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Philosophical Elements in Thomas Kuhn's Historiography of Science*

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ABSTRACT: To begin, the so-called 'selectivity of historical judgment' is discussed. According to it, writing history requires a comparative criterion of historical relevance. This criterion contains philosophical elements. In Kuhn's case, the criterion directs historical research and presentation away from Whiggish historiography by postulating a hermeneutic reading of historical sources. This postulate implies some sort of internalism, some sort of rationality of scientific development, and historical realism. To conclude, some consequences of Kuhn's anti-Whiggism are discussed.

Keywords: Historical relevance; Anti-Whiggism; internalism; rationality of science; historical realism.

RESUMEN: Para empezar, se discute la llamada "selectividad del juicio histórico". De acuerdo con ello, escribir historia requiere un criterio comparativo de relevancia histórica. Este criterio contiene elementos filosóficos. En el caso de Kuhn, el criterio aleja la investigación y la presentación histórica de la historiografía Whig al postular una lectura hermenéutica de las fuentes históricas. Este postulado implica alguna clase de internismo, de racionalidad del desarrollo científico y realismo histórico. Para concluir, se discuten algunas consecuencias de la postura anti-Whig de Kuhn.

Palabras clave: Relevancia histórica; postura anti-Whig; internismo; racionalidad de la ciencia; realismo histórico.

1. Introduction

For reasons that have a long and indeed plausible history, historians of science as historians are often not particularly interested in what philosophers have had to say about science and its history. This is nicely illustrated by what Thomas Kuhn said in the late 60s about his interactions with Carl Gustav Hempel:

No one in recent years has done so much to clarify and deepen my consideration of philosophical problems as my Princeton colleague C. G. Hempel. But my discourse with him and my acquaintance with his work does nothing for me at all when I work on, say, the history of thermodynamics or of the quantum theory. (Kuhn 1977a, *ET*, 12)

Thus, when I discuss philosophical elements in some particular form of historiography, I had better have persuasive reasons for why this topic might also be interesting for historians. In an attempt to provide such reasons, I shall start with a rather strong claim; namely, that no historiography, including the historiography of science, can be entirely philosophically innocent. The assertion is, in other words, that all historiography depends on and makes use of philosophical elements. The formulation that historiography "depends on and makes use of" philosophical elements may already be misleading, and I will come back to it later in this paper. But first, I shall defend my claim in general in the next section. In section 3, I will fill out these abstract considera-

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tion with some Kuhnian flesh. In section 4, I will develop the philosophical elements that are inherent in Kuhn's mode of doing history of science. In the final section, I will briefly discuss some systematic limits of anti-Whiggism.

2. *The necessity of criteria of historical relevance for historiography*

The fact from which I begin has many names. It has been called "the selectivity of historical judgment" and it has been described by the slogan: "In history, there is no democracy of facts". Kuhn spoke about "preconceptions about what is essential, what is not" and of "selective principles" of historiography (Kuhn 1971, 138, 142; also 1977a, *ET*, 14, 18). I shall use a more elaborate description which reads: Both in research and in presentation, all historiography needs *comparative criteria of historical relevance*. Such criteria are indispensable on the grounds that some decision must be made as to what belongs in a given historical narrative and what does not. The criteria must be comparative in that they allow for distinguishing degrees of importance. Stories may be told more or less comprehensively. Shorter versions need not omit essentials; neither must longer versions incorporate inessentials. Let me illustrate immediately. It has been estimated that in the comprehensive histories of all of natural science, something like 1 in 300 scientists who worked at a given time is mentioned (Graf-Stuhlhofer 1995). Of course in histories with higher disciplinary resolution, the percentage is much higher. Yet, the fact remains that not every scientist is mentioned along with everything he or she did. The reason is quite clear. A historical narrative is not, cannot, and should not be a 1:1 replica of the respective historical episode. Only the "important" figures and facts should be part of the story, and above I have called that importance "historical relevance". The varying degree of historical relevance cannot only be seen in the final narrative, but also in the research that precedes it. It governs what is investigated and what is not for a given project. It governs whether or not to rebuild an apparatus or re-perform a historical experiment. To illustrate again: If *a priori* insights are the main ingredient in superb science, then the question about what may have been the outcome of some experiment in the real world is not so important. If, however, empirical data play a pre-eminent role, then one may try to recreate a particular historical experimental situation. I am here alluding to the tension between Koyré and Settle with respect to Galileo (see, e.g., Brush 1995, 228).

It may be useful to distinguish different sorts of historical relevance (see Hoyningen-Huene 1993, 13-14). I think that there are roughly three sorts, but they overlap and interrelate. This latter fact is, however, not important in our context. The first sort of historical relevance is *factual relevance*. It selects material which is relevant in order for the history of a given topic to be told at all. Thus, talking about the history of special relativity necessarily involves some talk about electrodynamics. The second sort of historical relevance is *narrative relevance*. It selects for material which must be taken into account if the resulting text is to be a proper narrative. Such material includes those facts by which a historical report gains the narrative continuity that it needs (see Kuhn 1977a, *ET*, 8-9) or facts which make plausible what would otherwise be implausible. For example, take the delay in the start of an airplane caused by heavy snowfall which allowed two physicists from diverse areas talk to each other for several hours. If this,

in turn, led to deep innovations in one of their fields, then in order for the story of this field to be adequately told, this consequential incident must be included. The final sort of historical relevance is *pragmatic relevance*. It selects for material without which the pragmatic goal of a historical narrative cannot be realized. Thus, the content of a historical narrative is determined partly by the audience to which it is addressed, and partly by the effect it is meant to have on this audience. A narrative aimed at deepening the understanding of today's genetics may be different from one aimed at understanding some historical episode in genetics solely in its own time and in its own terms. Here, I am alluding to the difference between today's mainstream history of science and, for example, Ernst Mayr's concept of developmental history (see Mayr 1990).

Some of the elements of these sorts of historical relevance can properly be called *philosophical elements* of the respective historiographic work. By this I mean, roughly and very generally put, the usually implicit assumptions about history itself, or about proper historical research and presentation which influence historical work. It is clear that, for example, decisions about the general aims of historiography of science (which I just mentioned in the genetics example), or convictions about the influence of social factors on the content of science, qualify both as criteria of historical relevance and as philosophical elements of the respective historiography. I hope it is clear by now that historical work cannot proceed without them. Trying to avoid them in history is as illusory as the absolutely theory-free facts which the positivists sought as the epistemic basis of science.

Although it is not part of the main line of this paper's argument, at this point I want to insert a remark about the possibility of drawing philosophical lessons about science from studying its history. Many people have the impression that from the history of science, very little or even nothing philosophical about science can be gleaned for methodological reasons. But that was exactly what Kuhn tried to do. He tried to extract philosophical lessons about science from its history. One of the plausible reasons for why nothing of philosophical interest could possibly be extracted from the historical narratives about science would be that all that one could gain would be just those philosophical elements which one had plugged into the history oneself, in the form of comparative criteria of historical relevance. Kuhn saw this as a danger for Lakatos' program of an evaluation of methodological positions through history, given Lakatos' conception of internal history (Kuhn 1971, 140-143). But as in science, where theory-laden data do not lose their potential to challenge theories (even the very same theories by which they are impregnated), philosophical elements embodied in the criteria of historical relevance do not rule out the possibility that historical narratives can challenge philosophical conceptions of science. One of the reasons is, as already pointed out by Kuhn, that in historical work there are also operative selection principles which are neutral with respect to different positions in the philosophy of science (Kuhn 1971, 142). In the terminology introduced above, it is mainly the criteria of narrative relevance which qualify for that role because the historical data requested by them may turn out to be in severe tension with philosophical preconceptions of science. This is what Kuhn meant when he said that history was an "independent disci-

pline” (Kuhn 1971, 143; see also Kuhn 1977a, *ET*, 15-18), independent, at least to some degree, from the philosophical preconceptions of science the historian may hold dear. However, if one draws philosophical consequences about science from the study of its history, it is worthwhile to ask whether these consequences were actually already part of the criterion of historical relevance that one used in doing the respective history.

3. Kuhn's principal criterion of historical relevance: anti-Whiggism

Now I can come back to the main line of my argument, to the central element in Kuhn's criteria of historical relevance. It is his anti-Whiggish stance which is certainly not philosophically innocent. Its centrality concerns both Kuhn's historical and his more philosophical writings throughout his entire life. In this respect, it is almost unnecessary to quote the first sentence of *The Structure of Scientific Revolutions* which concisely states Kuhn's lifetime goal:

History, if viewed as a repository for more than anecdote or chronology, could produce a decisive transformation in the image of science by which we are now possessed. (*SSR*, 1)

Before analyzing the philosophical elements contained in Kuhn's anti-Whiggism, it is necessary to clarify what exactly is meant by Whiggism (or “Whiggishness”) and anti-Whiggism. This is all the more important as these terms have apparently changed meaning within the last few decades. What has not changed, however, is that “Whiggism” is undoubtedly a derogatory term. But today it seems that many more positions are called Whiggish than, say, 30 years ago, and this fact derives, as far as I can see, from the growth of social constructivism in the humanities. Today, any form of historiography that assumes scientific progress may be called Whiggish (Laudan 1990, 56), or any historiography that allows for judgements about the historical actors' getting it right or wrong in some sense (e.g., Margolis 1993, 188). The Whiggism which Kuhn attacked in his philosophical writings in the 60s and 70s was of a much more specific variety than the extremely wide conception of Whiggism today. Given today's usage of the term, Kuhn would be considered a Whig (which he would very much dislike), because he believed in scientific progress and did not doubt the possibility of evaluating the actions of past scientists as appropriate or inappropriate, relative to their paradigm, of course. First, I want to summarize the main elements of Whiggism in the sense it is used here.

In abstract terms, historiography is Whiggish if it is mainly oriented towards the present in the sense that it tries to understand and evaluate the past with respect to the present (McEvoy 1997, 3-4, 12; Turner 1990, 26); it “impos[es] our own intellectual categories and values upon the past” (Nickles 1995, 151). This tendency is also called the “presentism” or the “teleological structure” of Whiggish historiography because of its evaluation of past science “according to the extent and worth of their contributions to the present state of scientific knowledge” (McEvoy 1997, 12). More specifically, Whiggish historiography typically

- ascribes a specific function to the history of science, namely the improvement of present science, especially through the clarification of contemporary methods or concepts (Kuhn 1968, *ET*, 107; McEvoy 1997, 7-8);
- “search[es] for the origins of contemporary science in the past” (McEvoy 1997, 4) in the sense that past science contains the germs of present science;
- emphasizes “individual men of genius” (McEvoy 1997, 7) and produces “hero-myths” (Turner 1990, 24-25);
- has a tendency to describe historical episodes as battles between good and bad, between progressive and reactionary, between real science and metaphysics (McEvoy 1997, 23);
- asserts that in these controversies, crucial experiments very often play a decisive role (McEvoy 1997, 23);
- tends to describe the losers in scientific controversy as “knaves or fools” (Turner 1990, 25);
- evaluates old theories as either right or wrong; if they are right, they are still part of science, if wrong, they are a waste of time (Turner 1990, 23);
- holds a “eureka-moment version of scientific discovery” (McEvoy 1997, 7), that is, discoveries are typically events and not extended processes (see also Kuhn 1962a);
- conceives of scientific development as cumulative (McEvoy 1997, 6).

In addition, Whiggish historiography typically believes the following things about the Scientific Revolution:

- there was only one revolution in science: the Scientific Revolution; other revolutionary episodes in the history of science are dependent on it (for instance, the chemical revolution) (McEvoy 1997, 10, 15);
- there is an enormous discontinuity introduced by the Scientific Revolution; almost no continuities exist to earlier phases (McEvoy 1997, 23);
- the Scientific Revolution is characterized by the emergence of the Scientific Method (McEvoy 1997, 23).

It is quite clear that Kuhn opposed every single item mentioned. Instead, as is well-known, he advocated an anti-Whig form of historiography with the proclaimed goal of “display[ing] the historical integrity of [past] science in its own time” (*SSR*, 3). What was the main reason that he turned away from Whiggish historiography? Of course I do not want to answer this question biographically, rather I will try to unearth systematic reasons for his departure from this tradition. The reasons that Kuhn explicitly states, especially in *The Structure of Scientific Revolutions*, concern the experience of historians. More specifically, they concern the difficulties encountered by historians when executing the Whiggish historiographic program.¹

But I think that there is a reason, in some sense deeper, for his departure from Whiggish historiography. Whiggish historiography as a historiographic enterprise is

¹ *SSR*, 2-3; a more complete discussion of this issue can be found in Hoyningen-Huene 1993, 16-19.

only plausible against a particular philosophical background about the nature of science. It is plausible, and possibly even convincingly so, if and only if *present science is more or less in possession of the truth about its subject matter*. The privileged status of contemporary science would justify the evaluation of the past in terms of the present because in contemporary science, we have a yardstick that is qualitatively correct (and quantitatively at least not too far from the truth). Of course, professional experience strongly influences whether or not one believes in at least the approximate truth of contemporary science (or at least in its high probability). To most scientists on the one hand, it is more or less evident that later science is an improvement over earlier science, and that today's science is, in many aspects, just right—how else could one explain the success of science? This line of thought is known as the miracle argument.² To the trained historian on the other hand, the privileged status of contemporary science is dubious both on the grounds that this position is so strongly reminiscent of ethnocentrism of other kinds, and on the grounds that a careful reading of the sources of an older science often does not reveal a principally qualitative difference to today's science. Rather, scientists of past ages tried to make sense of empirical findings in much the same way as today's scientists do, and the main difference to present science is that they used some fundamental scientific concepts different from and not compatible with ours. And if some of the fundamental concepts of past science have been superseded by our current scientific concepts, couldn't it be that in the future some of our fundamental concepts will be superseded by still different concepts? If this were the case, then our current scientific concepts and our current scientific belief would be subjected to the same sort of historical change as all past science, resulting in a relativization of its privileged status. Clearly, in comparison to past science, present science is more powerful and therefore progressive. In comparison to future science, however, some of its fundamental concepts could appear as limited and inappropriate as some of the fundamental concepts of past science appear to us. As a consequence, the privileged status of present science could certainly not consist in its more or less possessing the truth about its subject matter, whereas past science got it just wrong.³

As I said, these convictions about the epistemic status of today's science may depend on one's specific professional experience, and for historians their genesis is probably best described as an interactive feedback process between preconceptions of science and concrete historical research. Thus, the description of these convictions as "philosophical elements" may be misleading insofar as it has an *aprioristic* flavor. It sounds as if the philosophical image of science is fixed first, and then the history of science is done accordingly. This is definitely not what I want to assert. As I said, the philosophical elements are in interaction with the historical experience made possible by them. Thus strictly speaking, neither the assertion that these philosophical elements

² There is a lot of literature on the miracle argument. Some references to it, together with my reservations against the miracle argument, can be found in Hoyningen-Huene 2011.

³ This line of thought exists in several variants. In philosophy it has famously been developed into the so-called pessimistic meta-induction. The classic source, from which most of the recent philosophical discussion on the pessimistic meta-induction originated, is Laudan 1984 [1981].

are *presupposed* in historical work is correct, nor that they are *implied* by it. Rather, their relationship should be described, in the ideal limit, as a state of reflective equilibrium resulting from multiple feedback loops.

With this proviso in place, I can now address the central philosophical element of Kuhn's historical work. His stance concerning the alternative mentioned before was quite clear and firm. Past scientific belief possesses the same sort of fundamental characteristics as present scientific belief: it provides detailed descriptions and explanations of the world, it is controlled by empirical data, and it is validated by scientific communities who are the exclusive arbiters in epistemic matters (compare Kuhn *SSR*, 42, 168). More specifically, past scientific belief cannot be described as having resulted from scientific "mistakes", whereas present scientific belief has avoided them (Kuhn 1970, *ET*, 279-280). Rather, scientists of all times have tried to improve the fit between their belief and empirical data in principally the same way. Of course, one has to spell out what the phrase "in principally the same way" means, but this is possible, especially for traditions of what Kuhn called normal science.

4. *Philosophical consequences of Kuhn's anti-Whiggism*

For Kuhn, anti-Whiggism had strong methodological consequences for his method of doing history. In particular, he approached the primary sources in a different way, that is, different from the way the earlier presentist tradition had. It was the particularly close reading of the sources, the *explication de texte*, that occupied center stage. This was, for Kuhn, Alexandre Koyré's main legacy. The aim was to reconstruct the positions of past scientists as authentically as possible. Or as Kuhn often put it,

to climb inside the heads of the members of the group which practices some particular scientific specialty during some particular period; to make sense of the way those people practiced their discipline. (1979, 122)

A clue to this sort of understanding is often provided by those passages that seem strange or outright foolish because they indicate a defective reading of one or more of the key terms of the text under scrutiny. In the Whiggish tradition, those passages were seen as irrelevant, or as pointing to some confusion that was also basically irrelevant for the further course of history. On the other hand, a hermeneutic reading, as this close reading of the sources is also called, assigns high historical relevance to those strange passages as it tries to uncover the different, original meaning of the text; i.e., different from the one that presents itself when read through contemporary conceptual spectacles (e.g. Kuhn 1968, *ET*, 110; Kuhn 1979, 126). In the following, for the sake of brevity I shall use the term "hermeneutic method" for this approach to historical sources.

As plausible as the hermeneutic method may be for anyone who is familiar with the practices in either history of philosophy or history of science, it contains at least three philosophical elements. These concern internalism, the rationality of science, and historical realism.

4.1 *Internalism*

I start with the *internalism* that is implied, at least as a strong working hypothesis, by the weight given to the hermeneutic method. This implication is fairly straightforward. If the historian's aim is to compose a story about the change of science in time, and if the main resource for this composition is a close reading of scientific sources, then it is presupposed that the main causal factors of scientific change can be found in those sources. As we are dealing with basic science, the content of the relevant scientific sources consists of material that is exclusively internal to science. Hence, internalism is presupposed, at least as a strong working hypothesis. This does not exclude the occasional influence of external factors but it implies that this is the exception to the rule.

How about justifications of internalism? I know that this very question may seem fairly outdated to some, but I still think that there is an interesting difference in two different modes of defending internalism. The first line of argument proceeds similar to the defense of anti-Whiggism. The internalism of some historian may be the result of an interactive feedback process between a preliminary internalist assumption which is confronted with scientific source material out of which a plausible narrative is to be built. This internalist assumption may continuously become more and more confirmed (or disconfirmed, for that matter) by this confrontation. Thus, for example, a substantive conviction about internalism in the basic sciences may result.

A second line of argument proceeds methodologically. In the best case, it does not involve any prejudice for or against externalism. According to this argument, internalism is a better heuristic maxim to begin with than externalism because more often than not, plausible and detailed internalist explanations for scientific change are more difficult to find than externalist ones. This holds at least if the standards for the plausibility of externalist explanations are not too high (which is the case in many standard publications: a structural parallelism between something internal to science and something external to science suffices; the question whether there is really a causal connection between the two is often not really seriously answered; see, e.g., Forman 1971). In addition, the adequacy of an internalist explanation is, in principle, not very difficult to check through comparison with the scientific record. Thus, if one can indeed produce a credible internalist explanation that is well-confirmed by the scientific record, there is little need to look for rival external explanations. Only if one has persistently failed to establish an internalist explanation for some scientific change, is one justified to search for externalist causes for this change. On the other hand, plausible external explanations for scientific change often seem simple to find, but exceedingly difficult to establish. To me, quite a few of the externalist explanations for scientific change that have been offered in the literature are not outright implausible, but I cannot get rid of the feeling that there are, or at least might be, alternative internalist explanations that have not been completely ruled out (see, e.g., Shapin and Schaffer 1985).

4.2 *Rationality of science*

In addition to this internalism, there is a rationality assumption about science contained in the hermeneutic method, again at least in the sense of a strong working hypothesis. A close reading of scientific sources is a necessary step for an *intelligible* reconstruction of a historical process only if the actions and beliefs described in those documents display some sort of rationality. Without such discoverable rationality, a close reading of those documents would not be instrumental for the composition of the respective narrative. Note, however, that I remained rather vague about the nature of the rationality presupposed. All I said was that some sort of discoverable rationality must be presupposed without which the historical sources lack the required intelligibility. As a matter of fact, in his exchange with Lakatos, Kuhn was quite explicit about his substantive assumption about scientific rationality. Here, he wrote:

I do not for a moment believe that science is an intrinsically irrational enterprise. What I have perhaps not made sufficiently clear, however, is that I take that assertion not as a matter of fact, but rather of principle. Scientific behavior, taken as a whole, is the best example we have of rationality. [...] [I]f history or any other empirical discipline leads us to believe that the development of science depends essentially on behavior that we have previously thought to be irrational, then we should conclude not that science is irrational but that our notion of rationality needs adjustment here and there. (Kuhn 1971, 143-144)

If one asks for a justification of this massive presupposition about sciences' rationality, I would again invoke interactive feedback processes which bring preliminary rationality (or irrationality) assumptions about science into contact with scientific source material. The respective assumptions may be confirmed or disconfirmed by this confrontation, or some modifications of these assumptions may be suggested.

Here, I can insert a short report about a disagreement about rationality that Kuhn and I had which we could never settle. I had the feeling that in the context of Kuhn's theory, the presupposed rationality of science could only mean some form of group rationality (see Hoyningen-Huene 1992, 496-499). This is a consequence of the fact that on Kuhn's views, science's principal agents are communities, not individuals. If communities are the principal agents of science, and if the enterprise resulting from community action is rational, then this rationality should be ascribed to the very same communities, not to individuals. But Kuhn strongly disagreed. To him, rationality had to be ascribed to individuals in principle. I see this point, especially from the perspective of action theory, and also from the perspective of the historian. The historian encounters rationality (or its lack) in papers, books, letters, reports, experimental set-ups and so on produced by individuals (or at most by small groups of individuals). However, it seemed natural for me to extend the realm of application of the term "rationality" to groups when it comes to a discussion of scientific development. Here, the principal actors are communities and their behavior may be rational or not.

4.3 *Historical realism*

After internalism and rationality I come to the last philosophical element connected with the hermeneutic method, historical realism. By "historical realism", I mean the doctrine that the historian can, in principle, find out the truth about history. There is

ample evidence that Kuhn adhered to this doctrine although he never articulated it pointedly and explicitly. Two things should be noted here. First, historical realism is not a strict analytic consequence of the hermeneutic method. However, historical realism can surely strongly motivate hermeneutics. If there exists a discoverable truth about historical processes, and if because of internalism, this truth is located in certain sources, then a close reading of those sources certainly makes much sense. Second, Kuhn's historical realism stands in considerable tension with his anti-realism about nature. As is well-known, on the one hand, for Kuhn the scientific process does not lead to convergence to the truth about nature; in fact, according to Kuhn, the concept of the distance of some body of scientific belief to the truth is meaningless in principle. Progress of science is not to be interpreted realistically, but only instrumentally. History, on the other hand, seems to be seen by Kuhn as capable of telling true stories, or at least approximately true stories which can be improved upon by further historical research. I do not believe that the tension between Kuhn's non-realism about nature and his historical realism is an outright and fatal inconsistency as some critics have suspected (see Hoyningen-Huene 1993, 128, fn. 308). But it certainly needs a prolonged argument in order to reconcile these two elements of Kuhn's position. This argument can only succeed if it plausibly establishes a dramatic difference in our opportunities to gain knowledge about nature, on the one hand, and about the human realm on the other. Whether such an argument exists seems to me an open and very interesting question (compare Hoyningen-Huene 1993, 128-130).

5. On the limits of anti-Whiggism

Finally, let me discuss some limits of anti-Whiggism. It is clear at the outset that the norm of anti-Whiggism represents some form of an idealized goal. All the characteristic maxims, like to forget completely about today's science, or to climb inside the heads of the historical actors, are psychologically outright impossible to adhere to if understood in a literal sense, while they are rather vague if understood metaphorically. In spite of this vagueness, however, there are some systematic limits of anti-Whiggism in the sense of some specific boundaries of the desirability of it.

The first limit of anti-Whiggism concerns the necessity to communicate the results of historical research to an audience who may not be familiar with the concepts of the past science in question. In order to communicate the specificity of the past conceptual system, how it differs from a familiar system must be elucidated, otherwise misunderstanding is unavoidable. Thus, contemporary science is needed at least as a contrast in order to communicate past science.

Second, an older scientific situation that was confusing for its own practitioners may become transparent when described in today's vocabulary. Of course, I do not suggest substituting the past descriptions by today's. But the nature of confusion in the past may become clearer by an additional redescription of the situation in today's terms, especially if the source of the confusion lies in the specific conceptual system used at the time.

Finally, as in many other situations, it seems worthwhile to distinguish questions from answers, that is, to distinguish Whiggish questions from Whiggish answers. This

difference has recently been stressed by Stephen Brush (1995, 220). Historical answers must not be Whiggish (with the constraints given above). But historical questions may be informed by the present without immediately becoming historically illegitimate. The question "Why didn't anyone notice Mendel's theory in the 19th century" is a question of undoubtedly Whiggish character. It is a question that may only be asked by someone unfamiliar with 19th century biology, but who knows about the relevance of genetics in the 20th century. Though the question would not have been asked in the 19th century, it does not seem to be an illegitimate question, and it can and should be answered in an anti-Whiggish sense.

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