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## Comments on the Precarious Relationship between History and Philosophy of Science

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The four papers in this issue illustrate both the extent of mutual entanglement between history of science and philosophy of science and the difficulty of achieving consensus on how the interchange between history of science and philosophy of science should work. They reveal some of the tensions between the aims and interests typical of historians and philosophers, tensions that often make exchanges like these unproductive. To overcome this problem, it is important to set the contexts and perhaps even the ground rules of collaboration so that the legitimate tensions involved are put to creative use. In order to make better sense of this suggestion, I address what was accomplished, and what was not, by the papers in this issue.

To begin, consider the differences in the aims of philosophy and history. A few stereotypical generalities are useful here, though the stereotypes need to be treated skeptically. Philosophical investigation tends to focus on abstract questions, formulated sharply in terms of larger philosophical issues and views. When philosophers turn to particular historical materials or case studies, they often begin with pre-established concepts and sometimes with expected conclusions in mind. The concepts employed often contain presuppositions about the nature of theory, evidence, and explanation, about the relation of experiment to theory, the objectivity and intellectual autonomy of scientific work, and the like. For this reason, philosophers working with historical materials risk overriding or misunderstanding the categories of the scientists whose work they study, especially when that work is historically or conceptually distant.

To avoid the resultant traps, it is crucial to enforce close attention to the context of the work under investigation, to the categories, concepts, and aims of the historical protagonists, and to key features of the contexts

that set their problems. Otherwise, considerations that the protagonists took to be decisive in resolving a dispute will be misclassified and misunderstood. Ignoring context is the path to anachronism, to the pseudo-history found in textbooks, and to the relegation of supposedly “real” history to Lakatosian footnotes—which, in spite of their pretense to capture history “wie es eigentlich gewesen ist,” *still* are often guilty of anachronism and misunderstanding. So, among the many reasons philosophers need help from historians, they need protection from being misled by bad—and seriously misleading—history.

Historians, in turn, sometimes focus so strongly on the local context of the work that they study that they miss the relevance of delicate epistemic or technical issues in the resolution of long-term debates. After all, even rather particular scientific communities, for example the eighteenth century electricians discussed by Friedrich Steinle, live and work across many of the contexts that historians study. Key debates are typically not confined to particular careers, laboratories, experimental systems, disciplines, or countries. A major research problem or program often spans more than a century and draws on multiple disciplines. On this large scale, when a scientific community rejects key arguments, switches dominant theory, reverts to long abandoned views, or abandons findings made with one instrument for those made with another (cf. Jutta Schickore’s paper), the main reasons need not be economic, ideological, or practical. Nor need power relations, influential as these undoubtedly are, be decisive. To understand what is involved in settling such debates one must often turn to such trans-contextual concepts as evidential and explanatory relevance. This remains true even though, as Schickore and Ofer Gal nicely argue, the concepts of “historical meta-epistemology” themselves need to be historicized.

Nonetheless, the long-term fate of research programs is often decisively affected by reasonably stable assessments of *evidential relevance*, *explanation*, and *inference to the best explanation* (common origin or otherwise). To make a good case for this claim, one would need to show that, in spite of some key changes in such meta-epistemological concepts, they can and do cross local contexts successfully. Such arguments *are* feasible. Indeed, Michel Janssen argues persuasively that common origin inferences played this sort of role in some familiar, important, widely separated historical cases. He argues that variants of current epistemological concepts played a crucial role in disparate episodes in widely separated historical periods, undamaged by the conceptual shifts revealed by historical meta-epistemology. There will inevitably be serious debates about some of his claims—and I will briefly challenge the all-out discipline- and period-independence of his common origin inferences as an “element of rationality.” Nonetheless,

however those debates turn out, his argument shows that well executed historical epistemology can foster productive collaboration and debate between history and philosophy of science. Indeed, the same can be said for all the papers in this issue. *The intertwining of history and philosophy of science depends on a stereoscopic vision of both historical agents' and our own epistemological categories. Such a vision opens up important questions regarding the contextually critical epistemological similarities and differences thus uncovered.*

To achieve such a stereoscopic vision, we need genuine two-way traffic between history and philosophy. Our papers can be nicely divided in this respect. Steinle's and Schickore's papers sketch out case studies pursued in their own right, with sophisticated attention to context and to agents' categories, and use these studies to develop philosophical points. In contrast, Janssen's and Gal's papers have philosophical starting points—they use their case studies to support philosophical or meta-philosophical claims, initially couched in pre-existing philosophical terms. This of course does not in any way impugn the depth or independent value of what they do with their case studies, but this is a different use of cases.

We thus have examples of two directions of work—proceeding from cases to conceptual claims and from philosophical stances to demonstrative instances illustrating those stances. One path takes the way up from history, the other the way down from philosophy. Both ways of working are, of course, legitimate ways of combining history and philosophy—but each also embodies the tensions I mentioned at the beginning and each faces different traps. Let me deal with them separately.

When one follows the way up, the historical material should be examined with minimal commitment to pre-established philosophical concepts. Yet, inevitably, cases are (and should be) chosen with regard to specific issues raised by philosophers (see Burian 2001). Steinle's and Schickore's case studies were surely chosen with philosophical aims—to address specific philosophical issues. Nonetheless, the categorizations and practices of the historical agents come first in their papers (as they should when one pursues the way up). This enables the historical work to highlight mismatches with current philosophical categories. Such disparities are a major source of tension for philosophically aware historians. And their history is suspect if it is too infected with *our* categories and concepts, *our* delimitation of domains of study, or *our* variants of such concepts as *error*, *evidence*, *experiment*, and *explanation*.

This problem is not insurmountable. For example, Steinle's paper reveals a general category of exploratory experimentation not well handled in Popperian, logical empiricist, and even Hacking- and Franklin-style new-experimentalist philosophies of experiment. He alludes to historical

studies of disparate domains in which related sorts of exploratory experimentation have been found. The various instances of exploratory experimentation historians have accumulated in the last decade force an unexpected issue upon philosophers of experiment, one that does not fit well with the categories they brought to their studies of experiment. This is a fine example of the historian leading the philosopher onto new ground.

One of Steinle's points about Dufay raises an interesting issue. He emphasizes the close association between exploratory experimentation and the development of a phenomenological theory in Dufay's work. Guided by the helpful comments of a referee, I note that, in general, "phenomenological" theories need not concern observables. The new concepts developed in the course of the exploratory experimentation sometimes reveal and stabilize phenomena not previously recognized. As Steinle's treatment of Ampère's stabilization of the concept of an electrical current circuit shows, the phenomena in question can be located at highly theoretical levels. The key point, then, is that exploratory experimentation stabilizes phenomena in a level-independent sense, neutral as to whether the phenomena are "theoretical" or "observable."

The change of concepts involved is often critical. To use an example from the 1950s with broad implications for molecular biology, Paul Zamecnik's group employed exploratory experimentation, following molecules and their components with (radioactive) tracers, to analyze protein synthesis biochemically. They showed that "soluble RNA" molecules (ultimately reinterpreted as transfer RNAs) were required to "activate" amino acids before they could be incorporated into growing protein chains. Following molecules through complex pathways is a kind of exploratory experimentation. Conceptually, the stabilization of the phenomena of activation turned on the way in which the experimenters, who sought one thing, found another, something that happened repeatedly in the work of the Zamecnik group (Rheinberger 1997, chaps. 10–11, particularly "Looking for India, Finding America," pp. 147–150). The general point I draw from this is that much recent work in molecular biology is a kind of natural history; what is at stake is the behavior of various interesting biological molecules in a variety of very particular circumstances. A great deal of exploratory experimentation, tracing molecules and their components through complex interactions, is required to delimit and describe the phenomena that need to be explained at the molecular level. A broad topic is opened up here, concerning, the prevalence, range, limitations, strengths, and weaknesses of such exploratory experimentation, its relationships to background knowledge, to changes in theory, to the various molecular mechanisms involved, and to the "identities" of the target molecules (or phenomena). The issues thus opened up call for

joint historical and philosophical investigation that, if successful, should stimulate a new category of historical studies of experiment. Detailed historical studies along these lines will do more to challenge orthodox philosophical accounts of the place of experiment within science than mere introduction of the category of exploratory experimentation.

Schickore's study of microscopy raises a more complex tangle of questions about how instruments are conceived, particularly whether some instruments systematically lead researchers into systematic error or misinterpretations of their own experiments. Like Steinle, Schickore uses historical materials to tease out the epistemological commitments of the protagonist(s) she studies before she commits herself to a particular epistemological analysis or meta-scientific characterization of the matter at issue. In the process, she opens the door to a new and potentially fruitful approach to the study of an aspect one of the most difficult topics in philosophy of science—the study of systematic error (and of instruments in causing it). And she does so by exploring the friction between the differing approaches to the sources of error in microscopic observations within a couple of generations in a key period of the development of microscopy and working outward from there.

In this respect, her paper and Janssen's are at opposite poles. He argues that leading protagonists in the debates over Copernican heliocentrism, Darwinian evolutionary biology, and special and general relativity make strikingly similar use of a kind of argument whose role in major theory transitions is not widely appreciated by philosophers—common origin inferences (*COIs*).<sup>1</sup> Further, he argues that the principals in these debates, in spite of the enormous conceptual and temporal differences between them, explicitly consider *COI* arguments as epistemologically decisive. As he is the first to acknowledge, a great deal of filling in is needed to make a full case for these claims. He needs to consider in detail the rhetorical roles played by the arguments he cites, to argue that the various figures whose positions he sketches did not actually deploy quite different concepts in spite of superficial similarities in their formulations, and much more along these lines. But this is only to say that important work is still needed on both historical and philosophical components of his argument—and that

1. *COI* inferences fit in a larger tradition that sees *unification* as a key value of explanation. In this tradition, a key explanatory virtue is reduction of the total number of *independent* phenomena requiring explanation. This goal can be reached by consilience of inductions (Whewell), explanatory unification à la Friedman or Kitcher (e.g., Friedman 1974; Kitcher 1981), and common cause explanations (e.g., Salmon 1984).

this will require the sort of cross-disciplinary collaboration that all of the papers in this issue consider necessary. Indeed, part of the appeal of his position is its forward engagement—a point in favor of the very concepts he deploys. In philosophy of science, his use (this issue, p. 489) of “striking” *MCOIs* (higher order common origin inferences, exemplified by Whewell’s consilience of induction, that call for explanation even if the first attempts at locating a common source or origin fail) illustrates this forward engagement. Against Whewell, he claims that a *MCOI* (e.g., a consilience of inductions) does *not* establish that the proffered explanation is correct. Rather, it establishes a burden on future explanations—they must provide a common source, origin, or structure to explain why the phenomena are so strikingly linked together (or explain away the apparent linkage?—RB). Again, as Janssen’s examples show (see also the discussion of Steinle above), the “phenomena” can be highly “theory-laden.”

I have a reservation touching on many of Janssen’s conclusions. Consider, for example, his conclusion 5, that “*COIs* capture an element of rationality in theory choice across disciplines, periods, and locales.” This claim can be interpreted innocuously: in the absence of countervailing considerations, *COIs* influence (and should influence) theory choice. But this is too weak for Janssen’s purposes. It does not clarify when and whether *COIs* will be recognized by historical agents, nor the different strengths that are (or should be) attributed to *COIs* as opposed to other considerations in different contexts. Clearly, Janssen’s claim is meant to be far more adventurous. Other things being equal, in all domains or periods, *COI*-inferences trump other considerations. On this strong reading, I believe his claim is false. For instance, it does not apply to all domains, e.g., not to certain parts of contemporary molecular biology. In that domain, as we have recently learned, certain key steps in embryological development are safeguarded against disruption by a kind of double or triple assurance (e.g., Tautz 1992 or Morange 2000). Background considerations make this plausible by reference to the sorts of disruption that organisms standardly encounter and must survive if they are to reproduce. The evidence is quite detailed—and can be illustrated by the failure of many gene knockout experiments to affect the development of the test organisms. These experiments were designed to disrupt major, well-understood steps in embryonic development. In many cases, the experimenters blocked a key step in a pathway standardly required to achieve a key developmental result, but an alternative pathway (often evolutionarily older) was activated that achieved the same effect via another route. This sort of built-in redundancy is common in organisms. Because evolution is a tinkerer and because organisms are tinkered together out of partly interchangeable

parts,<sup>2</sup> alternative causal pathways that achieve the same results by very different means are often available, both within a single organism and in different organisms. This sort of tinkering has the *prima facie* consequence that co-opted devices, with a common origin, may *seem* to explain common “downstream” effects, when, in fact, those effects are accessible by multiple pathways, pathways with different origins. The point is *not* against the power of common cause or common origin arguments in biology, but that Janssen does not make it clear how strong a conclusion to draw about the place and the reach of common origin and common cause arguments. This problem should be added to the budget of questions that require mixed historical and philosophical investigation.

Gal’s paper illustrates another philosophical agenda that depends on rich interaction with historical work. Gal’s focus is not on epistemology or meta-epistemology, but on ontology, specifically the alleged historicity of scientific objects. His position is, in a way, experimental, built on historical analysis of both science and recent philosophy and sociology of science. He emphasizes the contingency of the outcome of scientific controversy<sup>3</sup> and explores philosophical positions that seek to mesh contingency with various stances in the ongoing realist – anti-realist dialectic. He puts forward, tentatively, a form of historical realism drawn from Latour’s recent work. According to Latour, the objects of science, “*the real things*[...] are hybrids of natural law and social order” (this issue, p. 544). The argument turns on Latour’s principle of “super-symmetry” (Gal’s label): the objective outcomes of experiment on the one hand, and socially constructed scientific concepts and practices on the other, *co-determine* which scientific objects can possibly count as—or be (!)—real.

This twist on realism is distinctive: like science itself, scientific objects

2. This theme is developed in my unpublished MS, “Reconceiving animals and their evolution.” Among the striking findings of recent developmental biology I mention two: (1) the extent to which component mechanisms and materials are shared by a great diversity of animals (e.g., in governing the process of segmentation in organisms as diverse as annelid worms, frogs, fish, fruit flies, and mammals), and (2) the extent to which those very same component mechanisms and materials are recycled and reused in other pathways in development.

3. The recognition of contingency, itself, reflects historical and philosophical work—an historical argument that the shifting ontologies of science do not form a convergent series, and the fallibilist argument that all claims about the world are contingent and *could* have turned out otherwise if we, or the world, had behaved differently. Gal’s historical analysis of social constructivism adds a twist, according to which the “social” and the “constructivist” sides of that position turn out to be in conflict. According to Gal, the *contingency* of the constructivist stance is even more important to (most?) social constructivists than the addition of *social* to constructivism.

are historical entities. Experimental outcomes are not in our control, they are contingent on the behavior of the objects of science. But the objects change (or are exchanged?) along with the concepts and practices of science. Before Pasteur, in the absence of appropriate concepts or means to recognize germs, *there were no germs*. Counterintuitively, in the middle of the dispute over spontaneous generation, among the “real things” available to Pouchet were eggs, and germs to Pasteur. Beneath this surface, there lurks a quasi-Kantian question: how is it possible for there to be an objective science? Gal offers a quasi-Kantian answer on Latour’s behalf: the condition of the possibility of an objective science is that the objects and phenomena of science fall under the (pro-tempore) concepts of science. The concepts of science *are* pro-tempore; accordingly, the objects of science are historical entities whose existence is bounded by the concepts that make that existence possible. Yet the objects of science behave autonomously, which means that their behaviors enter into the contingent resolutions of scientific controversies.

This thoroughgoing historicity has several peculiarities, but Gal puts it forward for evaluation in a more-or-less scientific spirit: here, try this out, in spite of its counterintuitive features it may do better than anything else available in (re)constructing the ways in which the interactions between scientific objects and the concepts and practices of scientists can be understood. Gal’s presentation is “experimental” (my label) in that evaluation of this stance requires working out, e.g., how such a philosophy would handle disputes like those between realists and social constructivists and the one between Shapiro and Schaffer on the controversy between the Liège Jesuits and Newton. It also requires (as Gal acknowledges) some straightforwardly metaphysical work; it must accommodate the intertwining of subject and object and the temporal limitations it places on all scientific objects.

This Gal-Latour position helpfully raises a number of crucial questions. However, I do not think that it can accomplish the tasks it sets for itself. As I understand Gal’s account, super-symmetry claims that we can tell the story of the Copernican revolution (or electric fluids, microscopy, or germs) only from the standpoint of things as we know them, i.e., the standpoint of current science. I disagree. First, this claim underestimates the art of the historian, which can immerse us in perspectives other than our own. Second, it underestimates the pluralism of current science. As told from the perspective of different disciplines, the scientific topics covered in this issue will yield different histories. Science has no single standpoint, and we are quite practiced at using multiple perspectives to gain a better understanding of scientific objects. Third, the disunity of science

and scientific disciplines means that we are always in the middle of debates,<sup>4</sup> always faced with open questions about how best to interpret the ontology of the domains under examination. But we also have an enormous panoply of resources to enforce contact with the objects and processes about which we debate. We “triangulate” on them by means of different instruments, tools, theories, devices for localizing them, means for intervening or interacting with them, etc. This gives the objects about which we debate—epistemic objects (following Hans-Jörg Rheinberger), if you will—more than local identities. For that reason, Gal’s version of Latourian super-symmetry impoverishes us by dispensing with essential tools. Objects have their histories, and it is very important to recognize that they too are caught in secular historical change. But those histories are not coterminous with our knowledge of them; indeed, this is part of the autonomy of objects on which Gal’s and Latour’s arguments are based. If there are tuberculosis bacilli at all (and I remain supremely confident that there are!), they infected not only the people who lived after Pasteur, but also Egyptian nobles in the thirteenth dynasty (which we learned from forensic studies of mummies). Latour’s arguments notwithstanding, this is not a projection of our concepts on the past, but the application of a philosophy that can distinguish objects from knowledge of those objects. A realism that bars this distinction is no realism at all.

Each of the papers in this issue needs to intertwine history of science and philosophy of science to push its argument home. For all the differences in the methods and positions of the authors, the issues they open up cannot be resolved without recourse to a combination of historical and philosophical work. The papers (and the symposium from which they descend) will have succeeded if you, dear reader, join in the arguments that they open up.

### References

- Burian, R. M. 2000. “On the internal dynamics of Mendelian genetics.” *Comptes rendus de l’Académie des Sciences, Paris. Serie III, Sciences de la Vie / Life Sciences*, 323:1127–1137.

4. Gal emphasizes the need to start in the middle of things—i.e., in the world as currently understood. But this does *not* provide a unique perspective, nor unique constraints on what scientific objects can be. More to the point, it leaves open the ontological categories to which the scientific objects belong and the devices for indefinite reference to those objects. For example, if one follows the concept of the gene over a century (see e.g., Burian 2000; Falk 1986; Fogle 2000), one finds genes placed in very different categories and yet particular genes are adequately specified to allow for testing.

- . 2001. "The dilemma of case studies resolved: The virtues of using case studies in the history and philosophy of science." *Perspectives on Science*, 9:383–404.
- Falk, R. 1986. "What is a gene?" *Studies in History and Philosophy of Science*, 17:133–173.
- Fogle, T. 2000. "The dissolution of protein coding genes in molecular biology." Pp. 3–25 in *The Concept of the Gene in Development and Evolution: Historical and Epistemological Perspectives*. Edited by P. Beurton, R. Falk, and H.-J. Rheinberger. Cambridge and New York: Cambridge University Press.
- Friedman, M. 1974. "Explanation and scientific understanding." *Journal of Philosophy*, 71:5–19.
- Kitcher, P. 1981. "Explanatory unification." *Philosophy of Science*, 48:507–531.
- Morange, M. 2000. "Gene function." *Comptes rendus de l'Académie des Sciences. Serie III, Sciences de la Vie*, 323:1147–1153.
- Rheinberger, H.-J. 1997. *Towards a History of Epistemic Things: Synthesizing Proteins in the Test Tube*. Edited by T. Lenoir and H. U. Gumbrecht, *Writing Science*. Stanford: Stanford University Press.
- Salmon, W. 1984. *Scientific Explanation and the Causal Structure of the World*. Princeton: Princeton University Press.
- Tautz, D. 1992. "Redundancies, development and the flow of information." *Bioessays*, 14:263–266.