

Reflexivity, Relativism, Microhistory: Three Desiderata for Historical Epistemologies

Author(s): Martin Kusch

Source: *Erkenntnis* (1975-), Vol. 75, No. 3, WHAT (GOOD) IS HISTORICAL EPISTEMOLOGY? (November 2011), pp. 483-494

Published by: Springer

Stable URL: <http://www.jstor.org/stable/41476735>

Accessed: 14-10-2016 08:41 UTC

JSTOR is a not-for-profit service that helps scholars, researchers, and students discover, use, and build upon a wide range of content in a trusted digital archive. We use information technology and tools to increase productivity and facilitate new forms of scholarship. For more information about JSTOR, please contact support@jstor.org.

Your use of the JSTOR archive indicates your acceptance of the Terms & Conditions of Use, available at <http://about.jstor.org/terms>



Springer is collaborating with JSTOR to digitize, preserve and extend access to *Erkenntnis* (1975-)

Reflexivity, Relativism, Microhistory: Three Desiderata for Historical Epistemologies

Martin Kusch

Received: 27 September 2011 / Accepted: 27 September 2011 / Published online: 14 October 2011
© Springer Science+Business Media B.V. 2011

Abstract This paper tries to motivate three desiderata for historical epistemologies: (a) that they should be reflective about the pedigree of their conceptual apparatus; (b) that they must face up to the potentially relativistic consequences of their historicism; and (c) that they must not forget the hard-won lessons of microhistory (i.e. historical events must be explained causally; historical events must not be artificially divided into internal/intellectual and external/social “factors” or “levels”; and constructed series of homogenous events must not be treated as quasi-organisms). Ian Hacking’s work on styles of reasoning and Lorraine Daston’s and Peter Galison’s investigation into epistemic virtues are used to identify the costs of neglecting these desiderata.

1 Introduction

In this paper I shall develop three criticisms of Ian Hacking’s work on “styles of reasoning”. It is because of this work that Hacking is sometimes regarded as a key figure in “historical epistemology”. For instance, in 1994 Lorraine Daston called Hacking “one of the ... most able practitioners of historical epistemology”, and she did so with reference to one of his early papers on styles (1994, p. 283). Daston had, by that time, already adopted the term “historical epistemology” to characterise her own way of writing the history of science. Hacking himself has not only preferred the term “philosophical anthropology” but has gone on to propose “historical meta-epistemology” as a more adequate term for Daston’s research (1999, p. 54; cf. 2005/2006). I am with Daston: there is enough resemblance between Hacking’s investigations into styles and Daston’s historical studies of, say, epistemic virtues, to justify using the same label for both projects—and “historical epistemology”

M. Kusch (✉)

Department of Philosophy, University of Vienna, Universitätsstraße 7, 1010 Wien, Austria
e-mail: martin.kusch@univie.ac.at

captures the common ground well enough. Alas, it is precisely because of the proximity of Daston's to Hacking's work that, in what follows, I can make her and Peter Galison's recent joint work a secondary target of the criticisms that I shall develop of Hacking's historiography (Daston and Galison 2007). My positive overall goal in developing these critical perspectives is to make plausible three desiderata for historical epistemology and related historicist projects.

2 Hacking on Styles of Reasoning

I begin with a very brief summary of what Hacking calls "philosophical anthropology". The central inspiration for Hacking here was Alisdair Crombie's list of six "styles of scientific thinking": "(a) the simple postulation established in the mathematical sciences, (b) the experimental exploration and measurement of more complex observable relations, (c) the hypothetical construction of analogical models, (d) the ordering of variety by comparison and taxonomy, (e) the statistical analysis of regularities of populations and the calculus of probabilities, and (f) the historical derivation of genetic development" (Crombie 1981, p. 284; Hacking 1982/2002, p. 161). Crombie assumes that most of these styles have their origins in antiquity.

Hacking treats Crombie's ideas with great deference. He suggests "treat[ing] Crombie's list as given" and "canonical"—small modifications notwithstanding. Indeed, Hacking dismisses the question "why start with Crombie?" as "not right", and calls on his readers to focus instead on "what I can do with [his] list [of styles]" (2005/2006, L. 2, p. 4). Hacking praises Crombie for having shown us a middle way between writing about "the fleeting" and investigating the "quasi-timeless" (1992b/2002, p. 187). He admits that Crombie's emphasis on continuity does not fit well with his own allegedly "revolutionary temperament" but regards their differences on this score as easily negotiable (2005/2006, L. 3, p. 10).

Hacking makes a number of general observations concerning styles of reasoning. First, each style comes with its own objects, forms of evidence, sentences, laws, possibilities, ontological debates and self-stabilising techniques (1992b/2002, pp. 189–190). Self-stabilizing techniques are conventional ways of decision-making within a given style (1992c). Second, styles of reasoning are "self-authenticating": we become convinced that a style gets at the truth only by using that very style itself (1992a/2002, p. 191). Third, impressed by Scott Atran's work on innate principles of classification (Atran 1998), Hacking has more recently come to emphasise that styles of reasoning are "built on fundamental cognitive capacities"; and that "each [style] demands many ... modules of different types" (2007, pp. 6, 12, 16). Fourth, while every style is always born in "microsocial interactions and negotiations", it eventually becomes "autonomous", "independent of its own history", "a rather timeless canon of objectivity", and a "neutral tool" (1992b/2002: 188; 1999: 132–133). And yet, a style can survive in the long run only if it is anchored in social institutions. Thus the conceptual study of styles of reasoning needs to be complemented by "an external history of science" (2005/2006, L. 5, p. 7). Fifth, Hacking allows for styles that were not on Crombie's original list. The most

important of these newcomers is the “laboratory style”; it combines Crombie’s experimental and model-building styles (1992b/2002, p. 184). Hacking also talks of Paracelsian medicine and the Inquisition as styles of reasoning though he denies that dialectical materialism, psychoanalysis, moral reflection or the humanities qualify as well. Dialectical materialism and psychoanalysis are too much based on “the method of authority of the father”, and moral thought and the humanities lack techniques of stabilisation. (1982/2002, p. 171; 2007, p. 6; 2005/2006, L. 9, p. 4; 1996, p. 74; cf. Allen 1993).

For reasons that will emerge later, it is worth registering that Hacking’s very first paper on styles began with the sentence “I wish to pose a relativist question from within the heartland of rationality” (1982/2002, p. 159). Back in 1982 Hacking was impressed by the relativistic ideas that scientific sentences are meaningful only within a given style, and that there is no style-independent point of view from which a style of reasoning can be judged to be rational or irrational. However, in his later writings Hacking dismisses any implications of relativism. I shall review his reasons later.

3 Hacking and the Reflexivity Point

I now turn from summary to critical discussion, structured around three desiderata for historical epistemologies. The first desideratum might be called “the Reflexivity Point”: in the historical investigation of her thematic concepts (i.e. the concepts under study), the historical epistemologist must not ignore the pedigree of her operative concepts (i.e. her conceptual tools) (cf. Fink 1957). Hacking violates this rule to a considerable degree. He reflects little on previous uses of “style”; all we get—aside from much praise for Crombie—are passing references to Wölfflin (who allegedly “introduced [the term] into art history”), to Fleck, Husserl, Spengler, Mannheim, and the Nazis’ talk of a “Jewish style of thought” (2007, p. 4; 1982/2002, p. 162; 1992b/2002, p. 179).

This is not good enough. Consider for example Carlo Ginzburg’s paper “Style as Inclusion, Style as Exclusion” (1998). In this paper Ginzburg studies the political role the concept of style has played in Western art history (and since long before Wölfflin). One of the many disturbing features of Ginzburg’s account is how—at least since Winckelmann—*style* was increasingly modelled on biological categories like species or organism (1998, p. 33). And it did not take long before biological and social categories coalesced around race. Indeed, at least since the time of Riegl, style was often conceptualised as coextensive with race (1998, p. 41). Thus when Hitler lamented in 1933: “It is a sign of the horrible spiritual decadence of the past epoch that one spoke of styles without recognising their racial determinants ...”, he had not done his homework on German art history: the alleged racial determinants of style had been the focus for decades (1998, pp. 43–44). Ginzburg’s paper concludes by pointing out how racism and art history met in some of Paul Feyerabend’s wartime lectures (1998, pp. 43–45).

Crombie had no truck with racism, but for him too the concept of style functions as a tool of exclusion. Amongst historians of science Crombie belonged firmly in the

“internalist” and “continuist” camp.¹ He denied that there was a scientific revolution in the sixteenth and seventeenth century and he fiercely attacked “Protestant propagandists”—Max Weber and Robert Merton—for suggesting that the philosophical or scientific thinking of the period might have owed something to external factors, such as Protestant theology (Crombie 1994, vol. I, p. 79). For Crombie—just like for Duhem—internalism and continuism were thus of a piece with his strong Catholicism.² This is certainly how some of his colleagues in Oxford saw things. The view was widespread enough to even figure prominently in Crombie’s obituaries. Some Oxford dons regarded Crombie’s history-of-science teaching as “a Catholic plot to infiltrate the curriculum” (North 2004). And one colleague remarked that “no recognition could have been more fitting” for Crombie than his election to the *Pontifical Academy of Sciences* in 1994 (Fox 1997, p. 185).

It seems to me that to deny the importance of these issues is to revoke what is most important and valuable about historical epistemology. I am not of course suggesting that Hacking is a racist or Catholic missionary in disguise. My point is rather that in borrowing terms uncritically, one easily adopts ideas that—on reflection—one should reject. As far as the link between style and biology is concerned, note that the organism metaphor continues to live on Hacking’s texts. He is comfortable with writing that styles “mature”, “march” and “harness [...] [their] own techniques of self-stabilisation” (1992b/2002, p. 188, 194; cf. 1992a, p. 133, 154). More importantly, I suspect that the organism metaphor—combined with a genetic rather than an ecological perspective—informs Hacking’s one-sided attention for the origins of styles to the detriment of questions concerning their persistence.

Crombie’s internalism and continuism also have echoes in Hacking’s thought. Contrary to what Hacking alleges, one cannot easily combine Crombie’s guiding concept of style with an emphasis on ruptures. Note for instance, that whenever Hacking switches from a continuist to a discontinuist idiom, he denies doing history and admits to writing “myths”. The purposes of “philosophical analysis”, he says, are best served by “myths” about ruptures. For instance: “Here then is my myth: the air-pump marks the beginning of a form of life, ... and the beginning of ... the laboratory style” (1992a, p. 141; 2005/2006, L. 1, p. 8; L. 5, p. 16; 2007, p. 2). Moreover, to choose another example, it sits badly with Hacking’s self-proclaimed “revolutionary temperament” that, according to his own confession, his analysis of the laboratory style does not apply to research “at the frontiers of inquiry ... even when it is what Kuhn called normal science. ...” (1992c, p. 37)

4 Daston and Galison and the Reflexivity Point

Daston’s and Galison’s historical epistemology also fails to fully meet the demand for reflexivity. Put in a nutshell, Objectivity offers a history of the emergence of four

¹ Cf. Crombie (1952, 1953, 1981, 1994). Relevant secondary literature includes Ariew (1995), Cohen (1994), Cunningham and Williams (1993), Iliffe (1998), Magruder (1995), Pumfrey (1997), Schuster (1990).

² For Duhem, see e.g. Kusch (1991, pp. 24–26).

epistemic virtues in Western science and philosophy since the beginning of the eighteenth century: (1) “truth-to-nature” (essences not appearances must be represented); (2) “mechanical objectivity” (appearances are to be represented as they are); (3) “structural objectivity” (only abstract structures are objective); and (4) “trained judgement” (using their tacit knowledge, scientists must be able to find patterns in series of appearances). Each of these virtues—that can be identified as “ways of seeing” or “disciplinary eyes” in scientific atlases—had its *raison d’être* in the characteristic fears of a period-specific subject.

The lack of reflexivity in Objectivity takes two forms. Here is the first. Daston and Galison never pause to reflect on the question which of their four epistemic virtues most guides their own efforts—and with what right. Of course little analysis is needed to realise that truth-to-nature is paramount: Daston and Galison seek to identify typical, characteristic instances of their four virtues. And they neglect the demands of a more “objectively” minded historiography, for instance by offering no statistical data on the 2,000 odd atlases that form their corpus. But why should truth-to-nature be the appropriate form of knowing historical facts in today’s history of science?

Moreover, like Hacking, Daston and Galison pay little attention to the pedigree of their central operative concepts: “epistemic virtue”, “way of seeing”, and “disciplinary eye”. “Epistemic virtue” is introduced as if the term had been coined for the purposes of their investigation. And yet, both the term and the idea of using it to bring together ethics and epistemology have been central in much recent philosophical literature, especially in Neo-Thomist quarters (Zagzebski 1996). The term has even played a major role in philosophical-historical attempts to analyse the “genealogy” of our epistemic concepts and ideas (Williams 2002). More importantly, the emphasis on virtue in both epistemology and ethics is part and parcel of a revolt against rule-centred, “mechanical” or “procedural” conceptions of knowledge and morality (MacIntyre 1981). And this in turn suggests that the concept of epistemic virtue is not neutral with respect to the different ways of seeing at issue in Objectivity. The concept is too closely aligned with the tradition of trained judgement with its anti-procedural preoccupations. Most advocates of (mechanical or structural) objectivity would have protested against having their ideal described in terms of virtue. Similar concerns arise with respect to “ways of seeing” or “disciplinary eye”. “Ways of seeing” recalls John Berger’s classic Benjamin-inspired analysis of images in modern culture (Berger 1972); and “disciplinary eye” associates with Michael Baxandall’s modern classic *Painting and Experience in Fifteenth-Century Italy* (1972). Both of these books represent important alternatives to Daston’s and Galison’s approach; for instance, Baxandall’s focus on what the audience brings to the viewing of a painting could have lead to a much more reception-focused analysis of atlases.

5 Hacking and the Relativism Point

An appropriate title of my second desideratum would be the “Relativism Point”: historical epistemology must face up to the potentially relativistic consequences of its historicism.

Crombie tried to have it both ways. At times he advocated an “explicit cultural relativism”, the view that “effective thinking could be based on assumptions and have aims and motivations ... various and different from our own” (1994, vol. I, p. 5). At other times, Crombie stressed that there is only one “objective scientific truth”; that “the basic rationality” of our “scientific and scholarly tradition” as well as scientific problems have remained “essentially stable” (1994, vol. I, pp. 7–9); that there can be translation across all cultures and styles; and that the “Western tradition” alone has achieved scientific rationality (1994, vol. I, p. 22).

When Hacking first wrote about styles of reasoning, he was happy to move his project in the proximity of relativism. But in more recent texts he vehemently denies that the existence of styles of reasoning invites relativistic conclusions. Here I shall take issue with the latter position. I shall focus on epistemic relativism. Epistemic relativism is the view that, first, there are “no absolute facts about what belief a particular item of information justifies”; second, claims about the justificatory status of a belief contain an explicit or implicit reference to varying systems of epistemic principles; and third, there “are many fundamentally different, genuinely alternative epistemic systems, but no facts by virtue of which one of these systems is more correct than any of the others” (Boghossian 2006, 73).

As far as I can see, Hacking offers six reasons in support of the conclusion that his historicism about reason does not induce relativism. In the interest of brevity, I shall have to dispose of his reasons somewhat briskly.

Reason One: Relativism involves the thought that one and the same proposition can be true in one framework and false in another. But it is impossible for one and the same proposition to have truth-conditions in more than one style of reasoning (1982/2002, p. 175). Reply: Relativism does not need to be restricted in this way. Some familiar views that are often labelled “relativistic”—e.g. Kuhn’s theory—also work with the thought that one and the same sentence might have truth conditions in one framework but be meaningless in others (cf. Williams 1981, 1985).

Reason Two: The frameworks of a relativist are incompatible. But styles of reasoning are compatible (1992b/2002, pp. 181–183; 1992a, p. 137). Reply: This claim has some initial plausibility as long as we focus only on Crombie’s original list, perhaps adding Hacking’s laboratory style. But the case seems much less clear once we allow further styles of reasoning, such as Paracelsian medicine or the Inquisition. Moreover, even if the styles of reasoning within the thought of a single individual or group were always compatible, issues of incompatibility might well arise between the sets of styles possessed by two (or more) different individuals or groups. If my set of styles includes genetic derivation while yours does not, we might well experience our belief systems as in part incompatible.

Reason Three: Styles of scientific reasoning are now ubiquitous (2005/2006, L. 8, p. 4). Reply: Mere ubiquity does not refute relativism. The relativity of taste is not refuted once everyone is brainwashed into liking Coca Cola.

Reason Four: Styles of reasoning are based on innate capacities (2005/2006, L. 8, p. 4). Reply: Innate capacities do not refute relativism. The epistemic (cultural)

relativist need only insist that our innate capacities underdetermine which epistemic standards and practices different communities adopt.

Reason Five: A style of reasoning is not relative to anything (1992a, p. 135, 155). Reply: Irrelevant! The question is whether epistemic judgements and principles are relative to different styles of reasoning and whether all styles of reasoning are compatible.

Reason Six: In his *Pascalian Meditations* Pierre Bourdieu shows convincingly that historicism does not entail relativism (Hacking 2007, p. 8; cf. Bourdieu 1997/2006). Reply: He does not. Bourdieu confines himself to showing that it is possible for science to be both a system of power and a truth-seeking enterprise. No sensible relativist denies this.

To sum up: Hacking fails in his attempt to separate his historicism from epistemic relativism.

6 Daston and Galison and the Relativism Point

Daston and Galison too are eager to present their overall project as a form of non-relativistic historicism: “It is a misconception, albeit an entrenched one, that historicism and relativism stride hand in hand, that to reveal that an idea or value has a history is ipso facto to debunk it” (2007, p. 376). Or: “Far from relativizing these virtues, history exhibits their rationale ... Truth-to-nature, mechanical objectivity, and trained judgement all combat genuine dangers to knowledge: the dangers of drowning in details, of burking a fact to support a theory, of being straitjacketed by mechanical procedures” (2007, pp. 376–377).

I am not convinced. First, relativism is not scepticism. Sceptics debunk, but relativists—for instance those in the sociology of knowledge—insist that they do not (Bloor 2007, p. 253, 2008, p. 15). More importantly, note that, in their efforts to cleanse their project of relativism, Daston and Galison in fact end up revoking their historicism. In their central chapters they seek to show—in line with an unflinching historicism—that the different epistemic virtues only make sense against the background of the fears of a period-specific subject. But when, on the final pages of their study, Daston and Galison seek to dissociate themselves from relativistic elements in the social history of science, period-specific fears turn into eternal dangers for knowledge überhaupt. History is thereby reduced to the role of a searchlight that—at different times—picks out different virtues in a Platonic epistemic sky or heaven. This is no longer historicism.

Finally it is a-historical to claim that historicism and relativism do not go hand-in-hand. A century ago, and thus very much in the period at issue in *Objectivity*, the relationship between historicism and relativism was extensively discussed between the likes of Dilthey, Heidegger, Husserl, Nietzsche, Rickert, Simmel and Windelband. Whatever emerged from this eventually abandoned debate, it certainly included the insight that the historicist can avoid relativism only either by positing a *telos* of historical development or by treating the views of different periods as components of one overall truth (cf. e.g. Beiser 2007). Neither option now seems particularly attractive—at least not without much further argument.

7 Hacking and the Microhistory Point

My third desideratum for historical epistemology is the “Microhistory Point”: do not forget the hard-won lessons of microhistory! Amongst these lessons are: historical events must be explained causally; historical events must not be artificially divided into internal/intellectual and external/social “factors” or “levels”; and constructed series of homogenous events must not be treated as quasi-organisms.

I have already indicated that Hacking violates the last-mentioned condition: he comfortably speaks of a style of reasoning as an entity that “matures”, “marches”, becomes “autonomous” and “independent of its own history”, and persists “in its peculiar and individual way, because it has harnessed its own techniques of self-stabilisation” (1992b/2002, p. 188, 194; cf. 1992a, p. 133, 154). More importantly, treating entities as quasi-organisms usually comes with a one-sided attention for their genesis or origins and a neglect concerning their persistence and ecology. This is exactly what one finds in Hacking. While he emphasises the importance of a detailed social history for understanding the emergence of a new style of reasoning, he implies that, once the style has been born, it somehow transcends the domain of social contingencies.

I find related failures to reckon with the Microhistory Point in some of Hacking’s discussions of his laboratory style. The first oddity is that among the fifteen elements of the laboratory style there are no actions or interactions, no beliefs, no intentions, no groups and no interests. Although Hacking calls experimenters and their interactions “the most important ingredient of an experiment”, out of a thirty-five-page paper, they receive exactly sixteen lines (1992c, p. 51). Hacking justifies leaving out the human and social element with the remark that he is offering “a taxonomy of elements *internal* to an experiment” (1992c, p. 51, my emphasis). This makes matters worse: not just social-political interests, but *all* human actions, beliefs and intentions, are counted as “external” to experiments and the laboratory style.

In some of his most recent writings—from around 2005 onwards—Hacking has begun to acknowledge the importance of social order even for “mature” styles. He now grants that a style can become stable only if it has a solid social-institutional basis. As an example Hacking points to Steve Shapin’s and Simon Schaffer’s *Leviathan and the Air-Pump* (1985) and explains that the laboratory style was stabilised because of the new scientific academies and novel laboratory spaces, introduced in the sixteenth century and maintained ever since (2005/2006, L. 5, p. 16). So far, so good. Alas, Hacking’s recognition of the Microhistory Point is partial only. This can be seen from the fact that he characterises *Leviathan and the Air-Pump* as “an external history of science” (2005/2006, L. 5, p. 7). Hacking first presents his own internalist analysis of the laboratory style and then complements it with the allegedly externalist picture of Shapin and Schaffer. And this brings back the tired old dualism of intellectual content and social realm. It presupposes that the development of the intellectual content—the questions to which it gives rise, the answers it demands—can be determined and understood independently of how it is debated and negotiated in local, contingent and social circumstances. And this is

precisely what microhistory—for instance in the sociology of scientific knowledge—has thrown into doubt over the past 25 years. For the sociology of scientific knowledge the “experimental form of life” is not the external complement to the internal laboratory style of reasoning. A style of reasoning is *eo ipso* a way of ordering and organising people. And thus agreeing on a style and agreeing on a social order are congruent if not identical processes.

8 Daston and Galison and the Microhistory Point

Daston’s and Galison explicitly reject the Microhistory Point. I can best explain their reasoning by resorting to a couple of concepts prevalent in the French *Annales* School of the 1970s (Chaunu 1970; Furet 1985). Daston and Galison “construct” four temporally extended “series” of “homogenous” events, each series consisting of atlases or programmatic statements that make up one of their four epistemic virtues. Daston and Galison are eager to understand the sequence of the four series as wholes, and thus they give scant attention to the sequence of events within each series. In accordance with this perspective, questions about causal explanation are only raised concerning whole series. “Big” social causes like the Industrial Revolution are rejected as unspecific and as allegedly committing us to “a reductive base-superstructure model” in which “one ‘foundational’ level” (e.g. “interests of a social class”) causes “an ‘overlaid’ level of a strikingly different kind” (e.g. epistemic virtues). The small “local” causes of microhistory again are dismissed as over-specific; they are unable to explain the series as a whole. Daston and Galison conclude that only a second series of “homogenous” events as a whole, a series that moreover is “homogenous” with the first series, can help with the task of explanation. (This second series is made up of instances of one of their four conceptions of subjectivity.) And since causality allegedly jars with the demand for homogeneity, the field is left for what Daston and Galison call “intrinsic [i.e. non-causal] explanation” (2007, p. 197, pp. 376–378).

I have three objections. The first is that the case for a general heterogeneity of cause and effect has not been made. Second, Daston and Galison’s argument against using the Industrial Revolution as a satisfactory explanans is based on the assumption that economic relations and political interests are heterogeneous with respect to epistemic virtues, and that to explain the latter in terms of the former must be to reduce the latter to the former. But that is simply to presuppose, without further reflection, the dualism of internal and external factors or levels.

Third, Daston’s and Galison’s argument for intrinsic explanation rests crucially on their disregarding the sequence of events within each of their four series. But why should we suspend our historical curiosity at this point? Why not ask, say, which local causes brought about the first atlases constructed on principles of mechanical objectivity? Why not investigate the local causes for why these atlases became influential? Why not inquire into the local causes that made specific audiences prefer mechanical objectivity? And so on. It is unlikely that the causes we identify in answer to these questions will have much in common; it is unlikely that

they will form a series of homogenous events. But then again, why should they? Similar effects can be due to very different causes; and similar causes can—depending on background conditions—have very different effects. This is, after all, one of the most important lessons of microhistory. Put differently, epistemic virtues do not call for a new type of explanation. Their microhistorical explanation is both possible and—in the interest of genuine historical understanding—desirable.

9 Conclusion

In this paper I have tried to motivate three desiderata for historical epistemologies: the Reflexivity Point, the Relativism Point, and the Microhistory Point. I have introduced the three points via negation by identifying the costs of neglecting them in Hacking's, Daston's and Galison's work. In conclusion I should say why I have focused on these authors rather than some other historical epistemologists. The reason is simply that outside of the sociology of scientific knowledge no-one has challenged and influenced my own thinking about the history of science as much as Hacking, Daston and Galison have. Thus it should not come as a surprise that I was unable to resist the temptation to work out at which precise point our ways do part—at least for now.

Acknowledgments I am indebted to Uljana Feest and Thomas Sturm for organising the conference that provided me with the challenge to write this paper. For comments, questions and suggestions I am grateful to Raine Daston, Sarah Gore-Cortes, Ian Hacking, Jeff Kochan, Simon Schaffer, Catherine Wilson, and two anonymous referees.

References

- Allen, B. (1993). Demonology, styles of reasoning, and truth. *International Journal of Moral and Social Studies*, 8, 95–121.
- Ariew, R. (1995). Review of Crombie, styles of scientific thinking in the European tradition. *Isis*, 86, 82–83.
- Atran, S. (1998). Folk biology and the anthropology of science: Cognitive universals and cultural particulars. *Behavioural and Brain Sciences*, 21, 547–609.
- Baxandall, M. (1972). *Painting and experience in fifteenth-century Italy*. Oxford: Oxford University Press.
- Beiser, F. (2007). Historicism. In B. Leiter & M. Rosen (Eds.), *Oxford handbook to continental philosophy* (pp. 155–179). Oxford: Oxford University Press.
- Berger, J. (1972). *Ways of seeing*. London: Penguin.
- Bloor, D. (2007). Epistemic grace: Antirelativism as theology in disguise. *Common Knowledge*, 13, 250–280.
- Bloor, D. (2008). Relativism at 30, 000 feet. In M. Mazzotti (Ed.), *Knowledge as social order* (pp. 13–32). Aldershot: Ashgate.
- Boghossian, P. (2006). *Fear of knowledge: Against relativism and constructivism*. Oxford: Oxford University Press.
- Bourdieu, P. (1997/2006). *Pascalian meditations*. Cambridge: Polity. First published in French in 1997.
- Chaunu, P. (1970). L'histoire sérielle: Bilan et perspectives. *Revue historique*, 494, 297–320.
- Cohen, H. F. (1994). *The scientific revolution: A historiographical inquiry*. Chicago & London: University of Chicago Press.
- Crombie, A. (1952). *Augustine to Galileo: The history of science, AD 400–1650*. London: Falcon.

- Crombie, A. (1953). *Robert Grosseteste and the origins of experimental science, 1100–1700*. Oxford: Clarendon Press.
- Crombie, A. C. (1981). Philosophical perspectives and shifting interpretations of Galileo. In J. Hintikka, D. Grunder, & E. Agazzi (Eds.), *Theory change, ancient axiomatics and Galileo's methodology* (pp. 271–286). Dordrecht: Reidel.
- Crombie, A. C. (1994). *Styles of scientific thinking in the European tradition* (Vol. 3). London: Duckworth.
- Cunningham, A., & Williams, P. (1993). De-centring the 'big picture': The origins of modern science and the modern origins of science. *British Journal for the History of Science*, 26, 407–432.
- Daston, L. (1994). Historical epistemology. In J. Chandler, A. Davidson, & H. D. Harootunian (Eds.), *Questions of evidence: Proof, practice, and persuasion across the disciplines* (pp. 282–289). Chicago & London: University of Chicago Press.
- Daston, L., & Galison, P. (2007). *Objectivity*. New York: Zone Books.
- Fink, E. (1957). Operative Begriffe in Husserls Phänomenologie. *Zeitschrift für philosophische Forschung*, 2, 321–337.
- Fox, R. (1997). Alistair Cameron Crombie, 4 November 1915–1919 February 1996. *Isis*, 88, 183–186.
- Furet, F. (1985). Quantitative methods in history. In J. Le Goff & P. Nora (Eds.), *Constructing the past: Essays in historical methodology* (pp. 12–27). Cambridge: Cambridge University Press.
- Ginzburg, C. (1998). Style as inclusion, style as exclusion. In C. A. Jones & P. Galison (Eds.), *Picturing science, producing art* (pp. 27–53). London: Routledge.
- Hacking, I. (1982/2002). Language, truth, and reason. In I. Hacking (Ed.), *Historical ontology* (pp. 159–177). Cambridge, Mass.: Harvard University Press. First published in 1982.
- Hacking, I. (1992a). Statistical language, statistical truth and statistical reason: The self-authentication of a style of scientific reasoning. In E. McMullin (Ed.), *The social dimension of science* (pp. 130–157). Notre Dame, IN: University of Notre Dame Press.
- Hacking, I. (1992b/2002). 'Style' for historians and philosophers. In I. Hacking (Ed.), *Historical ontology* (pp. 178–199). Cambridge, Mass.: Harvard University Press. First published in 1992.
- Hacking, I. (1992c). The Self-vindication of the laboratory sciences. In A. Pickering (Ed.), *Science as practice and culture* (pp. 29–64). Chicago, IL: University of Chicago Press.
- Hacking, I. (1996). The disunity of the sciences. In P. Galison & D. J. Stump (Eds.), *The disunity of science: Boundaries, contexts, and power* (pp. 37–74). Stanford, CA: Stanford University Press.
- Hacking, I. (1999). Historical meta-epistemology. In W. Carl, & L. Daston (Eds.), *Wahrheit und Geschichte* (pp. 53–77). Abhandlungen der Akademie der Wissenschaften in Göttingen. Göttingen: Vandenhoeck & Ruprecht.
- Hacking, I. (2002). Inaugural lecture: Chair of philosophy and history of scientific concepts at the Collège de France, 16 January 2001. *Economy and Society*, 31, 1–14.
- Hacking, I. (2002/2003). Résumé des cours. Retrieved August 9, 2007, from http://www.college-de-france.fr/media/ins_pro/UPL35835_ihackingres0203.pdf.
- Hacking, I. (2005/2006). Raison et véracité: les choses, les gens, la raison. Retrieved August 9, 2007, from http://www.college-de-france.fr/default/EN/all/ins_pro/p1157460409944.htm.
- Hacking, I. (2007). *Finding out: Prolegomena to a theory of truthfulness and reasoning in the sciences*. Unpublished manuscript.
- Iliffe, R. (1998). Rational artistry. Review of Crombie, styles of scientific thinking in the European tradition. *History of Science*, 36, 329–357.
- Kusch, M. (1991). *Foucault's strata and fields: A study in archaeological and genealogical science studies*. Dordrecht: Reidel.
- MacIntyre, A. (1981). *After virtue: A study in moral theory*. London: Duckworth.
- Magruder, K. (1995). Review of Crombie, styles of scientific thinking in the European tradition. *Sixteenth Century Journal*, 26, 406–410.
- North, D. (2004). *Crombie, Alistair Cameron (1915–1996)*. *Oxford dictionary of national biography*. Oxford: Oxford University Press. Retrieved August 7, 2007 from <http://www.oxforddnb.com/view/article/62017>.
- Pumfrey, S. (1997). The scientific revolution. In M. Bentley (Ed.), *Companion to historiography* (pp. 293–306). London: Routledge.
- Schuster, J. A. (1990). The scientific revolution. In R. C. Olby, G. N. Cantor, J. R. R. Christie, & M. J. S. Hodge (Eds.), *Companion to the history of modern science* (pp. 217–242). London: Routledge.
- Shapin, S., & Schaffer, S. (1985). *Leviathan and the air-pump: Hobbes, Boyle, and the experimental life*. Princeton: Princeton University Press.

- Williams, B. (1981). The truth in relativism. In B. Williams (Ed.), *Moral luck* (pp. 133–143). Cambridge: Cambridge University Press.
- Williams, B. (1985). *Ethics and the limits of philosophy*. London: Fontana.
- Williams, B. (2002). *Truth and truthfulness*. Princeton: Princeton University Press.
- Zagzebski, L. (1996). *Virtues of the mind: An inquiry into the nature of virtue and the ethical foundations of knowledge*. Cambridge: Cambridge University Press.