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ARE PHYSICISTS' PHILOSOPHIES IRRELEVANT IDIOSYNCRASIES?

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ABSTRACT

This article argues that individual philosophical commitments of scientists can decisively influence scientific practice. To support this claim, two historical examples are presented, concerning controversies between physicists about central problems in their field. Confrontation of the theories of Kuhn, Lakatos, and Laudan with these examples reveals their inadequacy to explain the role of individual commitments. It is concluded that an adequate model of scientific change should exhibit a three-level structure.

1. *Introduction*

Many scientists, particularly theoretical physicists, occasionally indulge in philosophical reflection on the aims and methods of their profession. One only has to glance through Einstein's popular writings to find many examples, and if one might think he is an exception, read Bohr, Boltzmann, Feynman, Helmholtz, Schrödinger, Weinberg, and so on. Often these scientists gave their own individual answers to philosophical questions concerning science. What is the value of their views? Can they be significant for scientific practice, or should they be brushed aside as irrelevant?

Traditional philosophers of science gave the latter answer. They advanced *a priori* conceptions of rationality, comprising universal answers to epistemological and methodological questions. Scientists' actions should conform to this universal philosophy, while their words were ignored (except when these accidentally agreed with those of the philosopher in question). Since the historical turn in the philosophy of science, that state of affairs has of course changed. However, in my opinion, even historically oriented philosophers of science such as Kuhn and Lakatos do not fully appreciate the value of scientists' philosophies.

In this article I argue that individual philosophical commitments of scientists can decisively influence the development of science. To support this claim, I discuss two examples from the history of physics. Both examples concern a controversy between two individual physicists over a central problem in their discipline. The way they approached these problems, and the role of their philosophical commitments therein, is examined and compared. In particular, I analyze their opinions of (allegedly) *ad hoc* explanations, because these shed an especially interesting light on the heuristic role of physicists' philosophies. Subsequently, the results of these 'case studies' are confronted with three influential theories of scientific change, to wit those of Kuhn, Lakatos, and Laudan. It will turn out that these theories fail to provide satisfactory accounts of the cases. I investigate how a more adequate theory can be constructed.

In Section 2 the three theories of scientific change are reviewed with respect to their assertions regarding the heuristic role of philosophy. Section 3 presents the first example, the controversy between Maxwell and Boltzmann over the specific heat anomaly in the kinetic theory of gases, and discusses the merits of the theories of science in explaining this episode. In Section 4 the second example, the controversy between Pauli and Heisenberg over the anomalous Zeeman effect in quantum theory, is examined. Finally, Section 5 contains suggestions for an improved model of scientific change.

2. *Theories of science and the heuristic role of philosophy*

Everyone will agree that philosophy in some way influences science. It is the question as to the *precise* way in which this happens, that might be a subject of debate. Traditional philosophy of science asserts the existence of a set of stable and universal philosophical principles governing scientific development. A clear example is Popper's falsificationist methodology. On such views, the influence of philosophy is restricted to a universally valid 'philosophy of science', which may be called *constitutive* of science, *i.e.* underlying all scientific activities and results. However, since philosophers of science have turned to the history of science, they have become aware of the possible existence of *differentiating* philosophical influences, *i.e.* contextually different factors which may affect the direction and content of scientific development.

One important place where one might expect to find philosophical influences, is in the *heuristics* of scientists.[1] Today it is generally acknowledged that scientific discovery cannot be reduced to some algorithmic procedure. However, neither is it a completely arbitrary affair. Scientific heuristics guides the process of discovery without determining it. In a more precise definition of Radder (1991, p. 196): "heuristic rules [...] guide the search for new knowledge by drastically restricting the number of possible roads or by positively suggesting which general directions to take in the searching process". Obviously, philosophical views are possible sources for heuristic rules, both of the negative as well as of the positive variety.

The distinction between two different levels of possible influences of philosophy on scientific development — namely a universal level of constitutive influences and a contextual level of differentiating influences — can also be applied to heuristics. Some might contend that there is a universal heuristics guiding all scientific research, whereas others might hold that the heuristic function of philosophy is a purely contextual affair. Of course, one might also take an intermediate position by asserting that there are *some* philosophical influences which are universal, and thus constitutive of science, while *other* influences may be contextual, and play a differentiating role. As hinted at above, the unqualified thesis that philosophy affects science is trivially true, and it is indeed rather the question of to what extent this influence is either constitutive or differentiating (or both) that deserves to be answered. This paper will focus in the first place on differentiating philosophical influences.

In the examples discussed in the next sections the notion of adhocness has a prominent place. This is not a coincidence because, as we shall see, it depends on one's philosophical beliefs whether a specific hypothesis is considered as *ad hoc*, and also whether an *ad hoc* hypothesis is deemed objectionable.

An *ad hoc* hypothesis is typically 'isolated'. This characteristic can be conceived in two different ways: firstly, in the sense that its explanatory scope is restricted; secondly, in the sense that its construction is motivated only by the fact to be explained and not by some more general plan. The first conception of adhocness is put forward by Popper (1972,

p. 15-16), who defines an *ad hoc* hypothesis as a hypothesis which is not independently testable. Thus, Popper translates the idea of 'scope' into 'independent testability'. I will term this conception 'consequential adhocness'. The second conception places emphasis on the way in which the hypothesis was constructed, instead of its consequences. In this view, a hypothesis is deemed *ad hoc* if it is not embedded in a general plan, or, in other words, if it is not in line with accepted heuristic strategies. Accordingly, adhocness in this sense is dependent upon heuristics: a scientist's heuristics defines when a hypothesis is viewed as *ad hoc*. I will call this conception 'generative adhocness'.

In the remaining part of this section I compare three theories of scientific change with respect to their claims concerning the heuristic role of philosophy. The theories are Kuhn's theory of scientific revolutions, Lakatos's methodology of scientific research programmes, and Laudan's problem-solving model. Assuming the general claims of these theories to be well-known, my discussion is restricted to their (explicit or implicit) assertions about heuristics, adhocness, and the influence of philosophy on science.

2.1 Kuhn's theory of scientific revolutions

How can philosophy affect science, according to Thomas Kuhn's theory? First of all, paradigms are partly philosophical in character. The philosophical components of the paradigm are reflected in the disciplinary matrix, particularly in its 'metaphysical elements' and 'values' (Kuhn, 1970a, p. 185). The metaphysical elements may have their roots in ontological doctrines, while values are chiefly of a methodological nature. During the period of normal science the metaphysical elements are fixed, and though individual value differences might exist, they are usually not very important. Consequently, the role of philosophy during normal science is relatively static, but essential: its role is both heuristic and justificatory, since the acceptance of theories by the scientific community is determined by standards which are relative to the paradigm.

In periods of crisis and revolution philosophical ideas can be significant in a quite different, more dynamic manner. Scientists begin to consider alternative routes outside the paradigm and its fixed heuristic, and thus individual ontological and methodological ideas can influence scientific research. Kuhn states that it is "particularly in periods of acknowledged crisis that scientists have turned to philosophical analysis as a device for unlocking the riddles of their field" (Kuhn, 1970a, p. 88). Individual differences in the application and appraisal of values can become important factors in such crises. However, Kuhn does not make very specific assertions about the mechanisms of philosophical influences in crisis-like situations. When a revolution finally occurs, the philosophical commitments of the revolutionary scientists will be reflected in the character of the new paradigm. Moreover, because Kuhn (1970a, p. 150) claims that a scientific revolution is not "forced by logic and neutral experience" alone, these philosophical influences remain partly irreducible components of the paradigm.

In Kuhn's theory the character of heuristics in periods of normal science differs essentially from that in periods of crisis and revolution. During normal science the paradigm specifies clear, fixed heuristic guidelines for puzzle solving, which are embodied in the exemplars. In periods of crisis, however, the heuristics of the paradigm fails to perform its puzzle-solving tasks, and scientists will try alternative heuristic strategies. The choice of such alternative heuristics will be affected by the aesthetic preferences and value judgments of individual scientists. Furthermore, Kuhn's theory provides a generative conception of adhocness. When the puzzle-solving activities of normal science are guided by the standard heuristics, they yield 'expected' solutions, which are by definition not *ad hoc*, since they are not isolated but part of a general plan. By contrast, when scientists deviate from the heuristic rules in order to solve an anomaly while saving the paradigm, such attempts can be described as *ad hoc* adjustments (Kuhn, 1970a, p. 83).

2.2 Lakatos's methodology of scientific research programmes (MSRP)

In Imre Lakatos's MSRP the role of philosophy is to some extent the same as in Kuhn's theory, but in an important sense also far more restricted. The place where philosophy has a legitimate role to play is first of all in the hard core of research programmes (RPs). Because the hard core is, by convention, metaphysical in nature, it may have its origin in all kinds of philosophical beliefs. Ontological ideas are good candidates to constitute the basis of an RP. For example, the hard core of the kinetic theory contains the ontological ideas of atomism and mechanicism. As the positive heuristic is related to the hard core, the philosophical ideas underlying the hard core are in most cases also reflected in the positive heuristic. However, once an RP has started, the hard core and the positive heuristic normally do not change, and accordingly the role of philosophy remains quite static. This feature is reinforced by Lakatos's demand that the development of an RP must be predictable to some extent, that is, the positive heuristic must work according to a "preconceived plan" (Lakatos, 1976, p. 9).

This is not yet essentially different from Kuhn's account of the role of philosophy *within* a paradigm. However, there is one important difference, which is connected with the fact that MSRP also provides a normative methodology. In MSRP the rationality of historical episodes is assessed by means of a universal hypothetico-deductive methodology: if a theory is constructed within an RP, novel predictions have to be deduced from this theory, which have to be tested against experience. This is the only way in which scientific theories can be justified. When scientists accept a theory for other reasons, they are simply mistaken. As long as their acceptance is reconstructible as rationally warranted by MSRP's hypothetico-deductive methodology, this does not matter. However, if this is not the case, the scientist behaves irrationally. Thus, Lakatos claims that rational scientists adhere to a hypothetico-deductive (HD) methodology of justification, whether consciously or intuitively. In this respect, he disagrees with Kuhn, who permits the possibility of paradigm-relative methodological standards and individual differences in value appraisal. Lakatos's HD-conception of scientific rationality leads to his view of *ad hoc* explanations as the prime symptoms of the degeneration of an RP. Lakatos (1970, p. 175) defines three types of adhocness:

adhoc$_1$ theories which have no excess empirical content [*i.e.* novel predictions] over their predecessors.
adhoc$_2$ theories which do have such excess content but none of it is corroborated.
adhoc$_3$ theories which do not form an integral part of the positive heuristic.

Lakatos's *adhoc*$_1$ and *adhoc*$_2$ represent a consequential conception of adhocness, while *adhoc*$_3$ represents a generative conception. He explains his disapproval of the latter type as follows: "I define a research programme as degenerating even if it anticipates novel facts but does so in a patched-up development rather than by a coherent, pre-planned positive heuristic" (Lakatos, 1976, p. 11n). Thus, Lakatos's objection to *adhoc*$_3$ theories is related to his conception of heuristics as a preconceived plan, anticipating the future course of the RP. An *adhoc*$_3$ theory is objectionable because it is not in line with this plan. In other words, it negates the heuristic power of the RP: "Mature science consists of research programmes in which not only novel facts, but, in an important sense, also novel auxiliary theories are anticipated; mature science — unlike pedestrian trial-and-error — has 'heuristic power'" (Lakatos, 1970, p. 175). Thus, Lakatos deems the way in which new theories are constructed highly important. However, as Nickles (1987) observes, there appears to be an ambiguity in his criteria of appraisal. Lakatos denies the possibility of generative epistemic support. Instead, his view of epistemic support is purely consequential: only empirical facts which are not involved in the construction of theories (novel predictions) provide epistemic support for theories. It follows that, from a justificationary point of view, only *adhoc*$_1$ and *adhoc*$_2$ adjustments are truly objectionable.

This becomes even clearer when one considers Lakatos's claim that the positive heuristic can also be changed in a successful, progressive manner: "It occasionally happens that when a research programme gets into a degenerating phase, a little revolution or a *creative shift* in its positive heuristic may push it forward again" (Lakatos, 1970, p. 137). Thus, the essential difference between an *adhoc*$_3$ theory and a creative shift in the positive heuristic is that the latter restores the progressiveness of the RP, that is, it leads to (verified) novel predictions. However, since an *adhoc*$_3$ theory also anticipates novel facts, it can only be determined with hindsight whether changing the positive heuristic was an *adhoc*$_3$ move or a creative shift, namely by ascertaining whether the RP continued to make novel predictions after the change. Thus, while generative adhocness is disapproved of by Lakatos, it ceases to be objectionable if it leads to a sufficient number of novel predictions.

In contrast to its HD-methodology of justification, MSRP's methodology of discovery is not universal, but this does not imply that anything goes. On the contrary, scientists working in an RP have to follow specific 'discovery rules', supplied by the positive heuristic. Except for creative shifts, these rules cannot be changed. Actual methods of discovery should be identical to, or reconstructible as, the methodology specified by the positive heuristic. Consequently, Lakatosian methodology of discovery is to some extent a local matter: it may differ for different RPs. However, all scientists working in the same RP should use the same methods.

In conclusion, according to MSRP, the role of philosophy in scientific development is rather limited. To be sure, it has an essential part to play in the scientific enterprise, namely as a possible source of scientific research programmes. Ontological ideas may be part of the hard core of an RP, and will then also be reflected in its positive heuristic. However, there is no room for differences between philosophical ideas of individual scientists. Moreover, as MSRP claims that there exists a universal normative methodology, the local influences of philosophy on the character of RPs are in the end irrelevant to the dynamics of scientific development. This conclusion is, of course, in agreement with Lakatos's original aim, namely to save science from alleged Kuhnian irrationality, brought about by influences of this kind.

2.3 Laudan's problem-solving model.

In his book *Progress and Its Problems* Larry Laudan defends his problem-solving model of science by stating that, more than any other theory, it does account for external influences on science and even for the rationality of such influences.[2] He argues that most historians and philosophers have wrongly neglected the interaction between scientific and non-scientific disciplines which has in fact always existed (Laudan, 1977, p. 128, p. 174, and p. 213). What precisely does Laudan's theory assert about the role of philosophical factors in scientific development? I will answer this question by comparing the theory with Lakatos's MSRP, since Laudan's research traditions (RTs) have many similarities with Lakatos's RPs. Indeed, the role of philosophical factors within them is also to a large extent the same. Thus, an RT consists of an ontological and a methodological component, which seem roughly to correspond, respectively, to 'hard core' and 'positive heuristic'. Unlike a Lakatosian RP, the basic elements of an RT may change over time. This implies that in Laudan's theory the role of philosophy can be more dynamic. For example, a particular metaphysical doctrine which was formerly an unrejectable element of an RT, may later be rejected by a scientist without leaving the RT. However, such a change can occur only if it is discovered that this element was, after all, not essential to the problem-solving effectiveness of the RT (Laudan, 1977, p. 100).

An original feature of Laudan's theory, compared with Lakatos's, is his analysis of conceptual problems and his claim that these are at least as important as empirical problems. His account of the role of conceptual problems asserts the existence of a specific type of interaction between scientific and philosophical ideas, and, moreover, implies that philosophy can affect the direction of scientific development. This applies in particular to the categories of external conceptual problems arising from normative or worldview difficulties (Laudan, 1977, p. 57-64). In the former case there is an interaction between scientific results and contemporary methodological standards. In the latter case general non-scientific (*e.g.* philosophical) beliefs affect the acceptability of scientific results, and thus they might have some influence on scientific change.

Another important difference from MSRP is the status of methodology in Laudan's theory. Contrary to Lakatos, Laudan argues that we must evaluate historical cases using contemporary (and not our own) standards of rationality. He maintains that his theory permits one to do this "by exploiting the insights of our own time about the *general* nature of rationality, while making allowances for the fact that many of the *specific* parameters which constitute rationality are time- and culture-dependent" (Laudan, 1977, p. 130). The general, trans-cultural and trans-temporal, norms concern the problem-solving effectiveness of RTs, while the specific historical norms may define what is regarded as a problem, which methods one must use to solve it, etc. However, Laudan's views on the question of how precisely such an evaluation of the rationality of historical cases (for example, those in which scientific and non-scientific factors interacted) is to be carried out, are not very clear (see, *e.g.*, Laudan, 1977, p. 132).

Still, his approach has the general consequence that local philosophical factors can have a lasting influence on the development of science. The reason for this is that in Laudan's theory scientific results are appraised (justified) through a mixture of universal and local norms. Consequently, historical contextualities partly determine what counts as progressive and rational, and thus they affect the direction of science. This is a major difference from Lakatos's MSRP, in which local methods are considered irrelevant and progress is governed by a universal methodology. Finally, it must be noted that in Laudan's theory differences in the philosophical views of individual scientists play a negligible role. Within a specific RT at a specific time all scientists are by definition committed to the same philosophical assumptions and methods. It is only because of their possible variation in time that RTs leave some more room for contextualities than Lakatosian RPs.

Laudan devotes a separate section to adhocness, which he defines as follows: "a theory is *ad hoc* if it is believed to figure essentially in the solution of all and only those empirical problems which were solved by, or refuting instances of, an earlier theory" (Laudan, 1977, p. 115). This definition amounts to a consequential interpretation of adhocness. However, Laudan claims that consequential adhocness is not by definition objectionable. On the contrary, because it increases the number of solved empirical problems, it constitutes progress. Only if an *ad hoc* modification gives rise to conceptual problems which outweigh the increase in the number of solved empirical problems (which is often the case), then it is indeed objectionable. As Laudan (1977, p. 118) acknowledges, the Lakatosian notion of *adhoc*$_3$ applies to such a conceptually problematic situation. Thus, in Laudan's view, generative adhocness can be illegitimate. Laudan's attitude towards adhocness is related to his position concerning epistemic support. Since Laudan claims that problem-solving effectiveness is the only criterion for progress, predictive success is not a special epistemic virtue. If a theory makes a new prediction, which is verified, it simply adds a solved problem (which was not a problem before) to its list. It makes no difference whether an empirical fact is used in the construction of a theory or is an independent prediction: in both cases the result is a solved problem. Laudan's position is thus essentially different from Lakatos's, according to whom epistemic support for a theory is provided *only* by verified predictions and *not* by facts used in its construction. Accordingly, Laudan deems consequential adhocness legitimate, whereas for Lakatos it is illegitimate.

In order to assess the merits of the theories discussed above, they will be applied to two exemplary episodes from the history of modern physics in the next two sections.

3. *First example: Maxwell versus Boltzmann on the specific heat anomaly*[3]

The second half of the nineteenth century saw the development of the kinetic theory of gases, a theory attempting to explain the properties of gases from the hypothesis that they consist of small particles (atoms or molecules) in motion. Its chief proponents were James Clerk Maxwell and Ludwig Boltzmann, who contributed decisively to its scientific success. Around 1870, when the kinetic theory had established its reputation, both Maxwell and Boltzmann unconditionally endorsed the ontological assumptions underlying the kinetic theory: atomism, mechanicism, and materialism. The theory, however, confronted one serious empirical problem: the so-called specific heat anomaly. It turned out that, for many common gases, the experimentally determined value of the specific heat ratio did not agree with the theoretical predictions of the kinetic theory (on the basis of the equipartition theorem). This anomaly was considered a great obstacle for the theory, and many physicists searched for a solution.

In 1876, after several failed attempts, Boltzmann proposed a model which solved the specific heat anomaly. He assumed that the gases in question consist of molecules composed of two rigidly connected atoms. These 'dumb-bell' molecules possess five degrees of freedom, which, by applying the equipartition theorem, straightforwardly yields the correct value of the specific heat ratio. Unfortunately, Boltzmann's model was not without problems. In particular, it denied the molecules the possibility to vibrate internally, while this was deemed necessary in order to explain the existence of the experimentally observed spectral lines of the gases. Maxwell advanced this objection to Boltzmann's model and accordingly did not accept it as a solution of the anomaly. Boltzmann, however, refused to abandon his model and remained convinced that it constituted a good explanation, though he could not counter the objection.

I submit that this controversy between Maxwell and Boltzmann can be traced back to a difference in their philosophical views, particularly at the level of epistemology and methodology. Around 1876, at the time when Boltzmann presented his model, Maxwell was committed to correspondence realism and he advocated the method of deduction from the phenomena.[4] According to this method, one should "begin with the phenomena and deduce forces from them by a direct application of the equations of motion" (Maxwell, 1965, vol.2, p. 309). In this way one could obtain theories that provide a literal description of reality. Meanwhile, Boltzmann had developed an epistemological position which differed significantly from Maxwell's views. The difference concerned the relation between theory and reality: in Boltzmann's conception, a scientific theory is a model or picture which does not have to correspond to reality in all respects. Boltzmann's view of scientific method differed correspondingly from Maxwell's. He held that scientific research must begin with abstract theory, whereas Maxwell emphasized the primacy of empirical phenomena in scientific practice. More specifically, Boltzmann endorsed and practised the hypothetico-deductive method.

Maxwell rejected Boltzmann's solution because it could not be a literal description of reality, and because it was not 'deduced from the phenomena' (in fact, it seemed to contradict the phenomenon of spectral lines). Although this criticism was connected with physical problems of the model, the fact that Maxwell considered these problems important enough to reject the model, is directly related to his epistemological position. Moreover, he disapproved of Boltzmann's method, and consequently considered his solution objectionably *ad hoc*. Maxwell (1965, vol.2, p. 223) repudiated "theories investing the molecule with an arbitrary system of central forces invented expressly to account for the observed phenomena", of which Boltzmann's model clearly was an example. By contrast, Boltzmann's philosophy of science, in which abstract theories and the hypothetico-deductive method were central elements, allowed him to propose the model. He regarded it as a theory of molecular structure which, though defying a straightforwardly realist interpretation, did provide knowledge of the nature and behaviour of gas molecules.

It thus appears that, at least around 1876, the philosophical views of Maxwell and Boltzmann were similar in some respects and different in others. Their shared views concerned ontology: they were both atomists, mechanicists and materialists. These views were evidently characteristic of the larger scientific community in which both scientists participated. However, Maxwell and Boltzmann adhered to quite different, indeed opposing, views regarding epistemology and scientific method. This indicates the existence of different levels at which philosophical influences operate. Because the shared ontological views of Maxwell and Boltzmann are typical of the 'kinetic community' but are not constitutive of science as a whole, they are differentiating factors: they differentiate the kinetic community from other ones. In addition, there existed differentiating influences *within* the community itself, namely the individual differences with respect to epistemology and scientific method. These differences were significant enough to divide Maxwell and Boltzmann over the issue of the specific heat anomaly, thus giving rise to a further differentiation of heuristic, philosophical influences. In particular, they determined their attitudes towards *ad hoc* approaches. Maxwell's adherence to 'deduction from the phenomena' induced him to criticize *ad hoc* approaches such as Boltzmann's. By contrast, Boltzmann tried to solve the anomaly via hypothetico-deductive strategies, which may appear *ad hoc* since his hypotheses were especially devised to account for the recalcitrant data. In the light of his methodology and epistemology, however, this was not objectionable.

3.1 A Kuhnian account

In a Kuhnian analysis the development of the kinetic theory between 1860 and 1900 is a case of normal science, in which Maxwell and Boltzmann were occupied with the articulation of the kinetic paradigm. The role of philosophical ideas is relatively static during normal science, and is fixed by the disciplinary matrix, which embodies particular metaphysical commitments and heuristic tools. The former are beliefs such as 'gases are composed of particles in motion, subject to the laws of mechanics' and 'heat is kinetic energy', while the latter are given through exemplars, such as the derivation of simple phenomenological gas laws (*e.g.*, Boyle's law and Gay-Lussac's law of equivalent volumes). Thus, the fact that Maxwell and Boltzmann had shared ontological commitments (atomism, mechanicism, and materialism) is directly related to the fact that they were working within the same paradigm.

The specific heat anomaly is, in a Kuhnian view, simply a puzzle to be solved, and will not be considered a refutation as long as there is no general feeling of a state of crisis. This would apply to earlier, unsuccessful attempts to solve the anomaly. But was this puzzle solved when Boltzmann advanced his dumb-bell model in 1876? This is for the scientific community to decide, and shared commitment to the paradigm is the basis of agreement on the question as to whether a puzzle is solved or not. In particular, the question of whether Boltzmann's solution was *ad hoc* should have a unique answer, depending on whether or not he violated the heuristic rules of the paradigm.[5] It turned out, however, that Maxwell and Boltzmann did not agree in this respect. Kuhn's model cannot account for this controversy, except by assuming that Boltzmann did not solve the puzzle, but for some reason, *e.g.* a psychological one, refused to admit his failure. I have explained the controversy between

Maxwell and Boltzmann by referring to their very different epistemological and methodological ideas. Although Kuhn does not deny that such differences can exist during normal science, he deems them insignificant. It can be concluded that Kuhn's theory presents a too static, homogeneous picture: since the heuristics, and thus also what counts as *ad hoc*, is fixed within a paradigm, this controversy cannot be explained.

3.2 A Lakatosian account

For a Lakatosian account of the first example we can turn to Clark (1976), who analyses the history of the kinetic theory as the rational development of a single RP, which was progressive until 1880 and thereafter degenerated. The kinetic RP consisted of a hard core, which Clark (1976, p. 45) defines as: "the behaviour and nature of substances is the aggregate of an enormously large number of very small and constantly moving elementary individuals subject to the laws of mechanics". Thus, the hard core contained ontological ideas. The specific methods were formulated in the positive heuristic, which consisted of four directives. As in the Kuhnian account, the shared ontological beliefs of Maxwell and Boltzmann can be explained by their commitment to the kinetic RP. An important difference is that the Lakatosian theory assumes the additional, universal methodological aim of making novel predictions to explain the rationality of scientific development.

The specific heat anomaly was, according to Clark (1976, p. 82), at first "listed but set aside", because the RP of the kinetic theory was progressing in other directions, but became a serious problem when the positive heuristic of the RP had "run out of steam" (id., p. 87) and was lagging behind the rival RP of thermodynamics. In this latter, degenerating stage, attempts were made to solve the anomaly in an *ad hoc* manner. Clark (1976, p. 84) claims that Boltzmann violated the positive heuristic, and thus the method of the RP, when he proposed his dumbbell model, because "it was obtained from the previous models in a way which did not accord with the heuristic of the programme, for it did not comply with the principle that the interactions should obey the laws of mechanics". In Lakatosian terminology, Boltzmann's solution was *ad-hoc*$_3$, and therefore had to be rejected, even though it correctly predicted a 'novel fact', namely the correct specific heat ratio for polyatomic gases.[6] According to Clark, Maxwell objected precisely to this *ad hoc* character of Boltzmann's solution. In other words, the Maxwell–Boltzmann controversy is explained by the thesis that Boltzmann acted irrationally, while Maxwell's response was rational. However, this Lakatosian explanation ignores the crucial role of the opposing philosophical views of Boltzmann and Maxwell. In Lakatos's theory there is no room for such differences (at least not for their rationality), since the positive heuristic defines a single method, which is identical for all scientists working in the RP.

3.3 A Laudanian account

In Laudan's 'problem-solving model', the first example concerns the evolution of a single research tradition (RT). Since an RT consists of ontological claims and related methods for solving problems, it may be assumed that the kinetic RT is essentially similar to the Lakatosian RP. One significant difference, however, is that the basic elements of RTs, metaphysical as well as methodological, can change in time. Laudan appraises the rationality of RTs by determining their problem-solving effectiveness, in which the solution of conceptual problems has to be included. He will deal with Maxwell's and Boltzmann's shared philosophical commitments in essentially the same way as Kuhn and Lakatos.

As regards the specific heat anomaly, it should first of all be noted that the specific heat anomaly was, in Laudan's terminology, *not* an 'anomalous problem'. Although it was an empirical fact which refuted the kinetic theory, it was not anomalous, since there were no rival explanations of this fact by other theories. Thus, the specific heat anomaly was an 'unsolved problem'. However, Laudan's (1977, p. 30) thesis that "unsolved refuting instances are often of little cognitive significance", does not apply to this case: Maxwell (1965, vol.2, p. 433) considered the anomaly to be "the greatest difficulty the molecular theory has yet encountered", and it played an important role in the development of the kinetic theory.

Laudan's thesis is based on the claim that any theory can be arbitrarily adjusted to incorporate the refuting instance, so that its problem-solving effectiveness increases (Laudan, 1977, p. 119). Such an *ad hoc* modification is illegitimate only when it leads to a diminished problem-solving effectiveness in other respects. Now, Boltzmann's solution of the anomaly obviously solved a problem. His explanation was *ad hoc*, but was it illegitimately *ad hoc*? Laudan might claim that it was, because it led to an increase of conceptual problems, notably the problem of why the equipartition theorem did not apply to vibratory degrees of freedom.[7] The question is whether this new problem outweighed the success of solving the empirical problem. Whichever way this question is answered, Laudan's theory has difficulty in explaining the Maxwell–Boltzmann controversy, since Boltzmann's solution either constituted progress or it did not. What appears to be necessary for an explanation in Laudanian terms is a further relativization of the notion of a conceptual problem. If it is recognized that conceptual problems are relative, not only to RTs, but also to individual philosophical commitments, then Laudan's theory is capable of providing a straightforward account of the controversy: because of his epistemological and methodological position Maxwell held that the conceptual problems which Boltzmann's solution generated, outweighed its virtues, while Boltzmann himself viewed it the other way. Obviously, this modification of Laudan's theory implies also a 'relativization' of adhocness. Incidentally, since outcomes of problem-solving effectiveness calculations are, on this view, greatly contextual, it seems to undermine Laudan's general conception of rational progress.

4. *Second example: Pauli versus Heisenberg on the anomalous Zeeman effect*[8]

In the early 1920s quantum theory was based on Niels Bohr's atomic model, which described atoms analogous to a planetary system. According to this model, atoms consist of a nucleus around which electrons are moving in continuous orbits. The revolutionary quantum character of Bohr's model lies in the fact that only a discrete set of orbits is allowed and that electrons can 'jump' discontinuously from one orbit to another. Bohr's semi-classical theory, which is often referred to as the 'old quantum theory', successfully explained a host of empirical data on spectral lines (*e.g.* the Balmer formula). However, serious empirical problems remained, specifically the notorious 'anomalous Zeeman effect': the splitting of more complex spectral lines (doublets and triplets) in a magnetic field.

Wolfgang Pauli and Werner Heisenberg, both young and brilliant assistants of Bohr, were working intensely on this problem. In 1920

Heisenberg succeeded in adapting Bohr's atomic model in such a way that it yielded correct results for the anomalous Zeeman effect. However, his treatment violated many accepted quantum-theoretical laws and principles; most notably, it permitted half-integral values of quantum numbers, while a basic postulate of the theory asserted that they were integral-valued. Pauli considered Heisenberg's solution completely unacceptable, and he expressed his loathing of such *ad hoc* strategies to save the Bohr model:

> "atom physicists [...] have the characteristic in common that there is no *a priori* argument to be had from their theories that tells which quantum numbers and which atoms should be calculated with half integral values of the quantum numbers and which should be calculated with integral values. Rather this they can decide merely a posteriori by comparison with experience. I myself have no taste for this kind of theoretical physics [...]" (Pauli quoted in Serwer, 1977, p. 228).

In 1924 Pauli presented an alternative solution of the anomalous Zeeman effect: he ascribed the electron a peculiar 'two-valuedness' (*Zweideutigkeit*). This implied the existence of a new quantum number, which accounted for the Zeeman splitting. Contrary to other quantum numbers, this new number did not correspond with a classical magnitude. Obviously, one might criticize Pauli's solution for being at least as *ad hoc* as Heisenberg's. The latter's response was indeed utterly negative: Heisenberg called Pauli's move a swindle (Pauli, 1979, p. 192). For Pauli, however, it meant the start of a programme for the development of a new quantum mechanics, which would be completely independent of classical-mechanical concepts and of visualizable atomic models. Further elaboration of his proposal led Pauli to the famous 'exclusion principle'.

Again, as in the case of Maxwell versus Boltzmann, the controversy between Pauli and Heisenberg can be traced back to differences in their philosophical commitments. Pauli's methodology featured operationalism and an extremely critical style, which emphasized consistency and 'legitimacy'. Whether a theory was legitimate was, for Pauli, an intuitive matter, but important conditions appeared to be consistency with fundamental laws and unifying power, while *ad hoc* modifications to adjust theories to empirical data were illegitimate (see Serwer, 1977, p. 255). A consequence of Pauli's adherence to operationalism was his rejection of classical visualizability for the atomic domain (he objected to the use of the kinematical quantities of position and momentum since these were not operationally definable). While he rejected classically visualizable models, he searched for alternatives which would be *anschaulich* in the sense of intuitively intelligible.[9] Heisenberg's methodology was greatly different from Pauli's. Heisenberg took a pragmatic, even opportunistic, view of method: empirical adequacy was the goal that had to be achieved by any possible means. He was receptive to different kinds of philosophical ideas, but he did not employ them in a consistent manner and valued them mainly for their pragmatic virtues (see Cassidy, 1979, p. 189). At the ontological level, both Pauli and Heisenberg were realists, but an important difference was that Pauli radically rejected the ontology of classical physics, while Heisenberg had a relatively strong commitment to the semi-classical Bohr model.

Heisenberg's pragmatic method clearly showed in his *ad hoc* adaptation of the Bohr model to explain the anomalous Zeeman effect. Pauli's discovery of the exclusion principle was guided by his rejection of semi-classical, visualizable models and by his methodological views, particularly his aversion to *ad hoc* modifications of the old quantum theory. Nonetheless, his own solution of the anomalous Zeeman effect might seem to be *ad hoc* as well, since it postulated a new, strange property of the electron to account for the anomaly. Moreover, Pauli's approach permitted an arbitrary number of quantum numbers, as these were not connected with mechanical properties of the electron, and this seems to open the door to the *ad hoc* introduction of new quantum numbers to adjust the theory to empirical facts. Pauli's reasons for rejecting Heisenberg's approach and preferring his own were, first, his distrust of mechanical models, which was based upon his operationalism, and second, his demand for 'legitimacy'. In Pauli's view, Heisenberg's approach was doomed to fail because of its reliance on mechanical models, whereas his own approach was a radically new, non-mechanical programme, promising legitimacy. Thus, these aspects of Pauli's philosophy determined which solutions he regarded as *ad hoc* and which not. As in the Maxwell–Boltzmann controversy, it can be concluded that appraisals of scientific results, and conceptions of adhocness, are dependent upon personal philosophical views. This second example differs from the first, however, in the fact that no strongly shared philosophical views (constitutive of a 'quantum community') can be distinguished. Admittedly, the short example presented here cannot prove that claim, but it is confirmed by a more comprehensive analysis of the development of quantum theory (see De

Regt, 1993, Chapter 4).

4.1 A Kuhnian account

The development of quantum theory has often been characterized as a revolutionary period. Thus, it is not surprising that Kuhn (1970b, p. 256) considers this episode an "ideal case" for his theory of science. He distinguishes between two revolutions in the period between 1913 and 1927: the first began with Bohr's 1913 model and resulted in the paradigm of the 'old quantum theory'; the second followed the crisis of the early 1920s and resulted in the new quantum mechanics of Heisenberg and Schrödinger. Kuhn emphasizes that in times of crisis scientists often turn to philosophical analysis and individual values may become important. This was indeed the case in the controversy between Heisenberg and Pauli. Their methodological values were completely opposed: the pragmatic Heisenberg cared only about empirical adequacy, while the critical Pauli emphasized consistency and legitimacy. This difference, together with Heisenberg's ontological commitment to the Bohr model, led to their opposing attitudes toward *ad hoc* hypotheses. Moreover, Pauli's epistemological criticism of the semi-classical ontology of the old quantum theory is typical of revolutionary science; his radical rejection of all classical concepts contributed to the second quantum revolution. It may therefore be concluded that this example does indeed agree quite well with Kuhn's theory. However, it should be noted that this is partly due to the fact that Kuhn's assertions regarding scientists' behaviour in periods of crisis are not very specific, which is of course related to his view of crises as situations in which no precise rules for practising science exist.

4.2 A Lakatosian account

Lakatos (1970, p. 140-154) has analyzed the development of quantum theory in terms of MSRP. He asserts that Bohr started an RP, which degenerated in the 1920s and was superseded by the rival RP of Schrödinger. His account of the degenerative phase is dubious, as Kuhn (1970b, p. 258) and Radder (1982) have pointed out. Lakatos (1970, p. 154) claims that the degeneration led to "sterile inconsistencies and ever more *ad hoc* hypotheses", among which was Heisenberg's explanation of the anomalous Zeeman effect. Pauli's exclusion principle, on the other hand, constituted a creative shift in the positive heuristic (Lakatos, 1970, p. 153 and p. 137). This move did not help, however, for Bohr's programme continued to degenerate, and, according to Lakatos, it was superseded by the rival RP of Schrödinger's wave mechanics.

What are the implications of this account for the role of philosophy in this later stage? Firstly, since Heisenberg and Pauli (at least until 1925) both operated in Bohr's RP, they should employ shared ontological and methodological presuppositions, according to Lakatos. In reality, this was certainly not the case. In Lakatos's theory, Heisenberg's pragmatic attitude may be explained as the *ad hoc* response to the degeneration of the RP. Any positive heuristic role of Heisenberg's idiosyncratic approach is impossible in his theory. In fact, however, Heisenberg's pragmatic views were of crucial importance for these developments. The case of Pauli is another matter. By labelling the exclusion principle as a 'creative shift' in the positive heuristic, Lakatos implicitly admits that Pauli's philosophical ideas may have influenced this discovery, in agreement with my analysis. Pauli himself, however, did not regard his principle as merely an auxiliary hypothesis to save Bohr's RP. Instead, he considered the exclusion principle as a first result of a radically new approach to quantum theory, which was superior from a conceptual point of view. The only way in which Lakatos can make sense of this is either to dismiss these individual philosophical ideas as irrelevant idiosyncrasies, or to give up the idea that Bohr, Pauli, and Heisenberg were all in the same RP. The latter option renders the idea of an RP vacuous, and, moreover, excludes the possibility of interaction between these physicists. The former option is untenable for historical reasons. To be sure, it would leave open the possibility of interaction of different philosophical approaches to science, but it would deny the heuristic importance of such interactions.

4.3 A Laudanian account

Laudan's problem-solving model seems to be more promising to deal with the quantum case than Lakatos's, as RTs are more flexible than RPs. There are two important differences between Laudan's and Lakatos's theory: firstly, the ontological and methodological component of the RT may change in time, and secondly, there is an important role for conceptual problem solving. Related to the evolving character of RTs is Laudan's denial of the existence of crises and revolutions in the Kuhnian sense (see Laudan, 1977, p. 133-36) He argues that there is more continuity than Kuhn admits, and that the ontology or methodology of the RT may become the subject of debate not merely in specific periods (Kuhnian crises) but at any time.

A Laudanian account of the second example might run as follows. Bohr's RT, after great initial success, saw a decrease of empirical problem solving effectiveness in the early 1920s, and therefore its conceptual problems, particularly its relation to classical theory, became serious threats. At this time Pauli and Heisenberg entered the RT. Heisenberg was at first mainly concerned with the unsolved empirical problems, whereas Pauli also dealt with conceptual problems. Both their proposed explanations of the anomalous Zeeman effect were solutions to an empirical problem (both were *ad hoc*, but on Laudan's view adhocness is not necessarily objectionable). Pauli's proposal, in addition, attempted to solve a conceptual problem by changing the ontological and methodological basis of the RT.

This account is clearly more consistent than Lakatos's. However, there are still discrepancies. Firstly, a Laudanian RT must, at a *specific* time, have a fixed methodology, which was clearly not the case in the early 1920s. Whereas the influence of Pauli's idiosyncratic method is to some extent accommodated, the role of Heisenberg's pragmatic attitude is neglected. Secondly, the account does not explain the 'relativity' of the appraisal of *ad hoc* strategies. Laudan holds that it can be objectively determined which hypotheses are *ad hoc*, and, furthermore, that an *ad hoc* solution constitutes progress if it is not outweighed by conceptual loss. The case of Pauli versus Heisenberg, however, shows that application and appraisal of the notion of 'adhocness' depends on individual philosophical presuppositions.

5. *Towards an adequate model of scientific change*

The theories of Kuhn, Lakatos, and Laudan clearly fail to do justice to the two examples presented. In the first case, the reason for this is that the example reveals the importance of *individual* differences between the philosophies of Maxwell and Boltzmann, whereas according to the theories the development of the kinetic theory should be considered as a uniform process in which only *shared* philosophical commitments are heuristically significant. It is true that Maxwell and Boltzmann, despite their differences, also shared some philosophical commitments, particularly concerning ontology. These shared views were evidently typical of the community (paradigm, RP, RT) in which they participated. However, the theories at issue deny the possible existence, or at least the relevance, of individual differences within the communities. Thus, their conceptions of scientific communities are too rigid. Of course, one may suggest that Boltzmann started a new paradigm (RP or RT, respectively) when he proposed his model, but this escape route obviously renders the concept of paradigm (RP, RT) vacuous. The theories of Lakatos and Laudan also fail to provide adequate accounts of the second example. Again, the reason for this is the monolithic character of their units of analysis and their denial of the relevance of scientists' individual philosophies. In this case, Kuhn's theory is more useful, since its notion of 'crisis' applies very well to this episode.

What is needed is a theory of science which permits the existence of relevant differences between individual scientists. One might hope to find such a theory by adopting the naturalistic approach which has recently been advanced by Ronald Giere (1988) and Larry Laudan (1987, 1990).[10] On their view, one should explain and appraise judgments of individuals.[11] Moreover, they hold that rational rules are always 'hypothetical', *i.e.* they are of the form 'If you want to achieve Y, do X'. Accordingly, if one wants to explain actions of individual scientists, one has to take into account only *their* aims and background beliefs. This approach allows for an explanation of the controversy between Maxwell and Boltzmann, by arguing that their aims were not completely identical. Although at first sight they seem to be the same, namely the solution of the specific heat anomaly, a closer look reveals that their aims differed in some respects. Because of their different epistemological positions (concerning the nature of scientific theories), they had different criteria for what counts as a good solution and therefore had different aims. Consequently, they also used different methods to realize those aims. This explanation of the Maxwell–Boltzmann controversy can also be cast in terms of adhocness. In the naturalistic approach, the prohibition of *ad hoc* hypotheses is not a universal norm but an aim-dependent rule. Since Maxwell's aims differed from Boltzmann's (under the influence of their epistemological views), their attitudes towards the use of *ad hoc* hypotheses also differed. It will be clear that the second example can be accounted for in a similar manner.

While the naturalistic approach thus permits simple explanations of the controversies, it should be noted that this is partly due to the fact that its theoretical framework is so general that it seems to be able to incorporate any kind of influence on scientific development. Indeed, one may well believe that everything can be explained in this manner. There are almost no restrictive assertions as to the nature of scientific research. A second, related criticism is that the approach treats both cases in exactly the same manner, and that it does not explain the differences between the two episodes, neither in general nor with respect to different roles of philosophy. Nevertheless, since the examples have revealed the import of individual differences between philosophical views of scientists, the basic idea of the naturalistic approach is applicable. However, also the notion of a 'scientific community' remains useful for analyzing the heuristic role of philosophical ideas. Kuhn's paradigms, Lakatos's research programmes and Laudan_1's research traditions appeared to be too rigid descriptions of such communities. But perhaps it is possible to adapt their theories in such a way that this disadvantage disappears? I submit that this is possible, namely by means of a distinction between three levels at which philosophy may influence scientific research.

In my account of the first example I have already suggested the possible existence of different levels at which philosophical influences function. In other words, there may exist a 'fine structure' of differentiating levels. If we adopt the hypothesis that the activities of individual scientists are embedded in scientific communities, while we also wish to retain the possibility of individual philosophical differences within these communities, then we obtain three levels at which philosophy may affect scientific development:

Macro-level: philosophical ideas which are constitutive of science as a whole.

Meso-level: philosophical ideas which are differentiating, but constitutive of a particular scientific community.

Micro-level: philosophical ideas which are differentiating, and individually varying within a scientific community.

How are philosophical ideas distributed over these three levels? At the *macro-level*, one finds only a few, very general philosophical ideas, such as a general epistemological commitment to empiricism, which asserts that knowledge should be supported by experience in some way. Of course, this empiricism has to be supplemented with more specific ideas of how science employs experience and how scientific theories are related to experience. It appears, however, that scientists do have diverging views on this issue, which act as differentiating influences. The *meso-level* refers to scientific communities, which are defined as groups of scientists working on the same problems and using fairly similar methods and assumptions. I contend that, as regards philosophical ideas, a scientific community is held together by shared ontological commitments, in agreement with the theories of Kuhn, Lakatos and Laudan_1. In addition, however, one discerns a further differentiation of philosophical views within a community. Such *micro-level* differences concern, first of all, methodological ideas, such as commitment to inductivism or hypothetico-deductivism. The examples have revealed that methodological views of scientists may contribute decisively to their heuristics, and that sometimes individual scientists within a particular community adhere to highly diverging views concerning these methods. Moreover, it is the epistemological commitments of scientists which lie at the basis of their adherence to specific methodologies: when a scientist has a particular epistemological view of the status of scientific theories, it is reflected in his methodology.

This last remark may appear to be an almost trivial truth. There seems to be no reason why it should apply only for scientists and not for philosophers of science. Applying it to philosophers of science reveals why their universal methodologies fail to do justice to the history of science. Let me explain this. In Section 2, we have seen that the attitudes of Lakatos and Laudan_1 towards *ad hoc* theories are related to their specific views on epistemic support. Lakatos deems *ad hoc* theories objectionable, since he regards novel predictions as the only form of epistemic support for theories. Laudan_1, by contrast, holds *ad hoc* theories to be legitimate, since he believes that it is problem-solving which provides epistemic support.[12] In other words, adherence to a specific methodological rule concerning adhocness is dependent on particular epistemological presuppositions. It is therefore only reasonable to assume that also scientists themselves will appraise (or define) adhocness in accordance with their particular epistemological presuppositions. In the presented examples this has indeed been observed. The disagreements between Maxwell and Boltzmann, and between Pauli and Heisenberg, can be traced back to such epistemological differences.

In conclusion, it appears that, while scientific communities can often be characterized by shared ontological commitments, the epistemological views of individual scientists may be differentiating factors. Different epistemological commitments may lead to different views on method, which can be the reason for significant controversy. The three-level conception of the role of philosophy in scientific development permits a philosophical *pluralism* (at least concerning epistemology and methodology) within scientific communities, which itself is often of great heuristic value.

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NOTES

1. Some traditional philosophers of science, notably Popper, also acknowledge that individual philosophical views may have a heuristic function, but they deny heuristics any ultimate relevance to scientific development by invoking the distinction between context of discovery and context of justification. In this article, however, I will assume the possibility that heuristics is of interest to the philosophy of science. For an overview of the debate about the context-distinction and for arguments against it, the reader is referred to Nickles (1980, pp. 1-59).
2. Recently, Laudan has moved on to a more radical naturalistic view of science (see Laudan, 1987 and 1990). In Section 5 I will briefly discuss the merits of naturalistic theories.
3. In De Regt (1996) I have presented a more detailed account of this example.
4. Actually, Maxwell's philosophical views showed a gradual development.

Here I only consider his later views. For a discussion of his earlier philosophy (comprising the famous 'method of physical analogy') see De Regt (1996, pp. 33-39).

5. The answer seems to depend on whether the theorem of equipartition was an element of the disciplinary matrix.
6. This was a 'novel fact' only in the specific Lakatosian sense: although it was a known fact, it was 'novel' because Boltzmann's model was not designed to account for it.
7. Note that this objection to Boltzmann's model has similarities with the Lakatosian charge that the model was *adhoc*$_3$.
8. See De Regt (1993, pp. 112-132) for an extensive treatment of this example.
9. In De Regt (forthcoming) I analyze the problem of *Anschaulichkeit* in quantum theory in detail, specifically in relation to Erwin Schrödinger's views, which were opposed to Pauli's.
10. In order to avoid confusion, I will refer to Laudan's problem solving model as Laudan$_1$, and to his recent naturalistic theory as Laudan$_2$.
11. Giere and Laudan$_2$ call their approach *naturalistic* because they believe that such scientific judgment is a 'natural process' which can be studied empirically (*e.g.* by means of cognitive science or historical investigation).
12. This applies only to the 'consequential' conception of adhocness, not to Lakatos's notion of *adhoc*$_3$ and Laudan$_1$'s category of *ad hoc* theories which lead to conceptual problems.

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