpretation which is an arithmetically false statement.[16]

11 | We can now replace (D5) by the definition

(D5′) An inference (or rule of inference) is absolutely valid if, and only if, no statement preserving numerical counter-example of it exists.

The search for such a counter-example must, in every particular case, lead after a finite number of steps to a decision concerning the absolute validity of the particular rule in question.[17] (The method is somewhat similar to that of deciding a formula of
12 the propositional calculus by way of truth-tables.) It can be easily shown, | for example, that upon our definition (D5) of absolute validity which avoids any reference to truth, the rules (6.1g) and (6.2g) are absolutely valid, and similarly (6.6), and the rules analogous to it.[18] "

(This concludes the text-correction on pp 279–280. The remaining corrections are to the notes 16, 17 and 18 on these two pages, and a new note, 23a, on p. 284.)

13 | p 279, note 16, replace this note by the following new note:

For instance, a counter example of the rule "If a/b then b/a" is "If $2/1$ then $1/2$". The use of the words "arithmetically false" does not (as it may appear) re-introduce the words "true" and "false" which we intended to eliminate; the words we intended to eliminate refer to the statements a_1, a_2, etc. of the interpretations; those which we retain refer to such arithmetical statements as "$1/2$" i.e., "1 is at least as great as 2". (In other words, the problem is shifted to the metalanguage.)

p 280, note 17: replace this note by the following new note:

If the number of variables is considerable, the search for a counter-example may, of course, be long. But it is usually quite easy to design methods by which the procedure can be simplified. For example, in order to construct a counter-example of the invalid rule

"(If a/b then c/d) then (if $a, a_1/b$ then $c, a_1/d$)"

we first interpret the last consequent "$c, a_1/d$" by "$1, 1/2$". Since this does not lead to a counter-example of the rule we next choose "$1, 2/3$" instead of "$1, 1/2$"; and this, indeed, leads to the counter-example "(If $1/2$ then $1/3$) then (if $1, 2/2$ then $1, 2/3$)". (This in turn can be evaluated by the truth-table method.)

13 | p 280, note 18: Replace the words from "since" in line 4 of the note to "Altogether" in line 11 of the note by the word "But".

14 | p 284, line 23, insert a new note-sign "23a" after "*necessary*", and append to the bottom of the page the new note:

"But we may, if we wish (in order to avoid such definitions as (7.8) lay down that, for every numerical interpretation of the variables to which the definiendum refers, there exists one and only one true statement preserving numerical interpretation of the whole defining formula which renders this formula true. ('For every b' or 'For every c' etc., has to be interpreted – if we wish to avoid such definitions as 7.8 – by the phrase: 'For every number which exceeds the other numbers used in the interpretation by not more than 1.')"

30.6 Quine to Popper, 5 October 1948

Houghton Library. Typescript, 1p.

Oct. 5, 1948

Dear Professor Popper,

I am sorry to have delayed so long over your letter of June 13. Last summer I was much involved in preparing a draft of my work in progress (THEORY OF DEDUCTION) so that it could me mimeographed for my fall course. Toward the end of the summer I got married. When finally I did sit down to undertake a belated answer, I was away from your reprints and so couldn't understand your letter.

It is clear that the revisions which you propose constitute a great improvement, and meet various of the objections raised in my letter of April 19. I have no criticism to make of your revisions, as far as they go.

However, it seems to me that the criticism made in the last long paragraph of my letter of April 19 is still not met. Perhaps I am missing something.

I am returning your typescript of corrections, since it is an original; I hope my holding it so long hasn't meant great inconvenience.

I am sending you some reprints.

Sincerely yours,

30.7 Popper to Quine, 28 November 1948

Houghton Library. Typescript with letterhead of LSE, 1p.

November 28th, 1948.

Dear Professor Quine,

I wish, first of all, to congratulate you on the occasion of your marriage. I have been married for 18 years, and I can only say that I could not imagine myself living without my wife. (I say this even though she will have to type this letter.)

Next I wish to thank you for your off-prints. I cannot tell you how much I (and my best students) enjoy your writings; but these last papers on Ontology are just wonderful – sheer pleasure. There is no doubt in my mind that this is one of the most telling contributions of logic to philosophy yet. Besides, it alone brings fully to the fore the philosophical significance of Russell's theory of description (enforced by your earlier brilliant suggestion of a language in which all proper names are replaced by predicates which I now describe to my students as "proper-name-abstracts"). I can say, without exaggeration, that I have not enjoyed any philosophic application of logic (at least not since Tarski's Wahrheitsbegriff) as much as yours.

Thirdly, many thanks for your kind words concerning my attempts to repair my paper.

Very sincerely yours,
K. R. Popper

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Chapter 31
Popper's Correspondence with Heinrich Scholz

Karl R. Popper and Heinrich Scholz

Abstract Heinrich Scholz (1884–1956) was a German Protestant theologian, philosopher and logician who established the study of logic as an independent subject by founding Germany's first research institute for logic, in Münster. Since he also contributed to the historiography of logic, he was well acquainted with the work of Paul Hertz and Gerhard Gentzen. (For further information on Scholz cf. Peckhaus, 2018.) In the letter that we reproduce here, he points out what he regards as a close relationship between Popper's work and the work of Hertz.

Editorial note: The letter can be found in KPS Box 347, Folder 9.

31.1 Scholz to Popper, 17 November 1947

KPS Box 347, Folder 9. Postcard. Typescript.

Abs. Professor Dr. Heinrich Scholz
Münster/Westfalen, Westring 17 I
Deutsches Reich, Britische Zone

Herrn
Professor Dr. K.R. Popper
The London School of Economics and Political Science
London, W.C.2.

BTH 753/H

Münster, den 17.11.1947

Lieber Herr Popper, herzlichen Dank. Es ist schön, dass wir nun wieder mit einander verbunden sind und dass wir an einander teilnehmen können. Es scheint mir, dass hinter Ihrem Bericht wieder einmal ein echter Popper steht, und ich bitte Sie sehr, dass Sie uns nicht vergessen, wenn die angekündigte Publikation erschienen ist. Die Nähe, in der Sie zu den uns wohlvertrauten Konstruktionen von Paul Her⟨t⟩z und Herrn Tarski stehen erleichtert uns wesentlich den Durchblick durch ihren interessanten Entwurf. Inzwischen ist Herr Bernays im September 14 Tage hier

D. Binder et al. (eds.), *The Logical Writings of Karl Popper*,
Trends in Logic 58, https://doi.org/10.1007/978-3-030-94926-6_31

gewesen. Wir denken mit einer besonderen Freude noch immer an diese schönen Tage zurück.

Mit allem Guten für Sie
Ihr
Heinrich Scholz

Abs. Professor Dr. Heinrich Scholz
Münster/Westfalen, Westring 17 I
Deutsches Reich, Britische Zone

Herrn
Professor Dr. K.R. Popper
The London School of Economics and Political Science
London, W.C.2.

BTH 753/H

Münster, 17 November 1947

Dear Mr Popper, thank you very much. It is nice that we are now connected once again and that we can exchange our ideas. It seems to me that behind your report there is once again a real Popper, and I very kindly ask you not to forget us when the announced publication has appeared. The closeness in which you stand to the constructions of Paul Her⟨t⟩z and Mr Tarski, with which we are well acquainted, greatly facilitates our perusal of your interesting approach. In the meantime, Mr Bernays was with us for 14 days in September. We still think back to those nice days with a special pleasure.

With all the best for you
Yours
Heinrich Scholz

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Chapter 32
Popper's Correspondence with Peter Schroeder-Heister

Karl R. Popper and Peter Schroeder-Heister

Abstract Peter Schroeder-Heister (b. 1953) is a German philosopher and logician whose research focuses on the foundations of deductive reasoning, in particular in the field he has called *proof-theoretic semantics* (cf. Schroeder-Heister, 2018; Piecha and Wehmeier, 2022). He worked at the universities of Konstanz and Tübingen, where he has held a professorship in Logic and Philosophy of Language since 1989. Shortly after his Dr. phil. at the University of Bonn in 1981, which he did under the supervision of Gisbert Hasenjaeger (cf. this volume, § 13.10) with Dag Prawitz as an external adviser, he hit upon Popper's logical writings and realized that the inferentialism advocated by Popper bears significant relationships to the idea of proof-theoretic semantics. Subsequently, in 1982, he wrote a paper on Popper's theory of deductive inference stressing in particular the issue of demarcation of logical from extra-logical signs (later published as Schroeder-Heister, 1984). Sending a draft of this paper to Popper led to the correspondence reproduced here.

Editorial notes: The letters from Popper and photocopies or carbon copies of the letters sent to Popper were provided by Peter Schroeder-Heister. The letters received by Popper can be found in KPS Box 347, Folder 20.

32.1 Schroeder-Heister to Popper, 1 July 1982

Typescript on stationery of the University of Konstanz, 1p.

Sir Karl Popper
Fallowfield
Manor Close
Manor Road
Penn, Buckinghamshire
England

D. Binder et al. (eds.), *The Logical Writings of Karl Popper*,
Trends in Logic 58, https://doi.org/10.1007/978-3-030-94926-6_32

1. Juli 1982

Sehr geehrter Herr Professor Popper,

durch Zufall bin ich auf Ihre Aufsätze aus den Jahren 1947–1949 zur Begründung der Logik gestoßen. Die genaue Lektüre hat mir gezeigt, daß viele Ihrer Gedanken es wert sind, erneut in die Diskussion gebracht zu werden. Denn das fundamentale Problem, um das es Ihnen ging – die *Abgrenzung logischer von deskriptiven Konstanten* –, wird in den letzten Jahren im Zusammenhang mit dem wachsenden Interesse an philosophischer Semantik stark diskutiert, jedoch meist ohne Kenntnis Ihrer Arbeiten.

Daher habe ich beiliegenden Aufsatz zur kritischen Rekonstruktion Ihrer Gedanken und ihrer Einordnung in die moderne Diskussion verfaßt. Das Manuskript selbst ist vorläufig; insbesondere soll es noch einen Anhang zur Quantorenlogik erhalten. Trotzdem möchte ich es Ihnen senden, da Sie sich vielleicht für die Fortentwicklung Ihrer Gedanken interessieren. Sollten Sie Zeit haben, einen Blick hineinzuwerfen, wäre ich Ihnen für einen Kommentar dankbar.

Ich hoffe, das Manuskript in absehbarer Zeit endgültig fertigzustellen und an eine philosophische Zeitschrift zu senden, entweder an das „British Journal for the Philosophy of Science", „Mind" oder „Erkenntnis".

Ich reise übrigens heute abend noch ab nach Berlin, um an einem Kolloquium teilzunehmen, das der Physiologe Prof. Grüsser aus Anlaß Ihres 80. Geburtstages veranstaltet, und hoffe, dort einige Gedanken zu Ihrer Philosophie vortragen zu können.

Mit freundlichen Grüßen und herzlichen
Glückwünschen zu Ihrem 80. Geburtstag
Peter Schroeder-Heister

Sir Karl Popper
Fallowfield
Manor Close
Manor Road
Penn, Buckinghamshire
England

1 July 1982

Dear Professor Popper,

By chance I came across your articles from the years 1947–1949 on the justification of logic. A close reading has shown to me that many of your thoughts are worth bringing back into discussion. The fundamental problem you were concerned with – the *demarcation of logical from descriptive constants* – has been the subject of vigorous discussion in recent years in connection with the growing interest in philosophical semantics, but mostly without knowledge of your works.

Therefore I have written the enclosed article on the critical reconstruction of your thoughts and their integration into contemporary discussion. The manuscript itself is preliminary; in particular, it will be expanded by an appendix on quantifier logic. Nevertheless, I would like to send it to you, since you may be interested in the further development of your thoughts. Should you have time to take a look at it, I would be grateful for a comment.

I hope to finalize the manuscript in the foreseeable future and to send it to a philosophical journal, either to the "British Journal for the Philosophy of Science", "Mind" or "Erkenntnis".

By the way, I am leaving for Berlin this evening to take part in a colloquium organized by the physiologist Prof. Grüsser on the occasion of your 80th birthday, and I hope to present some thoughts on your philosophy there.

With kind regards, and heartfelt
congratulations on your 80th birthday
Peter Schroeder-Heister

32.2 Popper to Schroeder-Heister, 9 July 1982

Handwritten, 1p.

Fallowfield, Manor Close, Manor Road
Penn, Bucks,
England

Herrn Peter Schroeder-Heister
Fachgruppe Philosophie
Universität Konstanz
Postfach 5560
D-7750 Konstanz 1

Vertraulich[a]

9–7–82

Sehr geehrter Herr Schroeder-Heister,

herzlichen Dank für Ihren Brief vom 1. Juli 1982 und für Ihr großes Interesse. Ich wurde damals dadurch entmutigt, daß Alfred Tarski, den ich sehr verehre, diese Arbeiten nicht ansehen wollte. Ich hatte sonst niemanden.

Kurz vor seinem Tod lernte ich in Brüssel McKinsey kennen. Wir freundeten uns an: er sprach zuerst zu mir und sagte mir, daß seine Kritik[b] dieser Arbeiten nicht ganz fair war und daß doch mehr in ihnen steckt. Ich erinnere mich, daß wir von Brüssel zusammen nach Paris fuhren und dort im selben Hotel wohnten; auch, daß er mich zum Zug brachte als ich von Paris in die Schweiz fuhr. Er wollte, daß ich zu dieser Arbeit zurückfinde. Aber bevor das geschah starb er.

Aber es gibt natürlich einige interessante unveröffentlichte Resultate. Diese unter

[a] This letter was marked as "confidential", probably because it contained some personal information related to Tarski and McKinsey. As this information, which is more than three decades old, is in no way negative and highly interesting for somebody studying the reception of Popper's logical writings, the recipient of the letter considered it appropriate to release it for publication.

[b] For McKinsey's (1948) negative review cf. this volume, § 13.14.

meinen Papieren zu finden wäre eine große Arbeit. Eher könnte ich sie Ihnen vielleicht einmal beschreiben, am besten mündlich.

Aber ich bin gegenwärtig hauptsächlich an ganz anderen Dingen interessiert. Nur in *einem* Punkt berühren sich meine gegenwärtigen Interessen entfernt mit den damaligen: wann sind Definitionen uninteressant und wann interessant?

Falls Sie Ihr MS veröffentlichen, bitte zitieren Sie was ich in meinen *Ausgangspunkten*[c] über diese Episode sage.

Alles Gute

Ihr Karl Popper

⟨*Linker Rand:*⟩ Leider weiß ich nicht, wann ich dazu kommen werde, Ihre Arbeit zu lesen. Ich hatte eine Operation im März, eine im Ma⟨i⟩, und ich soll die nächste am 11. Juli haben.

Fallowfield, Manor Close, Manor Road
Penn, Bucks,
England

Herrn Peter Schroeder-Heister
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Postfach 5560
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Confidential[a]

9 July 1982

Dear Mr Schroeder-Heister,

Thank you very much for your letter of 1 July 1982, and for your great interest. I was discouraged at the time by the fact that Alfred Tarski, whom I admire very much, did not want to have a look at these works. I had no one else.

Shortly before his death I met McKinsey in Brussels. We became friends: he first spoke to me and told me that his criticism[b] of these works was not quite fair and that there was more in them after all. I remember that we went together from Brussels to Paris and stayed there in the same hotel; also that he brought me to the train when I departed from Paris to Switzerland. He wanted me to find my way back to this work. But before that happened he died.

But of course there are some interesting unpublished results. To find these among my papers would be a big job. Rather, I could perhaps describe them to you one day, preferably orally.

But at the moment I am mainly interested in other things. Only in *one* point my current interests touch remotely with those of that time: when are definitions uninteresting and when are they interesting?

If you publish your MS, please quote what I say in my *Unended Quest*[d] about this episode.

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[c] Popper (2012, § 25).

[d] Popper (1974c, § 25).

All the best

Yours, Karl Popper

⟨*Left margin*⟩
Unfortunatley, I do not know when I will find the time to read your work. I had an operation in March, one in May, and I shall have the next one on July 11th.

32.3 Popper to Schroeder-Heister, 10 July 1982

Handwritten, 1p.

Peter Schroeder-Heister
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Universität Konstanz
Postfach 5560
D-7750 Konstanz
West Germany

⟨*Top right corner:*⟩ Ich hoffe, Sie haben meinen ersten Brief[e] bekommen.

Penn, 10–7–82

Lieber Herr Schroeder-Heister,

Eben habe ich Ihre Arbeit durchgelesen. Ich finde sie ausgezeichnet. Sie haben ganz recht, mein erstes Ziel war die Charakterisierung der logischen Zeichen (formative signs). Aber dann schien es mir daß, falls man damit Erfolg hat, man gleich die Aussagenlogik begründen kann; und irgendwie geht das auch, wenn es auch ein gesonderter Schritt ist. (Aber darüber ein andermal mehr.)

Ihr Artikel ist ausgezeichnet geschrieben, auch rein sprachlich. Inhaltlich *scheint* mir, nach *einer* Lesung, alles richtig zu sein.

Philosophisch waren diese Untersuchungen (für deren teilweisen Mißerfolg ich mich schämte) sehr wichtig für mich, da sie mich in meinem *tentativen Antirelativismus* einigermaßen (= tentativ) bestärkten; insbesondere der Nachweis, der mir wichtig schien, daß in einer Sprache, die die klassiche Negation enthält (und das tut die intuitionistische Logik) „$\neg_{\mathrm{cl}} a /\!/ \neg_{\mathrm{int}} a$" gilt. Aber meines Wissens hat niemand außer Ihnen dieses Resultat gesehen. (Ich kenne die Literatur nicht, außer McKinsey.) Zu Ihrer Anm. 15): you will find that the metalinguistic use of $\forall$ and $\rightarrow$ needed for their full object-linguistic use is *greatly* restricted. [You mention $A \rightarrow (B \rightarrow A)$.] Hier liegt in der Tat etwas Ähnliches vor wie in Hilbert's Programm, eine finite Begründung für einen nicht-finiten Kalkül zu finden.

Besten Dank für dieses schöne Geburtstagsgeschenk!

Ihr Karl Popper

⟨*Left margin*⟩
Man braucht, so glaube ich mich zu erinnern, nicht einmal die ganzen positiv identischen Implikationsformeln

[e] The letter of 9 July 1982 (cf. § 32.2).

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West Germany

⟨*Top right corner:*⟩ I hope you did receive my first letter[e].

Penn, 10 July 1982

Dear Mr Schroeder-Heister,

I have just finished reading your work. I find it excellent. You are quite right, my first goal was the characterization of the logical signs (formative signs). But then it seemed to me that, if one succeeds with it, one can immediately justify propositional logic; and somehow this is possible, although it is a separate step. (But more about this at another time.)

Your article is excellently written, also in terms of language. Contentwise, after *one* reading, everything *seems* to me to be correct.

Philosophically these investigations (for whose partial failure I was ashamed) were very important for me, because they strengthened me to some extent (= tentatively) in my *tentative antirelativism*; especially the demonstration, which seemed important to me, that in a language, which contains classical negation (and this is the case for intuitionistic logic) "$\neg_{\mathrm{cl}} a /\!/ \neg_{\mathrm{int}} a$" holds. But to my knowledge, no one but you has seen this result. (I do not know the literature, except for McKinsey.) Concerning your note 15): you will find that the metalinguistic use of $\forall$ and $\rightarrow$ needed for their full object-linguistic use is *greatly* restricted. [You mention $A \rightarrow (B \rightarrow A)$.] There is indeed something similar here as in Hilbert's programme of finding a finite justification for a non-finite calculus.

Thank you very much for this nice birthday present!

Yours, Karl Popper

⟨*Left margin*⟩
One does not even need, I seem to remember, all the positive identical implication formulas.

32.4 Schroeder-Heister to Popper, 20 July 1982

Typescript on stationery of the University of Konstanz, 1p.

Sir Karl Popper
Fallowfield
Manor Close
Manor Road
Penn, Buckinghamshire

England

20. Juli 1982

Sehr geehrter Herr Professor Popper,

herzlichen Dank für Ihre Briefe vom 9. und 10. Juli. Ich habe mich sehr darüber gefreut, daß Sie sich trotz Ihrer Arbeitsbelastung und bevorstehenden Operation die Zeit genommen haben, meinen Artikel zu lesen. Ganz besonders freue ich mich natürlich, daß er Ihnen gefällt. Das ermutigt mich, ihn in den nächsten Tagen endgültig fertigzumachen und zur Publikation einzureichen.

Vielen Dank auch für Ihr freundliches Angebot, mir einige unveröffentlichte Resultate mündlich zu beschreiben. Ich werde es gerne annehmen, wenn sich bei mir die Gelegenheit einer Reise nach England ergibt.

Ich wünsche Ihnen gute Erholung von Ihrer Operation und nochmals alles Gute zum Geburtstag.

Mit freundlichen Grüßen
Peter Schroeder-Heister

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10 July 1982

Dear Professor Popper,

Thank you very much for your letters of 9 and 10 July. I was very pleased that you took the time to read my article despite your workload and impending operation. Of course I am especially pleased that you liked it. This encourages me to finalise it in the next few days and submit it for publication.

Thank you also for your kind offer to share some unpublished results orally with me. I will gladly accept it if the opportunity for a trip to England arises.

I wish you a good recovery from your operation and once again all the best for your birthday.

With kind regards
Peter Schroeder-Heister

32.5 Popper to Schroeder-Heister, 19 August 1982

Handwritten, 2p. Shortened.

Herrn Peter Schroeder-Heister
Fachgruppe Philosophie
Universität Konstanz

Penn, 19–8–82

Lieber Herr Kollege,

soeben kam (mit einer Unmenge von teilweise wichtigen Briefen) Ihr Brief vom 16. August[f]. Sehr schönen Dank. Ich finde ihn ausgezeichnet. Er scheint mir mehr als

[f] Letter accompanying a photocopy of an earlier version of Schroeder-Heister (1984) which was

vier fünftel (!) meiner alten Intuitionen, Ziele und Hoffnungen zu realisieren, und das übrige fünftel – was Sie „die Begründung der Logik“ nennen – möglich zu machen.

(Meine eigene „Begründung“ der *klassischen* Logik als *Organon der Kritik* ⟨Popper, 1972, p. 64⟩ ist, daß sie das stärkste System der retransmission of falsity ist, und daß wir in der Wissenschaft die schärfste Kritik brauchen.) (Für Beweise in der Mathematik ist es von Interesse, die *schwächsten* Axiomensysteme zu verwenden und auch die schwächsten Ableitungsregeln; und das macht z.B. die intuitionistische Mathematik und die Minimallogik interessant.) Selbstverständlich haben Sie die Erlaubnis, die beiden Fußnoten einzufügen[f].

Aus dem unpublished matter:

The fundamental difference between propositional logic and functional logic seems to be this: in functional logic, deductive or inferential power (/) is not enough. We have to *use*, in addition, =, to express identity of shape. Perhaps this can be done *after* defining = inferentially (i.e. object-linguistic identity). What we need is, for example,

$$a = b \rightarrow a/b \qquad a\binom{x}{y}\binom{y}{z} = a\binom{x}{z}\binom{y}{z}.$$

All the best
Yours sincerely
Karl Popper

⟨*Left margin*⟩
Another old note: I am not sure whether I can deduce:

$$a\binom{y}{x}/b\binom{x}{y} \rightarrow a\binom{y}{x}/Ax\left(b\binom{y}{x}\right)$$

[. . .]

Herrn Peter Schroeder-Heister
Fachgruppe Philosophie
Universität Konstanz

Penn, 19 August 1982

Dear Colleague,

Your letter of 16 August[f] just arrived (together with a huge amount of letters, some of them important). Thank you very much. I find it excellent. It seems to me to realize more than four-fifths (!) of my old intuitions, goals, and hopes, and to make the remaining fifth – what you call “the justification of logic” – possible.

(My own “justification” of *classical* logic as *organon of criticism* ⟨Popper, 1972, p. 64⟩ is that it is the strongest system of retransmission of falsity, and that in science we need the sharpest criticism.) (For proofs in mathematics it is of interest to use the *weakest* axiom systems and also the weakest inference rules; and this makes e.g.

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submitted to the British Journal for the Philosophy of Science. It contained a request for consent to publish two footnotes quoting from letters by Popper (= footnotes 2 and 25 in the printed version). The author remembers this letter, which apparently no longer exists.

intuitionistic mathematics and minimal logic interesting.) Of course, you have permission to insert the two footnotes[f].

From the unpublished matter:

The fundamental difference between propositional logic and functional logic seems to be this: in functional logic, deductive or inferential power (/) is not enough. We have to *use*, in addition, =, to express identity of shape. Perhaps this can be done *after* defining = inferentially (i.e. object-linguistic identity). What we need is, for example,

$$a = b \to a/b \qquad a\binom{x}{y}\binom{y}{z} = a\binom{x}{z}\binom{y}{z}.$$

All the best
Yours sincerely
Karl Popper

⟨*Left margin*⟩
Another old note: I am not sure whether I can deduce:

$$a\binom{y}{x}/b\binom{x}{y} \to a\binom{y}{x}/Ax\left(b\binom{y}{x}\right)$$

[...]

32.6 Schroeder-Heister to Popper, 14 March 1983

Typescript on stationery of the Institute for Advanced Studies in the Humanities, Edinburgh, 1p. Shortened.

P. Schroeder-Heister
bis 30. 4. 1983
(danach wieder:
Universität Konstanz
Fachgruppe Philosophie
Postfach 5560
D-7750 Konstanz)
14. März 1983

Sehr geehrter Herr Professor Popper,

[...]

Mein Manuskript über Ihre Arbeiten zur Logik und zur Abgrenzung der logischen Konstanten, das vom „British Journal for the Philosophy of Science" abgelehnt worden war, habe ich bei Dr. I. Grattan-Guinness eingereicht für seine Zeitschrift „History and Philosophy of Logic", in die es auch thematisch besser paßt. Ich bin zuversichtlich, daß es dort, evtl. in revidierter Form, erscheinen kann.

Ich werde an der Konferenz über „The Philosophy of Language and Logic" vom 28. – 30. 3. in Leicester teilnehmen und dabei die Gelegenheit haben, Ihren Vortrag am 29. März auf der gleichzeitig stattfindenden BSHS/BSPS - Konferenz zu besuchen. Vielleicht ergibt sich dabei die Möglichkeit, sie einmal kurz persönlich zu sprechen. Ich würde mich jedenfalls sehr darüber freuen.

Mit freundlichen Grüßen
Peter Schroeder-Heister

P. Schroeder-Heister
until 30 April 1983
(afterwards again:
Universität Konstanz
Fachgruppe Philosophie
Postfach 5560
D-7750 Konstanz)

14 March 1983

Dear Professor Popper,

[. . .]

I have submitted my manuscript about your works on logic and the demarcation of logical constants, which was rejected by the "British Journal for the Philosophy of Science", to Dr. I. Grattan-Guinness for his journal "History and Philosophy of Logic", into which it also fits better thematically. I am confident that it can appear there, possibly in revised form.

I will attend the conference on "The Philosophy of Language and Logic" on 28–30 March in Leicester and will thus have the opportunity to attend your lecture on 29 March at the simultaneous BSHS/BSPS conference. Perhaps this will provide an opportunity to talk to you in person briefly. In any case, I would be very happy to do so.

With kind regards
Peter Schroeder-Heister

32.7 Popper to Schroeder-Heister, 16 March 1983

Handwritten, 1p. Shortened.

Herrn Peter Schroeder-Heister
Institute for Advanced Studies in the Humanities
University of Edinburgh
17 Buccleuch Place
Edinburgh EH8 9LN

Penn, 16–3–83

Lieber Herr Schroeder-Heister,

schönen Dank für Ihren Brief vom 14. März. Ich freue mich sehr darauf, Sie am 29.3. kennen zu lernen, und nach meiner Vorlesung zu sprechen.

Es ist, natürlich, interessant für mich zu hören, daß Ihre Arbeit über die Abgrenzung der logischen Konstanten vom *BJPS* abgelehnt wurde. Ich kann nur hoffen, daß Dr. Grattan-Guinness weniger Vorurteile (gegen mich!) hat.

Wie Sie vielleicht bemerkt haben, bin ich nicht sehr stolz auf meine logischen Arbeiten! Aber auf meine Arbeiten über (oder gegen) induktive Wahrscheinlichkeit bin ich stolz. [. . .]

Herzlich,
Ihr Karl Popper.

Herrn Peter Schroeder-Heister
Institute for Advanced Studies in the Humanities
University of Edinburgh
17 Buccleuch Place
Edinburgh EH8 9LN

Penn, 16 March 1983

Dear Mr Schroeder-Heister,

Thank you very much for your letter of 14 March. I am very much looking forward to meeting you on 29 March and to speaking with you after my lecture.

It is, of course, interesting for me to learn that your work on the demarcation of logical constants was rejected by the *BJPS*. I can only hope that Dr Grattan-Guinness has fewer prejudices (against me!).

As you may have noticed, I am not very proud of my logical works! But I am proud of my work on (or against) inductive probability. [. . .]

Cordially,
Yours, Karl Popper.

32.8 Schroeder-Heister to Popper, 21 March 1983

Typescript on stationery of the Institute for Advanced Studies in the Humanities, Edinburgh, 1p. Shortened.

21. März 1983

Sehr geehrter Herr Professor Popper,

vielen Dank für Ihren Brief vom 16. März. Es freut mich sehr, daß ich Sie am 29.3. nach Ihrer Vorlesung in Leicester sprechen kann.

Mein Artikel ist inzwischen von „History and Philosophy of Logic" zur Publikation angenommen worden. Dr. Grattan-Guinness hält ihn für sehr gut und ist sehr interessiert daran. Er hat sich sehr viel Mühe gemacht, den Artikel selbst gründlich gelesen (in sehr kurzer Zeit) und eine Reihe wichtiger Verbesserungsvorschläge gemacht. Ich werde ihm die endgültige Fassung noch diese Woche zusenden. Herr Grattan-Guinness hat mich auch auf Ihren Artikel „Creative and Non-Creative Definitions in the Calculus of Probability“[g] aufmerksam gemacht, den ich noch nicht kannte und den ich in meiner Arbeit erwähnen werde.

[. . .]

Herzliche Grüße
P. Schroeder-Heister

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21 March 1983

Dear Professor Popper,

Thank you very much for your letter of March 16th. I am very glad to be able to talk to you on March 29th after your lecture in Leicester.

My article has now been accepted for publication by “History and Philosophy of Logic”. Dr Grattan-Guinness thinks it is very good and is very interested in it. He has taken a great deal of trouble, has read the article thoroughly himself (in a very short time), and has made a number of important suggestions for improvement. I will send him the final version this week. Mr Grattan-Guinness has also drawn my attention to your article “Creative and Non-Creative Definitions in the Calculus of Probability”[g], which I did not know yet and which I will mention in my paper.

[. . .]

Warm regards
P. Schroeder-Heister

32.9 Popper to Schroeder-Heister, 10 August 1983

Handwritten, 2p. Shortened

Dear Dr Schroeder-Heister,

[. . .][h]

Yours,
Karl Popper

P.S. One of my interests in doing these logic papers which you so kindly brought to life again was to show that things like (1) $\langle p(h, eb) = p(h \leftarrow e, eb)\rangle$ are part of the theory of deduction.

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[g] Popper (1963).

[h] This letter, as well as later correspondence between Popper and Schroeder-Heister, discusses the joint work by Popper and David Miller on probabilistic support and countersupport (Popper and Miller, 1983).

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Mit herzlichen Grüßen

Ihr

Karl Popper.

Concordances

Logic without Assumptions (1947)

K. R. Popper (1947b). Logic without Assumptions. In: *Proceedings of the Aristotelian Society* 47, pp. 251–292.

Proceedings	this vol.	Proceedings	this vol.	Proceedings	this vol.
251	83	265	92	279	101
252	84	266	93	280	102
253	84	267	93	281	103
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264	91	278	101	292	110

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New Foundations for Logic (1947)

K. R. Popper (1947c). New Foundations for Logic. In: *Mind* 56 (223), pp. 193–235.

Mind	this vol.	Mind	this vol.	Mind	this vol.	Mind	this vol.
193	113	204	123	215	132	226	141
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202	121	213	130	224	140	235	149
203	122	214	131	225	141		

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KNAW	Indagationes	this volume	KNAW	Indagationes	this volume
1214	561	153	1220	567	159
1215	562	154	1221	568	160
1216	563	155	1222	569	162
1217	564	156	1223	570	163
1218	565	157	1224	571	164
1219	566	158			

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KNAW	Indagationes	this volume	KNAW	Indagationes	this volume
173	44	167	179	50	174
174	45	168	180	51	175
175	46	169	181	52	176
176	47	171	182	53	177
177	48	172	183	54	178
178	49	173			

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KNAW	Indagationes	this volume	KNAW	Indagationes	this volume
322	111	181	327	116	186
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