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A weakness in Kuhn's regal argument

The significance of the Kuhn-loss thesis

Kuhn's alleged relativism is connected mainly with the following basic views of him. Kuhn claims, among other things, firstly, that scientific revolutions, alongside gains, involve losses, for example of explanatory power and of problem-solving ability. Secondly, he sees that as a consequence of incommensurability, no reduction relation can be constructed between successive paradigmatic theories. Thirdly, according to Kuhn, in the case of ontologies of such successive theories, one cannot speak of an approach to the truth. Fourthly, he claims that there are no surface measures for the nearness to truth of such theories.

In the following, I will concentrate only on the first claim, which is here called the *Kuhn-loss thesis,* or (K1) for short. What the Kuhn-loss thesis claims is this:

(K1) If (P,Q) is a pair of paradigms, a revolution between them, then the revolutionary transition from P to Q involves (alongside gains) losses (for example of explanatory power and of problem-solving ability).

The Kuhn-loss thesis (K1) is the gate through which Kuhn allows the social context of theory choice to enter. Moreover, this is the very thesis which invited Kuhn to use the term 'revolution', or as Kuhn himself puts the matter (1961: 184):

> "In fact, it is largely the necessity of balancing gains and losses and the controversies that so often result from disagreements about the appropriate balance that make it appropriate to describe changes of theory as "revolutions"."

If it were to turn out that the Kuhn-loss thesis did not hold, the significance of this for the study of the development of science and the growth of knowledge would be crucial — as of course for Kuhn's own theory. What consequences the collapse of this thesis would have for his theory , or how fatal they would be, need not be gone into here. Thus, it is not peripheric to take a closer look at the way, and the firmness of the way, that Kuhn has used when building the base for his Kuhn-loss thesis.

In the following I shall bring up a failure of Kuhn to have conclusively established the Kuhn-loss thesis. That Kuhn's argumentation is faint here is quite contrary to what he believes to have firmly shown (see e.g. 1970: 169; 1961: 184—185). My comments concern the examples Kuhn employs, regarding which I shall question whether they serve his theory if *that* theory is taken as the criterion. Of the examples available I have selected for consideration those two exemplars which play a crucial role in the main Kuhnian source material, *The Structure of Scientific Revolutions* (Kuhn 1970: e.g. 104—108, 148, 157) and elsewhere in Kuhn, too (consult for instance 1961: 181, 183—185). It is important here to notice that in the controversy which later was aroused by Kuhn's original and well-known works, Kuhn consistently with his earlier stand, still maintains that his examples, among them especially the first one of the two which I have in mind, provide the grounds for the Kuhn-loss thesis. For instance in his answer in his article (1976) to Stegmüller, who believes his Sneed formalization of Kuhn to have shown the Kuhn-loss thesis to be *erroneous*[1], Kuhn refers directly to the examples he has used, thus:

> "Consider, for example, just one of the many counterexamples I have developed elsewhere [Kuhn 1970: 107]." (1976: 192, italics added)

My analysis thus focusses firstly on this "one" example — after all, the author *himself* upholds it as a model case in wishing yet again to appeal to it. In conclusion I shall uncover the weakness of the other example and shall finally tentatively indicate the problematicity of Kuhn's exemplary store in general as evidence for the Kuhn-loss thesis.

Identifying the gap in the Kuhn-loss theory

In a scientific revolution, the former of two successive *theories* is lost. Losses of theories are consequent upon the incompatibility and incommensurability theses and are not in any way problematic or disputable in the present context[2]. In contrast to this, many find it at odds with intuition that in the course of the progress of science, *solutions* already found to problems should be lost, a point which Kuhn stresses:

> "Because the unit of scientific achievement is the *solved problem* and because the group knows well which problems have already been solved, few scientists will easily be persuaded to adopt a viewpoint that again opens to question many problems that *had* previously been solved." (1970: 169; italics added)
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> "... the new paradigm must promise to preserve a *relatively* large part of the *concrete problem-solving ability* that has accrued to science through its predecessors." (1970: 169; italics added)
>
> "... though new paradigms *seldom or never* possess all the capabilities of their predecessors, they usually preserve a great deal of the most concrete parts of past achievement ..." (1970: 169; italics added)

The above-cited passages speak of losses of problem-solving capacity concomitant with transfer from one paradigm to another. The wording induces the reader to conclude that such losses have in Kuhn's book already been *shown* to occur. And indeed, in an earlier passage on p. 148 Kuhn does mention two specified cases (these are my "two" cases) in connection with which he believes loss to have taken place alongside gain:

> "The transition to Lavoisier's paradigms had, like the transition to Newton's, meant a loss not only of permissible question but *of an achieved solution.*" (Italics added)

The former of these examples is precisely the one Kuhn (1976) appeals to in defense of his Kuhn-loss thesis. The exact formulation of the example is this:

> "Before the chemical revolution, one of the acknowledged tasks of chemistry was to account for the *qualities* of chemical substances and for the changes these qualities underwent during chemical reactions. With the aid of a small number of elemen-

> tary "principles" — which *phlogiston* was one — the chemist was to explain why some substances are acidic, others metalline, combustible, and so forth. Some success in this direction had been achieved. We have already noted that phlogiston explained why the metals were so much alike, and we could have developed a similar argument for the acids. Lavoisier's reform, however, *ultimately did away* with chemical "principles", and thus ended by depriving chemistry of some *actual* and much potential explanatory power. To compensate for this loss, a change in standards was required. During much of the nineteenth century failure to explain the qualities of compounds was no indictment of a chemical theory." (1970: 107; italics added)

Here, however, it is quite grounded to be suspicious about Kuhn's analysis. Is it really legitimate to use the phlogiston theory as a full-scale example of a mature science, the solutions of which were lost in the shift resulting from the victory of Lavoisier's chemistry? But Kuhn has anticipated such doubts and gives his definite answers to them:

> "It would be wrong to dismiss this *loss* of explanatory power by suggesting that the success of the phlogiston theory was only an accident which reflected no characteristic of nature. The metals do have common characteristics, and these can now be explained in terms of the similar arrangements of their valence electrons." (1976: 198, n. 12; italics added)

I will render Kuhn's answer unsatisfactory, however, by deciding in its substance to accept it but at the same time by throwing doubts on another matter, namely the *relevance* of this loss-example for Kuhn's *own* model of scientific change (and revolution). To begin with: the phlogiston theory did of course reflect certain characteristics of nature — there is no reason at all to question *that*. But, secondly, the fact that a theory reflects some characteristics of nature does not necessarily entail that this particular theory is paradigmatic or generated by a paradigm. A paradigm goes hand in hand with mature science, high-level normal scientific practice as a sociological indicator of the emerged paradigm. When normal science and its paradigm are lacking, the scientific sub-field in question may — if I interpret Kuhn correctly — however be steered by one or two entities (in the latter case there are competing schools in the field) which are aptly called *something much like a paradigm* (the term is Kuhn's, see 1970: ix) reflecting some characteristics of nature, as it were. And — roughly speaking — if the development of the discipline in question is followed back in time onto a still lower level of its history, the discipline may scatter into different natural philosophies which cannot give support to problem solving. (See Kuhn 1970: e.g. v—ix, 1—22.) Thus, thirdly, it must be realized that the fact that the phlogiston theory beyond question reflected some characteristics of nature and beyond question was practiced by a large group of scientists does not necessarily imply that phlogiston was a paradigm. And intuitively it would seem to me very odd to consider phlogiston as a paradigm because that would declare the phlogiston theory as an instance of mature science, examples of which are, say, the high-standard normal sciences of the astronomy of Ptolemy and the Einsteinian theory. However, be that as it may, phlogiston is not a clear-cut case of a paradigm. Then, finally and fourthly, I cannot but come to this: the pair (phlogiston, Lavoisier) which nicely pictures — and maybe most nicely of Kuhn's examples — the *nature* of the loss-phenomenon, does not, however, univocally at all lend support to the Kuhn-loss thesis because phlogiston, which may not be a paradigm at all, is at least not a clear-cut case of such an entity.

In the same context as his Lavoisier example, Kuhn mentions the emergence of Newton's paradigm as another example of the way former achievements may be lost in phases of scientific change (see Kuhn 1970:148; consult also pp. 104—105). Unfortunately enough, with this other example of Kuhn's we face, first, the same problem as with the former pair (phlogiston, Lavoisier): Kuhn compares Newton's famous para-

digm with the mechanico-corpuscular world view (the Cartesian view) which perhaps is not a paradigm. Besides, be the status of the mechanico-corpuscular view as it may, we in any case land into difficulties which may be uncovered by citing Kuhn 1961. There Kuhn uses this same particular example which he in *The Structure of Scientific Revolutions* (Kuhn 1970) persistently claims to exemplify the Kuhn-loss thesis, namely the pair (Cartesian view, Newton's theory), and writes:

> "Newton's theory of planetary and projectile motion was fought vehemently for more than a generation because, unlike its main competitors, it demanded the introduction of an inexplicable force that acted directly upon bodies at a distance. Cartesian theory, for example, had *attempted* to explain gravity in terms of the direct collisions between elementary particles. To accept Newton meant to abandon the *possibility* of any such explanation ..." (1961: 184; italics added)

Kuhn has of course established beyond question that such *possibilities* were lost. But what were that *actual* achieved solutions which were lost as Newton's theory gained the ascendancy, the fact of whose loss would in the required sense lend support to the Kuhn-loss thesis? Unfortunately enough, both Kuhn's main work (1970) and Kuhn 1961, which present the pair (Cartesius, Newton) as a model case of the Kuhn-loss thesis, lack evidence for an actually achieved but subsequently lost solution. This situation makes Kuhn's exemplar void even if the pair (Cartesius, Newton) was (to my mind) unnaturally accepted as an instance of a pair of sucessive paradigms.

Conclusions

I have above considered two model cases which Kuhn voices as thoroughly exemplifying instances of the Kuhn-loss thesis, the other important especially as in the hectic later Kuhn-controversy Kuhn, as a counter-argument to Stegmüller, still refers to this particular exemplar. Both of Kuhn's case examples, the pair (phlogiston, Lavoisier) and the pair (Cartesius, Newton) are, however, problematic. In order to serve their purpose, the pairs should have real paradigms as their first members (as they actually have as their second members), i.e. both phlogiston and the Cartesian view should be paradigms (as *are* Lavoisier and Newton). This would imply that the phlogiston theory and the mechanico-corpuscular world-view represent normal science and thus are on the same level of mature science as, say, Ptolemian, Newtonian and Einsteinian normal sciences. To me, that state of affairs would seem extremely odd. It is true, of course, that the border-line between a paradigm and a non-paradigm is — and taking the nature of the matter into consideration: cannot but be — fuzzy so that declaring phlogiston and the Cartesian view as non-paradigms is too complicated a task, at least in the present context. On the other hand, I cannot see how these cases could be declared as paradigms either, as Kuhn does, their *"best"* possible state being fuzzy border-line cases. Note moreover that the borderline being fuzzy does not perturb the fact that at the extreme ends of the "axis" there are clear-cut examples as well of non-paradimgs, and of paradigms in general and of successive paradigms in particular (e.g. the pair (Newton, Einstein) of successive paradigms). Why is it so that Kuhn — also in the later discussion — refers heavily to those fuzzy cases which might even prove to be non-paradigms? And this is not all: the other of the above cases, the pair (Cartesius, Newton) suffers in the context of the Kuhn-loss thesis from a defect which makes the whole example void independently of the Cartesian view being a paradigm or not: Kuhn shows only the loss of the potential *problem* field but has nothing to say about the achieved, but then lost *solutions*.

The Kuhn-loss thesis is a most important and fascinating — and in literature massively discussed — view of the development of (mature) science. The thesis presupposes the existence of such successive paradigms,

say R and S, that in the shift R/S the achieved solutions to problems are (partly) lost. Those examples of Kuhn which I have analysed above do not, however, if I am right, have the properties demanded and thus do not serve their purpose. If in Kuhn's works better examples are not to be found — and to my reading of Kuhn those better examples may be hard to be picked up — then there must be something wrong with Kuhn's regal argument.

NOTES

1. In the context of the structuralistic reconstruction of Kuhn which Stegmüller arrives at on the basis of Sneed's work, Stegmüller posits as his crucial implication that the theory that emerges victorious in a scientific revolution is capable of solving everything that its predecessor solved, and something more besides (Stegmüller, 1976: 218).
2. Theories are lost as objects of actualization (Kuhn 1970: 23—34: 92—110) but still can be retained as instruments for technology (see e.g. Verronen 1986: 147—167). Be it noted that the phenomenon of theory-loss is *not* to be met in mathematics: quite contrary to what for instance C. Parkinson has recently proposed, Kuhn's model cannot be applied to describe the groubth of mathematical knowledge (see Verronen 1992).

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