

# The Interrelations between the Philosophy, History and Sociology of Science in Thomas Kuhn's Theory of Scientific Development

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## ABSTRACT

The paper deals with the interrelations between the philosophy, sociology and historiography of science in Thomas Kuhn's theory of scientific development. First, the historiography of science provides the basis for both the philosophy and sociology of science in the sense that the fundamental questions of both disciplines depend on the principles of the form of historiography employed. Second, the fusion of the sociology and philosophy of science, as advocated by Kuhn, is discussed. This fusion consists essentially in a replacement of methodological rules by cognitive values that influence the decisions of scientific communities. As a consequence, the question of the rationality of theory choice arises, both with respect to the actual decisions and to the possible justification of cognitive values and their change.

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## I INTRODUCTION

Without doubt, Thomas S. Kuhn has been one of the dominant figures in the metasciences of the last three decades, be it the history, sociology or philosophy of science. In short, Kuhn's influence on the philosophy of science consists in turn from a normative-synchronous orientation to a more descriptive-developmental one, and in sociology of science from norms independent of the subject matter of the respective science to norms dependent on it. In spite of the fact that Kuhn has been widely read, it seems worthwhile to scrutinize his

views about the interrelations among the philosophy, sociology and history of science since much of what is being said about Kuhn is fairly superficial and in part just wrong. Kuhn has repeatedly deplored the inaccurate reception of his work though he does not deny his own contribution to it.<sup>1</sup> But the inspirational force of Kuhn's work on the interrelations mentioned does not appear to have been exhausted, and the problems inherent in it have apparently not been articulated sharply enough.

I shall deal with the interrelations among the philosophy, history, and sociology of science in Kuhn in the following four sections. First, I shall discuss why and in which sense the history of science provides a basis for the philosophy and sociology of science. Second, I shall treat the interrelation between the philosophy and sociology of science. Third, this will lead to the question of the rationality of theory choice since individual evaluations of competing theories can diverge. Fourth, we will have to ask whether cognitive values and their change is justifiable. I shall conclude with a summary.

## 2 THE HISTORY OF SCIENCE AS A BASIS FOR THE SOCIOLOGY AND PHILOSOPHY OF SCIENCE

The introductory chapter of Kuhn's *Structure of Scientific Revolutions* (hereafter SSR) opens with the following, often-cited sentence:

History, if viewed as a repository for more than anecdote or chronology, could produce a decisive transformation in the image of science by which we are now possessed (SSR, p. 1).

Kuhn's theory consists in viewing history as a repository for more than anecdote and chronology and in drawing consequences from this altered viewpoint, namely a new image of science and its development. But what is meant by 'history'? Kuhn means what he elsewhere calls 'the new internal historiography of science', a kind of historiography that has been institutionalized and professionalized only within the last three decades or so, mainly in the English-speaking world.<sup>2</sup> This kind of historiography intends to make up for what had already happened in other humanities in the second half of the nineteenth century: the overcoming of ethnocentric and presentist biases, that is the projection of the present into the past. It is those biases for which the so-called 'older internal historiography of science' has been reproached, and against which the new historiography reacts. But this older form of historiography determined, until recently, the common image of science entertained by laypersons, scientists and philosophers. This presentist sort of historiography allows the history of science to appear as a cumulative growth

<sup>1</sup> See Hoyningen-Huene [1989], p. 7, fn. 3 and p. 8, fn. 4 for references.

<sup>2</sup> Kuhn [1968], pp. 110–12, [1979], pp. 121–2, [1984], [1986]; compare also Hoyningen-Huene [1989], Chapter 1.

of knowledge in which later progress never essentially changes earlier knowledge, apart from an increase in precision or from some non-essential restriction.

But this image of science is deceptive. It is, according to Kuhn, as authentic as 'an image of a national culture drawn from a tourist brochure or a language text' (SSR, p. 1). In summary, this deceptive image originates through an assimilation of past science to present science by the older historiographic tradition, and this happens mainly in two ways. First, the selection of what will be part of the historical narrative is guided by the content of present science: only those elements of past science that are parts of present science are seen as historically valuable. Second, what is historically valuable (by the said criterion) is represented by means of the concepts of present science which may lead to serious distortion of older knowledge. In short: the older historiography of science does not allow for the possible strangeness and oddness of the older science, for its being substantially different from today's science—like the older anthropology that evaluated foreign cultures in terms of the values of the home culture.

According to Kuhn, we were possessed by such a deceptive image of science, and the philosophy, sociology, and history of science oriented themselves from such an image—and they perpetuated it. But neither the philosophy nor sociology of science must trust this deceptive image of science any longer, in the same way as talk of primitive cultures has become highly problematic. But how can we gain an undistorted image of past science? As in other areas where one tries to free oneself from ethnocentric and presentist biases, the key to an understanding of a foreign culture is a *hermeneutic* reading of *suitable* writings. Only contemporary writings are *suitable*, such as published sources like articles and textbooks, or unpublished material like letters, diaries, lab reports, etc. *Hermeneutic* reading of these sources tries to draw their sense as far as possible from them, as opposed to projecting concepts, problems and standards of today's science into them—even if this goal cannot be totally reached. Only a historiography with such a methodological set-up, which tries to recover the strangeness and oddness of some older science by stubborn hermeneutics, only such a historiography is allowed to produce the data that philosophy and sociology have to deal with. The image of science that results on the basis of such a historiography is, however, quite different from the usual one, and, more important in our context, it provokes quite different sociological and philosophical questions. I remind you of three fairly central points.

First, the development of a certain discipline can only be understood with reference to the relevant scientific community. It is not an abstract 'logic of scientific discovery', a universal methodology, *the* scientific method, that governs scientific work: science is not a rule-governed enterprise such that in a given situation the decision to be made will be uniquely determined, one and

the same for every participating scientist. In the jargon of game theory: science is not a one-person game. Rather, there is a system of cognitive values that influences individual decisions without determining them. Moreover, this system of cognitive values varies somewhat from community to community and in the course of time. This is one of the crucial points where Kuhn departs from the philosophical tradition, and he calls it the 'sociological basis of my position'. I will come back to this topic in detail in the following section.

A second result of the new internal historiography is the, by now, well-known distinction between two phases of scientific development, with respect to which very different sociological and philosophical questions must be asked. During *normal science*, for instance, fundamental theories are neither tested nor confirmed. It follows that the questions of confirmation theory that were of prime importance in standard philosophy of science cannot even be asked (compare Hoyningen-Huene [1989], Chapter 5). But also in *extraordinary science*, the typical confrontation of empirical data with one theory does not occur since we have here a comparison of at least *two* theories with respect to their relative problem-solving capacity (compare Hoyningen-Huene [1989], Section 7.4.a, pp. 231–3).

A similarly controversial result of the new historiography of science is the assertion that concomitant to theory change there is a more or less subtle change of scientific concepts which leads to a relationship of successive theories that Kuhn calls 'incommensurable' (compare Hoyningen-Huene [1989], Section 6.3, pp. 202–17, and Hoyningen-Huene [1990]). Whatever the exact sense of this word is: if there is such a thing as conceptual shifts in the course of theory changes—and whether there is is primarily a historical question—if there are conceptual shifts, a host of new questions must be asked in the philosophy of science. For instance: in which way is communication and argument possible across the revolutionary divide (compare Hoyningen-Huene [1989], Section 7.5, pp. 245–51)? What can scientific progress mean if it is not exclusively to be understood as cumulative (compare Hoyningen-Huene [1989], Section 7.6, pp. 251–6)? What could scientific rationality be (compare Hoyningen-Huene [1989], Section 7.4.b, pp. 233–8)?, and more.

By sketching these three topics I wanted only to show that and how the sociology and philosophy of science are dependent on the history of science. The upshot is: the history of science already determines, among other things, the realm of questions that can, in a sociological or philosophical perspective, be sensibly asked with respect to science. But now I shall turn to the question how the relationship between philosophy and sociology of science has to be conceived.

### 3 THE INTERRELATION BETWEEN THE PHILOSOPHY AND SOCIOLOGY OF SCIENCE

It is well known that Kuhn quite shocked the philosophy of science by a number of assertions,<sup>3</sup> one of which disputes the autonomy of the philosophy of science. This assertion can be articulated in two ways.

First, Kuhn asserts that the separation of the context of discovery from the context of justification is not justified. It is well known that this separation is the starting point of both logical positivism and critical rationalism. According to these traditions, the context of discovery is the subject matter of empirical, metascientific disciplines like psychology, sociology and the history of science. The philosophy of science, on the other hand, deals with epistemological questions, with questions of critical evaluation of knowledge claims; its subject matter thus belongs to the context of justification. The epistemological problems can and must be treated independently of empirical questions. I will not analyse further, let alone defend Kuhn's assertion that the separation of these so-called contexts cannot be upheld. The reason is that the context distinction is affected by notorious obscurities which have seduced many critics of the distinction, including Kuhn, to obscure criticism.<sup>4</sup>

In a different terminology but with identical intention, Kuhn has articulated his criticism of the claim of autonomy of the philosophy of science in the following way:

[T]hough science is practiced by individuals, scientific knowledge is intrinsically a *group* product and . . . neither its peculiar efficacy nor the manner in which it develops will be understood without reference to the special nature of the groups that produce it. In this sense my work has been deeply sociological, but not in a way that permits that subject to be separated from epistemology (Kuhn [1977a], p. XX).

The claim is that the philosophy and sociology of science cannot be practised independently of each other. But before coming to the argument for this claim, one possible misunderstanding must be removed. It is not meant that *each and every* question in the sociology of science can only be answered after some marriage of convenience with the philosophy of science. Of course, Kuhn would never deny that, for instance, many of the questions that belong to the chapter of sociology entitled 'Science and Society' can be treated autonomously by sociology. What is meant, however, is first that the treatment of *some* allegedly pure philosophical questions, such as those about the dynamics of theories or about scientific progress, necessarily involves sociological

<sup>3</sup> For an account of the main shocked reactions see, e.g., Stegmüller [1973], pp. 153–83, [1974], Hacking [1981], pp. 1–5.

<sup>4</sup> For a reconstruction of Kuhn's criticism of the context distinction, see Hoyningen-Huene [1989], Section 7.4.c, pp. 238–45; for a general discussion of the distinction, see Hoyningen-Huene [1987].

aspects. Second, it is meant that the treatment of *some* allegedly pure sociological questions, such as those about the values governing the behaviour of scientific communities, necessarily involves epistemological aspects. But what does the envisaged fusion of philosophy and sociology of science look like in detail?

Kuhn uses two basic assumptions that import sociology into the philosophy of science. The first assumption states, as I have already said earlier, that *communities* and not individuals should be seen as the basic agents of science, its subject (compare Hoyningen-Huene [1989], pp. 19–23, 73, 88–9, 152–4, 196). The second assumption is built upon the first one. It states that these communities must be characterized by the specific cognitive *values* to which they are committed (compare Hoyningen-Huene [1989], Section 4.3.c, pp. 148–54). The opposite positions to these assumptions are, of course, logical positivism and critical rationalism. In both these positions, the principal agent, the subject of science, is the *individual*. This individual is not *committed to values* but *obeys rules*—at least as long as he or she behaves rationally. By ‘rules’ Kuhn means in this context algorithms, that is uniquely executable instructions. But what is the difference between the subject of science being individuals obeying rules and communities being committed to values?

Let us look at the situation where a single scientist has to make a decision between competing theories or hypotheses, a situation for which the alternatives in question can easily be juxtaposed. For both logical positivism and critical rationalism it is a necessary condition of the rationality of such a decision that it follows well-defined rules. As a consequence, every individual that rationally reaches his or her decision must come to the same result. Whether the rules invoked are deterministic or only probabilistic does not make a difference: whether the decision for A and against B is cogent or only based on, say, 80 per cent probability does not matter. In both cases, the only rational decision is the one for A. For instance, if a rational decision in the theory choice situation is the one that maximizes empirical content, and if there are methodological rules for determining empirical content, then it is plain that the rational decision is independent of the deciding individual, it is inter-subjective: everybody makes the same choice.

In opposition to this view, we shall not suppose any longer, according to Kuhn, that rational scientific decisions are rule-governed, that is that they follow algorithms. Rather, these decisions are influenced by the cognitive values to which the respective community is committed. The important point here is that a decision which is *influenced* by values is not necessarily *determined* by them. This implies that different individuals influenced by the *same* values may come to *different* decisions.

In order to understand the consequences of this conception of scientific decisions, let us consider the values to which, according to Kuhn, a scientific community is committed. Typically, there is a consensus with respect to

*accuracy*, that is that a statement derived from theories shall be accurate, both qualitatively and quantitatively; this is a value of prime importance though it does not necessarily have determinative force by itself. Another value is *consistency*, that is that a theory shall have no internal contradictions, and that it should be consistent with other accepted theories. Other values concern the desired large *scope* of a theory, its *simplicity*, and its *fruitfulness*: the theory should point to the discovery of new phenomena, or to new relationships between known phenomena.

Compared to classical philosophy of science, this is not a revolutionary list of values. The point is, rather, that in general these (and possibly other) cognitive values do not determine scientific decisions. There are two reasons for the missing determinative power. First, each of these values can be interpreted somewhat differently by different members of the same scientific community. For example, what simplicity means exactly and which aspects of a theory are primarily meant is not uniquely fixed by the commitment of a community to this value. Second, two cognitive values can, in their application, contradict each other which makes relative weighing necessary. But their relative weight is, again, not determined by the commitment of the community to the list of values.

But if the cognitive values do not determine the decision of the scientist, how can he or she ever reach a decision in the situation of theory choice? The answer is that theory choice becomes possible through the influence of *additional* values contributed by the individual scientist which may vary strongly from one member of the community to the next. For example, the individual's professional experience contributes in various ways, for instance the kind, length and success of the individual's work experience in other fields. In addition, extra-scientific persuasions of a philosophical or even religious kind may play a role. Finally, individual personal characteristics may contribute to the decision—fear or joy of risk, preferences, etc. Analysis of the theory choice of an individual scientist thus results in two kinds of active values: cognitive values to which the whole community is committed, and individual values varying within the community.

For Kuhn, the indeterminacy of systems of communal cognitive values is by no means an imperfection that should in principle be removed; rather this indeterminacy plays a vital role for scientific development. The main reason is that *actual* theory choice in science is almost always risky: scientists must usually make up their minds with which theory to work in a situation where these theories are not yet fully worked out, that is where nobody is absolutely sure which theory will be the final winner. In situations like this it is vital that some scientists work with one theory and some with the other; otherwise the true potential of both theories can not be ascertained. In Kuhn's words, the situation '*requires* a decision process which permits rational men to disagree' (Kuhn [1977b], p. 332). Otherwise, science is in danger either of becoming

petrified within one tradition, or of jumping from one theory to the other without ever exhausting their respective potentials. The missing determinative force of the cognitive values is just the right way to cope with the unavoidable risk of choosing a theory at a comparatively early stage of its development, or of sticking with its older opponent. The community as a whole is undecided but prepares a decision by ascertaining the potential of the candidates.

#### 4 THE RATIONALITY OF THEORY CHOICE I: THE DIVERGENCE OF INDIVIDUAL CHOICES

Even if some functional advantage of the divergence of individual decisions in the theory choice situation is granted, there remain pressing questions with respect to the rationality of an enterprise in which individual decisions so strongly embody subjective elements. Kuhn has very often been accused of making science an irrational or subjectivist enterprise. It has also been said that his sociological or historicist view just misses the essential element of science, namely its *justified* claim to knowledge. Kuhn has repeatedly rejected these accusations, but with little success. In the picture commonly labelled as Kuhn's, scientific revolutions are events whose outcome is exclusively determined by propaganda, conversion and the dying out of opponents, and not by sober comparative evaluation of the achievements of theories, or other arguments that articulate good reasons for theory choice. Thus, the question has to be asked whether in the Kuhnian framework theory choice is a rational affair or not. In pursuing this question, I shall turn to the philosophical aspects of Kuhn's theory which supposedly are united with the more sociological ones treated so far.

I start with the question whether the individual decision in the theory choice situation under the description given by Kuhn has to count as rational or irrational. Let us consider the complex of values that influences this decision. Here we can distinguish communal cognitive values and individually differing values. With respect to the communal values, one is probably inclined to classify the decision as rational; in this respect, the decision is based on values like accuracy, consistency, scope, etc. which form a rational basis for theory choice. Being somewhat more cautious, it may be said that the decision is rational in this respect *if and only if* these values are a rational basis for theory choice. Whether they are or not will be discussed in the following section. But how about the individually varying values contributing to the decision? Surely, not all of these values can be classed as irrational. No doubt, it is not irrational to be influenced by experience derived from one's own scientific work in other areas, for instance by success with certain types of theories. As a consequence, professional experience in other areas will contribute to the individual value system active in the theory choice situation. An assessment of individual values of aesthetic, religious, philosophical or psychological origin



with respect to reason is more difficult. Perhaps one would be inclined not to classify them as unambiguously irrational as long as they only colour the communal values without overpowering them—but, of course, the border between 'to colour' and 'to overpower' is not sharp.

All right, but perhaps the following consideration shows that the individual choices, as depicted by Kuhn, can certainly not be classified as rational. This consideration starts with something like the 'unity' or 'exclusiveness' of reason—whatever reason is. If something—a consideration, or an argument, or a position—if this something legitimately counts as rational, then its opposite cannot, in the same respect, also be rational. If it is rational to do A in a certain situation, then doing non-A in the same situation cannot also be rational in the same respect. Therefore, if it is rational with respect to the progress of science for one scientist to choose theory A in the theory choice situation, then it cannot be rational for another scientist to choose theory B in the same situation. Following this line of argument, one has to conclude that Kuhn's statement that the theory choice situation 'requires a decision process which permits rational men to disagree' is inconsistent.

I think that the consideration about the unity or exclusiveness of reason is correct in general; but its application to the case in question is incorrect. The reason is that the situation of theory choice is *not* identical in every respect for the different scientists involved, although the choice is to be made between the same theories. This is grounded in the fact that the factors *reasonably* seen as relevant for the choice may vary from one scientist to the other, for instance the experience one has had with certain types of theories. Of course, it is reasonable to allow for such experiences in the theory choice situation. Therefore, the mere fact of a divergence of theory choices among different scientists is no indication of irrationality; rather, it may be a sign of different information bases used in the decision.

But, of course, the earlier consideration is still valid that the individual decisions contain something a-rational and potentially irrational by incorporating aesthetic, religious, philosophical, and psychological elements. But, looked at more closely, it seems to me that this fact is less important than it appears with respect to the question of the rationality of theory choice. The reason is that in the Kuhnian framework the principal agent in science, its subject, is not the individual but the group. Therefore, it seems to me, the question of the rationality of theory choice must be asked with respect to groups, not with respect to individuals.

But, first of all, a paradox seems to emerge at this point. If the individual scientists come to divergent decisions, what sense does it make to speak about the decision of the group? 'The decision of the group' can only mean a more or less uniform decision of its members—after all, the group does not exist beyond and above its individual members. This paradox dissolves once one realizes which processes of scientific development are our subject matter. We are, at

the moment, analysing processes which begin with disagreement concerning theory choice and end with a new agreement about the comparatively best theory. During the disagreement phase, different scientists have decided to work with different theories (which is analytically true). This work generates in the course of time such an amount of empirical and theoretical arguments in favour of one theory that a new consensus concerning this theory emerges (compare Hoyningen-Huene [1989], Section 7.4.b, pp. 233–8). Only if such a consensus emerges, that is, only if the community really reaches a decision, can the question about the rationality of this decision be asked.

But the new consensus has to be qualified in two respects. First, the *same* result in the individual decision processes which constitutes the end of the disagreement does not imply that the individual choices are also based on *exactly the same* reasons. Rather, the individually varying values still contribute to the choices, and they function as additional (or rather integrated) reasons for the choice. But after the phase of disagreement, so many arguments in favour of one candidate have piled up that whatever the individual value system consists in, everybody makes the same choice. Or rather *almost* everybody, I should say, which brings me to my second qualification of the new consensus. The consensus reached may be a little less peaceful than I have depicted it. To a certain degree, the new consensus may eventually be reached in the way that some dissidents are excluded from the community, or that the group separates. But this happens in the fringes of the consensus-forming process; it is certainly not its main determinant. What is essential, however, is the fact that the new consensus is mainly based on the collective system of cognitive values. To cite Kuhn on this matter:

To understand why science develops as it does, one need not unravel the details of biography and personality that lead each individual to a particular choice, though that topic has vast fascination. What one must understand, however, is the manner in which a particular set of shared values interacts with the particular experiences shared by a community of specialists to ensure that most members of the group will ultimately find one set of arguments rather than another decisive (Kuhn [1970b], SSR, p. 200).

## 5 THE RATIONALITY OF THEORY CHOICE II: THE JUSTIFICATION OF COGNITIVE VALUES AND THEIR CHANGE

Now we can turn to the question of the rationality of scientific development once more. We wanted to clarify whether and, if yes, in which sense the decisions of scientific communities in the theory choice situation can count as rational decisions. A decision of this kind can, as we have seen, be justified relative to the system of cognitive values. Is the decision therefore a rational decision? Obviously, the fact that the decision can be justified with recourse to cognitive values, is not by itself sufficient for its rationality. Rather, the

cognitive values themselves must be good reasons, 'rational' reasons which make the decision a rational one.

In which sense could the cognitive values to which a scientific community is committed be good reasons for theory choice? The possibility of a positive answer seems to be foreclosed in the framework of the Kuhnian theory. As Kuhn repeatedly states it in the 1960s and 1970s: 'Some of the principles deployed in my explanation of science are irreducibly sociological, at least at this time' (Kuhn [1970a], p. 237). Statements like these seem to indicate that Kuhn is unable to accept anything that goes beyond an empirical account of cognitive values, primarily a philosophical justification. But in fact, this is not at all Kuhn's view, at least not in the late 1970s and the 1980s. In his argument with Carl Hempel who was his colleague at Princeton for many years, he provided extensive discussion of the problems of justification of cognitive values (see especially Hempel [1977], Section 8, [1981], [1983], Kuhn [1983]).

It seems to me that Hempel and Kuhn agree on the *possibility* of a justification of cognitive values, and perhaps also on the fundamentals of the *means* of justification. The justification of cognitive values has to have recourse to an ultimate goal of science: the commitment to cognitive values is then the means which guarantees that theory choice is made in accordance with this ultimate goal. Hempel states this view in the following way:

Science is widely conceived as seeking to formulate an increasingly comprehensive, systematically organized, world view that is explanatory and predictive. It seems to me that the desiderata [this is Hempel's term for cognitive values, P.H.] may best be viewed as attempts to articulate this conception somewhat more fully and explicitly. And if the goals of pure scientific research are indicated by the desiderata, then it is obviously rational, in choosing between two competing theories, to opt for the one which satisfies the desiderata better than its competitor (Hempel [1983], p. 91).

Thus, with recourse to an ultimate goal of science the problem of the justification of cognitive values can be approached within the Kuhnian image of scientific development. This is, at the same time, also an approach to the rationality of theory choice which is, as I have said earlier, essentially based on cognitive values.

What I find most fascinating about this approach is the prospect of a solution of a related problem in which sociological and philosophical aspects are also intertwined. It is the problem of the *change* of cognitive values in time, and of their *difference* in different scientific communities at the same time. Kuhn has described change and difference of cognitive values, but I think he has not answered the question how change and difference of cognitive values can be understood. Let me now focus on the question of change in time of cognitive values; the question of value difference among communities can be treated

analogously. What does the change of a system of cognitive values consist of? Well, a single value may change with respect to its content, and the weight of a value may change within the system of values (Kuhn [1977b], pp. 335–6). For example, in the course of the development of modern science the value ‘accuracy’ seems to have changed from a more qualitative sense to a more quantitative or numerical one, and the relative weight of this numerically understood value ‘accuracy’ seems to have increased within the system of values. The *description* of such a value change is, of course, a matter for sociologically sensitive historians of science, or historically sensitive sociologists of science. We, however, would like to discuss the problem of the *explanation* and, possibly, the justification of such a value change.

There are two principal ways for an explanation of value change in science. The first seeks the factors responsible for value change external to science. For instance, extra-scientific values may change for whatever reasons, in turn causing the scientific values to change. This case may be very interesting to the sociologist but it is less interesting to the philosopher. The second possibility is the philosophically interesting one, namely that the cognitive values to which some scientific community is committed change because of processes internal to science.

Typically, according to Kuhn, changes in the system of cognitive values occur in the aftermath of theory changes (Kuhn [1977b], p. 336). This fact strongly suggests that changes in the cognitive values are somehow *caused* by the theory change, that is that they have causes internal to science. Then the question arises how this change of values can be understood as a consequence of theory change, and whether such a change may count as justified. The latter question asks whether this sort of value change may be rational. Thus, we have here a second-level question about scientific rationality; the first-level question is the one about the rationality of theory change which is governed by values, and the second-level question is the one about the rationality of value change.

At first, the justification of value change by recourse to theory change seems entirely impossible. Theories belong to the descriptive sphere whereas cognitive values belong to the normative sphere, and a justified transition from the descriptive to the normative does not seem to exist. Thus, how should theory change justify value change? The puzzle dissolves once one pays attention to the fact that cognitive values relate to the ultimate goal of science which was, in Hempel’s words, ‘an increasingly comprehensive, systematically organized, world view that is explanatory and predictive’. The values ‘articulate this conception somewhat more fully and explicitly’, as Hempel says; thus they concretize this ultimate goal, or they are something like execution procedures for it. In order to accomplish this task, the values have to be realistic in the sense that they don’t postulate something not realizable. For instance, the cognitive values of empirical science must not postulate that the

theories sought after can be demonstrated like mathematical theorems; though we would like theories of this kind, they seem to be beyond human reach. Now, since the values must claim to be *realizable* execution procedures for the ultimate goal of science, besides their normative content, they must also have factual content. For realizability of posits depends on properties of the world; in other words, certain properties of the world must enter the cognitive values. To give a simple illustration: once one believes that the world is a deterministic mechanism, the ultimate goal of science will, for instance, be concretized in the attribution of a high value to deterministic theories. If, on the other hand, one does not believe in strict determinism any longer, the value attributed to deterministic theories will diminish.

The result in this: since the cognitive values of science are a sort of execution procedure for its ultimate goal, they must be realizable; the realizability of cognitive values (in the sense given) depends on properties of the world; since the theories of science formulate knowledge claims about the world, they inevitably and reasonably enter the cognitive values of a particular community. Thus, if in a scientific revolution knowledge claims about the world change, some of the cognitive values also may change since they tell us how to realize the ultimate goal of science in the real world.

## 6 CONCLUSION

To conclude, let me summarize a few points. One aspect of the interrelation between historical sociology and philosophy consists in a philosophical analysis of data supplied by sociology about cognitive values and their change. Can the observed change of values be understood and justified as a consequence of theory change? Obviously, this analysis presupposes the existence of the required data. On the other hand, without such a consideration the observed change of values is meaningless with respect to the specificity of science. Thus both disciplines, or rather perspectives, are mutually dependent. Finally, both disciplines react back upon the historiography of science. Historiography invariably needs criteria for the historically essential, that is what has to be researched and included in the historical narrative (compare Hoyningen-Huene [1989], pp. 24–5, with references). From the model given, it would be useful for historians to pay increased attention to value change as an essential part of scientific development (compare Kuhn [1977b], p. 335). And wherever such a change is observed, one must ask the question whether it is a consequence of processes internal to science or not.

Thus the circle of mutual dependencies among the three meta-scientific disciplines closes. Let me make one final remark. Perhaps the line of argument given may help to unite two complementary sides of science which do not seem to fit together. I have in mind, on the one hand, the undoubtedly historical character of the scientific enterprise, and, on the other hand, the scientific

claim for knowledge which has an atemporal element. The ultimate goal of science to produce general, explanatory theories about the world may well be beyond historical change; yet it is of vital importance to science. The cognitive values, however, that concretize this goal in an operationally meaningful way are themselves in part historically relative since they are dependent on what one believes about the world at some particular moment of time.

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