

# Subliming and Subverting

## *An Impasse on the Contingency of Scientific Rationality*

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### Abstract

What is special about the philosophy of history when the history is about science? I shall focus on an impasse between two perspectives – one seeking an ideal of rationality to guide scientific practices, and one stressing the contingency of the practices. They disagree on what this contingency means for scientific norms. Their impasse underlies some fractious relations within History and Philosophy of Science. Since the late 1960s, this interdisciplinary field has been described, variously, as an “intimate relationship or marriage of convenience”, a “marriage for the sake of reason”, a “troubling interaction”, a “precarious relationship”, or one at risk of “epistemological derangement”.

My paper has three aims. First, I characterise two idealised perspectives in this impasse: the sublimers and subversives. Then I describe a curious dynamic in which each perspective accuses the other of, simultaneously, claiming too little and too much. Subliming is dismissed for being scholastic, and condemned for being imperialist. Subverting is disparaged for being merely sociological, then criticised for being irrational or relativist. Here I draw on and extend the analyses of science studies in Kitcher (1998), Zammito (2011) and others. Second, I argue that sublimers and subversives often talk past each other. Their mutual dismissals and condemnations arise partly because each perspective misconstrues the other’s claims. Each fails to see that the other is interested in different problems and interprets concepts differently. Third, I suggest that my analysis clarifies some current disagreements and old debates about contingency in science. It may also apply to debates on the contingency of norms in non-scientific realms.

### Keywords

scientific method – scientific practice – rationality – contingency – relativism – history of science – philosophy of science – historiography of science

## 1 Introduction

What is special about the philosophy of history when the history is about science? I shall focus on an impasse over the nature of scientific rationality. The impasse is between two perspectives – one which seeks an ideal of rationality in scientific practices, and one which stresses the contingency of the practices. They disagree on what this contingency means for the norms of scientific rationality. From one perspective, the norms are prized because they result from a constant struggle with human limitations. From the other perspective, the norms appear as counterfeits, exposed for their false claim to transcend human conditions.

This impasse underlies some fractious relations within History and Philosophy of Science. Since the late 1960s, the field has been described, variously, as an “intimate relationship or marriage of convenience”, a “marriage for the sake of reason”, a “troubling interaction”, a “precarious relationship”, or one still at risk of “epistemological derangement”.<sup>1</sup> One source of tension is the methodological dilemma that confronts historians and philosophers of science. Either we overlook the contingency of scientific practices and focus on “the disembodied history of scientific ideas”; or we neglect scientific norms and settle for “a lobotomized history of scientific institutions”.<sup>2</sup> Of course, we

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- 1 These descriptions are from R. N. Giere, “History and Philosophy of Science: Intimate Relationship or Marriage of Convenience?”, *British Journal for the Philosophy of Science* 24, no. 3 (1973): 282–97; L. Krüger, “History and Philosophy of Science – a Marriage for the Sake of Reason”, in *Abstracts 6: 6th International Congress for Logic, Methodology and Philosophy of Science* (Hannover: Dr. Bonecke, 1979), 108–112; C. Pinnick and G. Gale, “Philosophy of Science and History of Science: A Troubling Interaction”, *Journal for General Philosophy of Science* 31, no. 1 (2000): 109–125; R. M. Burian, “Comments on the Precarious Relationship between History and Philosophy of Science”, *Perspectives on Science* 10, no. 4 (2002): 398–407; J. Rouse, “Epistemological Derangement”, *Studies in History and Philosophy of Science Part A* 36, no. 4 (2005): 835–847. I also learnt from the surveys in J. H. Zammito, *A Nice Derangement of Epistemes: Post-Positivism in the Study of Science from Quine to Latour* (Chicago: University of Chicago Press, 2004); E. McMullin, “Integrating History and Philosophy of Science”, *2007 & HPS Conference Paper, 2007*, URL=<http://philsci-archive.pitt.edu/3607/> (accessed 10 May 2013); J. Schickore, “More Thoughts on HPS: Another 20 Years Later”, *Perspectives on Science* 19, no. 4 (2011): 453–481; S. Mauskopf and T. Schmaltz (eds.), *Integrating History and Philosophy of Science: Problems and Prospects* (Dordrecht: Springer, 2012).
  - 2 These alternatives are described and criticised by L. Laudan, “The History of Science and the Philosophy of Science”, in R. C. Olby *et al.* (eds.), *Companion to the History of Modern Science* (London: Routledge, 1990), 47–59. He is quoted in D. M. Miller, “The History and Philosophy of Science History”, in Mauskopf and Schmaltz (eds.), *Integrating History and Philosophy of Science*, 40.

hear regular calls to integrate both approaches. Their persistence, however, suggests that we have yet to find a theoretically compelling way to bring rationality and contingency together.

I am a philosopher looking in, and learning from, this interdisciplinary field. When I attend seminars in history or sociology of science, I see how difficult it is for philosophers to talk to historians and sociologists. Each discipline has its own interests and concepts. In this paper, I shall use some of these divergences as tools for analysing the theoretical impasse on the contingency of scientific rationality.

First, I characterise two idealised perspectives in this impasse: the sublimers and subversives. I explain their different aims in studying science, and how these aims are related to their methods and results. Then I describe a curious dynamic in which each perspective accuses the other of, simultaneously, claiming too little and too much. Subliming is dismissed for being scholastic, and condemned for being imperialist. Subverting is disparaged for being merely sociological, then criticised for being irrational or relativist. Second, I argue that sublimers and subversives often talk past each other. Their mutual dismissals and condemnations arise partly because each perspective misconstrues the other's claims. Each fails to see, or forgets, that the other is interested in different problems and interprets concepts differently. Third, I suggest that my analysis clarifies some current disagreements and old debates about contingency in science. It may also apply to debates about the contingency of norms in non-scientific realms.

## 2 Impasse on Scientific Rationality

This impasse over the nature of scientific rationality is between sublimers and subversives. I use them as ideal intellectual types, akin to Isaiah Berlin's hedgehogs and foxes.

Of course, like all over-simple classifications of this type, the dichotomy becomes, if pressed, artificial, scholastic and ultimately absurd... like all distinctions which embody any degree of truth, it offers a point of view from which to look and compare, a starting-point for genuine investigation.<sup>3</sup>

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3 I. Berlin, "The Hedgehog and the Fox: An Essay on Tolstoy's View of History", in H. Hardy (ed.), *The Proper Study of Mankind* (New York: Farrar, Straus and Giroux, 2000), 437.

Each perspective is a blend of historiographical interests and philosophical commitments. Both present a simplistic contrast that should not be pressed too far, whether in classifying individuals or groups. An individual who engages with history or sociology of science may go through subliming and subversive phases. A group is likely to contain subliming and subversive elements, though one perspective may be dominant. Here is a sociological conjecture: currently, History and Philosophy of Science attracts more sublimers, while History of Science and Science and Technology Studies appeals to more subversives.<sup>4</sup>

My goal, however, is not to classify. It is to use the perspectives of subliming and subverting as a starting point – to identify, and then to interpret, a dynamic in debates about the norms of scientific rationality and the contingency of scientific practices.

### 2.1 *Aims and Methods*

So who are the sublimers? I borrow the term “subliming” from Wittgenstein, who speaks of the philosophical tendency to “sublime the logic of our language”.<sup>5</sup> The sublimers I am interested in study scientific practices, not linguistic ones. Their aim is to discover an ideal of scientific rationality. This ideal consists of norms that can guide scientific practices, such as framing hypotheses, conducting experiments, making observations, collecting data, constructing models and testing theories.

To deduce these norms, sublimers use *abstraction*. When they study successful scientific practices, they draw out a pattern of reasons from its background of causal conditions. These reasons are explicated in terms of scientific norms. The causal conditions may be social, cultural or economic in nature. From the subliming perspective, what matters is that they are not cited in reasons attributed to the scientists: this suggests that the conditions are not constitutive of the norms. They are as irrelevant as the scientists’ breakfast and bedtime. Once the relevant norms are identified, they can be tested to see how well they illuminate and improve other practices.

4 See Miller, “The History and Philosophy of Science History”, §3.2, for illuminating comparisons of these disciplines.

5 L. Wittgenstein, *Philosophical Investigations*, trans. G. E. M. Anscombe, 3rd Edition (London: Blackwell Publishing, 1953). Here is his example at §38: when we study how names refer to things, we look for the ideal structure that some names share. We thereby neglect other names, which have “many different kinds of use . . . related to one another in many different ways”. See also §89: “In what sense is logic something sublime? For there seemed to pertain to logic a peculiar depth – a universal significance. Logic lay, it seemed, at the bottom of all the sciences.”

Subversives, on the other hand, want to uncover the contingency of scientific practices. From their perspective, a practice is contingent if its meaning depends on some human conditions – broadly construed to include the accidents of human nature and history. This contingency, in turn, reflects the contingency of norms which guide the practice.

For instance, the meaning of many scientific norms depends on conditions such as the communicative ability and general trustworthiness of humans, and the particular interests distributed among groups of scientists. Without these conditions, the norms would lose their point. They would no longer be meaningful in guiding scientific behaviour. From the subversive perspective, we might say that these conditions enact the norms. Their discovery is subversive in at least one sense: it challenges any conception of scientific rationality that denies a role to such enactive conditions.

Subversives use the *contextualisation* of scientific norms. They interpret the social and historical conditions under which norms guide practices – often the very conditions cast aside as irrelevant from the subliming perspective. Which conditions count as enactive? These conditions are to be distinguished, however imperfectly, from enabling conditions such as the scientists' breakfast and bedtime.<sup>6</sup> Nutrition and sleep make it possible for scientists to use the norms. But they do not thereby contribute to the meaning of the norms.

To identify enactive conditions, subversives look for a slack in reasoning, where the scientists' reasons are insufficient to explain their behaviour in a practice. If the slack appears systematically, then the norms associated with those reasons cannot fully account for the practice. This suggests that enactive conditions may be at work. Subversives then look for a pattern of social or historical conditions that picks up the slack, helping to make sense of the scientists' behaviour. Once these enactive conditions are identified through interpretation, they can be tested to see how well they explain, or at least clarify, other practices.

A fuller study would include more details about abstraction and contextualisation; I elaborate some of them later. Here I have only sketched how the aims of subliming and subverting are related to their methods and results. Table 1 sums up their main differences.

In the rest of this paper, I want to focus on a persistent dynamic that arises between both perspectives. I will use two insightful surveys by Philip Kitcher

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6 I thank Jeroen Bouterse for urging me to clarify this distinction, though it is not one he endorses.

TABLE 1 *Differences between the subliming and subversive perspectives*

	The subliming perspective	The subversive perspective
<i>Aims</i>	Ideal of scientific rationality	Contingency of scientific practices
<i>Methods</i>	Abstraction from practices (reasons vs. causal conditions)	Contextualisation of norms (enactive vs. enabling conditions)
<i>Results</i>	Rational norms that guide scientific practices	Social or historical conditions that make scientific norms meaningful

and John Zammito as stalking horses.<sup>7</sup> Kitcher works primarily in philosophy of science, Zammito in intellectual history and historiography of science. They do not represent the “average” subliming and subversive. Indeed, I see both as dissidents from their natural perspectives, forging a middle ground for the critical study of science and its history.

In his 1998 survey, Kitcher evaluates debates in the “science wars” between one side that focuses on “realist-rationalist” phenomena and another side that focuses on “socio-historical” phenomena. His own position is in “the marginalized middle”.<sup>8</sup> From it, he rehearses Mercutio’s cry (“A plague o’ both your houses!”), while noting that Mercutio is nevertheless “closer to the Montagues than to the Capulets”. Zammito finds Kitcher’s position on science studies to be “thoroughly compatible” with his own.<sup>9</sup> His 2011 review aims to “revive” History and Philosophy of Science as a “locus of practice”.<sup>10</sup> He calls for a conception of science history that allows for structural constraints which are “real and binding”, and novelties which arise at the “concrete level of event and agency”.<sup>11</sup>

Both Kitcher and Zammito criticise the excesses of subliming and subverting, while retrieving insights from each perspective. I agree with much of what

7 P. Kitcher, “A Plea for Science Studies”, in N. Koertge (ed.), *A House Built on Sand: Exposing Postmodernist Myths About Science* (New York: Oxford University Press, 1998), 33–51, and J. H. Zammito, “History/Philosophy/Science: Some Lessons for Philosophy of History”, *History and Theory* 50, no. 3 (2011): 390–413. From the latter, I draw several quotations by other authors.

8 Kitcher, “A Plea for Science Studies”, 50.

9 Zammito, *A Nice Derangement of Epistemes*, 268.

10 Zammito, “History/Philosophy/Science”, 392, note 8.

11 *Ibid.*, 413.

they say. My aim is to extend their middle ground by clarifying a dynamic that they neglect and, to some extent, reiterate.

## 2.2 *Dismissals and Condemnations*

What is this dynamic? It is a series of mutual dismissals and condemnations. I begin with the dismissals. Sublimers are dismissed for being *scholastic* in their approach, while subversives are disdained for being merely *sociological*.

From the subversive perspective, the sublimers' ideal of scientific rationality looks too abstract. It is not useful in the analysis of science. According to Zammito, the "abstract-universal norms thematized in philosophy of science" seem "increasingly irrelevant".<sup>12</sup> This is how he criticises the "unified theory of science" proposed by the positivists:

The grand-scale "normative" prepossessions of the "Received View" in philosophy of science... have proven ultimately incongruous with *any* effective descriptive or explanatory investigations of concrete areas or problems.<sup>13</sup>

So he rejects positivist norms for being irrelevant, in some ultimate sense, to analysing "concrete" issues. In their place, he promotes a "far more disunified, situated, and contingent theory of empirical inquiry", which is drawn from the fields of historicised epistemology and science studies.

From the subliming perspective, some social conditions identified by subversives are poorly connected to scientific practices. For instance, Kitcher criticises the sociological approach in which scientists are conceived as "brain dead from the moment they enter the laboratory to the moment at which they leave".<sup>14</sup>

Curious stories are then told about the ways in which class or gender, toilet training or religious education, political disputes in the wider society and large cultural styles determine the character of a researcher's work.<sup>15</sup>

When subversives connect social conditions and scientific work with "so broad a brush", they may be legitimately accused of "scientific ignorance". It is not

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<sup>12</sup> *Ibid.*, 409.

<sup>13</sup> *Ibid.*, 391. All italics are in the original quotations.

<sup>14</sup> Kitcher, "A Plea for Science Studies", 38.

<sup>15</sup> *Ibid.*, 39.

obvious how their conclusions are meaningful to philosophers and others who study scientific practices; they are, in this sense, merely sociological.

Many historians of science will be sympathetic to Zammito's complaint. David Miller characterises their view in the following way:

The philosophers' pursuit of rationality, says the historian, fails to say anything definitive about the essentially contextualized human condition. Hence, the philosophical study of science is ill-founded and uninteresting.<sup>16</sup>

On the other hand, philosophers of science are likely to share Kitcher's concern. Here is how Miller elucidates their view:

Philosophers . . . see no profit in complete contextualization and they are not interested in antiquarian description for description's sake. They want the payoff for their own, always present-day, essentially rational selves. Hence, the historical study of science is ill-founded and uninteresting.<sup>17</sup>

He reports that, even at conferences organised to bridge this methodological divide, there is "a remarkable amount of confusion and even disdain regarding the aims and activities of the other camp".<sup>18</sup>

Next are the mutual condemnations. Sublimers are condemned for being *imperialist*, while subversives are criticised for being *irrationalist* or *relativist*. From the subversive perspective, sublimers seem to impose their ideal of rationality on scientific practices. Even as Zammito dismisses the relevance of positivist philosophy, he denies that it is "authorized to impose *a priori* rules upon empirical inquiry".<sup>19</sup> He finds support from two other theorists. The first is Joseph Rouse, who criticises the desire of some philosophers to establish "epistemological sovereignty". Their sovereign is supposed to serve as "an impartial referee among conflicting claims", acting "in accord with rules of rational method".<sup>20</sup> Second is Hans-Jörg Rheinberger, who echoes Gaston Bachelard's

16 Miller, "The History and Philosophy of Science History", 39.

17 *Ibid.*, 40.

18 *Ibid.*, 30.

19 Zammito, "History/Philosophy/Science", 391.

20 J. Rouse, "Beyond Epistemic Sovereignty", in P. Galison and D. Stump (eds.), *The Disunity of Science: Boundaries, Contexts, and Power* (Stanford: Stanford University Press, 1996), 403.



view that philosophers ought not to “dictate to scientists the conditions of possibility and the norms of their knowledge”.<sup>21</sup>

From the subliming perspective, subversives appear to be seduced into scepticisms about reason and reality. Kitcher cites the view that they are “dedicated to undermining scientific authority”.<sup>22</sup> Insofar as this view is accurate, he blames “some bad philosophy that has been strikingly influential in contemporary history and sociology of science (and occasionally in some contemporary philosophy of science)”.<sup>23</sup> For instance, the underdetermination of theory by evidence is “dramatically overblown” by subversives. They use it to support a sceptical dogma: “no system of beliefs is constrained by reason or reality, and no system of beliefs is privileged”.<sup>24</sup> Zammito explicitly endorses Kitcher’s diagnosis of this error. He adds that the dogmas of post-positivist philosophy “simply do not justify the extravagances of radical postmodernism in science studies”.<sup>25</sup>

### 3 Analysis of Impasse

I have suggested that, underlying the impasse between sublimers and subversives, there is a dynamic of dismissals and condemnations. Each perspective criticises the other for claiming too little, then for claiming too much. The result is that both oscillate, in tone, between disdain and hysteria about the other.

Now I propose a partial explanation for this dynamic. Sublimers and subversives often talk past each other because they are interested in different problems and interpret concepts differently. Each perspective construes the other’s claims in terms of its own interests and conceptual interpretations. Seen from the wrong perspective, the other’s claims can appear either trivial or false – thereby provoking either dismissal or condemnation.

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21 H.-J. Rheinberger, *On Historicizing Epistemology* (Stanford: Stanford University Press, 2010), 21.

22 Kitcher, “A Plea for Science Studies”, 33. A precursor: David Hume may be interpreted as highlighting the enactive conditions for inductive norms. He has also been accused of being irrationalist, or at least sceptical, about induction.

23 *Ibid.*, 39.

24 *Ibid.*, 41 and 45.

25 Zammito, *A Nice Derangement of Epistemes*, 268–9.

### 3.1 *Being Scholastic and Imperialist*

Consider why sublimers are dismissed for being scholastic. Their ideal of rationality is said to be too abstract for analysing scientific practices. But too abstract for whose purpose? From the subliming perspective, the ideal's norms are used to make sense of, and later guide, some codifiable aspects of scientific practices. They should be judged by how well they illuminate or improve those aspects of science. Being insufficient to explain other aspects of science does not make them irrelevant *per se*. Being abstract, in itself, also does not make them irrelevant. Indeed, an account of "concrete areas or problems" that strains to avoid any abstraction is likely to be overwhelmed by incidental detail – a different risk of scholasticism.

Subversives themselves resort to a degree of abstraction when they describe how social or historical conditions enable the practices and enact the norms of different scientists. They too face the risk of obscurity that comes with abstract description. Kitcher highlights this with a parody of the subversives' terminology:

Let us mix in some Lacan, some Lyotard, a dash of Deleuze. Let us play with Derrida. Let us have actor networks, mangles of practice, emergent dialectical surfaces, multivocalized polygendered postphallogocentric transcategorially sensitive discourses...<sup>26</sup>

He finds this sociological discourse to be "as closed off from the phenomena that were once central to the field as some philosophical investigations of the 1950s with their exclusive obsessions with the blackness of ravens."

However, it is not surprising that the sublimers' ideal of rationality appears irrelevant to subversives. First, some subversive philosophers emphasise the contingency of scientific practices in order to provoke a re-valuation of norms. Rheinberger presents Feyerabend's view as an extreme case: "Contingency intervenes in the actual course of history in a way that lies beyond any critical-rational schematism".<sup>27</sup> Second, historians and sociologists of science often focus on the human conditions under which scientific norms work. These norms cannot help to explain their own conditions. Rather they form a framework in which interesting questions can be raised about their contingency.

Such historians and sociologists can find unexpected allies among philosophers who do not advertise subversion. Beginning in the 1980s, Richard Boyd

26 Kitcher, "A Plea for Science Studies", 45.

27 Rheinberger, *On Historicizing Epistemology*, 63.

has built a case for the “radical contingency of scientific methods”.<sup>28</sup> According to Boyd, these methods are “profoundly theory dependent”. They reliably guide scientists to the truth only insofar as the background theories that facilitate their use are “approximately true”.

The emergence of successful modern scientific methodology as we know it depended upon the logically, epistemically, and historically contingent emergence of a relevantly approximately true theoretical tradition.<sup>29</sup>

This theoretical tradition is one of the central conditions that enact our scientific norms. So it cannot be explained as the product of the norms. In this sense, the corpuscular theory of matter in the seventeenth century was not the product of any reliable and theory-independent chemical methodology. Rather, “scientific reliability in chemical methodology was only made possible by its adoption”.<sup>30</sup> Boyd’s example shows that a theorist can seek to explicate, even vindicate, scientific norms without having to deny their contingency.

Next, what about the condemnation that sublimers are imperialist – that they presume to impose norms on science? This is puzzling for two reasons. First, most sublimers do not claim any *a priori* or infallible authority over scientific rationality. They identify norms by studying how scientists behave. These norms are open to testing. And they can be rejected if they do not help to clarify or improve scientific practices.

Even the logical positivists, who are criticised by Zammito, derived norms partly from their substantial knowledge of how physicists theorise about the world. Boyd emphasises this “naturalistic strain” in the work of the positivists and their successors:

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28 See R. Boyd, “Scientific Realism and Naturalistic Epistemology”, *PSA: Proceedings of the Biennial Meeting of the Philosophy of Science Association* 1980, Volume 2 (1980): 613–62; “How to Be a Moral Realist”, in G. Sayre-McCord (ed.), *Essays on Moral Realism*, (Ithaca: Cornell University Press, 1988); “Realism, Approximate Truth, and Philosophical Method”, *Scientific Theories* 14 (1990): 355–91; “Metaphor and Theory Change: What Is ‘Metaphor’ a Metaphor For?”, in A. Ortony (ed.), *Metaphor and Thought*, 2nd Edition, (Cambridge: Cambridge University Press, 1993), 481–532.

29 Boyd, “Realism, Approximate Truth, and Philosophical Method”, 365–6.

30 Boyd, “Scientific Realism and Naturalistic Epistemology”, 636.

... it was (and is) utterly routine for the results of *a prioristic* reasoning concerning the foundations of science to be abandoned when found to be incompatible with actual scientific practice.<sup>31</sup>

Their rational reconstruction of scientific inquiry involved the interpretation of sound scientific methods and well-confirmed theories. It evidently depended on “detailed consideration of the work of actual scientists”, though it could not draw on the vast range of historical, sociological and ethnographic resources available today. Unfortunately, this salutary aspect of the positivists’ method has been obscured by some of their better-publicised doctrines. For instance, Boyd acknowledges that the incipient naturalism in their work seems “out of keeping” with their programmatic distinction between empirical and conceptual inquiry.

Second, the sublimers’ method of abstraction does not logically exclude any practices from consideration. Others are free to propose norms that are drawn out from neglected or new practices. Indeed, this task must be open-ended. As Kitcher points out, in science, “canons of reason and evidence also progress with time as we discover not only more about the world but also more about how to learn about the world”.<sup>32</sup> He cites recent discoveries in statistical inference and methodology.<sup>33</sup> Even sublimers must revise their ideal of rationality to reflect these evolving practices.

Why then do some subversives believe that the sublimers want to “impose” or “dictate” an ideal of rationality to others? One possible, if partial, answer: they are reacting to a practical, rather than logical, consequence of abstraction. Abstracting a set of norms usually leads us to neglect related practices that are not codified by them. This is, I believe, key to Wittgenstein’s worry about subliming. It is implicit in a complaint lodged by two sociologists of science, Donald McKenzie and Barry Barnes:

Philosophical models of proper evaluation are irrelevant to the historian’s task. Indeed, with their typical stress upon the formal, abstract properties of verbal argument, they can even impede an adequate naturalistic understanding of actual judgments ...<sup>34</sup>

31 *Ibid.*, 617. He cites an early and explicit appeal to such considerations in C. G. Hempel, *Aspects of Scientific Explanation: And Other Essays in the Philosophy of Science* (Free Press, 1965), chapter 4.

32 Kitcher, “A Plea for Science Studies”, 36.

33 *Ibid.*, 37.

34 D. MacKenzie and B. Barnes, “Scientific Judgment: The Biometry-Mendelism Controversy” in B. Barnes and S. Shapin (eds.), *Natural Order: Historical Studies in Scientific Culture*

Note how the basis of their criticism shifts from irrelevance to impediment. To non-logicians who are attuned to historical or sociological interests, this impediment may be no less significant than logical exclusion.

Kitcher offers some evidence to support this subversive response. He describes how philosophers of science, between the 1930s and 1960s, fixated on the justification of true scientific beliefs.

Prevailing pictures of justification tended to identify relatively simple forms of inference that made it puzzling how any rational person could ever have opposed the great achievements in the history of science.<sup>35</sup>

These philosophers neglected practices that explain why there had been “protracted scientific debates” over these great achievements. These practices include taking measurements and building models. They also focused on practices in physics, rather than those in biology and chemistry. Besides the “rebellious sociologists” named by Kitcher, subsequent generations of philosophers criticised and began to compensate for this neglect.<sup>36</sup> Jutta Schickore reminds us that, in the 1960s and 1970s, many philosophers of science sought to bring “abstract axiomatic philosophy” back into “contact with actual science”.<sup>37</sup>

### 3.2 *Being Sociological and Irrationalist*

What exactly do sublimers mean when they dismiss subversives for being sociological? First, they may be marking a disciplinary boundary. In informal settings, some philosophers disparage details about the context of a practice as “merely sociological”. Their criticism suggests that the conditions forming the context are accidental to the practice; by default, they do not interest philosophers who want to focus on what is essential.

But this dismissal is premature. Sociological inquiry can cast light on social dimensions that may be essential to scientific practices. Some philosophers

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(London: Sage, 1979), 191–2. They are quoted in Zammito, “History/Philosophy/Science”, 409.

35 Kitcher, “A Plea for Science Studies”, 43. I thank Jouni-Matti Kuukkanen for impressing on me the significance of this point.

36 See, for instance, the essays collected in H. Putnam, *Philosophical Papers: Volume 1, Mathematics, Matter, and Method*, 2nd Edition (Cambridge: Cambridge University Press, 1979); P. Kitcher, *In Mendel's Mirror* (Oxford: Oxford University Press, 2003). The naturalistic trend has flourished in contemporary philosophy of science; see the review of this trend, and its problems, in Steven French and Michela Massimi, “Philosophy of Science: A Personal Peek into the Future”, *Metaphilosophy* 44, no. 3 (2013): 230–40.

37 Schickore, “More Thoughts on HPS”, 477.

of science already focus on these neglected dimensions.<sup>38</sup> For instance, Boyd argues that social mechanisms are relevant to the “criticism, testing, acceptance, modification, and transmission of scientific theories and doctrines”. Thus they have a role “not only for the sociology and history of science, but for the epistemology of science as well”.<sup>39</sup> In his survey, Kitcher highlights research on the contribution from “values of the broader society” and “social relations and structures”.<sup>40</sup> Their contribution is guided by norms that should interest sublimers. To progress, this research needs “rich descriptions of particular instances”.<sup>41</sup> These may well be supplied by historians and sociologists with subversive interests.

Second, sublimers may be raising a metaphysical challenge. Through contextualisation, subversives discover social conditions that enable scientific practices and enact scientific norms. But, to some philosophers, the metaphysical significance of these conditions needs to be clarified. Are the conditions causally or constitutively related to scientific knowledge? A causal relation indicates that, if the relevant conditions were absent, we would lack that scientific knowledge. A constitutive relation implies that the nature of our scientific knowledge is partly defined by those conditions. If the conditions are constitutively related to scientific knowledge, are they related via norms, concepts, objects or facts?

These questions involve metaphysical distinctions between causal and constitutive relations, and between their appropriate relata. Philosophers who raise the questions are interested in a metaphysical account of enabling and enactive conditions. It is up to metaphysicians to decide how best to use “rich descriptions of particular instances” to answer these questions.

Are subversives interested in these questions? Very few of them say that the social conditions they investigate are constitutive of scientific knowledge. Most prefer to speak of the “social construction” or “shaping” of norms, concepts, objects and facts. Paul Boghossian interprets their claims in strictly constitutive terms:

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38 P. Machamer and L. Osbeck, “The Social in the Epistemic”, in P. Machamer and G. Wolters (eds.), *Science, Values and Objectivity* (Pittsburgh: University of Pittsburgh Press, 2004), 78–89; Helen Longino, “The Social Dimensions of Scientific Knowledge”, in E. N. Zalta (ed.), *The Stanford Encyclopedia of Philosophy*, Spring 2013, URL=<http://plato.stanford.edu/archives/spr2013/entries/scientific-knowledge-social/>.

39 Boyd, “How to Be a Moral Realist”, 318.

40 Kitcher, “A Plea for Science Studies”, 46.

41 *Ibid.*, 50.

our social construction theorist is not interested in cases where, *as a matter of contingent fact*, some fact is brought into being by the intentional activities of persons, but only in cases where such facts could *only* have been brought into being in that way. In the intended technical sense . . . it has to be *constitutive* of a given fact that it was created by a society if it is to be called “socially constructed”.<sup>42</sup>

By contrast, in his survey of social constructionism, Ron Mallon interprets its “core idea” to be that:

some object or objects are caused or controlled by social or cultural factors rather than natural factors, and if there is any core motivation of such research, it is the aim of showing that such objects are or were under our control: they could be, or might have been, otherwise.<sup>43</sup>

This leaves room for philosophers, including Mallon himself, to construe constructionist claims in terms of either causal or constitutive relations between social factors and constructed objects.

Like Mallon, I am not so confident that the historians and sociologists who make constructionist claims intend them in Boghossian’s “technical”, which is to say metaphysically loaded, sense. First, subversives tend not to distinguish sharply between knowledge and the process of acquiring knowledge. Here is an example of this blurring, taken from Zammito: he describes recent studies of scientists “as emphasizing increasingly the local and developmental character of their learning practices, thus making the conception of knowledge dependent upon historical experience”.<sup>44</sup> From this subversive perspective, contextualising how something is learnt is also historicising what comes to be known. But, without a firmer distinction in place between what is known and how it is learnt, the sublimator’s metaphysical challenge loses its point. Indeed, Zammito does not specify if the dependence he points to is causal or constitutive in nature.

42 P. Boghossian, *Fear of Knowledge: Against Relativism and Constructivism* (Oxford: Clarendon Press, 2007), 17.

43 R. Mallon, “Naturalistic Approaches to Social Construction”, in E. N. Zalta (ed.), *The Stanford Encyclopedia of Philosophy*, Winter 2013, URL=<http://plato.stanford.edu/archives/win2013/entries/social-construction-naturalistic/>.

44 Zammito, “History/Philosophy/Science”, 390. He is summarising Rheinberger’s presentation of “thought about the nature of science by leading scientists and their interpreters over the course of the twentieth century”.

Second, subversives often explicate their constructionist claims in terms of the meaningful or significant role played by social conditions in scientific practices. This role is assessed by how the social conditions help to make sense of scientific practices. For this interpretative purpose, what matters is that the scientific practices “could be, or might have been, otherwise” if the social conditions were otherwise. It does not matter to them whether this contingency arises from causal or constitutive relations.

Similarly, it is not obvious that subversives are interested in the metaphysical distinction between constructed norms, concepts, objects and facts. All these constructions signify the contingency of scientific practices – the main locus of their inquiry. This may explain why some subversives feel no need to distinguish sharply between different targets of construction.<sup>45</sup> Here, for instance, Rouse moves without qualm from talk of norms to talk of things:

Normativity arises from practical involvement *in* a situation whose subsequent development is not yet determined: there are real possibilities for making a (significant) difference in how things *subsequently* turn out. The difference those possibilities can make transforms the situation . . .<sup>46</sup>

He does not ask whether the “significant” difference made by norms to things arises from a causal or constitutive relation.

Some subversives also draw on a tradition in French epistemology of science that includes Gaston Bachelard, Georges Canguilhem and Michel Foucault. According to Rheinberger, this tradition studies “the historical conditions *under* which, and the means *with* which, things are made into objects of knowledge.” To do so, it focuses on “the process of generating scientific knowledge and the ways in which it is initiated and maintained”.<sup>47</sup> Why does this “process of generating scientific knowledge” cast light on the making of “objects of knowledge”? Because, in this tradition, the objects of interest are partly defined in terms of the changing norms and concepts of the scientific process. This is a different, also technical, sense of “objects”, not noted by Boghossian or Kitcher.

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45 See I. Hacking, *The Social Construction of What?* (Cambridge, MA: Harvard University Press, 1999), chapter 1. He discusses the tendency to conflate the construction of a concept with the construction of a phenomenon picked out by the concept.

46 J. Rouse, *How Scientific Practices Matter: Reclaiming Philosophical Naturalism* (Chicago: University of Chicago Press, 2003), 26.

47 Rheinberger, *On Historicizing Epistemology*, 2–3.



As Rouse explains, such objects “only exist within phenomena”;<sup>48</sup> Rheinberger calls them “epistemic things”.<sup>49</sup> Subversives who follow this tradition are rarely interested in how things are in themselves.<sup>50</sup>

Next, consider the criticism that subversives are irrationalist or relativist. This is problematic for two reasons. First, few subversives denigrate scientific rationality *per se*.<sup>51</sup> Most subversives want to explain how scientific norms operate under social or historical conditions. So they look for conditions that enact scientific norms. Their search need not debase the success of those practices, nor threaten scientific rationality. As Boyd demonstrates, contingency in scientific method does not entail its corruption. Even those subversives who are interested in reform want to reconceive scientific norms, rather than reject them outright. They often do so by proposing changes to the norms’ enactive conditions.

Here is a passage from Barry Barnes and David Bloor, cited by Boghossian as an typical example of “postmodern relativism”:

For the relativist there is no sense attached to the idea that some standards or beliefs are really rational as distinct from merely locally accepted as such. Because he thinks that there are no context-free or super-cultural norms of rationality he does not see rationally and irrationally held beliefs as making up two distinct and qualitatively different classes of thing.<sup>52</sup>

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48 J. Rouse, *Engaging Science: How to Understand Its Practices Philosophically* (Ithaca: Cornell University Press, 1996), 274.

49 Rheinberger, *On Historicizing Epistemology*, 86.

50 On the French tradition’s connection with Kant, see G. Canguilhem, “The Death of Man, or Exhaustion of the Cogito”, in G. Gutting (ed.), *The Cambridge Companion to Foucault*, trans. C. Porter, 2nd Edition (Cambridge: Cambridge University Press, 2006), 74–94.

51 An anonymous referee reminded me that, in its early phases, the conflict between sublimers and subversives was marked by sharp disagreement over scientific methods. Some subversives suggested that these methods worked primarily as retrospective justifications for scientific consensus. This theme appears, for instance, in H. Collins, *Changing Order: Replication and Induction in Scientific Practice* (Chicago: University Of Chicago Press, 1992). In a new afterword, Collins clarifies that his analysis of scientific method as “social through and through” does not constitute “an attack on science”. It is instead a critique of the “canonical rational-philosophical model” of science.

52 B. Barnes and D. Bloor, “Relativism, Rationalism and the Sociology of Knowledge”, in M. Hollis and S. Lukes (eds.), *Rationality and Relativism* (Cambridge, MA: MIT Press, 1982), 27–8.

At first glance, this may read like a manifesto for irrationalism. But Barnes and Bloor do not deny that there are rational norms. Nor do they claim that norms bind us “merely” when they are locally chosen. Instead, they challenge – as most subversives do – a specific conception of rationality that requires “really rational” norms. These norms are supposed to be “context-free or super-cultural”, working without what I have called enactive conditions. Barnes and Bloor also do not say that “rationally and irrationally held beliefs” are equally valid. Instead, they believe that both classes of beliefs are acquired through contingent practices; I take this to be one sense in which the beliefs are not “distinct and qualitatively different”.

Nothing so far supports a naive relativism about scientific norms. Barnes and Bloor do not say that the norms can only be “locally accepted”, in the sense that they are determined by one culture or constrained within one context. From the subversive perspective, enactive conditions need not be interpreted as metaphysically necessary conditions. Instead, a different set of conditions could enact the same norms. So different cultures need not produce different scientific norms.

Second, the subversives’ method is misconstrued as their sceptical dogma. Kitcher, for instance, blames this scepticism partly on a philosophical idea that is “over-interpreted”: the underdetermination of theory by evidence. He acknowledges that subversives reject “context-independent standards of good reasoning”.<sup>53</sup> But he interprets this rejection as an exaggerated claim about underdetermination. Why? He believes that subversives need this claim about underdetermination to support scepticism. They supposedly infer that “the world can have no bearing on what scientists accept.” So their appeal to underdetermination is the “reformulation of a form of scepticism – tantamount to the freshman reader of Descartes who demands to be shown that it is inconsistent to suppose that one is alone in the universe”.<sup>54</sup>

But subversives rarely claim that scientists are free from constraints exerted by the world. Some of them even model how material constraints crop up in scientific practices as a kind of “resistance”.<sup>55</sup> Admittedly, for most subversives, these material constraints are not in focus. They are primarily interested in how the meanings of scientific practices depend on human conditions. Yet even those influenced by the French epistemologists see themselves as

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53 Kitcher, “A Plea for Science Studies”, 41.

54 *Ibid.*, 42.

55 See A. Pickering, *The Mangle of Practice: Time, Agency, and Science* (Chicago: University of Chicago Press, 1995); Hans-Jörg Rheinberger, *On Historicizing Epistemology: An Essay* (Stanford: Stanford University Press, 2010), 31.

uncovering the conditions under which “things are made into objects of knowledge” – things that are, presumably, in the world.

So how has this misunderstanding arisen? Contrary to Kitcher, I do not believe that most subversives use underdetermination as a philosophical premise. Rather it is their methodological tool. As I explained earlier, they look for systematic instances in which scientific norms cannot fully account for a scientific practice. From their perspective, this underdetermination suggests that the norms may be enacted by social conditions. It furnishes a clue. To demonstrate that such conditions are at work, subversives must find a pattern of conditions that help to make sense of the scientists’ behaviour in that practice and others.

Their clue is obscured when we assume that scientific norms and worldly constraints suffice to account for a scientific practice. This explains why, in their interpretations of scientific practice, some subversives disavow any appeal to nature or how the world is. In doing so, they are not being metaphysically provocative – neither denying that the world exists nor denying that the world constrains scientific practices. Rather they are adopting a methodological stance in order to look for social conditions that enact scientific norms.

#### 4 Applications

To sum up: I have identified a dynamic of dismissals and condemnations from the impasse between sublimers and subversives. And I have explained this dynamic in terms of their misunderstanding of each other’s methodological interests and conceptual interpretations.

My method is abstraction, used with some philosophical abandon. I highlight only those tendencies and trajectories of thought that clarify the dynamic I am interested in. Two caveats are needed. First, I neglect many methodological and conceptual differences between the philosophers, historians and sociologists whom I classify roughly into sublimers and subversives. I also ignore surprising similarities, such as the distaste for traditional metaphysics shared by the logical positivists and many subversives. Second, I do not claim that my analysis fully accounts for their mutual dismissals and condemnations. It is unlikely that a persistent dynamic between two perspectives – with overlapping disciplinary, social and political affiliations – is just the result of misunderstandings.<sup>56</sup>

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56 These affiliations are explored with different nuances in Zammito, *A Nice Derangement of Epistemes*; J. Golinski, *Making Natural Knowledge: Constructivism and the History of*

Like any abstract analysis, its value lies partly in applications. It is plausible that at least some aspects of this dynamic will appear in debates on the contingency of norms within non-scientific realms, such as art and morality. Here I propose two other applications. First, does my analysis illuminate other aspects of the dynamic? For example, each perspective's dismissal and condemnation can now be interpreted as responses to the other's criticisms.<sup>57</sup> When subversives are dismissed for being merely sociological, it is not surprising that some of them are dismissive of sublimers in return. This is especially likely if they interpret sublimers to mean that subversive interests are not significant *per se* – rather than not yet, or not obviously, significant in metaphysical terms.

Subversives tend to focus on practices that are not codified by the sublimers' norms. Some of these practices may be guided by hidden norms. If the sublimers hastily dismiss these subversive interests, then they may be unduly neglecting some scientific norms. Their response adds to the subversives' impression that subliming arguments are imperialist.

When sublimers are condemned for being imperialist about scientific norms, some of them will interpret this to mean that subversives are cavalier about these norms. Moreover, subversives tend to neglect metaphysical distinctions that matter to some sublimers. They do not draw clear distinctions between knowledge and the process of acquiring knowledge, between contingent and constitutive relations, and between constructed norms, concepts, objects and facts. If sublimers enforce these distinctions, some subversives will accuse them of being scholastic. Their reaction only reinforces the sublimers' impression that subversive arguments lack rigour.

Second, does my analysis clarify more specific disputes over the contingency of scientific practices? I started to explore the history of subliming and subverting when I spotted a similar dynamic of dismissals and condemnations in recent discussions on scientific models.<sup>58</sup> One perspective elucidates

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*Science* (Chicago: University of Chicago Press, 2005); Miller, "The History and Philosophy of Science History".

57 This extends a line of reasoning from Kitcher, "A Plea for Science Studies", 49. He argues that one side's over-extension of "genuine insights" leads to the other side's over-generalisation of "good points".

58 See M. Morgan and M. Morrison (eds.), *Models as Mediators: Perspectives on Natural and Social Science* (Cambridge: Cambridge University Press, 1999); N. da Costa and S. French, "Models, Theories, and Structures: Thirty Years On", *Philosophy of Science* 67 (2000): S116–S127; D. Portides, "Models", in S. Psillos and M. Curd (eds.), *The Routledge Companion to Philosophy of Science* (London: Routledge, 2008), 385–395; A. Gelfert (ed.), "Model-Based Representation in Scientific Practice", *Studies in History and Philosophy of Science* 42, No. 2 (2011).

the norms that govern model-building, while the other perspective emphasises the conditions and processes involved in model-building. Again, one perspective is sometimes criticised for being scholastic, the other for being merely or potentially sociological. I conjecture that my analysis can also help to explain some current disagreements about the role of social values in science.<sup>59</sup> In this debate, the threats of imperialism, irrationalism and relativism are more salient.

I am interested to see if the impasse between subliming and subverting can cast fresh light on old arguments. Earlier I discussed an example from Barnes and Bloor, on the point of relativism in sociology. Consider, in another example, the critical theorist Max Horkheimer's insistence that the "historically conditioned" nature of scientific thought is not in conflict with its validity and truth. Here is his version of the subversive creed:

That all our thoughts, true and false, depend on conditions that can change . . . in no way affects the validity of science. It is not clear to me why the fact of *Seinsgebundenheit* [i.e., of being historically conditioned] should affect the truth of a judgement – why shouldn't insight be just as *seinsgebunden* as error?<sup>60</sup>

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59 See Machamer and Wolters (eds.), *Science, Values and Objectivity*, and the review by S. French, "Science, Social Values and Straw Positions", *Metascience* 14 (2005): 465–468.

60 Cited in D. C. Hoy and T. McCarthy, *Critical Theory* (Oxford: Blackwell Publishers), 10. I owe this example to Alexandra Serrenti. Another intriguing example, which cropped up during conference discussion with Arianna Betti, is the debate on psychologism in logic, as framed by Gottlob Frege and Edmund Husserl.