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Kuhnian Turn in Scientific Rationality†

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It is well-known that Kuhn opted for a procedure of theory choice based on scientific values, as an alternative to the once-mainstream formal approaches to theory evaluation. However, his value-based view of theory choice faces two unresolved problems, that is, the problem of excessive methodological permissivism and the problem of consensus formation. So I propose and argue for a revisionary version of Kuhn's value-based view on theory choice. In my revisionary version, I suggest that the strategies of methodological divergence or convergence and their associated tactics of adjusting the weights of scientific values need to be adopted in due time as methodological constraints, and further argue that those strategies and tactics will contribute to resolving the problem of excessive methodological permissivism and that of consensus formation, while maintaining the advantage of Kuhn's original view in handling the problem of producing rational disagreement in theory choice. Then I lay out two important implications such revisionary version seems to have on the nature of scientific rationality and in turn of scientific methodology. One is that the rationality of scientific community in theory choice is prior to that of individual scientists. And the other is that what I call soft methodology results from my revisionary proposal.

【Keywords】Thomas Kuhn, scientific rationality, theory choice, scientific values, problem of consensus formation

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1. Introduction

Kuhn was critical about the once-mainstream formal approaches to theory evaluation, particularly logical empiricists' probabilistic confirmation theories and Popper's falsification theory. Alternatively, he offered a procedure of theory choice based on scientific values, and took one of its main merits to be that it allows for rational disagreement among scientists in theory choice. However, his value-based view of theory choice tends to imply that any theory choice made by scientists be taken to be rational. I call this radical implication the problem of excessive methodological permissivism. Another difficulty with Kuhn's value-based view of theory choice is that it seems to have a hard time in answering the question of how scientists can and do converge in their theory choice throughout scientific revolutions. I call this difficulty the problem of consensus formation.

My diagnosis is that the lack of methodological constraints on individual scientists' practicing scientific values is mainly responsible for both problems. To deal with these problems, I propose and argue for a revisionary version of Kuhn's value-based view on theory choice. In my revisionary version, I suggest that the strategies of methodological divergence or convergence and their associated tactics of adjusting the weights of scientific values need to be adopted in due time as methodological constraints, and further argue that those strategies and tactics will contribute to resolving the problem of excessive methodological permissivism and that of consensus formation, while maintaining the advantage of Kuhn's original view in handling the problem of producing rational disagreement in theory choice. Then I lay out two important implications such revisionary version seems to have on the nature of scientific rationality and in turn of scientific methodology. One is that the rationality of scientific community in theory choice is prior to that of individual scientists. And the other is that what I call soft methodology results from my revisionary proposal.

2. Kuhn's Turn in Scientific Rationality

In the historical discourse regarding contemporary philosophy of science, it almost goes without saying that the so-called historical approach, which Thomas Kuhn and his book *The Structure of Scientific Revolutions* (hereafter, *SSR*) are representative of, played a decisive role for the demise of logical empiricism, the mainstream philosophy of science in the 20th century. This historical common sense is usually associated with the idea that Kuhn's philosophy of science was squarely antithetical to logical empiricism as the received philosophy of science in the middle of 20th century. Such an association is no longer taken for granted. I think that we can find a more balanced assessment regarding the relation between Kuhn's philosophy of science and logical empiricism in John Earman's statement that "I aim to pay homage to both Carnap and Kuhn by noting some striking similarities and also some striking differences. These similarities and differences are useful in helping to focus some of the still unresolved issues about the nature of scientific methodology."[1]

What is Kuhn's stance on scientific rationality and how is it similar to or different from that of the once-mainstream philosophers of science like logical empiricists or Popper? It is widely agreed that

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1) Earman (1993), p. 9.

the main locus of scientific rationality, if science is rational at all, lies in the way theory evaluation or choice is made by scientists. Then what was Kuhn's view about theory evaluation or choice? It can be easily noted that Kuhn was very critical about the received views of theory evaluation, particularly two of them.[2] One is what he called probabilistic verification theories, and the other is Popper's falsification theory. Particularly when he discusses "the probabilistic verification theories", Kuhn refers to Ernest Nagel's book (1939), *Principles of the Theory of Probability*, Vol. I, No. 6, of International Encyclopedia of Unified Science, pp. 60-75, where Carnap, Hempel and Reichenbach are dealt with as key proponents of probabilistic approach to theory evaluation. As such, there is no doubt that it was logical empiricists and Popper who were main targets in Kuhn's critical discussion about the existing views of theory evaluation. So we are led to think that Kuhn stood in opposition to what logical empiricists and Popper took scientific rationality to be. What was the existing view of scientific rationality that Kuhn was against?

Although logical empiricists and Popper sharply disagreed about the legitimacy of inductive reasoning and, in turn, the notion of confirmation, I think they shared a common view about the nature of scientific rationality as follows:

(T1) Scientific theories can be evaluated in a comparative manner, using justifiable logical reasonings, on the basis of common empirical data.
(T2) As a matter of fact, scientists make theory choices mostly based on such an evaluation.
(T3) Scientific rationality obtains by means of (T1) and (T2).

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[2] See Kuhn (1962/2012), pp. 145-47.

Why did Kuhn reject the received view on scientific rationality? In his critical discussion about logical empiricists' probabilistic verification theories and Popper's falsification theory, he mentioned some technical reasons why he found those existing views troublesome. But Kuhn's discussion of normal science and scientific revolutions brings forward more fundamental reasons why logical empiricists' and Popper's views of theory evaluation and in turn their common view of scientific rationality, formulated as (T1)~(T3), cannot be maintained. First of all, according to Kuhn, scientists in normal science are absorbed in puzzling-solving activities and, as such, not interested in theory testings. He thinks that is the way normal science is conducted. Furthermore, for Kuhn, most of the scientific activities belong to normal science. If that is the case, most of the time scientists fail to abide by (T2). Some people might decline to accept such a conclusion by saying that it is not that scientific activities in normal science fail to abide by (T2) but just that they are irrelevant to theory testings. Which side is right then? It depends upon how we read (T2). For the conclusion that normal science does not refute (T2) but the former is just irrelevant to the latter, (T2) should be read as follows:

(T2*) If scientists get interested in theory testing at all, they make theory choices mostly by implementing (T1).

However, such a reading of (T2) misleads the nature of methodological disagreement between logical empiricists or Popper and Kuhn. The nature of their disagreement is well illustrated in Popper's critical comments on Kuhn's view of normal science. Popper criticizes Kuhn as follows:

> I believe ⋯ that Kuhn is mistaken when he suggests that what he calls 'normal' science is normal. ⋯ few, if any, scientists who are recorded by the history of science were 'normal' scientists in Kuhn's sense. In other words, I disagree with Kuhn both about some historical facts, and about what is characteristic for science.[3]

Thus Popper's criticism on normal science is twofold. First of all, normal science is against what is characteristic for science. What is characteristic for science? Popper believes "that science is essentially critical; that it consists of bold conjectures, controlled by criticism, and that it may, therefore, be described as revolutionary."[4] On the contrary, "[Kuhn] believes in the domination of a ruling dogma over considerable periods; and he does not believe that the method of science is, normally, that of bold conjectures and criticism."[5] Thus Popper is methodologically against normal science since it is quite opposite to how scientific activity he thinks should be conducted. Moreover, scientific activities like normal science are, according to Popper, relatively rare in the history of science. He admits that Kuhn's distinction between normal science and scientific revolution is important but still believes that "it needs qualification."[6] For, according to Popper, Kuhn's distinction "seems to fit astronomy fairly well," but "it does not fit, for example, the evolution of the theory of matter; or of the biological sciences, since, say, Darwin and Pasteur."

Given Popper's above-mentioned oppositions to Kuhn's notion of normal science, the proper way of reading (T2) will be like the following:

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3) Popper (1970), pp. 53-54.
4) Ibid., p. 55.
5) Ibid.
6) Ibid. p. 54.

(T2†) Most of the time scientists are and should be interested in theory testing, and moreover, make theory choices mostly by implementing (T1).

If (T2†) is the proper reading of (T2), then Kuhn's normal science clearly fails to abide by (T2), and in turn refutes the traditional view of scientific rationality based on it.

What about the other part of scientific activities, namely, scientific revolutions then? According to Kuhn, if something like theory testing occurs at all, it will be during scientific revolutions where old and new paradigms compete against each other. The notorious problem with the traditional notion of scientific rationality is then Kuhn's claim that competing paradigms during scientific revolution are incommensurable. The basic idea of Kuhn's incommensurability is that there is no common measure for evaluating competing scientific theories.

Kuhn's view of normal science and scientific revolution and its negative implications against the traditional notion of scientific rationality aroused vicious counterattacks on it. One major charge against Kuhn's normal science is that it makes scientific activities dogmatic and parochial so that they lose their allegedly objective character. Even during scientific revolutions where scientists are taken to make theory choices, it is charged that Kuhn's incommensurability makes competing theories incomparable so that scientists' commitment to old or new paradigm becomes nothing but the product of mob psychology.[7] Thus, Kuhn's thesis of incommensurability, understood by his critics, seemed to refute (T1). The overall conclusion of the critics is then that Kuhn's alternative view of science makes scientific

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7) Lakatos is representative for charges of this kind. See Lakatos (1970), p. 178.

activities irrational. The critics' not-so-secrete strategy is to argue by reduction to absurdity that scientific activities are not irrational and hence Kuhn's views cannot be right.

What was Kuhn's stance on this rather heated debate? Obviously he had two options. One is to bite the bullet and to say that no, scientific activities are not so rational. Although this option has been favored by some of Kuhn's sociological followers, it is not what he did take. The other option that Kuhn himself opted for was to say that yes, scientific activities are typically rational, and thus the critics misunderstood his position. But how is it possible for Kuhn to admit that science is rational, while his views of science seem to refute both (T1) and (T2)? Kuhn's way out in this apparent impasse was to claim that it is the notion of scientific rationality itself but not his view of science that needs to be changed. His new task is then to propose an alternative notion of scientific rationality that he can persuade people to adopt for its traditional counterpart.

What is Kuhn's alternative notion of scientific rationality and how is it grounded? Kuhn would not deny that the main locus of scientific rationality lies in the context (or process) of theory choice. At this juncture, Kuhn himself raises the following question[8]: What are the characteristics of a good scientific theory? He enumerates five characteristics such as accuracy, consistency, scope, simplicity and fruitfulness, which he takes to be all standard criteria for evaluating the adequacy of a theory. Then he suggests that "together with others of much the same sort, they provide the shared basis for theory choice."

As Kuhn himself admits, his criteria for theory evaluation are at least nominally not quite different from their traditional counterparts. Then how is Kuhn's view of theory choice different from the

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8) Kuhn (1977), pp. 321-22.

traditional one? The idea is that his criteria "function not as rules, which determine choice, but as values, which influence it."[9] The main difference between Kuhn's value-based procedure for theory choice and its traditional counterpart is then that, contrary to the latter, the former allows for disagreement among scientists as to which theory to choose, and further that such disagreement is regarded as rational. Kuhn takes such consequence to be not a shortcoming of his theory choice procedure but rather its merit. Thus Kuhn's alternative notion of scientific rationality is much more tolerant than its traditional counterpart.

3. Unresolved Challenges to Kuhn's Turn

Even if it is admitted that scientific values play an important but untraditional role in theory choice, there seems to remain a serious challenge to Kuhn's effort for securing scientific rationality. It is the challenge that his own incommensurability thesis was taken to bring about. As far as incommensurability was understood to imply the impossibility of comparing competing theories, rational theory choice seemed to be beyond the reach of scientists. Against this negative understanding of his thesis, Kuhn charged the critics with misunderstanding his genuine intention. According to his rebuttal, he had in mind just a local incommensurability like the following:

> Most of the terms common to the two theories function the same way in both; their meanings, whatever those may be, are preserved; Only for a small subgroup of terms and for sentences containing them do problems of translatability.[10]

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9) Kuhn (1977), p. 331.

If two competing theories are just locally incommensurable in the just-mentioned manner, scientists will have a good possibility of sharing common ground (particularly, common empirical data) for evaluating those theories. This means that incommensurability does not necessarily exclude the possibility of comparing two competing theories in straightforward manner on the basis of common empirical data. As a matter of fact, we can confirm that such comparison is available between two theories competing during Kuhn’s representative scientific revolutions such as Copernican revolution or Chemical revolution.[11]

Thus Kuhn’s local incommensurability may seem to allow for restoring the traditional mode of scientific rationality based on (T1). However, Kuhn is likely to warn that such appearance should not be exaggerated. For, as far as he is concerned, two major barriers lie ahead for any attempt to restore the traditional rationality on a full scale. One of them is what is called Kuhn loss.[12] Kuhn loss refers to the phenomenon that some of the problems solved in a pre-revolutionary normal science are no longer taken to belong to the list of problems to be solved or come to remain unsolved in a post-revolutionary normal science. The source of Kuhn loss is again the incommensurability of the pre- and post-revolutionary normal sciences, particularly the situation where the proponents of competing paradigms disagree about the list of problems that scientists should

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10) Kuhn (1983), pp. 670-71.

11) See Cho (1996).

12) According to Kuhn (1962/2012), “new paradigms seldom or never possess all the capabilities of their predecessors”(p. 169). For Kuhn’s own examples, see Kuhn (1962/2012), pp. 99-100. The expression “Kuhn loss(es)” was introduced by Heinz Post (1971).

resolve. Why did we get accustomed to the idea of scientific progress as cumulative growth? Perhaps it has something to do with Kuhn's observation that "there are losses as well as gains in scientific revolutions, and scientists tend to be peculiarly blind to the former."[13)]

The other barrier to restoring the traditional mode of scientific rationality is the way scientific values influence theory choice. Contrary to the algorithmic criteria for theory evaluation that backed up the traditional rationality of science, criteria as scientific values allow for different interpretations and different weightings. Kuhn described the situation as follows:

> Individually the criteria are imprecise: individuals may legitimately differ about their applicability to concrete cases. In addition, when deployed together, they repeatedly prove to conflict with one another; accuracy may, for example, dictate the choice of one theory, scope the choice of its competitor.[14)]

This means that, although scientists nominally share certain scientific values, they may reach different conclusions about theory choice depending on how they interpret and/or weight those values.

Two issues are involved in our discussion about theory choice. One issue is how scientists do make theory choices. The other is how scientists should make theory choices. The former issue is descriptive in character, and the latter is normative (or prescriptive). Kuhn's view on the descriptive issue is that scientists sometimes disagree in their theory choices, but most of the time they agree. For Kuhn thinks that his normal science/scientific revolution distinction is descriptively right. Then, regarding the descriptive aspect of how scientists agree in theory choice, Kuhn and the then-mainstream philosophers of

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13) Kuhn (1962/2012), p. 167.

14) Kuhn (1977), p. 322.

science may be taken to be very much in accord. For, as Laudan suggests aptly, the view that science is a very consensual activity has been widely accepted by philosophers of science and sociologists during the 1940s and 1950s.[15] And such an overview fits well with the strenuous effort by the then-mainstream philosophers of science to figure out rules of scientific methodology which they thought are mainly responsible for producing consensus in science, and interestingly enough, even with Kuhn's notion of normal science. Further, given that both sides share the consensual view of science, they are not likely to be in discord about the descriptive aspect of how scientists disagree in theory choice. That is to say, they are likely to concur that, as a matter of fact, scientists occasionally disagree in theory choice.

How about their normative view on the way scientists agree or disagree in theory choice? It is about this part of the matter that Kuhn and his mainstream philosophical foes are likely to disagree. First of all, Kuhn's traditional foes will think that if scientists disagree in their theory choices, some of them are right but others are wrong. For given an appropriate algorithm for theory evaluation, one of the competing theories will turn out be better than the others, and then choosing the former is methodologically justified but choosing any of the latter isn't. Further such a differentiation will be taken to be the job of an appropriate algorithm for theory evaluation. On the other hand, Kuhn's suggestion is that if scientists disagree in theory choice, all (or both sides) of them might be right. For given his value-based procedure for theory evaluation, any of the competing theories could be preferred on its own ground and choosing any of them is methodologically permissible. But isn't such permissiveness methodologically irresponsible and equal to giving up scientific

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[15] Laudan (1984), pp. 3-13.

rationality? Kuhn's reply is likely to be this: Scientists' disagreement in theory choices is a prerequisite for scientific revolution, and hence, if scientific revolution is an essential part of scientific development then any acceptable scientific methodology must be able to produce such disagreement in theory choice in due intervals; furthermore any desirable notion of scientific rationality should be tolerant enough to make such disagreement in theory choice rational.

Whose view is more desirable, Kuhn's or his traditional philosophical foes' in question, about occasional disagreement among scientists in theory choice? The major difference between Kuhn's view and his traditional foes' is that while the latter is obliged to take sides with one of the competing theories at the expense of all the others, the former is allowed to give methodological rationale to all the competing theories. That is, in the context of theory choice, Kuhn's view is methodologically permissive but its traditional counterpart is methodologically strict. Supposing that methodological permissiveness is preferred at least on the initial stage of competition among theories particularly when the old theory is in crisis, I think we may concur with Kuhn that "recognizing that criteria of choice can function as values when incomplete as rules has, I think, a number of striking advantages. ⋯ it allows the standard criteria to function fully in the earliest stages of theory choice, the period when they are most needed but when, on the traditional view, they function badly or not at all."[16]

However, the methodological permissiveness characteristic of Kuhn's alternative view on theory choice is not always a virtue but seems to create its own problems. One of them is an excessive methodological permissivism in theory choice. According to Kuhn, "every individual choice between competing theories depends on a

[16] Kuhn (1977), p. 331.

mixture of objective and subjective factors, or of shared and individual criteria."[17] Here objective factors are nominally shared scientific values, and subjective factors are individual interpretations and weightings of those values. Why introduce these subjective factors? For the shared criteria "are not by themselves sufficient to determine the decisions of individual scientists."[18] Kuhn does not offer any methodological constraint of whatever interpretation or weighting each scientist adopts and in turn whichever theory he or she chooses. Moreover, every resulting choice is taken to be rational on its own way. Then, whether intended or not, the eventual outcome seems to be an excessive methodological permissivism.

The other, though related, problem is the reverse of the original rational dissensus problem. The problem arises from a methodological requirement of Kuhn's own normal science. Earlier I mentioned that, for Kuhn, disagreement in theory choice among scientists is a prerequisite for scientific revolution. The contrary is the case for normal science. For normal science requires overall agreement in theory choice. The question is then how scientists regain consensus in theory choice from dissensus formed during the initial stage of scientific revolution.[19][20]

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17) Ibid., p. 325.

18) Ibid.

19) I am not the first person to bring up this problem. Laudan is the major precursor for the discussion on the problem of consensus formation. See Laudan (1984), pp. 16-19; Laudan (1996), p. 91 & pp. 234-35.

20) In attempting to solve the problem of consensus formation and other problems related to scientific rationality, Laudan (1984) proposed the so-called reticulated model of scientific rationality. In one of my earlier works (Cho 2006), I had an opportunity to present a rather extensive discussion of the significance and limitation of his reticulated model. So, in this essay, I restrain a further discussion on it.

According to Kuhn in the 1st edition of *SSR*, “conversions will occur a few at a time until, after the last holdouts have died, the whole profession will again be practicing under a single, but now a different, paradigm.”[21] How is such conversion induced? His initial answer went like this. “Individual scientists embrace a new paradigm for all sorts of reasons and usually for several at once. Some of these reasons … lie outside the apparent sphere of science entirely. Others must depend upon idiosyncrasies of autobiography and personality. Even the nationality or the prior reputation of the innovator and his teachers can sometimes play a significant role.”[22] Here Kuhn seems to argue for the indispensable role of extrascientific factors in theory choice.[23] Perhaps in the light of such statements, Hacking (1999) takes Kuhn to be committed to what he calls the external explanation of [scientific] stability. According to Hacking, “The constructionist [like Kuhn] holds that explanations for the stability of scientific belief involve, at least in part, elements that are external to the professed content of the science. These elements typically include social factors, interests, networks, or however they be described.”[24] Hacking’s just-mentioned statement is actually about the externalist approach to how scientists maintain their consensus on scientific beliefs. But if such an approach is adopted for solving the problem of consensus maintenance, it will naturally apply to handling the problem of consensus formation, i.e., the problem of regaining scientists’

21) Kuhn (1962/2012), p. 152.

22) Ibid., p. 153.

23) This sort of argument inspired his sociological followers to claim that it is social factors that ultimately determine theory choice. Later Kuhn tried to distance himself from those sociological radicals. But it is hard to deny that his original discussions in *SSR* allowed ample leeway for sociological extrapolations.

24) Hacking (1999), p. 92.

consensus on scientific beliefs. Perhaps that is why Wray was led to conclude that "according to Hacking, this is Kuhn's view on consensus formation in science."[25] However, Kuhn's suggestion about the role of extrascientific factors in theory choice, even if we accept it wholeheartedly, shows at most that they contribute to determining which theory each individual scientist decides to choose. And it hardly shows that those various extrascientific factors do and cannot but cooperate in producing the overall agreement in theory choice among all or most scientists involved in a particular scientific revolution.

Fortunately or not, despite Kuhn's statements about the influence of social factors on theory choice, the externalist construal of his view on consensus formation in science is not something that he is likely to endorse. Such a stance is pretty clear from Kuhn's confession in his lengthy interview of 1995 that "It constantly surprises people in England that I'm an internalist."[26] As a matter of fact, Kuhn's more refined discussion of theory choice is very much focused on the role of scientific values.[27] The question is, then, did Kuhn's value-based view of theory choice solve the problem of consensus formation? My answer is, not quite. For, as far as concerned with Kuhn's own discussion of the matter, it did not improve the problematic situation as much as needed. Many readers of Kuhn's 1st edition of *SSR* got the impression that each paradigm has its own criteria for theory evaluation, and hence scientists' arguments for their chosen theory cannot but be circular. That is why some of the critics charged that Kuhn's view of theory or paradigm choice invited a radical sort of relativism. Kuhn's later proposal for

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25) Wray (2011), p. 154.
26) Kuhn (2000), p. 287.
27) See Kuhn (1962/2012), "Postscript-1969"; Kuhn(1977).

the critical role of values in theory choice was perhaps helpful in alleviating the radically relativistic image of his original view. For he suggested that, whatever paradigm scientists commit themselves to, they share certain scientific values as common criteria for theory evaluation. But I think its success remained partial. For Kuhn took theory evaluation in his value-based procedure to run as follows. Although scientists were supposed to share scientific values at least nominally, their application was taken to depend upon each individual scientist's interpretation and weighting. As such, Kuhn's value-based procedure was fairly successful for producing scientists' "rational disagreement" in theory choice, but it remained unsuccessful in showing how they do and should regain "rational agreement" in theory choice.

Interestingly, contrary to my half positive and half negative assessment of Kuhn's value-based view on theory choice, Wray seems to have reached the conclusion that Kuhn's view of theory choice succeeded in handling the problem of consensus formation. His supporting discussion consists of two parts.[28] The first part is that subjective factors like individual variations in scientific value induce different scientists to work with different theories. The second part is that different theories are developed to the point where it becomes clear which theory is epistemically superior. Here Wray's first part is not different from the process I alluded to earlier where scientists disagree about theory choice due to individual variations in scientific values. On the other hand, in the second part of his supporting discussion, Wray brings up the idea that "in the process, new evidence is amassed, and, in time, the epistemically superior theory emerges as the victor."[29] And he tries to illustrate this idea

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[28] See Wray (2011), p. 162.
[29] Wray (2011), p. 162.

by his case study of the contemporary revolution in geology which occurred in the 1960s and led to the acceptance of the theory of plate tectonics.[30] Intended or not, however, this part of his discussion reminds us of a traditional picture where competing theories are evaluated on the basis of accumulating evidences and a unanimous decision is made about which theory is the most superior. If that is the case, I am afraid that he falls back on too easy a solution. What's more puzzling is that, in his second part of the discussion, he rarely mentions scientific values and their operation. Fortunately, Hoyningen-Huene's related discussion[31] is helpful for clarifying the situation. According to him, "after the phase of disagreement, so many arguments in favor of one candidate have piled up that whatever the individual value system consists in, everybody makes the same choice."[32] What Hoyningen-Huene is suggesting here seems to be that, during the phase of disagreement, variations in value induce individual scientists to make differing theory choices but, after the phase of disagreement, those scientists make the same theory choice due to "many arguments" in favor of one particular theory regardless of variations in value. In Wray's discussion, the expression "epistemic factors [operating on new evidence]" was used instead of "many arguments". After all, Hoyningen-Huene, joined later by Wray, seems to have suggested a dualistic view of theory choice, that is, the view that, during the phase of disagreement, values dominate theory choice but, after the phase of disagreement, epistemic factors

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30) See Wray (2011), pp. 191-200. I'd like to thank one anonymous referee for drawing my attention to this part of Wray's discussion, although we seem to disagree about how to assess it.

31) Wray's discussion of Kuhn's view on theory choice is relying on this part of Hoyningen-Huene's work (1992).

32) Hoyningen-Huene (1992), p. 496.

like arguments dominate theory choice. What underlies such a dualistic construal of Kuhn's view on theory choice is Hoyningen-Huene's peculiar idea that, even during the period of normal science, variations in value among individual scientists exist as they were in the process of scientific revolution, but only in latent form.[33] However, it is questionable that Kuhn would have endorsed such a dualistic construal of his own view on theory choice. For it will not be, to say the least, so preferable for Kuhn to let scientific values dormant during normal science and as a result to make his value-based view of theory choice less than complete. The option more preferable for Kuhn will be, then, to say that scientific values are always operative in scientific inquiry, but variations in value among individual scientists during normal science are not substantial enough to result in different theory choices.

4. Proposing a Revisionary Version of Kuhn's Value-based View on Theory Choice

In the rest of the discussion, first of all, I'll ask whether a revisionary version of Kuhn's value-based view on theory choice can handle both the problem of excessive methodological permissivism (i.e. the problem of taking every theory choice to be rational) and the problem of consensus formation (i.e. the problem of regaining rational agreement in theory choice) while maintaining its advantage in handling the rational dissensus problem (i.e. the problem of producing

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33) See Hoyningen-Huene (1989), p. 235. So, according to Hoyningen-Huene, "Dissent in extraordinary science is thus the manifestation of differences that exist [latently] in normal science, ⋯ "(p. 235)

rational disagreement in theory choice), and then explore what implications such revisionary version might have on the nature of scientific rationality and in turn of methodology of science.

First of all, let's try a diagnosis of the two problems under consideration. My hunch is that both the problem of excessive methodological permissivism and the problem of consensus formation seem to have a common origin. The problem of excessive methodological permissivism arises from the situation where individual scientists are given the freedom of interpreting and weighting scientific values but Kuhn offers no methodological constraints for regulating such freedom. Allowing individual scientists to have their own interpretations and weightings of values serves as the mechanism of inducing the divergence in theory choice. But methodologically unconstrained freedom in doing so begets the result of making every theory choice be rational, which is all about the problem of excessive methodological permissivism. Furthermore, such unconstrained freedom in handling scientific values may be effective for producing divergence in theory choice, but it is likely to create an impassable barrier for reproducing convergence in theory choice. For it will be virtually impossible that, without methodological constraints, the various divergence-inducing individual contingencies do cooperate to yield convergence in theory choice among all or most scientists in the community. Thus the lack of methodological constraints on individual scientists' practicing scientific values seems to be mainly responsible for both the problem of excessive methodological permissivism and the consensus formation problem.

My next question is then whether a revisionary version of Kuhn's value-based view on theory choice can handle both problems better than its original counterpart. If any can, what is it going to be? In our previous diagnosis of the two problems, the tentative solution suggested itself. That is to bring out certain methodological constraints on how individual scientists apply scientific values in theory choice, if we have good reasons to do so.

In the original version of Kuhn's value-based view, it is just individual scientists who are supposed to decide how to interpret and weight scientific values. Such a supposition is partly correct. For, on the descriptive level, it will be ultimately each individual scientist who decides his or her own interpretations and weights of shared scientific values. It is all but certain that each individual scientist's decision will be exposed to various influences from intra- and extra-scientific factors. What is notably lacking in Kuhn's own discussion is to give considerations to the normative level of the matter. A normative question in this context will be something like this: what interpretations and/or weights of shared scientific values will be desirable or needed on a given stage of scientific inquiry? First of all, scientific values seem to have different degrees of variability in their interpretation. For example, the interpretation of accuracy, particularly, quantitative accuracy is likely to have the least degree of variability among scientists. On the other hand, the interpretation of simplicity is known to have a somewhat notorious degree of variability. From now on, however, I will suppose for the sake of argument that in each given context of discussion do scientists share common interpretations of scientific values,[34] and

[34] As one anonymous referee aptly points out, this may be a pretty big supposition. Perhaps there have been occasions when pre- and post-revolutionary scientists disagreed about how to interpret some of the scientific values under discussion. However, I doubt that there are good reasons for us to think that both groups of scientists usually do or should disagree about interpretations of scientific values in the way that such disagreement serves as a major hindrance to consensus formation among

restrict our discussion to weightings of scientific values and their changes.

Even regarding the question of weightings, it will be virtually impossible to give definitive and quantitative answers. However, qualitative but graded answers are needed and likely to be available. Going back to Kuhnian crises of normal sciences, for example, the need for an alternative theory will be in order. As long as the existing weightings of scientific values in normal science remain intact and continue to tightly constrain scientists' theoretical imagination and judgments, it will be extremely difficult for any alternative theory to emerge. Hence, depending upon how seriously the old theory is in trouble, the existing weightings of scientific values need to be changed and in turn their constraint should be loosened to facilitate the emergence of an alternative theory.

One natural way of motivating and handling the above-mentioned normative question is to pay attention to the goal-directed character of scientific activity, if it has such a character at all. Of course it will have to be something shared by scientific community as a whole and characteristic of scientific activity in general. Does science have such a character? Kuhn is likely to answer with positive nodding. In the final chapter of *SSR*, he specifies "the essential characteristics" of scientific communities as follows:

> The scientist must, for example, be concerned to solve problems

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scientists involved. As such, though I won't argue for it in this already-lengthy essay, I tend to think that variations in interpreting scientific values are quite less likely than variations in their weights to seriously hinder the process of consensus formation during scientific revolutions. I look forward to having an opportunity to discuss this matter in some detail.

> about the behavior of nature. In addition, though his concern with nature may be global in its extent, the problems on which he works must be problems of detail. More important, the solutions that satisfy him may not be merely personal but must instead be accepted as solutions by many. ⋯ This small list of characteristics common to scientific communities has been drawn entirely from the practice of normal science, and it should have been. That is the activity for which the scientist is ordinarily trained. Note, however, that despite its small size the list is already sufficient to set such communities apart from all other professional groups. And note, in addition, that despite its source in normal science the list accounts for many special features of the group's response during revolutions and particularly during paradigm debates. ⋯ The scientific community is a supremely efficient instrument for maximizing the number and precision of the problem solved through paradigm change.[35)]

It will be within easy reach from these statements to conclude that, at least implicitly for Kuhn, the basic goal shared by scientific communities and thus characteristic of scientific inquiry in general is to maximize the number and precision of the problems solved. However, some scholars would object to the idea of attributing a goal-directed character to scientific inquiry. For example, Kantorovich suggests that we should dispense with the notion of goal in understanding the nature of scientific inquiry.[36)] This suggestion is based on his evolutionary theory of science, which models the development of science on natural selection and views science as a continuation of biological and cultural evolution. As a result, according to Kantorovich, scientific inquiry need not be taken as goal-directed just as biological evolution isn't and need not be. Further Kantorovich is quite likely to think that Kuhn did share such

35) Kuhn (1962/2012), pp. 168-69.

36) See Kantorovich (1993), Chapter 4.

a view, given that he presented an evolutionary picture of scientific progress in the last chapter of *SSR*.

Whether the notion of goal is in fact dispensable for understanding the nature of scientific inquiry is one thing, and whether Kuhn is committed to such an idea is another. In this essay I do not intend to delve into the former issue[37] but restrict my concern to the latter issue. It is true that, after a lengthy and passionate discussion about the nature and necessity of scientific revolutions, Kuhn make a puzzling proposal for the evolutionary understanding of scientific progress. I used the term "puzzling" because Kuhn's discussion of scientific revolutions unmistakably highlights the revolutionary character of scientific change, whereas his evolutionary proposal for scientific progress could implicate him in the idea that scientific change is gradual. However, such an appearance is perhaps misleading. As far as Kuhn maintains his characteristic distinction between two kinds of scientific inquiry, namely, normal science and scientific revolution, it will be self-defeating for him to incorporate the gradualistic character of biological evolution into his view of scientific change. As such, it would be an overinterpretation to think that, in his discussion of scientific progress, Kuhn is literally assimilating the process of scientific inquiry to the process of biological evolution.[38]

On the other hand, what Kuhn clearly repudiated is the idea of "seeing science as the one enterprise that draws constantly nearer to some goal set by nature in advance."[39] And, in repudiating "some

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[37] In my earlier work, Cho (2006), I had an opportunity to present my stance related to this issue, though not on a full scale.

[38] For a recent scholarly exchange surrounding Kuhn's evolutionary analogy, see Renzi (2009); Reydon & Hoyningen-Huene (2010).

[39] Kuhn (1962/2012), p. 171.

goal set by nature in advance", what Kuhn specifically have in mind is something like "some one full, objective, true account of nature". Here one sweeping statement of describing Kuhn's proposal for a very unconventional notion of scientific progress could be to say that he wanted to remove goal-directed character from scientific inquiry. However, as I have already mentioned, Kuhn cites as a requisite for membership in a professional scientific group the characteristic that "the scientist must, for example, be concerned to solve problems about the behavior of nature." And scientists are not zombies. They deliberately intend and act to solve scientific problems. Then it seems quite proper to say that scientists pursue the goal of solving problems about the behavior of nature as many as possible.

How should we resolve this dilemmatic situation? It will be helpful to consider why Kuhn was led to purge goals from scientific inquiry. The most notable goal traditionally set for science was seeking a true account of nature. But, from Kuhn's point of view, this traditional goal did not fit well with the historical pattern of scientific change involving incommensurability and further seemed dispensable for the practices of scientific inquiry centering on problem solving activities. So he was compelled to leave the traditional goal of seeking truths about nature out of scientific inquiry.[40] Interestingly, in doing so, he seems to have been induced to take a further step of purging goal-directed character itself from science. However, as I mentioned above, problem solving activities in science usually involve scientists' intentional decision and action based on the consideration of means-ends relationship, and as such, they seem to be well qualified for goal-directed activities. Then disqualifying problem solving activities for goal-directed activities cannot but be regarded as an overstep in regulating the usage of the expression "goal-directed",

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[40] For a dissenting view against this move, see Bird (2000), Chapter 6.

although it is admitted that problem solving activities are different from truth seeking activities in that scientific problems and solutions originate from an existing scientific inquiry rather than are preset by nature and further in that the progress of problem solving activities is measured by the distance from their pre-existing state of achievement rather than by the distance toward the ultimate truths of nature.

Having said that, we'll suppose for the sake of argument that maximizing the number and precision of the problems solved is the basic and common goal built into scientific activities. Then such a goal will serve as the ground for handling the questions like why and how the weightings of scientific values should change in the process of scientific inquiry.

Why should the weightings of scientific values need to be changed? It is mainly because there will be certain changes in scientific activities needed to maximize the number and precision of the problems solved. Then the substantial question will be what changes in scientific activities, particularly in weightings of scientific values, are needed to achieve the just-mentioned goal of science? As I mentioned earlier, in the situation where the existing scientific activities in a Kuhnian normal science continue to fail in resolving notable anomalies, the need for an alternative theory will be in order. In order to make such a need to be realized, there will have to be some change in the existing weightings of scientific values that have contributed to blocking the emergence of new theories. As such the strategy of methodological divergence (or tolerance) will need to be adopted. But not every divergence from the existing weightings of scientific values will do. Only certain specific tactics of changing the existing weightings of scientific values will be able to meet the need of allowing or even inducing an alternative theory with good prospects. Then what will be those specific tactics?

Kuhnian normal science, in the process of its development, that is, in the process of solving more and more problems (or puzzles), is very likely to display a general tendency to become more and more complex. Hence increasing the weighting of simplicity will contribute to undermining the monopoly of the existing theory and give a comparative advantage to a new theory. Similarly, decreasing the weighting of consistency will be needed to give a chance for a new theory to emerge as an alternative to the existing theory. For it will not be realistic to expect that from the beginning we come up with a new theory totally different from the existing theory. Usually a new theory will go through repeated piecemeal changes from the existing theory. Then transient versions of a new theory are likely to be internally inconsistent due to the mismatch between old components and new components of the theory.[41] So it will take time for a new theory to secure its internal consistency. What is worse will be the external inconsistency of a new theory with its neighboring theories. Such a situation is something expected since a new theory, being inconsistent with the old theory, will be hardly consistent with its neighboring theories that have been consistent with the old theory.[42] Perhaps it will take more time for a new theory to secure an external consistency with its neighboring theories. For the neighboring theories will have their own grounds and lifespans.

Both tactics of changing the weightings of scientific values are well illustrated in the early stage of the Copernican revolution. The state of Ptolemaic astronomy in the 16th century was, according to Kuhn, a scandal. As time goes on, more and more epicycles had to be added to save increasingly accurate data for planets revolving

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41) For example, consider Bohr's theory of atomic structure on its initial stage.

42) Copernican astronomy will be one prominent example.

around the Earth. As a result, Ptolemaic astronomy became exceedingly complex. The Copernican turn, that is, the shift from earth-centered universe to sun-centered universe brought with it a significant theoretical simplicity by reducing the number of epicycles used[43] and that of rather arbitrary auxiliary hypotheses. It is a fairly conventional understanding that Copernicus and his early followers accentuated the gain in theoretical simplicity of the sun-centered astronomy. It means that the increased weighting of simplicity in theory evaluation was quite instrumental in those astronomers' adopting the sun-centered astronomy. However, the tactic of increasing the weighting of simplicity may not have been good enough for the sun-centered astronomy to have its own chance. For the sun-centered astronomy, if literally taken, was outright inconsistent with the then-existing world view and its associated theories such as Aristotelian physics. Such a situation is not something peculiar to the Copernican astronomy. Facing the threat of self-inconsistency or inconsistency with other existing theories will be the fairly common fate of revolutionary new theories. Hence the methodological tactic of decreasing the weighting of consistency needs to be adopted for a new theory like the sun-centered astronomy to let it have its own chance to compete with the incumbent theory.[44]

Once a new theory emerges and succeeds in establishing itself as a

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43) Considering the disputes about the number of epicycles used in the Ptolemaic astronomy and in the Copernican astronomy, we may need to add the discussion about the reduction in the number of mathematical devices such as deferent, major epicycle, minor epicycle, eccentric or equant.

44) One anonymous referee points out that my discussion of the Copernican revolution is quite concise and so remains less than complete. I'm willing to concur and look forward to having another opportunity to discuss that matter in more detail.

respectable rival to the existing theory in the sense that the former theory fares better than the latter theory at least for some of the scientific values, the strategy of methodological divergence previously adopted in the process of inducing the emergence of the new theory will need to be replaced in due time by the strategy of methodological convergence that is expected to result in the return to the normal scientific weightings of values. More specifically, the above-mentioned two methodological tactics of changing weightings of the values, namely one of increasing the weighting of simplicity and the other of decreasing the weighting of consistency, will need to be gradually reversed so that the normal scientific weightings of values eventually come to be restored. The methodological motive of adopting such weights-adjusting tactics was to give a try to a new theory in the situation where the old theory was in trouble. Here the try was not to take sides with the new theory no matter what but just to give it a chance for competing with the old theory. In order for a new theory to emerge and to have a chance to compete with the incumbent theory, the former theory has to show that it can and does solve at least some of the notable anomalies which the latter theory has kept on finding troublesome. But that is not good enough for the new theory to maintain its status as a respectable rival to the existing theory. In due course it should solve many of the problems that the existing theory was credited with having solved. To have the upper hand on the old theory, the new theory will eventually have to show that it can solve, equally or better, most of the problems belonging to the achievements of the old theory, as well as at least some of the problems that troubled the latter theory. In doing so, however, the new theory itself is likely to come to lose its original simplicity gradually and become quite complicated so that the adopted tactic of increasing the weight of simplicity will put the new theory in an unfavorable situation. Hence as the competition between the new theory and the old theory goes on, the weighting of simplicity needs to be gradually lowered back perhaps to its original weighting. On the other hand, as time goes on, the new theory should be able to secure its internal and external consistencies. Hence the weighting of consistency needs to be gradually increased. Otherwise, the competition between the new and old theories will become unfair. For keeping the weighting of consistency indefinitely low will give an one-sided advantage to the new theory. Thus the methodological motive of reversing the two tactics under discussion is to make the competition between the new and old theories a fair one and to see which one wins over the other. If one theory comes to appear to win over the other through fair competition, then the apparent winner will deserve more resources for scientific activities.

It is time to consider the nature and status of the above-discussed methodological strategies of divergence or convergence and their related tactics of adjusting weights of scientific values. Discussing over those strategies and tactics, we had in mind what scientific activities constantly aim at. If those strategies and tactics are effective in maximizing the number and precision of the problems solved, then they will be assigned normative force on methodological grounds. Of course, in order that those strategies and tactics contribute to maximizing the performance of problem-solving activities, they will have to be adopted and practiced by individual scientists. But the justification of weights-adjusting tactics will not depend solely on how many individual scientists practice them. How much effective certain weights-adjusting tactics are in maximizing the number and precision of the problems solved will rely partly on the problem-solving capacity of a theory chosen as the result of adopting those tactics and partly on the problem-solving ability of individual scientists practicing those tactics. Then those weights-adjusting tactics inducing or enforcing a theory with relatively greater problem-solving capacity will be methodologically desirable and thus have a normative force, independently of whether they are actually practiced by scientists.

How does the normative force of weights-adjusting tactics operate? Let's take a Kuhnian normal science as the default (or the starting point), where scientists share quite homogeneous scientific values. But once normal science falls into a Kuhnian crisis, the need for an alternative theory will be in order. In order to make such a need realized, only a handful of scientists will have to adopt interpretations and weightings of scientific values that facilitate the emergence of a new theory. It means that scientific values shared by the scientists become divergent in their interpretations and weightings. Still the majority of scientists will stand by the existing application of scientific values, but the handful of scientists will run the risk of adopting the changed application of them. During the Kuhnian crisis, where the need for an alternative theory is in order, not every changed application of scientific values will do, but a changed application in tune with the above-mentioned two tactics of changing weights of the values, namely one of increasing the weight of simplicity and the other of decreasing the weight of consistency, is more likely to be apt.

For a new theory to be a starter at all, it will have to show at least a local (or partial) superiority over the existing theory. The typical way of displaying the local superiority will be to show that new theory can solve at least some of the notable anomalies which have troubled the existing theory for many years. Otherwise, the new theory will have to prove that it can score better than the existing theory with respect to at least some of the shared scientific values.

Initially the new theory will be benefited from the strategy of methodological divergence, specifically, the tactics like increasing the weight of simplicity or decreasing the weight of consistency. But such a benefit is just temporary and won't be permitted indefinitely. The methodological motive of such strategic benefit is not to take sides with the new theory, but just to give it a chance of preparing itself for a fair competition with the existing theory. Hence, as I suggested earlier, the just-mentioned weights-adjusting tactics will have to be reversed in due time. Thus, as time goes on, the way scientists apply their shared scientific values will have to become mostly homogeneous again.

Why these reversing tactics? The answer is likely to be based on the following considerations:

(1) The basic goal of scientific inquiry is to maximize the number and precision of the problems solved.
(2) Normal science is a very efficient instrument for maximizing the number and precision of the problems solved.
(3) Normal science embodies its characteristic mode of applying scientific values.
(4) The normal scientific mode of applying scientific values is indispensable or comparatively more efficient for its maximizing the number and precision of the problems solved.
(5) Hence, the ultimate competition between an existing theory and its alternative should or had better be conducted, sharing the normal scientific mode of applying scientific values.

Then, in order to share the normal scientific mode of applying scientific values, the strategy of methodological divergence and its associated weights-adjusting tactics adopted for inducing and accommodating a new theory will need to be reversed in due time so that its mode of applying scientific values may converge with that of the existing theory. Further, once it is admitted that the main goal of scientific inquiry is to maximize the number and precision of the problems solved and further that scientists cannot but rely on Kuhnian value-based procedure for theory choice, the above-mentioned strategies and tactics of applying scientific values will have to serve as methodological constraints which turn out to be indispensable or at least efficient means for achieving that main goal of science.

What will be the consequences of taking those strategies and tactics as methodological constraints? The strategy of methodological divergence, specifically, the strategy of adopting in a constrained manner the tactics of increasing the weight of simplicity and of decreasing the weight of consistency will have the effect of inviting some, though just a handful, of the scientists to explore a new theory. Suppose, however, that no individual scientist is or too many scientists are willing to abandon the existing theory for an unknown new theory. Both situations are not in tune with the just-mentioned strategy and its associated tactics operating as methodological constraints. Under such methodological constraints in operation, then, it will be no longer the case that either the monopoly of the existing theory indefinitely continues or every alternative theory choice is taken to be rational. Thus the strategy of methodological divergence and its associated tactics of applying scientific values, when aptly adopted as methodological constraints, will serve as the ground for solving the rational dissensus problem, as well as what we called the Kuhnian problem of excessive methodological permissivism. Similarly, the strategy of methodological convergence and its associated tactics of reversing both increased weight of simplicity and decreased weight of consistency in due time, if aptly adopted as methodological constrains, will induce the previously diverged mode of applying scientific values to converge gradually with its normal scientific mode and let one of the competing theories emerge as the victor through fair competition. As such, they will contribute to the relevant scientific community's regaining its consensus in theory choice, and thus to solving the consensus formation problem.

5. Implications of My Revisionary Proposal

From now on, I will pay attention to how the above-mentioned strategies and associated tactics of applying scientific values work as methodological constraints. Basically they work on the community level. What I mean by "working on the community level" is this: Those strategies and tactics suggest, for example, that it is desirable for just a small portion of the relevant scientific community to explore a new theory when the existing theory continues to fail to meet the expectation of the community; however they do not do it directly to individual scientists but on the community level. Suppose that during a Kuhnian crisis no individual scientist attempts to explore an alternative theory, or in other words, no scientist dares to adopt the strategy of methodological divergence and its associated tactics of increasing the weighting of simplicity and decreasing the weighting of consistency but sticks to the existing normal scientific mode of applying scientific values. Then in the light of those methodological constraints on applying scientific values for a Kuhnian crisis, the relevant scientific community as a whole will be blameworthy but not particular individual scientists. Why? The majority of scientists belonging to the scientific community are expected to stay with the existing normal scientific mode of applying scientific values. In such a situation, why should a particular scientist be responsible for taking up the burden of adopting the changed mode of applying scientific values that is suitable for exploring a new theory? It won't be fair to ask one or other particular scientist to become either a hero, if lucky, or a scapegoat, if unlucky.

If we are right about the way the strategies and associated tactics of applying scientific values work as methodological constraints, what implication does it have about the nature of scientific rationality? It seems to suggest that the primary unit for attributing scientific rationality should not be individual scientist but scientific community. What do I mean by saying that scientific community is a primary unit for scientific rationality? As I mentioned above, during a Kuhnian crisis only a handful of scientists are expected to explore and adopt a new theory, but it won't be appropriate to blame one particular scientist or another for not doing so. However, it will be still open and appropriate to blame a scientific community for not being so rational, if none of the community members is willing to adjust his or her existing mode of applying scientific values and to explore a new theory. During the period when a new theory continues to catch up with the problem-solving performance of the existing theory but is not yet winning over it in a decisive manner, it will be desirable that the resources of scientific inquiry, including the number of scientists committed to a particular theory, are appropriately divided between the old theory and the new theory at least roughly in proportion to their problem-solving records. Suppose, however, that the ratio of those resources divided between the two theories is significantly different from the ratio of their problem-solving records. Then blaming the whole scientific community for not being so rational will be in order. Furthermore, those individual scientists committing themselves to one theory or another beyond the number of scientists appropriate in the light of its problem-solving record could be blamed for not being so rational. Still, blaming individual scientists for making an overcommitment to one theory or another will be derivative from blaming the relevant community for a similar reason. Thus it will be difficult or even impossible to make an evaluation of individual scientist's commitment to a particular theory without regard for his or her fellow scientists' preceding commitments to one theory or another. From this consideration I conclude that the rationality (or irrationality) of scientific community in theory choice is prior to that of individual scientists.

In the rest of the discussion I will argue that our earlier discussion concerning the way the above-mentioned strategies and tactics of applying scientific values work as methodological constraints has an interesting implication about the nature of scientific methodology. What the traditional philosophers of science before Kuhn expected from scientific methodology is to produce a unique conclusion in theory evaluation and, on that ground, to enforce unanimous decision in theory choice. They thought such methodological aim could be achieved by algorithmic procedures of theory evaluation. Various proposals for that aim were made but turned out to be largely unsuccessful. Kuhn's alternative proposal, that is, his value-based procedure for theory evaluation and choice turns back against the just-mentioned methodological aim. Instead it allows scientists to disagree in theory choice, and takes it to be its merit. My revisionary version of Kuhn's proposal also allows scientists to disagree in theory choice but in a constrained manner. Thus methodologically normative forces are exerted on the way scientists disagree in theory choice. That is where the strategy of methodological divergence and its associated tactics of adjusting value weightings find its role. On the other hand, the trouble with Kuhn's value-based view has been that it fails to make explicit how to regain the overall agreement in theory choice among scientists. Here what I called the strategy of methodological convergence and its associated tactics of adjusting value weightings in my revisionary version of Kuhn's view are expected to play the role of inducing overall agreement in theory choice among scientists in due time. The process of solving the consensus formation problem involves, if I am right, not only some strategy and tactics of applying scientific values as methodological constraints but also the basic goal of scientific inquiry, that is, the goal of maximizing the number and precision of the problems solved.

From the preceding discussions about the roles that the strategies of methodological divergence or convergence and associated weights-adjusting tactics play in the context of theory evaluation and choice, I think we can draw the conclusion that those strategies and tactics satisfy the following conditions for adequate scientific methodology:

(M1) An adequate scientific methodology should allow scientists to disagree in theory choice in a constrained manner during Kuhnian scientific revolutions.

(M2) An adequate scientific methodology should give a qualified new theory a chance of having a fair competition with the existing theory. Here a new theory is qualified if it initially solves at least some of the notable problems that the existing theory has failed to solve despite its repeated attempts and further it makes a sustained progress in solving the problems that the existing theory has succeeded in solving.

(M3) An adequate scientific methodology should induce in due time virtually all or most scientists of a relevant scientific community to make an agreement in theory choice.

As I mentioned earlier, the traditional contemporary philosophers of science before Kuhn, particularly logical empiricists and Popper, opted for a logic-driven algorithmic approach to theory evaluation, and in doing so, wanted to yield a unique conclusion, that is, a consensus in theory choice. Scientific methodology that embodies such a logic-driven algorithmic approach deserves being called hard (or strong) methodology. On the other hand, the scientific methodology ensuing from my revisionary version of Kuhn's value-based approach does not rely on quantitative measures of how much theories are confirmed or corroborated.[45] Further it does not involve the idea that, whenever needed, we can at least in principle make a comparative judgment about which theory is better than the other and enforce scientists to choose the better one in a unanimous fashion. However, it still serves as the source of supplying methodological norms for inducing disagreement as well as agreement in theory choice among scientists, and thus satisfies (M1), (M2) and (M3). Hence, I suggest we call it soft methodology.

45) I'd like to add that my revisionary version under consideration would not exclude but incorporate quantitative measures of theory confirmation or corroboration, if they are available at all.

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과학적 합리성에서의 쿤적 전환

조 인 래

쿤이, 이론 평가에 대한 당시 주류 과학철학의 형식적 접근들 대신, 과학적 가치들에 기반한 이론 선택의 절차를 채택했다는 것은 잘 알려진 사실이다. 그러나 가치에 기반한 그의 이론 선택론은 두 가지 문제, 즉 과도한 방법론적 방임주의의 문제와 합의 형성의 문제에 직면한다. 이 문제들을 해결할 수 있는 방안을 모색하는 과정에서 나는 가치에 기반한 쿤의 이론 선택론에 대한 수정안을 제시하고 이를 옹호하고자 한다. 특히 나는 이 수정안에서 방법론적 일탈 또는 집중의 전략 그리고 그것과 연계하여 과학적 가치들의 가중치를 조정하는 전술들이 이론 도입 및 평가의 과정에서 방법론적 제약으로 채택될 필요가 있으며, 그럴 경우 이 방법론적 전략 및 전술들은 앞서 언급된 과도한 방법론적 방임주의의 문제와 합의 형성의 문제를 해결하는데 기여한다고 주장한다. 나아가서 나는 그러한 수정안이 과학적 합리성과 과학적 방법론에 대해 가지는 것처럼 보이는 두 가지 중요한 함축들을 제시하는데, 그 중 하나는 이론 선택의 과정에서 과학자 공동체의 합리성이 개별 과학자들의 합리성에 선행한다는 것이고 다른 하나는 나의 수정안에 따른 과학적 방법론이 몇몇 적합성 조건들을 충족시킬 뿐만 아니라 연성 방법론에 해당한다는 것이다.

주요어: 토머스 쿤, 과학적 합리성, 이론 선택, 과학적 가치, 합의 형성의 문제