*WALTER HOERING**

ON JUDGING RATIONALITY

THE RATIONALITY of science, largely taken for granted in the days before Fleck, Hanson, Feyerabend and Th. S. Kuhn, has become highly problematical through their work. Three kinds of arguments have been advanced against the idea of straightforward cumulative development of science. (1) Meaning variance: the idea that *prima facie* identical observation terms occurring in different (non-equivalent) theories are bound to have different meanings, since non-translatable theoretical terms of the two theories will influence the meaning of observation terms in different ways. This cuts off the very basis for a neutral comparison of theories apparently talking about the same range of phenomena. (2) Irrationality of change of standards: if we look upon scientific development as governed by standards of rationality, then a change of these very standards occurring in revolutions cannot be explained in a similar way. (3) Kuhn loss: revolutions bring about a shift of interest away from the blind spots of the new theory: instances, where the old theory was successful and the new theory is not. Seen from this angle, the new theory cannot cogently be argued for — at least not without introducing hypotheses about future successes in the presently neglected areas.

If these arguments are correct then across scientific revolutions we can have rationality neither in the sense of cumulative development with respect to phenomena expressed in a neutral observation language, nor in the sense of scientific development being guided by a set of persistent criteria of rationality. But they cannot be quite correct, since the arguments of the first and third kind at least partly cancel each other (*cf.* also the section on rationality criteria).

Since we do not want to tie (ir)rationality too closely to the concept of revolution itself, we shall aim at an independent characterisation. A first step is to decide to what kinds of entities one wants to ascribe rationality.

What Can Be Rational?

Actions Not

John chops off his right index finger. Is this rational? . . . Yes, it is. That's

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how John wants to avoid being drafted into the army . . No, it isn't, because in John's country draft avoiders are executed.

Roberto drops stones from a building. Rational? . . . Yes, if done to discover the laws governing the free fall of bodies and nobody is around who may be hurt . . . Not really, if there are better ways of discovering these laws.

So it seems that in life as in science one should avoid judging isolated actions. Their rationality will very much depend on the context, on the plan followed by the agent, on his strategy.

Strategies, Yes, Conditionally

The situation just described for actions may also occur with strategies. *E.g.* a strategy for systematically searching the mountains of southern Tibet for traces of the yeti may be utter nonsense in the context of normal bacteriological research. This changes, however, if we want to get comprehensive information concerning the influence of cold temperatures and high altitudes on the bacterial fauna of primates.

Thus a strategy which does not make sense if embedded in one large strategy may be perfectly rational in the context of some other comprehensive strategy.

The difficulty just encountered disappears however, if we only compare total or maximal strategies. But others remain. They center around the problem of applying the conceptual framework of game theory to 'games of science'. If one thinks of science as a two-person game, scientist playing against nature, one should be informed about the possible moves of the two participants. But this is difficult already for reasons of language. For describing their conditional decisions one will either have to use just ordinary language or that language plus the language of some scientific theory. In the first case it will be difficult to separate scientific from non-scientific actions (of the scientist). In the second case one cannot avoid presuppositions and hypotheses attached to the concepts of the theory involved.

If *e.g.* we want to find out the relative advantages of different methods of weighing, we will have to assume that bodies do not change their weight under certain conditions and we will have to take into account the buoyancy of substances in air.

Strategies of science thus have to be described in the terminology of the science in question, which implies that the description of a situation in theoretical terms is to be taken as a shorthand for a multitude of factually equivalent descriptions in observation terms, the equivalence of these descriptions and their relation to the theoretical description being at least partly given by interpretation rules and theoretical laws.

Even so it will be difficult to formulate a strategy in the sense of a *complete* set of conditional decisions giving answers of the scientist to *each* possible move of nature. But this is not necessary: knowing a large number of laws of nature, we know many of her conditional responses in our game of science. If we can localize the gaps of our knowledge, we can set up fairly complete strategies for finding and checking of new laws for limited areas. Whether such a strategy is falsified by yielding a false law, is an empirical matter.

Methodologies Not

It will not be so easy to falsify methodologies — as Lakatos uses the term — or it will be too easy. On the one hand they are 'characterized by rules governing the (scientific) acceptance and rejection of theories or research programmes',[1] on the other (without being overtly probabilistic) they are not supposed to be 'mechanical'. This 'non-mechanical' construction can surely be used as a protective belt against falsification. A methodology thus is a large bundle of strategies, so large in fact that some of them will yield true laws and some not — no matter what the world looks like.

This can be seen most easily for falsificationism or inductivism. Supposedly these methodologies contradict each other. But they agree in vagueness. Even if taken in one of their more elaborate forms, they do not provide a unique selection among the theories consistent with (unfalsified by or constituting generalizations of) the experimental data hitherto gathered. There are always many mutually inconsistent ways of subsuming a finite set or singular sentences under a general law or theory — which of course is unfalsified by them. This has been known to scientists all along and has been made quite explicit for philosophers by N. Goodman.[2]

In other words: methodologies contain adjustable parameters which could only be determined using additional restrictions foreign to the methodologies themselves. These would be, *e.g.* for inductivism, the choice of predicates to which to apply the inductive rule and the size (at least) of the inductive basis required in each case; for falsificationism, the minimal length (at least) of severe test series, which may well depend on the kind of law tested; for the methodology of research programmes, the choice of the hard core and the degenerative period allowed before switching the programme.

To sum up the contents of this section. One cannot argue seriously for or against a methodology as long as it can comprise contradictory strategies. Methodologies are by their very generality and irreducible vagueness no appropriate subjects for judgements of rationality.

Now that we have delimited the domain of objects eligible for judgements of rationality, we can further enquire into the logical structure of this concept.

[1]Lakatos, 'History of Science and its Rational Reconstructions', *Boston Stud.* **8** (1971), 92. R. C. Buck and R. S. Cohen (eds.).

[2]Goodman, *Fact, Fiction and Forecast*, 2nd edn. (New York, 1965).

A Logical Form for Rationality

Do we want to conceive of rationality as a qualitative, comparative or quantitative concept? What are the relevant variables, if it is to be a relation? That's what we mean, when we ask about the 'logical form' of rationality.

We shall not try to give a final answer at once, but will rather use a method of successive approximations. We shall first propose a rather crude relational structure and then try to improve it gradually, by doing more work on the variables, splitting them up, specifying them more clearly, etc.

We start with construing rationality as a comparative concept ('more rational than'), relating possible reactions of a scientist or a scientific community to a situation: it is more rational for A to react to situation B with C than with D. Agent, action and situation will of course have to be discussed in greater detail. We shall not argue for adopting just the variables mentioned. It is obvious, however, that all of them are essential in the sense that there are a and b such that $R..a... \not\leftrightarrow R..b...$ (other places in R being kept constant).

We now discuss the argument places in turn.

Comparative Rationality

The most cogent reason for adopting a comparative concept of rationality is that it is not affected by the introduction or invention of new kinds of action or strategies. This would be different for the qualitative concept. In more detail: if we deem C to be more rational than D, this relation will not be changed by the advent of a new action or strategy E no matter whether it is more rational or less rational than C or D or both. In the qualitative case, however, a strategy at first labelled as rational might have to be relabelled as irrational after the introduction of a new and much more rational strategy. Thus one sees that comparative rationality is more stable than the qualitative concept. But it is also more adequate to a situation of choice, and more informative.

Processes unequivocally determined by laws of nature are considered neither rational nor irrational. Only when we refer to actions based on decisions do we speak of rationality. Implicit in every decision is a preceding decision concerning the range of alternatives contemplated. With respect to them we have to try to emancipate ourselves from pre-existing habits and dispositions in order to avoid choice by prejudice or instinct, which would not in fact involve any decision but would resemble a blind natural process. 'Decision' implies that there are at least two possible courses of action to choose from. Thus there is an element of comparison also in the apparently purely qualitative locution (I): 'It is rational to do A in situation S'. This can be brought out by rephrasing the sentence using the comparative concept (II): 'In situation S there is no more rational alternative action or strategy available than *A*'. Thus it is easy to retrieve a qualitative relation from a comparative one. The opposite is impossible or at best leads to a rather trivial result, as one can see reading (II) as the defining condition for a comparative concept. That would agree with the generally accepted view, that comparative concepts are refinements of corresponding qualitative ones.

Situations, Actions and Strategies

Although actions and situations occupy different argument places in our rationality relation, they are closely related. Since even a fairly complete description of the actual situation of some agent is out of the question, one has to select the relevant traits — relevant that is for the kind and context of action envisaged.

So let us, without aiming at completeness, briefly go through some contexts of scientific action, before we settle down to treating in more detail the theoretical context of accepting and rejecting laws or strategies on the basis of previous experiences and accepted metalaws. First we have the experimental context of the planning and carrying out of experiments. This as a rule presupposes an effort of a technical kind involving the construction and building of apparatus. Evaluating the accomplishments and abilities of persons characterizes the context of personal decisions. These kinds of action and also actions in a political context, like influencing public opinion in favour of science in general or of a specific project, are all part of the strategies of present-day science.

It seems that these activities are easier to accommodate in a philosophy of science based on values, actions and strategies, than in a theory which instead focuses on observations and on accepting or rejecting laws.

In this paper, however, we shall mostly limit ourselves to strategies in this latter, narrower area.

As can be seen from the above enumeration, 'situation' is meant to cover all determinants of action. Another method of grouping them, which cuts across the first one, is relevant in a more theoretical way. It distinguishes *internal* from *external* determinants. Among the first we count the *value system* adopted and the *laws* and *metalaws* (laws about laws) accepted.

If we insist on assigning rationality to single actions, we can in the present framework do so, provided we speak of rationality relative to a strategy and include that strategy in the set of internal determinants.

The *external* part of the 'situation' will consist of the relevant traits of the actual situation surrounding the agent, as mirrored in his past observations (observation reports).

Agents

As agents we envisage either individual scientists or scientific communities.

How individual strategies which do not look particularly rational may combine into a more rational collective strategy will be sketched in the section on irrationality.

Rationality Again

Can the ingredients of our preceding discussion be put together in such a way that we get an explication of rationality in terms of them? Let us try!

R. It is more rational for A to react to situation B with C rather than with D *if and only if* it follows from B that the outcome of applying C to $\bar{B}$ is preferred by A to the outcome of applying D to $\bar{B}$.

Glossary

$\bar{B}$ = the actual external and internal situation described by B (it would not make sense to apply a strategy to the description of a situation).

the expected outcome . . . = the outcome of applying C to $\bar{B}$ derivable from (laws, metalaws, external situation in) B and the description of C.

C and D are understood to be strategies. A strategy should tell you in each situation that might possibly occur what to do. For practical reasons no strategy actually employed will be complete. So strategies assign actions to (most) situations. This assignment can be thought of as effected by an (in)deterministic Turing machine in the case of (in)deterministic strategies.

indeterministic Turing machine (TM): an indeterministic TM assigns to its argument a finite set of values — not one value, as a deterministic TM would. The idea is that in each run of a realisation of an indeterministic TM one value is selected arbitrarily from this finite set.

outcome: this could be either the situation encountered after applying A (or B), or, if we can go on indefinitely applying these strategies, the sequence thus obtained.

prefer: in the first of the two cases just mentioned this would be the preference relation determined by the valuation of situations, which we presuppose; in the second case one would have to transfer the preference relation in a reasonable way to sequences of situations.

This problem of transferring preference from elements to sets and sequences becomes still more serious if we consider indeterministic strategies and laws. Such a strategy assigns to a situation s a set of situations (the set of situations obtainable from s by any course of events admitted by strategy and laws) in the terminating case, and sequences of such, in case the application of the strategy goes on indefinitely. The problem of transferring orderings has obvious solutions in special cases which depends on the decision function chosen, but no general solution. Thus one will have to accord neutral preference to pairs of strategies in many cases.

The complications encountered should not deceive us about the preliminary nature of our analysis. What we have obtained is just the rough outline of a possible more exact explication of comparative rationality.

Truth and Strategies

Our aim in the preceding sections was to let the outlines of an explication of rationality become visible as quickly as possible. In this section we want to catch up with some details.

Strategies

A systematic procedure of switching from one strategy to another in certain situations should also be a strategy. The explication of strategy given in the last section is deficient in this respect. This can be remedied easily by an inductive definition: effective (in)deterministic partial functions assigning actions to situations are strategies. Situations are strategies. Finite sets of strategies are strategies. Effective (in)deterministic partial functions from strategies to strategies are strategies.

Metalaws

We want to give some examples of metalaws, in order to prepare for a more exact treatment in the section next but one. When we claim the Lorentz-invariance of the basic laws of physics, or when we maintain that all radioactive materials decay exponentially, even when we view the general gas equation as covering all instances of Charles' law, are we using laws about (other) laws or, as we say, metalaws? The example of the gas law shows us that the same law of physics may alternatively be viewed as law or as metalaw. Invariance claims, on the other hand, are most naturally rendered as metalaws, since they involve the operation of substitution in certain expressions of a mathematical language of physics, and substitution is normally taken to lie outside that language.

Laws About Strategies

When we say *e.g.* that a certain falsificationistic strategy is equivalent to a certain inductivistic strategy, we propose a law connecting these two strategies. True laws of that kind can be useful for selecting new strategies. Therefore we shall extend our explication of truth to cover these laws also.

Lifting Truth

We have seen before that the process of lifting the preference relation from elements to sets and sequences is not unproblematic. Transferring the concept of truth on the other hand from a valuation of the atomic sentences of a basic (observation-)language to its first- and higher order metalanguages, to strategies and laws about them seems to be quite straightforward. The first steps have been described in Hoering.[3] We start with a language L_0 containing a finite number of predicates and denumerably many individual constants and with a valuation V of all atomic sentences. This specifies in the usual way truth values for all closed expressions of L_0. In the first metalanguage L_1 we want to be able to say that classes of sentences of L_0 are true (made true by V). Therefore it shall contain means of naming expressions of L_0, predicates applying to these names, *e.g.* a truth predicate T_0 expressing truth-in-L_0. We can choose a Gödel-numbering to produce the names, and arithmetical predicates for addition and multiplication to construct predicates to apply to the numerical names. The truth definition for L_1 comprises the truth definition for arithmetic, the true (by V) instances of T_0x, and proceeds from these atomic sentences to a valuation of all closed sentences of L_1. Going on to truth definitions for L_2, and more generally L_n, presents no difficulties.

In the case of strategies the only problem is how to represent situations and actions. Let us assume that we can observe finite sets of atomic sentences (their truth values, that is): these are the situations; and that we can react by accepting the truth or falsity of a further atomic sentence (which characterises the action taken). Actions thus are signed (negated or unnegated) atomic sentences, situations finite sets of them. We can now follow the clauses of our definition of strategy. Finite sets of strategies are true iff (if and only if) each of them is true. Strategies are true (by V) iff they take true situations or strategies into true actions or strategies. Whoever has followed us thus far, will have no problem with filling in the remaining explication of truth for laws about strategies.

Relative Rationality of Classes of Strategies

Let us now return to earth, to simple strategies for predicting the truth-value of singular sentences, some of them not even using laws, but random guesses, some using falsifiable laws, and some falsifiable metalaws. That this sequence of kinds of strategy proceeds from less to more successful ones is to be expected. It is interesting, however, that this can also be proved.[4] If one takes the expected number of successful predictions as measure of quality, then, under very general assumptions, strategies employing metalaws fare better than the corresponding ones without them, and these in turn fare better than purely random strategies.

[3]Hoering, 'Normale Wissenschaft und Paradigmawechsel durch die Brille eines Wissenschaftstheoretikers gesehen', *Die Bedeutung der Wissenschaftsgeschichte für die Wissenschaftstheorie, Studia Leibnitiana,* Sonderband 6 (1977), W. Totok (ed.).

[4]Hoering, Induktion, Spiele, Metagesetz (Ms, 1974), available on demand.

In this field of comparing classes of strategies under rather weak conditions one can indeed get comparisons of rationality on purely theoretical grounds.

Application to Case Studies

What benefit can the historian of science derive for his work, from constructions like the ones presented above? He certainly cannot apply them as they stand. But they can direct his interest. You find things more easily, if you know what to look for. And values, dispositions, strategies, metalaws are certainly worth looking for, since they are functional parts of the machinery of science. On the other hand, a fairly clear appreciation of the structure of these concepts can make us wary of the places where we might want to but should not jump to conclusions. Thus, for instance, the correct retrieval of a probabilistic strategy from some actual course of events is a highly improbable matter.

Values and Strategies

A Boundary Condition

When we are doing science, we *can* do this against the biological and personal values governing our personal well-being. If some people do so, this *may* even be good for mankind. But not if all do. The values of science must not contradict the values of survival. This is not to say that the search for truth will be most effective, if conducted as a search for the useful. Indeed, it is rather the other way round!

We see from our examples that the question of scientific rationality can arise on (at least) two levels. (1) Rationality of (a strategy of) science with respect to the values of survival of a person or a group. (2) Rationality (of a scientific strategy) within science, with respect to an existing framework of scientific values and assumptions.

In both cases the attribution of rationality will depend crucially on hypotheses, laws and assumptions which can only be made plausible or probable but which cannot be proved in a final way. In both cases the 'values from above' must match the 'values from below'; the effects must match the costs if a strategy is to be called rational.

Values Without Decision Rules Have No Value

The role of values should not be overestimated. They only influence actions via decision rules and there are several of them: maximizing the expectation value, minimum regret, etc. So one can view valuations as theoretical concepts, receiving their interpretation only through decision rules. We suspect that any strategy given as set of conditional decisions, and resulting from one decision rule and a valuation of situations, could also be obtained from some (any?) other decision rule plus suitable valuation.

Non-Linear Orderings are Unavoidable and Lead to Indeterministic Strategies

Even if value systems do not tell the whole story, there is a kind of ordering which is bound to influence the decision rule: non-linear ordering. This type of order is unavoidable. It will be obtained for instance: (1) If you are comparing theories with regard to their ranges of application, you are using in effect the partial ordering of sets by inclusion. (2) If you compare theories with respect to several independent criteria, *e.g.* exactness, simplicity and range of application, you are led to a direct product of the value orderings of these components: you will prefer one theory to some other, if it excels over the other in at least one of these criteria and fares at least equally well with respect to the other. You will have no preference (*i.e.* incomparability of the theories) if two criteria yield incompatible orderings.

In these cases it is easy and seems adequate to formulate indeterministic tie-break rules for incomparable options — more adequate at least than an artificial linearization by an *ad hoc* deterministic rule. Thus it seems to be a natural move to introduce indeterministic strategies.

Application to Case Studies

It will not be possible to derive the value system of some historical agent from his actions, if one is not sure about his or her decision rule. And this will not normally be the case. The same point was made in Stegmüller.[5]

Forms of Irrationality

In order to make our account of rationality come out more clearly by way of contrast, let us describe some kinds of irrationality. We may be inclined to accord irrationality to somebody else's actions, because we don't share his or her beliefs and values. But there are also kinds of irrationality, characterised by objective criteria.

Acting Against Plan

If you act against the advice of the strategy you have chosen in accordance with your value system, what you do in effect is to fall into some other strategy which may no longer be consistent with your values. On the one hand you may be in for surprise discoveries as was, *e.g.* Fleming; on the other you may be punished by incurring unnecessary expenses. The first possibility seems to be less frequent than the second. False conscience, being ill-informed about the

[5]Stegmüller, *Erklärung und Begründung* (Berlin, 1969).

strategy one is actually following, can be regarded as a subspecies of acting against plan.

Contradictory Values

If a sequence of actions is valued higher or lower than the sum of its components, the strategy resulting from such a valuation is likely to be less than optimal. One may regard it as an advantage of our description of values and strategies that such a contradiction can be made explicit.

Dogmatism

It seems fair to say, that no theory or method has survived a lengthy period of time without modification of content or of range of application.

If we accept this as a basis of induction, we should beware of *final* acceptance of any such presupposition; we should not accept a strategy which in a final way bars certain areas of the space of possible laws or methods. Thus, to this extent at least, 'methodological anarchism' can be given a completely rational basis.

Conflicts of Rationalities

The fact that different people can have strongly divergent views and values is *not* — in our opinion — a threat to the overall rationality of science. On the contrary! It ensures, that the problems are attacked on a broad front, as it were, and that 'far out' ideas get a fair chance of examination.

The historical picture of course gets distorted and imparts a slanted view of the collective strategy of science, if 'losers' are forgotten and 'winners' remembered.

If there is a tie between two fractions of a scientific community for some period of time, as there was one between the adherents of the corpuscular theory of light on the one hand and of the undulatory theory on the other in the time between Newton and Fresnel, this can be taken to be the objectivized expression of a situation, which is often not so easy to realise for the individual mind, namely that the arguments of either side are not conclusive.

Problems of Justification

Laws

It has been known since the days of Hume that a logical justification of laws from observation is impossible. If we need a contigent premiss to justify our acceptance of laws, how can we justify this premiss — without invoking a further premiss etc.?

There are just two decisions you have to make to free yourself from the immobilizing fear of infinite regress. (1) Resolve to accept *some* laws without justification in the strict sense. (2) Accept contingent general premisses or metalaws. You want to do induction or falsification in *this* world, not in every possible world. At the level of active science these decisions are generally taken without hesitation or reflection. At the level of philosophy of science the first one is part of the falsificationistic procedure and the second is only apparently avoided by it. Namely: if you decide to accept laws if severely tested and unfalsified, this amounts to the hypothesis that a certain range of testing is sufficient. Thus there may be many laws justifiable relative to observation and one general premiss, which itself has no stronger justification than successfully performing this service. (Newton's second law seems to play structurally a similar role as the general premisses, which govern falsification or induction. It is necessary for arriving at special laws, the force laws.) This procedure of relative falsification is fallible of course but self-correcting. It may be iterated by collecting many general premisses and looking for their justifying general premiss. This yields a hierarchical structure closely related to our hierarchy of strategies.

Rationality Criteria

The problem of rationally explaining the change of criteria of rationality, which was mentioned in the introduction, arises only if one misreads higher order laws as criteria in the sense of necessary conditions. It is true that today we expect new laws of physics to conform, for instance, with conservation of energy and Lorentz-invariance. We (some of us grudgingly) accept statistical laws. Not long ago it was different, when Galilei-invariance and deterministic character were expected. If higher order laws simplify the search for new laws so reliably that one uses them as guidelines for some considerable period, that does not prove them to be true, and they should not to be used as criteria for new laws. The only valid criteria for a law are correct prediction of observations and simplicity (in that order). This fact must not be obscured by the possibility of disagreement about what to count as observation or what to regard as an observation term. We can always, if necessary, go back to descriptions of observations and apparatus in ordinary language or a quite uncontroversial fragment of it. (For a somewhat more detailed argumentation see Hoering[6] or Rapp[7].)

Thus the criterion for accepting a law of science never changes; what may change is the place where it is applied, lower or higher in the hierarchical network of laws, the accompanying hypotheses, and the secondary, tertiary etc., 'criteria', which can never be assumed to be final.

If in fact we do not change rationality standards, argument (2) of the

[6]Hoering, Hypotheses attached to theoretical concepts, talk given at the Helsinki Colloquium, December 1977, *Acta Philos. Fennica* **2** (1978), 179.

[7]F. Rapp, 'Observational Data and Scientific Progress', *Stud. Hist. Phil. Sci.* **11** (1980), 153–162.

introduction is void. But similarly, the first kind of argument would collapse if the above-mentioned reduction to ordinary language can be shown to work. Arguments of the third kind, it seems, cannot be so globally dimissed. They demand careful analysis in each special case.

Evaluating Case Studies

When we try to assess the rationality of a certain historical development, we are just undertaking a special kind of case study. So the general problems besetting the evaluation of historical case studies will also be our problems. Let us mention three of them.

1. *Bias of presentation.* It is generally agreed that a completely neutral description of 'what happened' is almost impossible except in the most trivial cases. If there is no unbiased description, one should be on the alert and take precautions against special kinds of bias, consciously or unconsciously introduced.

2. *Circularity of method.* That instance *a* confirms theory *T* may itself be taken as instance confirming a certain theory of confirmation — or isn't this procedure circular? — More generally: in the process of trying to evaluate case studies for the purposes of the philosophy of science, we often have to presuppose the solution of problems, which philosophy of science itself has not yet satisfactorily solved.

3. *Testing fuzzy theories.* If one tries to put to test conceptions like Popper's, Kuhn's or Lakatos's on the development of science, one cannot but be impressed by the ease with which they can be stretched to fit given cases. This may partly be due to the existence of theoretical terms within these conceptions. But similarly, attempts to make some of these conceptions more precise[8] use (theoretical) concepts (like 'core'), whose interpretation in concrete cases is not uniquely determined. One way out is to test the (fuzzy) theory plus one of its more definite interpretations, even if one has to face the likely objection, that one has not tested the original theory.

If one tries to formulate a common trait of the problems outlined, one can say that they agree in showing some indeterminateness of method or interpretation. One way of dealing with such a situation is to apply to it a method of trial and error. For that — and also for any other method for dealing with the situation — one has to know what constitutes an error, or else at least to be able to compare errors, to estimate their relative magnitude. This consideration could be given a more precise form and turned into a condition which seems to be reasonable to require of theories to be tested, like Kuhn's, and relations to be specified, like rationality. We shall not do this here, but only remark that for our relation of rationality we do have a means for

[8] Stegmüller, *Theorie und Erfahrung* II (2) (1973), 221, 224ff; English edition: *The Structure and Dynamics of Theories* (Berlin: Springer, 1976).

comparing its specializations with cases studied, namely the set of actual scientists' decisions correctly reproduced or at least admitted by the relation.

It should be obvious that in a comparative situation like this, isolated case studies have about the same value as the verification of a law by one single measurement. What one needs here are *comparative case studies:* the testing of several (meta-historical) theories or specifications of rationality in several cases.

Summary

We have tried to prepare a basis for discussions of rationality by proposing to reconstruct this concept in the form of a comparative relation: it is more rational for A to react to situation B with strategy C than with strategy D. Choosing this form for judgements of rationality makes strategies their subject, not isolated actions, and obliges one explicitly to acknowledge the agent's situation (values, beliefs etc.). A proposal for a concrete rationality relation has not been put forward. We have, however, discussed in some detail the role of value systems and strategies as ingredients of the relation. It is not claimed that the variables selected have already been chosen in an optimal way. Possible lines of improvement were indicated and also connections with the general problems of historical case studies.

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