Primitive Rays and Aetherial Air: On the Impossibility of Theory-Free Experiments

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Abstract

In this paper, I offer a differentiated philosophical interpretation of the relationship between experiment and theory. The claims that all scientific experiments are theory guided or that all experiments are explicit tests of existing theories about the objects in question, prove to be implausible. Yet theories are important for experimental practice for two reasons. The (immediate or later) significance of experiments is affected by the theoretical context in which they are situated. Moreover, performing and understanding experiments depends on a theoretical interpretation of what happens in materially realizing the experimental process. Through an examination of Newton's prism and Boyle's air-pump experiments, the last point is argued here in detail. It goes both against explicit claims made by some philosophers of experiment and against possible empiricist conclusions that might be drawn from such claims.

1. Introduction

The 1980s and 1990s have generated a wave of novel and interesting work on scientific experimentation. A number of philosophers have made significant contributions to this subject. One of the issues that have been treated in some detail is the relationship between experiment and theory. The result has been a criticism of theory-dominant accounts of science, based on the claim of the relative autonomy of experimental science. In some cases, the rejection of theory-first approaches has led to a certain revaluation of empiricist views. In this paper, I provide a close examination of these issues. My conclusion is that the claimed relative autonomy of experimental practice does not imply that theory-free experiments are possible. Thus, an interactive account of the relationship between experiment and theory proves to be more adequate than the views that experimentation is either fully theory-independent or strongly theory-guided.

I begin with a point of method. If we want to put forward a specific claim about an issue in the philosophy of scientific experimentation (for instance: ‘theory-free experiments are impossible’), we need to make explicit what we take to be an experiment. One suggestion might be that an experiment is what scientists call an experiment. Consistently following this suggestion, however, would imply that we simply take for granted the intuitions and conceptualizations of the scientists. Given that scientists and philosophers pursue different goals, this is—generally speaking—not recommendable. For example, many scientists do not bother to distinguish between experiments in which the object under study is being manipulated and 'experiments' in which this is not the case. Such a distinction, however, is philosophically quite relevant. Another approach might be to have scientists' practice, instead of their words, decide on what to call an experiment. Unfortunately, this does not work either, since what scientists do does not unambiguously fix what it is that they do. Thus, the question of what counts as an experiment—when and where does it start or end? what to include and what to exclude?—cannot be answered by merely observing what is going on in scientific practices. We also need to add our own understanding, which may then be adjusted and improved in the process of learning more about scientific experimentation.

In the present discussion of the relationship between experiment and theory, I build on my earlier account of scientific experimentation (see Radder, 1988, ch. 3; Radder, 1996, chs. 2 and 6). According to this account, an experimenter tries to realize an interaction between an object and some apparatus in such a way that a stable correlation between some feature of the object and some feature of the apparatus will be produced. If the experiment succeeds, two aims have been achieved simultaneously. First, a stable experimental (or, object-apparatus) system has been materially realized; second, it has proved to be possible to obtain some knowledge about relevant features of the object by taking note of correlated features of the apparatus. In addition, since scientific practice does not consist of isolated experiments performed by solitary experimenters, we have to examine the ways in which individual experiments are embedded and used in broader experimental and theoretical contexts. Hence, if we want to establish what counts as an experiment, we need to investigate the wider function and significance of stable experimental systems and experimentally acquired knowledge.

To develop this basic account we need to deal with a number of further issues in the philosophy of scientific experimentation. In this paper I focus on some of these issues, to wit the ones pertaining to the experiment-theory relationship. In section 2, I introduce three proponents of the view that theory-free experimentation is possible and does occur regularly. Section 3 contains the main argument of the paper. I discuss and evaluate four more specific claims about the relationship between experiment and theory. Two rather strong versions of the theory-first claim are shown to be implausible. Yet, in two other ways, experiments prove to be theory-dependent. The concluding section offers some additional observations to provide further support for the main argument of the paper.

2. Theory-Free Experimentation?

Several philosophers have emphasized the relative autonomy of experimentation from theory. Ian Hacking, for instance, develops the following line of argumentation. He discusses the claim that 'an experiment must always be preceded by a theory' and proposes to distinguish between a weak and a strong version of that claim.

> The *weak version* says only that you must have some ideas about nature and your

> apparatus before you conduct an experiment. ... There is however a *strong version*. ... It says that your experiment is significant only if you are testing a theory about the phenomena under scrutiny. (Hacking, 1983, 153-154)

Next, Hacking argues that the weak version is quite plausible or even trivial. According to his prior understanding of experimental practice, ‘experiments’ without ideas are meaningless and hence not experiments at all. In contrast, the strong version of the claim—advocated by Justus von Liebig and Karl Popper, among others—is said to be unwarranted on the basis of examples from the history of science.[1] Thus, many noteworthy phenomena would have been experimentally investigated independently of any theoretical interpretation. In particular, Hacking mentions a number of experiments from the early history of optics, between 1600 and 1800.

More recently, the issue of the theory (in)dependence of experimentation has been examined in somewhat more detail. Friedrich Steinle discusses the question of whether experiments are always ‘guided’ by theories. An experiment is theory-guided if it is being explicitly planned, designed, performed and used from the perspective of particular theoretical claims about the objects under scrutiny (Steinle, 1998, 286 and 289). Theory guidance in this sense lies somewhere between having an idea and testing a theory. It is much more specific than the former but includes more than the latter. Next, Steinle introduces the notion of exploratory experiments and he argues that this type of experimentation is not theory-guided in the above sense, even if it does require reflection and deliberation. He illustrates his arguments with a discussion of some early electromagnetic experiments carried out in 1820 by André-Marie Ampère. In addition, he claims that exploratory experimentation occurs in the practice of other scientists and other sciences as well.

A further recent contribution has been made by Michael Heidelberger. He uses the term theory ladenness and makes a distinction between ‘theory ladenness through theoretical interpretation’ and ‘theory ladenness through reference to a prior understanding’ (Heidelberger, 1998, 85-86). In the same spirit as Hacking and Steinle, he admits that experiments do depend on prior understanding, which entails certain expectations about the performance of the experiments. But he denies that experimentation always requires a prior theoretical interpretation of the relevant phenomena and instrumentation. Heidelberger develops this view as follows. He proposes a classification of instruments into three categories: productive instruments (e.g., an air pump), constructive instruments (e.g., a wind tunnel) and representative instruments (e.g., a thermometer). On this basis, he claims that

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[1] Already in 1978, Peter Janich advocated a similar view:

> A high respect for academic traditions on the one hand and a certain disdain for technical application on the other led historically to the belief that experiments are *nothing but* a means for gaining true knowledge of nature although experiment played an important role from the very beginning of men's use of tools and caused the development of a technology independent of any theoretical claims. (Janich, 1978, 18)

experiments that employ productive or constructive instruments only are fully theory-free.

These three views share two basic claims. First, they assume that there is a philosophically significant contrast between 'ideas' (or 'understanding') on the one hand and 'theories' of nature and apparatus on the other. Second, they adduce examples from the history of science which are meant to exemplify experiments led by ideas or understanding but not by theories. In the next section, I offer an analysis of some of these examples, which shows that they cannot support claims of theory-free experimentation. The contrast between ideas and theories will be dealt with in the final section.

3. On the Relationship between Experiment and Theory

The issue of the relationship between experiment and theory is complex and the claim of this paper is that it demands a more differentiated treatment than has been advanced so far. The challenge is to take account of both the specific features of experimental activities and the various roles played by theoretical interpretations. In this section, I discuss four different claims about how experiment and theory relate to each other. The first three are relatively uncontroversial. Hence I will deal with these rather quickly and focus on the claim that involves a substantial disagreement with the three views summarized in the previous section.

(i) *All experiments are explicit tests of existing theories about the objects in question*. This is the strongest version of the claim that experimenting is theory-dependent. It completely subordinates experimental inquiry to theoretical research. It can be safely concluded, though, that this claim is most certainly false on any reasonable understanding of what is taken to be an experiment (see the studies mentioned in the previous section, among many others). among many others). Yet, the fact that experiment involves much more than theory testing does not, of course, mean that testing a theory may not be an important goal in *particular* scientific settings. Thus, Gerd Lüer (1998, 200-204) argues that so far psychological experiments have had too much of a life of their own. In psychology, an exclusive focus on experimental methodology has been at the expense of a theoretical understanding of underlying psychological mechanisms. Accordingly, Lüer claims that psychological experiments, if they are to become more informative, should be developed in closer connection to explanatory theories.

(ii) *All experiments are theory-guided*. That is to say, they are planned, designed, performed and used from the perspective of one or more theories about the objects in question. Since 'guidance' is meant here in the rather strong sense of a continuing and specific impact of theories on various activities during the successive stages of the experimental process, this claim is too extreme to be plausible. Steinle is certainly right in arguing that not all experiments are theory-guided in this sense. But again, this does not preclude that cases of theory guidance can be found in scientific practice. Steinle himself (1998, 282-284) discusses the example of Ampère's experiments to test his hypothesis that all magnetism is caused by electric currents circulating in material bodies. Modern particle physics is another area where theory-guided experiments occur regularly.

(iii) *The (immediate or later) significance of experiments is affected by the theoretical context in which they are situated.* This claim involves a broader role for theories than the first two. The significance of an experiment is said to depend, to a smaller or larger degree, on its theoretical relevance. Consider, again, the early experiments on the interaction between galvanic electricity and magnetized needles performed by Hans Christian Oersted and, among others, Ampère. The immediately acknowledged significance of Oersted's original experiments largely derived from the fact that they realized a kind of phenomenon that appeared to be at odds with the dominant theoretical discourses. An important point was that there seemed to be a non-Newtonian—that is, a non-central—force at work (Gooding, 1990, 29-36). More in particular, as Steinle (1998, 274-277) notes, the existence of an interaction between electricity and magnetism did not fit either in Siméon Poisson's theoretical account of electricity. Another illustration of the above claim is provided by Evangelista Torricelli's well-known experiment, performed in 1644. Torricelli poured mercury into a glass tube and then inverted the tube in a dish filled with the same substance. A crucial question concerned the nature of the space left at the top of the mercury column: is there a vacuum or not? Thus, the experiment derived its theoretical significance from its direct bearing on the long-standing dispute over the possibility of a vacuum (Shapin and Schaffer, 1985, 41). In these two cases, the theories in question were neither being tested nor guiding the realization of the experiments in all sorts of ways. Instead, they increased the theoretical relevance of these new kinds of phenomena and thus strongly stimulated their continuing experimental exploration.

The significance of an experiment may also change in the course of time as a consequence of further theoretical developments. In this case, the same material realization of an experiment gives rise to different theories of the experimental process and different theoretical results (cf. Radder, 1988, ch. 3, and Radder, 1996, ch. 2). An illustration can be found in Carrier (1998, 186-189). He discusses the experiment by which Hippolyte Fizeau measured the shift in the interference pattern of light when it passes through moving water. Carrier shows that, in the course of time, three conceptually disparate interpretations of this experiment were proposed: Fizeau's own ether drag account (1851), Hendrik Lorentz's approach in terms of the interaction between light and the charged constituents of matter (1892), and Albert Einstein's explanation by means of the relativistic addition of velocities (1905). Hence, the significance of Fizeau's original experiment changed fundamentally as a consequence of reinterpretations through radically new theories.

On the basis of such cases, the claim that the immediate or later significance of experiments is affected by the theoretical context, is plausible enough. The present claim, however, is not necessarily incompatible with the accounts of Hacking, Steinle and Heidelberger, because the relationship between experiment and theory is less tight than in the case of the first two claims. As we have seen, it even applies to Steinle's example of Ampère's exploratory experiments. Unwittingly, Hacking (1983, 156) offers another example. He notes that the phenomenon of double refraction in Iceland Spar surprised 17th-century physicists, and that this surprise was, in part, caused by an apparent conflict with the theoretical laws of refraction.

(iv) *Performing and understanding experiments depends on a theoretical interpretation of what happens in materially realizing the experimental process*. This is a view I have held myself for a long time through claiming that experimentation involves both material realization and theoretical interpretation (Radder, 1988, 59-76; Radder, 1996, 11-12). As I explained in the Introduction, experimenting involves, at least, the material realization of an interaction between an object and some apparatus, in such a way that a stable correlation between some feature of the object and some feature of the apparatus will be produced. In these terms, the claim is that materially realizing a stable correlation and knowing what can be learned about the object from inspecting the apparatus depends on theoretical insights about the experimental system and its environment. Thus, these insights pertain to those aspects of the experimental process which are relevant to obtaining a stable correlation. It is not necessary, and in practice it will usually not be the case, that the theoretical interpretation offers a full understanding of each and every detail of the experimental process.

This particular form of theory dependence, however, is at odds with the account advocated by the authors whose views I summarized in section 2. Hacking, for instance, writes:

> It remains the case ... that much truly fundamental research precedes any relevant theory whatsoever. (Hacking, 1983, 158)

And Heidelberger states:

> Normally, the experimental use of productive and constructive instruments does not presuppose a theoretical interpretation. (Heidelberger, 1998, 87; my translation)

As I noted before, the view that much experimental inquiry precedes, or does not presuppose, theoretical interpretation is claimed to be supported by examples from the history of science. Hence, to assess the plausibility of this view I will examine two of the mentioned cases in some detail.

I start with some experiments from the early history of optics. Hacking (1983, 156) tells us that Isaac Newton's experimental observations of the dispersion of light preceded any theoretical interpretation and Heidelberger (1998, 82) includes the prism in his category of theory-free, productive instruments. In many of Newton's experiments on the dispersion of light, carried out between the 1660s and the 1720s, the prism was his major instrument. One series of experimental trials involved the use of a single prism to produce colored light projected on a screen. A further, very important, series of experiments investigated what happens when light of a specific color that goes out from a first prism is being sent through a second one. The following account of Newton's prism experiments shows that they depended in at least three ways on a theoretical interpretation of what was going on in the material realization of the experimental processes (see Schaffer, 1989; see also Hakfoort, 1986, ch. 2).

A first question concerns what may be learned from inspecting the apparatus, i.e.

looking at the colors on the screen, about the object under study, i.e. the light before it impinges on the prism. Only if the outgoing light is, basically, of the same nature as the incoming light, are we able to learn something about the latter by examining the former. Scholastic theories of light, however, usually distinguished two kinds of color: apparent colors produced from modified light (e.g., in prisms) and real colors disclosed, but not produced, by light. Hence, on the basis of these theories prisms were inappropriate for probing the nature of real light and real colors. That is to say, on this view prismatic colors are mere artefacts of the instrument and not intrinsic constituents of light. In his studies on optics, however, René Descartes had criticized the distinction between real and apparent colors, and claimed that all colors were apparent, or secondary. In this respect, Newton followed him. As Schaffer (1989, 73-74) concludes, it was only in this changed theoretical context that prisms could acquire a key new role in the analysis of color.

Second, there is the notion of a light ray. The idea of rays travelling in straight lines and capable of being refracted upon entering another medium was employed routinely in analyzing experiments by Newton and many others before him. It played a role in the performance of the experiments as well. In many experiments a 'single ray' was needed. For this purpose, light should be passed through a slit of the right dimensions, while the room should otherwise be as dark as possible. According to Newton, such conditions were crucial for realizing a stable experiment that could be reproduced by others (see Schaffer, 1989, 88 and 93). Thus, the theoretical model of light as consisting of rays, travelling along straight lines and refrangible on entering a different substance, structured the way in which Newton performed and understood his experiments on the dispersion of light.[2]

Third, there was the more specific notion of primitive rays, each possessing a particular refrangibility. This notion was very important to Newton. He stated that the existence of such rays could be demonstrated by means of the two prism experiment: a ray that comes out of the first prism is primitive (or, uncompounded), if its color does not change when it is passed through the second prism. It proved quite difficult, however, to realize this experiment in a stable manner. Several experimenters from different countries failed to reproduce it. In response, Newton claimed exclusive authority for his own theoretical interpretation by arguing that his critics had failed to produce a primitive ray in the first place. Thus, Newton stuck to his theory that light is a heterogeneous mixture of differently refrangible, primitive rays, which he said had informed all his prism experiments since about 1666 (Schaffer, 1989, 71). This, however, was not the only possible interpretation. Robert Hooke, for instance, did succeed in reproducing Newton's experiments, but he interpreted them on the basis of his own vibrational theory of light and color (Schaffer, 1989, 86-87).

Let us now turn to a second case of claimed theory-free experimentation. As we have seen, Heidelberger distinguishes between productive, constructive and representative instruments. Both productive and constructive instruments aim primarily at the creation of stable phenomena. Air pumps, prisms and particle accelerators are examples of productive

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[2] For an insightful account of the theoretical nature and the practical uses of this model, see Toulmin, 1967, especially 16-28.

instruments, while Leyden jars and wind tunnels exemplify constructive instruments. Experiments employing productive or constructive instruments only are claimed to be theory-free. That is to say, neither is it the proper aim of such experiments to test a presupposed theory or even the relevant prior understanding, nor does this prior understanding imply a theoretical interpretation of the experiments (Heidelberger, 1998, 86-87). In this paper I will leave aside the issue of the adequacy of the proposed typology of instruments (for a discussion of this issue, see Buchwald, 1998, 384-391). Instead, I will examine a case that is put forward by Heidelberger as a prime example of a theory-free, productive instrument, to wit the air pump.

Consider, more specifically, the case of the air-pump experiments performed by Robert Boyle during the late 1650s and early 1660s (see Shapin and Schaffer, 1985, chs. 3 and 4). This case shows that both the working and the possible outcomes of Boyle's productive experiments with the air pump were directly dependent on theoretical assumptions. These assumptions were made explicit by Thomas Hobbes in his criticisms of these experiments. Boyle's central claim was that his air pump produced a space that was (almost) totally devoid of air. This claim required that the routine leakage of air was ‘negligible’. Hobbes, however, argued on the basis of his theory of the nature and composition of air that the pump leaked, not just slightly and incidentally—as was conceded by Boyle—but in a significant and consistent way (Shapin and Schaffer, 1985, 115-125). According to this theory, common air is a mixture of different substances: earthy or aqueous fluids and pure or aetherial air. Because of the presence of the latter, air is infinitely divisible. Consequently, an absolutely impermeable seal is impossible, and thus the air pump is bound to leak. In addition, Hobbes claimed that his theory was able to account for the results of Boyle's experiments just as well. In one trial, for instance, a candle placed in the receiver of the air pump went out after some time of pumping. And while Boyle accounted for this on the basis of the absence of air, Hobbes simply claimed that the candle had been extinguished as a consequence of the violent circulation of the air caused by the pumping. In this way, Hobbes intended to show that (the results of) Boyle's experiments depended on questionable theoretical assumptions.

What does this episode from experimental science tell us with respect to Heidelberger's claims? Heidelberger is right in as far as Boyle did not intend his air-pump experiments to be an explicit test of either an already available theory or his prior understanding. Thus, the case confirms the earlier point that not all experiments are meant to test theories or other knowledge claims.

There is more to this case, though. Consider the claim that ...

> ... the air pump produces a vacuum, which human beings could not experience in nature without this instrument. (Heidelberger, 1998, 81; my translation)

Yet, this claim has not always been evident. In Boyle's days, the conclusion that the space in the receiver was a vacuum was highly contestable, because of the big theoretical controversy between plenists and vacuists about the possibility of a vacuum in nature. Hence Boyle, who wished not to go beyond ‘matters of fact’, intentionally refrained from drawing that conclusion. Thus, he understood a vacuum to be a space (almost) totally devoid of air, and not a space without any bodies at all (Shapin and Schaffer, 1985, 46).

However, the claim that the air-pump experiments demonstrated the existence of a vacuum in Boyle's sense depended on theoretical interpretation as well. The conclusion that he had produced a space devoid of air required that Boyle took a stance in the ongoing theoretical debate on the nature and constitution of air. In his reply to Hobbes's criticism, Boyle conceded that it was possible that air had an aetherial part and that this part might always be left in the receiver. But he denied that the presence of this aether could have an impact on his experimental results, since the aether was not experimentally 'sensible' (see Shapin and Schaffer, 1985, 178-185). The general point is that the correlation between features of the apparatus (e.g., the going out of the candle) and corresponding features of the object under study (e.g., the space devoid of air) is stable only, if the experimental set-up constitutes a closed system (for this notion, see Radder, 1988, ch. 3; Radder, 1996, ch. 6). To know whether or not a system is closed requires a theoretical interpretation of the possible interactions between the (object-apparatus) system and its environment. In the case under discussion, Hobbes argued that there was such an interaction (the intrusion of outside air into the receiver) *and* that it disqualified the claimed experimental results. Boyle, in contrast, admitted that there might be such an interaction but he argued that it was *irrelevant* anyway for the production of stable experimental matters of fact.

In the above discussion the examples have been deliberately chosen from early experimental science. After all, here the view that experimentation may be theory-free seems to have an initial plausibility. Surely, such a view will be far less likely when the objects under study are further removed from ordinary experience and the apparatus is more advanced and complex. Heidelberger claims that the particle accelerators of modern physics enable a theory-free experimental production of new constituents of matter. It is, however, not hard to show that performing and understanding these experiments does require a lot of theory, about the particles themselves, about the operation of the instruments and about the interaction between the particles and the instruments (see, e.g., Morrison, 1990, 6-14).

4. Ideas versus Theories?

I conclude the paper with a short discussion of the distinction between 'ideas' (or 'understanding') and 'theories'. As we have seen, such a distinction is being used by Hacking, Steinle and Heidelberger to support their view that experiments may be guided by ideas or prior understanding and still be essentially theory-free. Making a distinction between ideas and theories is part of a more general strategy of differentiating the notion of theory.[3] A

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[3] Thus, Hacking (1983) introduces a (further?) contrast between high-level and low-level theories. On the basis of this contrast, it is claimed, two important philosophical consequences of the doctrine of theory ladenness can be avoided: the vicious circularity that would arise if all tests of a theory were laden with that theory itself; and the anti-realist conclusion that scientific knowledge cannot be about a human-independent reality, if it were

full assessment of the philosophical merits of this strategy is clearly beyond the scope of this paper. Hence, I will limit myself to a brief discussion of the immediate implications of the preceding sections.

One reason for the restricted value of the contrast between ideas and theories is philosophical in nature. The main purpose of the early proponents of theory ladenness was to demonstrate the inadequacy of empiricism. That is to say, they opposed all forms of epistemology according to which scientific knowledge is founded on theory-free observations or experiments. Since their criticism works as well in the case of ideas (or prior understanding) as in the case of presupposed theories, any appeal to this distinction for empiricist purposes is unwarranted. Thus, whatever the further merits of the 'new empiricism' (see note 3), it cannot include the classical empiricist claim that scientific knowledge is, or should be, justified on the basis of 'given' experience.

Next, it will be clear that a strict definition of what a theory is, and hence how it contrasts with a mere idea about nature and apparatus, is hard to come by. Yet, some attempts have been made. Thus, Hacking (1983, 175) sees a theory as 'a fairly specific body of speculation or propositions with a definite subject matter'. And Steinle (1998, 285) conceives of theories as systems that aim to account for entire domains of unobservable entities. On these views, however, Newton's interpretation of the nature of light and color and Hobbes's account of air surely qualify as theories. Thus, the claims that Newton's experiments on the dispersion of light and Boyle's air-pump experiments were led by ideas but not by theories prove to be historically wrong.

More generally, the lesson is that claims about the theory (in)dependence of experiments must be accompanied by *detailed* historical evidence. In addition, the distinction between ideas and theories may induce a certain presentist bias. Simply put, using this distinction may engender a tendency to interpret yesterday's theories as mere ideas, while current—and especially microscopic—theories are seen as genuinely theoretical. If the philosophical claims about theory (in)dependence are to be historically adequate, this presentist pitfall should be avoided.

Finally, on the basis of the arguments in this paper, I conclude that performing and understanding scientific experiments depends on theoretical interpretation. This conclusion is supported by the philosophical analysis of experiments as attempts to realize stable correlations between specific features of objects and apparatus. Moreover, Newton's prism and Boyle's air-pump experiments show that even cases which appear to be theory-free at first sight prove, on closer inspection, to rely on theoretical interpretation in various ways. Yet, it should also be clear that the claims in this paper do not imply a theory-first approach, neither in a logical nor in a temporal sense. An interactive view of scientific experimentation, in which the mutual dependencies between material realization and theoretical interpretation

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completely dependent on ever-changing high-level theories. The resulting position has been characterized as a 'new empiricism'. See Rouse, 1987, 9-12; see also Carrier, 1998, 179-181, on the similarities with logical empiricism. (I would like to thank Francesco Guala for a helpful discussion about these issues).

are systematically taken into account, appears to be the most adequate.

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