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AGAINST HISTORICISM

Critical Remarks on Thomas Kuhn's Conception of Science and Its Reception in Parapsychology

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ABSTRACT: The origins and essentials of Thomas Kuhn's historicist conception of rise, change, and fall of scientific theories and of entire scientific branches are described in the first part of this contribution. An attempt is made to show that this conception has been adopted almost unanimously by scientists engaged in parapsychological research. Some unpleasant consequences and dangers of this one-sided reception of Kuhnianism are pointed out. It is demonstrated that Kuhn's theory embodies several serious shortcomings and that it only applies to deductive models of justification. It is suggested that Kuhn's conception be replaced by an alternative model that, by means of a recourse to elementary practices of predication and action, allows for proper foundation and justification of scientific propositions. Some consequences of this alternative conception are pointed out and contrasted with some of the disastrous implications and consequences the Kuhnian conception entails for science in general and parapsychology in particular.

In my comments (Hövelmann, 1981) on K. Ramakrishna Rao's attempt to point out a fallacy in David Hume's treatise "Of Miracles" (Rao, 1981b), I argued that Popper (1934/1959) successfully rejected as being defective on logical grounds the concept of induction that was considered appropriate by Logical Positivists of the Vienna Circle and, especially, in Reichenbach's probability theory of induction. Therefore, Popper urged that induction be replaced by his deductive method of testing. In his reply to my comments, Rao (1981a) correctly points out, however, that the Popperian program likewise is not free of serious shortcomings.

For decades, Critical Rationalists, that is, adherents of the Popperian school of philosophy of science, have been thoroughly discussing what they call the deductive model of justification. They

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came to the insight that, with regard to the problem of an appropriate basis of justification, the model runs directly into several insurmountable difficulties. It was found that any deductive attempt to pull oneself out of the swamp of uncertainty and to find a solid basis of justification leads into a threefold impasse. The deductive model of foundation and justification (a) must make use of circular argumentation, which is logically faulty (*circular argumentation* here means that after invoking a number of successive justificatory statements in support of a given proposition, one is forced to call in the original proposition itself to support one of these justificatory statements); or (b) it leads into an infinite regress of justificatory attempts, which is practically impossible because human beings are mortal (*infinite regress* means that for any justificatory statement that is called in in support of a scientific proposition, a further justificatory statement can be requested, and so forth ad infinitum); or (c) it leads to an arbitrary suspension of the postulate of justification or to the dogmatic choice of a particular basis of justification that itself is unfounded (*dogmatic choice* means that the chain of justificatory statements is arbitrarily broken off at one point and that the latest statement is chosen as the justificatory basis).

Hans Albert (1980, pp. 11–15), a German adherent of the Popperian school, aptly called this treble alternative of dead ends *Münchhausen-Trilemma* after Münchhausen, a (fictive) baron and notorious liar who reported to have pulled himself out of a swamp by his own tuft. This trilemma is inescapable if solid foundation and justification are sought to be reached on the basis of a deductive model. Therefore, Critical Rationalists decided no longer to judge theories by their internal (logical) consistency but rather by their steadfastness to thorough scrutiny.

An alternative proposal, which proved to be most influential among "working scientists" (McConnell, 1983, pp. 192–193) as well as within the philosophy of science (Lakatos & Musgrave [Eds.], 1970), was put forward by Thomas S. Kuhn in his book *The Structure of Scientific Revolutions*, which was first published in 1962 by the University of Chicago Press (Kuhn, 1962). Kuhn, who had initially conceived his concept as a novel theory of historiography of science,[1] holds that theories and entire scientific branches develop beyond and quite

[1] Therefore, it is at least questionable to talk about Kuhn's "philosophy" of science as has become customary in recent years.

irrespective of *any*[2] attempt at justification. As I will shortly try to point out, however, Kuhn's way out of the Münchhausen-Trilemma turns out to be hardly more adequate than that of the Critical Rationalists. To the contrary, it shares many of the major shortcomings of its predecessors and even adds a few further quite insufficient ideas.

Historicism: Kuhn's Views of the Development of Science

As most parapsychologists are quite familiar with the Kuhnian conception of rise, change, and fall of scientific theories and entire scientific branches (I am also going to deal with this familiarity below), I will restrict myself to a relatively brief outline of the most important aspects of this conception.

According to Kuhn (1962), who is considerably influenced by the work of Fleck (1935/1979, 1983),[3] there are two alternating phases in the process of science: normal science and extraordinary science. The term *normal science* designates the familiar type of research based on commonly accepted explanatory models, which are referred to as *paradigms.*[4] Such paradigms are conceived as sufficiently unprecedented, successfully attracting an enduring group of adherents, and sufficiently incomplete to leave work for those adherents. *Extraordinary science*, on the other hand, takes place in the transitional phases between two paradigms. The following periods mark the development of any scientific discipline:

1. In a preparadigmatic period, more or less random and casual data gathering takes place drawing heavily on what is readily at hand from other disciplines or from everyday or workmanlike experiences.

2. In a normal scientific period, the data gathered in the first period are arranged in an admittedly incomplete paradigm, which,

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[2] Stephen Braude, chairman of the session at the P.A. convention where I presented the earlier draft of this paper, remarked that he believes that I have misunderstood Kuhn in this respect. Contrary to my claim, said Braude, Kuhn's theory does allow for a scientific proposition to be adequately founded and justified within a paradigm once that paradigm has been accepted by a group of scientists. Braude is quite right, but that is not what I am talking about. Foundation within a paradigm is foundation in a very restricted sense at best, because the accepted paradigm itself remains unfounded and, according to Kuhn, must necessarily remain unfounded.

[3] Fleck's philosophy of science was much more versatile, however, than Kuhn seems to have realized (Schäfer & Schnelle, 1983, pp. 10 and 19).

[4] For a discussion of the ambiguity and obscurity of Kuhn's use of the term *paradigm*, see Masterman (1970).

nevertheless, must be more powerful than its eventual competitors. Research activities as well as political and financial matters are organized; specialization occurs; important problems are distinguished from less important ones; results of research are recorded in textbooks; and so on.

3. Anomalies that are contrary to textbook opinion are discovered.

4. If anomalies question the core of the paradigm, or if they are of practical importance, or if enough time has passed since the anomalies were first reported, a crisis arrives and results in the sapping of the paradigm and in the loosening of the rules governing scientific practice within that paradigm.

5. Extraordinary research is conducted to give structure to the anomaly, thus (often suddenly) creating an entirely new paradigm.

6. A revolutionary struggle between the old and the new paradigm takes place, and the older one is eventually replaced in whole or in part by an incompatible new one. That means: revolutions of this kind are noncumulative changes. The field is completely reconstructed from its foundations. Because each group of researchers argues on the basis of its own paradigm in that paradigm's defense, paradigms remain totally incompatible, and mutual factual agreement proves to be impossible. In some sense, groups who accept different paradigms live in different worlds. Besides logic and experiment, force is needed to settle the issue.

7. After one paradigm has won the struggle, scientists return to normal science. Textbooks are rewritten, which causes the false impression of a cumulatively advancing science. Science is conceived as a nonteleological, rule-guided, puzzle-solving activity, the development of which must not be described in terms of foundation and justification but in those of sociology or social psychology.

Historicism in Parapsychology

In 1968, Robert McConnell was the first one to bring Kuhn's model, which is out to replace foundation and justification by questionable historicism, to the attention of scientists engaged in parapsychological research (McConnell, 1968; also see McConnell, 1966, 1976). McConnell's 1968 paper was introduced by an editorial note, which I would like to quote full length here. It reads:

> We are privileged to present a hitherto unpublished synopsis of an important contribution to the understanding of science and particularly to the understanding of the *beginnings of any new field.* Although psi

> phenomena are not discussed in this book, we believe that attitudes toward them will be profoundly influenced by the *study and acceptance* of Professor Kuhn's ideas. (p. 321; italics added)

In the light of the seven points just listed, it is quite obvious why the editor of the *Journal of the A.S.P.R.*, the late Laura A. Dale, was so enthusiastic about the Kuhnian conception: parapsychology was and remains a "new field," opposition was and remains strong, and because parapsychologists wish their field to get a firm footing in the world of science, they sooner or later need an adequate understanding of the way science develops.

To cut a long story short, parapsychologists took the above suggestion to heart. They did study Kuhn's ideas and, instead of calling them scandalous as, in my opinion, they should have done, they did accept them. A factor further accelerating this one-sided acceptance has certainly been that several leading figures in the field set an example by readily adopting Kuhn's conception (Pratt, 1974, *in toto*; Thouless, 1972, pp. 100–102). Pratt (1979, p. 26) used Kuhnian concepts and Kuhnian terminology to identify "a paradigm crisis within parapsychology." As far as I can see, today the vast majority of parapsychologists still highly esteem or more or less explicitly subscribe to Kuhnian opinions, as for instance Kornwachs (1975), McClenon (1982), Nilsson (1975, 1976), or Winkelman (1980). Others, such as Edge (1976, 1977, 1978a, 1978b), Stanford (1977), and Thakur (1977), who find themselves in basic agreement with Kuhn's opinions, have tried to apply more or less modified versions of the Kuhnian conception of a paradigm and of his views of scientific change to the internal and external sociological and political circumstances of parapsychological research.

More than a dozen years after he epitomized the Kuhnian conception for the information of the parapsychological community, McConnell (1981) briefly recapitulates its influence on the discussions within the field, and he states:

> Kuhn's elucidation of "preparadigm science" has brought a new understanding of parapsychology as an emerging field. (p. 225)

And in his latest book, McConnell (1983, p. 198), for whose contributions to parapsychology I otherwise have nothing but the highest respect, assures us that "Kuhn's basic idea seems inescapable." It is obvious that McConnell's whole book is strongly influenced by Kuhn's views of development in science. In chapter 18 of that book, McConnell (1983, pp. 192–207), under the heading "Thomas Kuhn, Historian and Heretic," provides a new introduction to Kuhnian thoughts. In part, it closely resembles his 1968 paper. This chapter starts with the following paragraph:

> In the present chapter, I shall ask you to examine the nature of progress in science, beginning with the following long-established ideas. As you read these statements, are there any with which you would disagree? Think about each one for a moment as you go down the list.
> 1. Science is the accumulation of truth concerning nature.
> 2. As science progresses, our total understanding converges toward reality.
> 3. The unchanging laws of nature are waiting to be discovered (by some combination of luck, diligence, and insight).
> 4. The purpose of scientific research is to make new discoveries.
> 5. The past accomplishments of science are described in its textbooks (by which each new generation of scientists takes its task from its predecessors).
>
> In some important aspect, each of these five statements is untrue. Or at least, there is a historian of science . . . who says they are untrue. His name is Thomas Kuhn. (p. 192)

Like Kuhn (and maybe McConnell) and to McConnell's probable surprise, I very strongly disagree at least with the first four of these statements. I disagree with them because they merely reflect commonly accepted, but nevertheless highly naïve and short-sighted empiristic and realistic misconceptions of the goals of science. However, these four statements can all be rejected very easily on the basis of rational counterarguments, which need not make any use of Kuhnian opinions.

Parapsychologists' tendency to comfort themselves by subscribing to Kuhn's historicistic views has not gone unnoticed by the outside world: a caricature in *New Scientist* (1979) features a parapsychologist who, leaning out of a window of his institute, is desperately asking all passers-by: "Buddy, can you spare a paradigm?" (p. 311). Finally, it can be inferred from a paper by Reber (1982–1983), which contains several serious misconceptions of science as well as of parapsychology, that this critic, at least, also shares Kuhnian views. And even an unrestrainedly radicalized version of the Kuhnian conception of the development of science is now available in the works of the British sociologists Collins and Pinch (1979, 1981, 1982).

Besides two of my own papers (Hövelmann, 1983b, in press), I came across only very few pieces in the parapsychological literature appearing to express at least moderate disagreement with Kuhn's historico-sociological views of science. By far the most unmistakable rejection of parapsychologists' enthusiasm about Kuhn's conception is to be found in John Beloff's reaction to the paper by Thakur (1977):

> I would like to go on record as a skeptic with regard to the Kuhnian concept of a paradigm. For that reason I rather regretted that Professor Thakur spent so much of his paper discussing what I consider to be essentially the idle question of whether parapsychology is or would become a normal science in the Kuhnian sense. . . . There has been an enormous amount of controversy, of a rather tedious kind, as to whether psychology has ever acquired a paradigm, whether behaviorism was a paradigm, and so on. None of this debate, it seems to me, led anywhere and I think it would be far better to simply skip this whole approach and ask more important questions. (Beloff, 1977, p. 210)

Moreover, I found three book reviews in the parapsychological literature in which the authors are criticizing the consequences of uncritically adopting Kuhn's conception. It may be an overinterpretation to say that they are critical of Kuhn, but they doubtlessly are highly skeptical about Collins and Pinch's radicalization of Kuhn's approach. Thus, Gregory (1982) writes:

> There are problems and ambiguities about paradigmatic language, now well recognized . . ., but unceremoniously brushed aside by Collins and Pinch. . . . If things are as Collins and Pinch say, then the whole endeavor of psychical research, of judging carefully whether and when and under what conditions and why something happens, is a futile waste of time, or at any rate a rather masochistic game perpetuating a sort of Sartresque *huis clos* in which parapsychologists explain away their failures and skeptics explain away parapsychologists' successes, the two sides fruitlessly chasing each other's tails for ever and ever and ever. . . . There is, we are told, no such thing as rational argument over and above social convention. (Gregory's italics; pp. 310–311)

Similarly, in a review of the same book, Stokes (1983) writes:

> The authors themselves adopt the relativistic view of rationality (i.e., that there is no demonstrably correct form of reasoning), and they also explicitly espouse a radical interpretation of Kuhn's theory of scientific revolutions. . . . Their position in this regard seems far too radical. (p. 84)

And Stokes continues:

> It should be noted that the relativism proposed by Collins and Pinch constitutes a danger to parapsychology. If parapsychologists in large numbers were to adopt such a relativistic position, the field would be in danger of isolating itself from science and diverging from acceptable scientific practices. It would then truly deserve to be a laughing stock for the orthodox scientific community. Finally, if the authors' relativistic position is correct, what reason does a reader have for accepting their arguments as valid or the form of rationality employed by them as the correct one? In this respect, the relativistic position in some sense refutes itself. (p. 85)

Stokes had already criticized Collins and Pinch's relativistic position in an earlier review, where he wrote:

> According to Collins and Pinch, what is a scientific truth depends as much on power struggles and fund raising as on rational argument and empirical evidence. This is social determinism taken to its most absurd extreme. If this view is accepted, then the world really was flat a millenium ago. (Stokes, 1980, p. 370)

Despite these rare exceptions, which exhibit a basically critical attitude toward Kuhnian ideas or their radicalization, at least Kuhn's own somewhat less radical conception has been avidly seized on by a very great number of parapsychologists. The high esteem of the Kuhnian way of looking at science appears to be almost omnipresent and unanimously shared by the people engaged in parapsychology. It is fairly obvious that parapsychologists feel very well understood by this historian of science. Now it is my aim here to thwart this total agreement a bit. My criticism will turn not only against the obscurity of parts of the Kuhnian conception—as, among others, Masterman (1970) has done[5]—but also against the conception as a whole. As far as I can see, parapsychologists' appeal to Kuhnianism serves (or may be serving) several functions for their field. I will only mention three of them:

1. Many parapsychologists seem to believe that Kuhn is picturing a pathway—and some parapsychologists may even consider this the only possible pathway—to future legitimacy of parapsychological research. Obviously, they think that Kuhn has adequately described the way science develops, and they hope that, after a future paradigm shift, they will be the adherents of the paradigm that has won the "struggle for life." Accordingly, many seem to be less concerned with scientific truth and rational argumentation than with ways to find the most strategic position on the battlefield of science, which they hope to reach in time to avoid a Custerian Little Big Horn. I think that this attitude involves a twofold naïveté and shortsightedness, as it is not only based on Kuhn's theory of developments in science but, even worse, on a serious misunderstanding of that defective theory. Parapsychologists, who appeal to Kuhnian historicism in this way, are

[5] Margaret Masterman's paper was one of the most important criticisms of the 1962 edition of Kuhn's book. It had a considerable influence on Kuhn's "Postscript" of 1969, in which he suggested several terminological clarifications (Kuhn, 1970, 1974). In this postscript, *paradigm* is used in two major senses: in a global sense, relabeled *disciplinary matrix*, it includes paradigms and other aspects of scientific activity; and in a particular sense, relabeled *exemplars*, paradigms are aspects of the disciplinary matrix.

courting disaster: according to Kuhn, it is, by definition, impossible to predict future paradigm shifts. Because the very nature of paradigm shifts lies in their success, they can only be observed after the fact. Therefore, appeals to Kuhn cannot give any comfort to emerging scientific disciplines. Quite to the contrary—and that is why I think that these parapsychologists are throwing boomerangs—Kuhnianism can even be used to justify orthodox intolerance toward such emerging fields, as it provides establishment science with a historicist pretext for its constant refusal to take deviant claims seriously.

2. In the opinions of some parapsychologists, the Kuhnian conception may readily serve as a welcome excuse for the fact that, hitherto, they have constantly failed in all their attempts to establish their field as a legitimate and well-recognized branch of science. Thus, the ways science is believed to develop can be held responsible for all such failures; and parapsychologists can consider themselves the innocent and defenseless victims of a ruthless and merciless social and/or political process. Moreover, the critics of the field can be viewed as being those people who just happen to be adherents of a fully developed paradigm that is (still) disposing of the power to successfully combat against aspiring preparadigmatic fields such as parapsychology. One can even afford to more or less politely ask the critics to "eventually die out," as Max Planck put it in his autobiography.

3. Adherence to the Kuhnian model seems to dispense scientists (and parapsychologists, for that matter) from the obligation to advance tightly reasoned scientific propositions, because development of science and acceptance of supposed scientific truths need no longer be regarded as a question of foundation and justification but as one of social power. Thus, proper justification of scientific propositions can be viewed as being secondary or even an unnecessary accessory.

Scientific Propositions: Factual Success Versus Foundation and Justification

As I have tried to demonstrate in my brief summary of the Kuhnian conception, sociological and historical reflections in science and the recourse to factual developments are now to replace foundation and justification of scientific propositions. This is surprising, at first sight, because "foundation" and "justification" traditionally were supposed to denote methodical or systematical rather than mere historical securing of such propositions. Where previously a recourse to (relatively) safe starting points was thought to be possible, it is now enough simply to make reference to the factual course of theory building and theory enforcement. As I have emphasized in the beginning, this is a symptom of the apparent crisis in those concepts of foundation and justification as they are held by the "analytically" oriented schools in philosophy of science (logical positivism, critical rationalism, structuralism, and their various offspring). Strictly speaking: after Kuhn, valid justification in a strict sense does no longer exist in science. Questions as to whether a theory is a good and sound one are now answered by making reference to the fact that this theory did factually prevail (or fail).

In my view, this Kuhnian criticism of the concept of justification is nothing but a highly intelligent excuse for the failure of the deductive model of justification. Kuhn's conception, which was intended to avoid the dogmatism with which the third lemma of the aforementioned Münchhausen-Trilemma is unsolvably connected, and according to which every possible statement that may be called in for justificatory purposes is to be viewed as "theory-laden" and, hence, itself in need of adequate justification, is itself dogmatic. The Kuhnian criticism of justification only applies to deductive models of justification. By definition, *deduction* necessarily implies that certain linguistically composed statements, sentences, propositions are derived from other, more general statements, sentences, propositions that of course are linguistically composed themselves. That means that, in the last resort, the unavoidability of this linguistic composition of all possible statements in the process of deduction is the reason why the deductive model of justification runs directly into the unpleasant Münchhausen-Trilemma: on the one hand, "language-free deduction" is contradictory in terms; on the other hand, adequate justification of any linguistically composed statement is obligatory.

As I observed in the preceding, Kuhn's criticism of the concept of justification only applies to such deductive models. It completely loses sight of the fact that adequate foundation and justification of scientific propositions are well possible by means of a recourse to elementary practices of predication and action which, on the most basic level, can dispense with linguistically composed statements and therefore do not lead into the Münchhausen-Trilemma. For obvious reasons it is impossible here to go into the details of a theory of elementary predication and a theory of action (Kamlah & Lorenzen, 1973; Lorenzen, 1969) or to develop a whole theory of foundation and justification. But the following outline should suffice to explain the essentials in the present context.

In science, human beings, who are able to act, to speak, and to learn by way of acting and speaking, are trying to solve problems that they and other human beings are confronted with. In the ideal case, scientists are directly or indirectly attempting to help their fellow human beings to live a better, an easier, or a healthier life. What else should be the purpose of scientific research?[6] These goals can only be reached if the actions scientists perform are successful. So, why not, in a pragmatic[7] sense, base our scientific propositions on what scientists are actually doing when they do scientific research? Why not base them on the actions they perform, as, for instance, their experimental actions in the experimental sciences? The problems the deductive model is confronted with can easily be avoided that way. That is because, in the last resort, any scientific proposition, say, in the experimental sciences can be reduced in a finite number of steps to the actions the experimenter performs. The success of these experimental actions functions as the criterion of the truth of scientific propositions about the experiment in question. In this sense, the action of scientists can serve as the justificatory basis for scientific propositions. If the utility of a certain action is also questioned by an incredulous objector (as the propositions based on this action may have been), then this action must simply be performed to demonstrate whether it is appropriate for reaching the success (producing an effect, and so forth) that has been claimed in the propositions that were based on that action. If this action succeeds, the propositions are justified. In everyday life, we all do not have the slightest problem with this procedure of justifying our claims. If, to use a very primitive example, our claim that under such and such circumstances it is possible to light a candle is questioned by someone, then, in the last resort, we are forced to actually perform the action of lighting a candle under the specified circumstances to demonstrate whether our claim was justified. If this action succeeds, our claim is justified. With a recourse to the actions scientists perform, which can always be reached by a finite number of steps, we are equipped with a reliable, language-free basis for a proper justification of scientific propositions, which cannot be subjected to further justificatory

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[6] Some critical comments on claims to the effect that the purpose of science is to "discern reality," or to explain the world, the universe, human destination, and what not, are to be found in Hövelmann (in press, section III.3).

[7] I am somewhat hesitating to use the term *pragmatic* in this context, as it is liable to be misunderstood as a reference to William James's pragmatism or Charles Sanders Peirce's pragmaticism. While the position outlined in this paper is doubtlessly influenced to some degree by the philosophies of Peirce, James, and Dewey, there are also a lot of very important differences.

requirements. Kuhn clearly fails to consider this way out of the Münchhausen-Trilemma. The problem of foundation and justification of scientific propositions is solvable. Kuhn has only shifted and obscured it.

An historically organized practice as that advocated by Kuhn is continually in danger of privately becoming a stylish, but cheap, defense of poor science. If the quality of a theory is not primarily judged by the justification of that theory but rather by the way it has superseded competing ones, then it becomes possible that a theory is considered a proper one simply because it superseded rivaling ones that way. Considerable parts of current philosophy of science as well as of current practice in various scientific branches indicate that many philosophers and scientists have already ceased from striving for a constructive means of theory building in favor of a mere analysis of factually existing theories, which seem to be conceived as natural necessities. Many philosophers of science have obviously chosen to resign themselves to being mere chroniclers of science. Proposals of a better, methodically proceeding theory building are no longer taken into consideration. The validity of theories, thus, becomes dependent on factual agreement based merely on the alleged fact that "the scientific community knows what the world is like" (Kuhn, 1962, p. 5). That would mean that the dispute about the claim that, say, "theory A is valid whereas theory B is not" is finally settled by a majority vote of the "scientific community."

Kuhn assumes that science, which for him always means factually prevailing scientific theories and orientations, is accomplishing its tasks. In his view, however, neither these tasks themselves nor the methodical way they are accomplished require critical judgment. Thus, foundation and justification turn out to be mere historical coincidences. Methodological rules as well as standards and criteria of the distinction between "rational" and "irrational" are regarded as repressive curtailment of scientific practice. Evidently, this kind of argumentation helps the scientist and the philosopher to get over any deficiency in the justification of their theories. Justificatory judgment of scientific undertakings is thus replaced by an internal, historicist judgment of the historical process of gaining scientific knowledge.

Doubtlessly, study of the history of scientific disciplines can be of the utmost importance to these disciplines—I wholeheartedly agree with Alvarado (1982) on this question; and Kuhn himself has shown in some of his case studies that scientific changes frequently occur in a way that is anything but rational. The question is, however, whether we content ourselves with this unpleasant fact and view it as an indispensable necessity or look for ways to avoid such developments in the future. In Kuhn's conception, the valuable insight into the importance of the history of science comes down to a practice that restricts itself to merely recording and analyzing historically contingent factual developments. In the end, even historical laws are being formulated. History of science is seen as conforming to laws of the history of theories and theory building, and this history of theories, in turn, is conceived as a succession of systematical concepts that presupposes genetic regularities and rests especially on the consideration of normal and revolutionary phases in the development of science. From this point of view, the prevailing of a theory appears similar to a natural selection. The "struggle for life" of theories is described in terms of variation and selection of competing theories relative to the underlying conditions of a common historical practice, which, in turn, is thought to be a natural necessity. No wonder that Toulmin (1961, pp. 110–111) recommends that historians and philosophers learn from biology;[8] and Kuhn (1962, p. 146) expresses himself in a very similar way. Here, the history of criteria and standards of rationality, which are cultural products, is written in terms of a natural history of scientific developments. That is the reason why, in contexts relevant to and in need of adequate foundation and justification, one is simply referred to factual developments. Systematic argumentation, which once has been obligatory in cases where the validity of theories was at issue, is no longer required. Ultimately, science completely renounces the distinction between well-founded and justified scientific theories on the one hand and those that factually prevail on the other.

Contrary to this Kuhnian conception, I would hold that it is well possible to reconstruct (in the sense just specified) justificatory steps of a theoretical development, instead of merely looking at the results of such a development. Because each form of scientific knowledge necessarily points to a more or less strictly performed methodical and stepwise construction (otherwise, we would not at all speak of scientific knowledge!) and because, as a matter of course, questions as to

[8] Elsewhere (1982, 1984), I have comprehensively criticized the use of natural scientific concepts, theories, and terminologies for the description and explanation of cultural developments. The way such concepts are used in so-called evolutionary epistemology is particularly irresponsible. People tend to forget that scientific research is a cultural activity; even the development of a natural science is a cultural (not a natural) event. Therefore, it cannot be described and explained in terms of natural scientific theories, which are intended for application to entirely different objects and, thus, are subjected to different criteria of validity.

the validity of our propositions are unavoidable, such endeavors to reconstruct justificatory steps of theory building must primarily aim at a critical judgment of the practical orientations that are at the roots of the development of scientific knowledge. Then it becomes possible not only to find out such orientations, but also to critically argue against or in favor of them and their expediency. Thus, the Kuhnian historiography of theoretical developments can, at least in large parts, be replaced by a critical foundation and justification of these developments. Among others, this view implies the following interrelated advantages for a rational understanding of science in general and of parapsychology and its position vis-à-vis the established sciences in particular:

1. A very essential part of human work need not be conceived as being ruled by more or less contingent and irrevocable natural necessities. This implies that human beings engaged in scientific research (rather than some mysterious mechanisms underlying and governing scientific developments) are responsible for what they are doing and saying: *in concreto*, this means that they are obliged to provide adequate justification of the propositions resulting from their research upon request. Scientific research can be performed in a rational and reasonable way, and proper justification can be given to scientific propositions. Rational argumentation based on propositions that have been justified in the way sketched above rather than Kuhn-type historico-sociological processes should guide scientific research activities.

2. Historically contingent scientific developments for which one cannot argue systematically need not be conceived as governing the process of science.

3. Limited contingent parts of theories, which may nevertheless exist, can be distinguished from well-founded and justified ones. Such parts must either be given up or reformulated in a rationally justifiable way.

4. Faulty developments (in parapsychology as well as in the established sciences) can be characterized as such. As long as all possible developments of scientific disciplines and theories are taken to be equally valid—as implied in the Kuhnian conception of rise, change, and fall of scientific theories and explicitly stated in an entirely unsupportable way by Collins and Pinch—such characterization proves impracticable.

5. Central parts of predominating scientific theories and practices can be substantially criticized and improved by means of appropriate suggestions for reorganization.

6. The Kuhnian as well as the Popperian analytico-descriptive conceptions are totally lacking any normative or prescriptive aspects.[9] The aforementioned alternative proposal, however, not only implies that it is possible to criticize scientific orientations, practices, and theories; it also implies that such orientations, practices, and theories can be subjected to rational discourses[10] in the interest of reaching a mutually agreeable description of the goals scientists wish to pursue and to set norms as to how such orientations, practices, and theories should look relative to these mutually agreed upon goals and relative to the postulate to guarantee scientifically acceptable research. That means that even more can be done than criticizing logical faults or lacking falsifiability of theories.

7. All revolutionary pretensions can be given up, because even very important scientific changes can only be based on rationally justifiable arguments in favor of such changes. The frequently heard argument that parapsychologists were forced into the role of scientific revolutionaries by the revolutionary character of their subject matter and their findings, is rooted in a simple linguistic slovenliness. Subject matter or scientific findings are never revolutionary *sui generis*; what can be revolutionary are actions. If these actions, however, make use of a commonly accepted methodological canon, they are, by definition, unrevolutionary (see Hövelmann, 1983b, pp. 128–129; in press).

From my argumentation so far, it may be concluded that in parapsychology (as in any other branch of science) we can learn from our history not only in the sense that we will eventually know how the present situation has come about, but also in a sense closely connected with our actions as scientists, that is, in a sense that enables us to critically change our own practice where this turns out to be desirable in order to guarantee rational scientific conduct. Moreover, we will certainly be better able to point out to the critics of the field by means

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[9] In this respect, I do not agree with Douglas Stokes, who believes that "Popper's falsifiability theory" belongs to the "prescriptive philosophies of science" (Stokes, 1983, p. 92; his italics). Popper is very clear about his opinion that his falsifiability theory is an adequate description of how science actually proceeds. What is "prescriptive" in Popper's philosophy is a limited part of the language in which he describes what he believes scientists are actually doing. He completely shares the view of logical positivists that factual scientific practice at any given time is to be viewed as the best justified practice at that time. He was (and apparently still is) convinced that all that philosophers of science can do is to analyze, describe, and affirm what scientists (and physicists in particular) are actually doing.

[10] For a definition of *rational discourse*, see Hövelmann (1983a, pp. 496–499).

of well-founded arguments not only that they are wrong (in case they are wrong), but also why they are wrong (in case they are wrong).

As far as I can see, especially in parapsychology there is no reason for dispensing with solid foundation and justification of our practices, our theories, and our argumentation in favor of an obscure historicist conception according to which relatively contingent historical constraints are telling us what to do next.

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KUHN AND PARAPSYCHOLOGY

Some Critical Remarks on Hövelmann

By T. J. Pinch

Thomas Kuhn's seminal writings in the history and philosophy of science have had an enormous impact on how we have come to understand the development of science. As John Ziman (1983) remarked recently when presenting Kuhn with the J. D. Bernal medal at the Society for Social Studies of Science: "We are all Kuhnians nowadays." This points not only to the extraordinary extent of Kuhn's influence but also to the remarkable property of his ideas—they tend to mean all things to all men! Kuhn himself has become all too aware of this latter facet of his work, having spent the last decade trying to distance himself from self-styled "Kuhnians" (including Collins and Pinch).[1] This led us to comment in our recent book on the application of Kuhn's ideas to parapsychology that "Kuhn n'est pas Kuhnian!" (Collins & Pinch, 1982).

It is clear that in any discussion of Kuhn's ideas or their influence we should be careful about which reading of Kuhn is being referred to. Throughout our own work in the sociology of science, we hope to have made it clear that it is Kuhn's ideas interpreted through the writings of Winch and Wittgenstein that have informed our analysis. Hövelmann is correct in describing our views as "radical," but it is misleading to suggest that this is an "uncritical" reading of Kuhn. Indeed, we have argued that it is the uncritical reader of Kuhn who is led into apparent paradoxes. The need for care in interpreting Kuhn is obvious when it is recalled that Kuhn himself in a recent essay has argued that his ideas are quite consistent with the critical-rationalist tradition in the philosophy of science of which Hövelmann stands in such awe (Kuhn, 1977).

The fact that Kuhn's ideas can be seen to be quite consistent with the creeds of the best rationalist philosophers alerts us to the dangers of imputing any straightforward influence to Kuhn's ideas. Presumably, if the rationalist interpretation of Kuhn was widespread in parapsychology then Hövelmann, rather than offering condemnation,

[1] For reviews of Kuhn's work in the sociology of science, see Pinch, 1979, 1982.

would be full of praise for Kuhn-inspired parapsychologists and their enlightened attitude toward epistemology.

What type of influence does Hövelmann impute to Kuhn's ideas in the case of parapsychology? He writes that Kuhn's ideas can lead parapsychologists to become "less concerned with scientific truth and rational argumentation" and "to dispense scientists (and parapsychologists for that matter) from the obligation to advance tightly reasoned scientific propositions." In short, it seems that under the influence of Kuhn scientific standards will drop and scientific reasoning may be abandoned altogether. This, however, is a reading of Kuhn with which we would disagree. Nowhere does Kuhn suggest that there is any justification to act in any other manner than to follow the accepted canons of scientific evidence and methodology shared by the scientific community. Such canons and methods may not be decisive (especially during moments of paradigm revolution), but to abandon them is clearly to abandon any chance of being accepted as part of science.

The difficulty here comes from the stark choice that Hövelmann presents as arising from Kuhn's work; namely, that scientists must either exercise rationality or crass social power. The subtlety of Kuhn's argument is that the canons of scientific method *are* the means by which social power is exercised. If power did lie outside of scientific method, then clearly it would be possible to bring about a scientific revolution with just money and influence. And no one (apart perhaps from Hövelmann) suggests that this has occurred in the history of science or indeed that it is a serious possibility.

Given the variety of interpretations of Kuhn, it is, of course, possible that Hövelmann's point still stands and that some (misguided) parapsychologists have read Kuhn in this way. Hövelmann, however, does not persuade us that this is the case. He presents not a single example of such sloppy Kuhn-inspired practice in parapsychology. Indeed, he actually praises the work of one of the parapsychologists who is most inspired by Kuhn, R. A. McConnell. He writes that for his "contributions to parapsychology I otherwise have nothing but the highest respect." Perhaps Kuhn's ideas have influenced parapsychological practice a little less than Hövelmann suggests.

This leads us to ask the more interesting question, which is what exactly has the influence of Kuhn been on parapsychology? And here we would suggest that it is rather similar to the influence Kuhn has had on psychology, sociology, and anthropology. In all these fields, Kuhn's work has been avidly read and discussed. All these disciplines have questioned whether they were going through a crisis, undergoing a revolutionary paradigm shift, or whether they were merely in a preparadigmatic state.[2] In short, there has been a lot of "paradigm agonizing" but little else. We can do no better than quote Beloff (1977) on this point:

> There has been an enormous amount of controversy, of a rather tedious kind, as to whether psychology has ever acquired a paradigm, whether behaviorism was a paradigm and so on. None of this debate, it seems to me, led anywhere. . . . (p. 210)

For psychology, could be read sociology, anthropology, or indeed parapsychology.

It is in the realm of ideology where Kuhn's ideas have been influential. Indeed, a point made by Hövelmann about Kuhn's ideas being used as an *excuse* seems to be about right. What Kuhn conveniently provided was a resource to be drawn on, particularly by those who felt overshadowed by the physical sciences. They either felt that their ideas had been unreasonably rejected by an established orthodoxy or that, if only they had a paradigm, then scientific progress would follow. Such ideas could be used to justify their perceptions of the status of their science, but this is not the same as the ideas being used to distort their science. Indeed, it is the remarkably *unproductive* nature of debates over paradigms and the like that deserves attention rather than their subversive potential. Hövelmann seems to have overestimated the impact any philosopher or sociologist can make on actual scientific practice. Such ideas may be important for scientific ideology, but they are hardly likely to bring significant changes in a body of practices built up in an ongoing research tradition.

Hövelmann also seems to be confused over the issue of analytical intention. The analytical intentions of sociologists in their attempts to understand scientific development are different from the intentions of scientists in carrying out science. For example, we have argued in our own writings in the sociology of science that sociologists should suspend judgment as to the truth or falsity of the beliefs being studied. We seek a symmetrical, impartial explanation for both "true" and "false" beliefs, and we do not want to fall into the trap of assuming that adherence to "true" ideas needs no explanation whereas adherence to "false" ideas is to be explained in terms of social epidemiology or "false consciousness." The wisdom of adopting such an approach, especially in areas like parapsychology where the "truth" of matters is yet to be settled, is obvious. However, because we, as *analysts*, are

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[2] For a discussion of Kuhn's impact in sociology, see Martins, 1972. The most useful general extension of Kuhn's ideas to sociology of science is to be found in Barnes, 1982.

truth-neutral does not mean that scientists should not look for what they regard as true belief. After all, the job of scientists is to find out what the truth of the natural world is! Hövelmann simply seems to have confused the analytical aims of our work (and more pertinently Kuhn's) in understanding science with the aims of the scientists of *doing* science. Thus, when he suggests that Kuhn's analysis and our own writings mean that parapsychologists can no longer distinguish mistakes in their own field, he confuses the differing analytical endeavors of sociologists and scientists.

Finally, it should be noted that a section of Hövelmann's paper is used for the purposes of arguing for a philosophical theory of science based on predication and action. This, it is claimed, is to be preferred to the "obscure" writings of Kuhn. Parapsychologists, it seems, are to be chastised for neglecting this theory in favor of Kuhn. This seems to be somewhat absurd since this particular theory can hardly have been available to most parapsychologists. Indeed the theory appears to be of such obscurity that it cannot be spelled out clearly in Hövelmann's paper.

But from what Hövelmann does say about the theory, it would seem to have at least one major shortcoming. It does not solve the very problem that Hövelmann notes to be the major difficulty of critical-rationalism—the infinite regress caused by the theory-ladenness of observation statements. As we understand it, the theory he advocates stresses that ultimately it is *performance* that counts in science. The emphasis is put on action and performance to avoid references to theory-laden propositions. For example, the performance of the action of lighting a candle under specified circumstances demonstrates whether a claim about candle lighting is verified. The problem with this is that performances have to be *interpreted* and *given meaning*. In interpreting the outcome of a performance there is nothing to stop the "specified conditions" being challenged. For example, if we try to demonstrate candle lighting and we fail, one possible meaning is that we have found a new property of candle lighting; on the other hand, it is more likely that we did not allow for the background conditions such as the wind. When a new discovery is claimed in science, there is no way of telling whether it is a genuine discovery or some property of the background conditions. This, of course, is the Duhem-Quine thesis and, because all tests designed to resolve the issue further themselves involve background assumptions, no test can resolve the dilemma. Recent studies of physics seem to indicate that this questioning of background assumptions is just what does occur when a new discovery is claimed (see Collins, 1981). Thus, the appeal to performance does not seem to remove the infinite regress of the theory-ladenness problem that has besotted the critical-rationalist philosophy of science.

In conclusion, we welcome parapsychologists' enthusiasm toward ideas in the sociology of science. If this enthusiasm, however, leads parapsychologists to believe that they can dispense with scientific method, they are misreading our work. But because Hövelmann presents no evidence that any reputable parapsychologists have taken Kuhn or ourselves as warrant to abandon scientific method, our advice is probably redundant. Hövelmann seems to have constructed a straw man in an attempt to persuade parapsychologists to adopt his own obscure and inadequate philosophy of science.

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