

Commentary 03  
Causes and contingencies in the history of science:  
a plea for a pluralist historiography

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Looking back on the historiographical ruminations of Sam Lilley and Clifford Truesdell, one is struck by the continuing relevance to our profession of the issues they were grappling with. The nature of explanation in the history of science and the requisite skills and 'tools' of historians of science are still up for grabs, even if the categories (causes-effects, external-internal) in which Lilley and Truesdell framed those issues may sound hopelessly dated, relics of a bygone historiographical age. A striking feature of Truesdell's piece is his impassioned advocacy of connoisseurship as a prerequisite for good 'scholarship'. The historian of science is expected to be intimately familiar with a daunting variety of subjects, from the nitty-gritty of hydrodynamics to the canon of classical literature. The unrealistic demands Truesdell makes on the prospective scholar could very well drive him or her 'into a madhouse'. As an ideal, however, erudition retains its appeal. Witness the following passage by an eminent contemporary historian:

The historian needs . . . discernment, an ability to identify the extraordinary, whether good or bad, in our terms and theirs, in the writings, artifacts, and actions of the past. Cultivating this discernment is the work of a lifetime. It requires not only studying the material of immediate interest, say the scientific papers of some heroes, but also reading in general history and the literature of the age, listening to its music, looking at its art and architecture. In a word, an historian should be a connoisseur. (Heilbron, 2007, p. 75)

The similarity with Truesdell's exhortations to connoisseurship is striking. As Jutta Schickore and Hasok Chang point out, though, it is

not at all clear that Truesdell practiced what he preached. In his essay there are indications that he did not. He shows an ambivalent attitude towards the context of scientific practice. Notwithstanding that he calls for a total immersion in the past, in all its complexity, he relegates the contextual aspects of the scientific enterprise to 'ancillary social sidelights' (Truesdell, 1973, see p. 22 of this issue).

He shows a similar ambivalence towards anachronisms. On the one hand, he seems to be sensitive to the fact that the meanings of scientific terms change over time. On the other hand, however, he insists on the necessity of translating past scientific texts into a modern idiom (see p. 25 of this issue). His unregenerate Whiggism offends our historiographical sensibilities: 'Our scholar must select what is science; within science he must select what is permanent; within what is permanent he must select what is true'. (see p. 28 of this issue) Even though he goes on to qualify this statement, it is clear that an emphasis on the 'permanent' and the 'true' is central to his historiography. This attitude is common among scientists with an interest in history, who think that the 'history of science is . . . distinguished from political or artistic history . . . in that the achievements of science become permanent' (Weinberg, 2005, p. 39). Historians of science, on the other hand, have long realized that a preoccupation with the 'permanent' and the 'true' is a serious obstacle to historical understanding.

Turning to Lilley's piece, one is still impressed by his plea for striking a balance between internal and external explanations of scientific activity. Internalism and externalism are seen by Lilley as two complementary causal explanatory strategies. The relative explanatory weight that one should attach to external or

internal factors depends on the specifics of the particular case. For instance, the development of astronomy in the 17<sup>th</sup> century was motivated, according to Lilley, by the needs of navigation, whereas the discovery of Neptune in the middle of the 19<sup>th</sup> century could be explained fully in terms of the internal dynamic of astronomy. As he puts it, ‘the causal mechanism leading up to Adams’ and Leverrier’s discovery will be found within science itself—chiefly in the mathematical tools which their predecessors had created. In such cases, the historian looking for causes and influences, would merely expose himself to ridicule if he devoted more than a small fraction of his efforts to examining the social environment of the mid-19th century’ (Lilley, 1953, see p. 8 of this issue).

Two issues here have to be disentangled and commented upon. The first is Lilley’s conflation of historical interpretation with causal explanation. The demand for causal explanations is not peculiar to Lilley’s historiography. Several prominent historians and sociologists of science have insisted upon causal accounts of belief formation. Paul Forman, for instance, in his classic essay on Weimar culture and quantum mechanics suggested that ‘the historian . . . must insist upon a causal analysis, showing the circumstances under which, and the interactions through which, scientific men are swept up by intellectual currents’ (Forman, 1971, p. 3). A few years later, David Bloor elevated the demand for causal explanations of scientific beliefs to a central tenet of his ‘Strong Programme’. More recently, Steven Shapin has argued that it would be important to specify and defend ‘the modes of causative action presupposed by’ internalist and externalist accounts of scientific change (Shapin, 1992, p. 348).

I have to admit straightaway that causal talk in the context of the history of science makes me uneasy. What do we mean by causality in this case? Is it possible to explain, in causal terms, how the content of scientific knowledge is formed? The claim that a belief was caused by intellectual, social, or cultural factors implies that the belief in question was determined by those factors. Given those factors, that belief was bound to emerge and be established. However, this is rarely, if ever, the case. Rather the factors

in question function as enabling conditions, as resources and constraints that are brought to bear on the problem situation faced by the historical actors. Thus, I would frame the problem of historical explanation in terms of resources and constraints. On the one hand, the availability of certain resources provides opportunities for action and enables the emergence and consolidation of certain beliefs. On the other hand, constraints set the limits within which scientific practice is played out. Within the bounds provided by the available resources and constraints, scientific development may follow alternative paths (cf. Wise 2007).

Notwithstanding his fascination with causality, in certain places Lilley waters down his demand for causal explanations and talks, instead, about enabling conditions—conditions that make a development possible. For instance, after a review of the debate about the relative importance of the scholar and the craftsman for the scientific revolution he concludes: ‘Granted that only in the 17<sup>th</sup> century would social conditions allow the consummation of the marriage between scholar and craftsman’ (Lilley, 1953, see p. 14 of this issue). This sensible statement points the way to an adequate conceptualization of explanation in the history of science: for explanatory purposes one has to understand how certain conditions made a development possible.

The second issue that is still worth thinking about is the internalism-externalism distinction. Presumably our discipline has reached a stage where this distinction has been put to rest. Charles Gillispie encapsulated a widespread dismissive attitude towards the distinction in question, by characterizing it as the origin of a ‘passing schizophrenia’ (Gillispie, 1991, p. 97; cf. Shapin, 1992, p. 345). However, as Shapin has convincingly argued, there are many unresolved issues concerning the internalism-externalism debate. What is at stake in that debate, according to Shapin, is nothing less than a full-blown account of scientific change: ‘[T]he most coherent way to formulate’ externalism and internalism is by viewing them ‘as [different] *theories of scientific change*’ (Shapin, 1992, p. 346). This construal of the debate helps us to understand and appreciate Lilley’s project, which was nothing less than ‘understanding

the causal mechanism that lies behind scientific development' (Lilley, 1953, see p. 16 of this issue). In practice most historians adopt an eclectic stance, very much in the spirit of Lilley's, and attempt to synthesize internal and external factors within an integrated analysis (or narrative). This eclecticism, however, 'cannot help resolve . . . a debate about the validity of the theories' (Shapin, 1992, p. 346). Perhaps none of those theories, as they stand, is valid. A satisfactory account of scientific change would have to provide, I think, an integrated framework, where each of those factors would find a place. Be that as it may, the internal-external distinction is not easily dispensable. Under various guises (e.g. as a distinction between the content and the context of scientific knowledge), it continues to affect historiographical practice.

A final remark about Lilley: he points out that external factors introduce an element of contingency in the development of science. The lines of research that are deemed worthy of pursuit are often selected on the basis of contingent social factors. An eloquent expression of the contingency that Lilley had in mind has been given by Richard Rorty:

We need to see the constellations of causal forces which produced talk of DNA or of the Big Bang as of a piece with the causal forces which produced talk of "secularization" or of "late capitalism." These various constellations are the random factors which have made some things subjects of conversation for us and others not, have made some projects and not others possible and important." (Rorty, 1989, pp. 16-17)

Rorty attributes the contingent character of scientific development to various 'causal forces', which 'produce' certain kinds of discourse and focus the resources of the scientific community on the investigation of certain issues, at the expense of others. Thus, causality and contingency are two sides of the same coin. In this respect, the development of science is supposed to be like every other historical development.

The contingency highlighted by Lilley and Rorty may be crucial from a historiographical (and a political) point of view. From an epistemological perspective, however, it is inno-

uous, since it does not undermine the validity of the results established by scientific research. In this respect, Lilley's and Rorty's 'contingentism' is less radical than certain other versions of that thesis. In the version defended by Mario Biagioli, for instance, 'local contingencies' are supposed to enter into the formation, establishment, and rejection of scientific beliefs (Biagioli 1996, p. 198). The available evidence and the accepted methodological prescriptions do not suffice to determine the form and content of scientific knowledge. Rather, evidential and methodological constraints permit a range of beliefs, and the epistemic gap between evidence and belief is closed as a result of contingent historical and social circumstances.

The attribution of a crucial epistemic role to historical contingency has also been a hallmark of *Science Studies*. Harry Collins and Andy Pickering, for example, have claimed that our knowledge would have been different if our contingent needs and interests had been different.<sup>1</sup> Thus, they tie contingency with counterfactual claims about the development of science.<sup>2</sup> On this particular issue, historians and sociologists of science have parted company with scientists, who have strong inevitabilist intuitions about scientific development. Steven Weinberg, for instance, thinks that '[a]s developed by Einstein in 1915, general relativity appears almost logically inevitable' (Weinberg, 2005, p. 33).

The dispute between inevitabilists and contingentists is essentially a philosophical one, whose core is the realism issue (cf. Hacking, 1999). Inevitabilists tie their thesis to a realist, teleological intuition: Science aims at an accurate representation of the entities, processes, and laws of nature, which admit of a unique true description, a 'fixed point' toward which science marches inexorably (Weinberg, 2005, p. 39). Contingentists, on the other hand, are anti-realists, who drive a wedge between the instrumental achievements of science and its putative closeness to the way nature actually is.<sup>3</sup>

This is not the place to attempt a philosophical evaluation of the contingency thesis. Even though I think there are difficulties in that thesis, whose scope thereby has to be reduced,<sup>4</sup> I cannot deny its fruitfulness from a historiographical point of view. Detailed empirical studies

by sober historians and philosophers of science have showed that historical contingency has been operative in crucial episodes of the history of science. For example, Jim Cushing, following in Forman's footsteps, has attributed the dominance of the Copenhagen interpretation of quantum mechanics among physicists to historical contingency (Cushing, 1994). More recently, Gregory Radick has suggested that there was nothing inevitable about the 'discovery' of genes and explored alternative paths that biology might have taken (Radick, 2005).

These studies suggest, I think, that we cannot settle in an *a priori* fashion which kinds of factors are relevant to understanding a past scientific episode.<sup>5</sup> The kind of understanding that we aim at depends on the questions that we are interested in answering. And those questions, in turn, delineate the kinds of sources that we have to explore and determine the skills and 'tools' that we need. Pace Truesdell, there is no unique set of tools that should be part of every aspiring historian's training. This is one of the points in Jutta Schickore's essay worth taking home.

This plea for a pluralist historiography is in tune with the current state of our discipline.<sup>6</sup> History of science has reached a stage where there is no consensus about the kinds of questions that have to be asked. A comparative glance at the kinds of histories published in the *Archive for History of Exact Sciences* and in *Isis* would suffice to make my case. However, I do not deplore this state of affairs. A plurality of historiographical perspectives can be an asset, making possible the exploration of various complementary aspects of the scientific past.<sup>7</sup> On the other hand, there is widespread consensus in our discipline that all those different approaches should share a common methodological commitment: a sensitivity to the historical actors' categories.

Let me close with a remark on one strand in this pluralist historiography that is close to my heart: integrated history and philosophy of science. My brief comments on contingency indicate that (implicit or explicit) philosophical positions about the nature, scope, and aim of science enter historiographical practice. If our aim is to understand historically scientific knowledge, as some historians still insist (Darrigol 2007), our views on its sources and validity

will inevitably affect our historical accounts of how it was produced and legitimated. Furthermore, philosophical questions may play a heuristic historiographical function. Questions about measurement and scientific progress, conceptual change and the ontology of science, or the nature of scientific observation have motivated and guided recent historical work.<sup>8</sup> These issues are important, even if philosophy of science, as it stands, may not always be helpful in addressing them.

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### NOTES

1. Note, however, that they disagree about the extent to which those contingencies are 'structured' or essentially random. See Pickering (1987).
2. The links between historical contingency and counterfactual history are explored in an illuminating way in Ben-Menahem (1997).
3. Gregory Radick has denied that there are necessary conceptual connections between inevitabilism and realism, on the one hand, and contingentism and antirealism, on the other (Radick, 2005). It remains the case, however, that, as a matter of fact, those connections are present.
4. See Arabatzis (2008), p. 165. Cf. also Hacking (2000).
5. By the way, this is not all that different from what Lilley was arguing 55 years ago.

6. For an eloquent 'plea for tolerance and pluralism in historiographical methods and approaches', see Fox (2006). The quote is from p. 410.
7. Hasok Chang advocates a related, though not identical, kind of historiographical pluralism. See Chang, unpublished manuscript.
8. See, respectively, Chang, 2004; Andersen, Barker, and Chen, 2006; Arabatzis, 2006 and Schickore, 2007.

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