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Flavia Padovani
Alan Richardson
Jonathan Y. Tsou *Editors*

Objectivity in Science

New Perspectives from Science and Technology Studies

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Chapter 1
Introduction: Objectivity in Science

Jonathan Y. Tsou, Alan Richardson, and Flavia Padovani

While few would question the importance of the objectivity of science for providing a well-supported factual basis upon which policy decisions can be reliably made, it is far from clear what scientific objectivity is or how it should be achieved. In recent decades, questions regarding the objectivity of science have become increasingly salient in framing public debates about science and science policy: for example, can we trust medical research when it is funded by pharmaceutical companies? Or, whose research in climate science meets the standards of scientific objectivity? At the same time, the objectivity of science has become an increasingly important topic among historians and philosophers of science, as well as researchers in related fields in science and technology studies. In the wake of Karl Popper's (1972) account of objective knowledge and Thomas Kuhn's (1977) landmark analysis of scientific values in connection with issues of scientific objectivity and rationality, philosophers of science have attempted to clarify questions concerning the role of values in theory choice, the distinction between epistemic (or "cognitive") and non-epistemic (or "social") values, and the ways in which different kinds of

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values (including non-epistemic values) contribute to the objectivity of science.[1] By contrast, historians of science have offered rich historical analyses that aim to clarify the changing historical meanings of objectivity by examining the emergence of particular scientific ideals in specific episodes in the history of science.[2] These historical studies have revealed the complex, multifaceted, and ultimately contingent nature of the ideals that contribute to our current notions and understandings of scientific objectivity. Finally, sociologists and anthropologists of science have offered analyses that explicitly bring into question specific understandings of scientific objectivity as, for example, the disinterestedness or value neutrality of scientific work, by revealing the role of social processes—including the workings of structures of credit, rhetorical practices in science, and the pressure of funding regimes—in the production of scientific knowledge.[3] Taken together, these investigations offer compelling reasons for thinking that scientific objectivity is much more complicated than one might have imagined. Two emergent themes from the science and technology studies literature are especially important in this regard.

The first of these themes comes largely from philosophy of science, but philosophy of science that is informed by sociology of scientific knowledge and by feminist criticism. This theme can be summarized as follows: The standard account of the objectivity of science throughout much of the twentieth century was value-freedom. Science is able to serve as an objective source of unbiased information precisely because either the individual scientist is able—qua scientist—to transcend all social, moral, and political values, or more plausibly, the institution of science is able to insulate itself from social values that would bias it and render it subjective. With the historical turn in philosophy of science after the work of Thomas Kuhn and others and with the rise of sociology of scientific knowledge, which took the class-based interests of scientists and of the institutions of science for granted, such claims have come to seem increasingly implausible. It is far too easy to see moral and social values suffusing all the past achievements of science, including those that we still endorse as well as those we no longer endorse. The social and moral values at the heart of Darwin's theory of evolution are just as evident in his account of natural selection, which is still understood to be largely correct, as in his advocacy of proto-eugenicist social policies, which we largely reject. Scientists and the institutions of science bear within their bodily frames the indelible stamp of the times, places, and social structures in which they have arisen.[4]

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[1] For some examples, see Salmon (1980), Hempel (1983), Laudan (1984), McMullin (1988, 1993), Longino (1990, 2002), Kitcher (1995, 2001), Okruhlik (1994), Machamer and Douglas (1998), and Solomon and Richardson (2005).

[2] See especially Proctor (1991), Daston and Galison (1992, 2007), and Porter (1995).

[3] For example, see Latour and Woolgar (1979), Collins and Pinch (1993), and Latour (1999).

[4] For a classic account of the value-free ideal, see Proctor (1991). For more recent elaborations of various philosophical perspectives on the value-free ideal, see Lacey (1999), Machamer and Wolters (2004), Kincaid et al. (2007), and Douglas (2009). For an account of the development of sociology of scientific knowledge by one of its most distinguished practitioners, see Shapin (1995).

With a similarly motivated but even more empirical eye, feminist critics of science have measured the community of science against the ideals that every community claims to hold dear.[5] The value-free ideal suggests that the community of science should be open equally to all voices suitably trained in the material and methods of science. The scientific community, one would think, would then be in the vanguard of equity. But this is notably not the case. Women and ethnic minorities are visibly less well represented in various scientific communities than in many other professions; indeed some of the sciences (as well as philosophy) are among the least gender-balanced disciplines within the academy. Thus, the model of the open community of inquiry that grounds much of the account of scientific objectivity as value-freedom seems importantly ungrounded empirically. By its own lights, given the value-free ideal, the scientific community seems to be biased.[6]

What should be done in light of such concerns about the value-free ideal of objectivity? One response—one alleged to be common in the science and technology studies community, but in fact quite rare within that community—would be to deny the objectivity of science: objectivity is value-freedom, but no person and no institution is value-free, not even scientists and the institutions of science. Science is one more interested political actor in an on-going socio-political power struggle, nothing more and nothing else.[7] While this response is rare within the science and technology studies community, it is *halfway* exemplified in many responses to science in the arena of public discourse. It is not at all uncommon to hear this move being made with respect to science that a critic does not believe. For example, climate change skeptics point to the behaviour of climate scientists as revealed in the leaked email in the Climategate scandal: surely, scientists who engage in the "tricks" involved in the hockey-stick graph or who so revile and seek to shut down their critics by not sharing their data reveal themselves as not objective.[8] Such climate science cannot be reliable, must be beholden to special interests, and its results can be set aside in the policy realm. Such a response usually does not go so far as to call all objectivity impossible, since the science, if any there be, backing the views of the critics is usually endorsed as objective. The impossibility

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The Darwin industry cannot be summarized effectively, but a sense of the variety of approaches to contextualizing Darwin's achievements can be seen in Ruse and Richards (2008).

[5]For some important contributions, see Harding (1986, 1991), Longino (1988, 1993), Keller (1989), Haraway (1988), Tuana (1989), Code (1991), and Lloyd (1996).

[6]For a representative essay on gender bias within science see Ceci and Williams (2011). An older but more expansive and, sadly, not really superseded treatment is Sonnert and Holton (1995).

[7]For the charge against the science and technology studies community see, for example, Koertge (2000) and Gross and Leavitt (1994). An examination of both the work discussed at length and the degree to which that work is actually understood in such scholarship indicates that the views decried were never actually endorsed by the leading members of the science and technology studies community.

[8]For discussion of the Climategate scandal, see Montford (2010), Ryghaug and Skjølsvold (2010), and Leiserowitz et al. (2013).

of value-free objectivity becomes, when deployed in this way, a dialectical game that renders finding a factual common ground impossible.

The response of the science and technology studies community to the objections to the value-free ideal as an account of the objectivity of science has largely been along a different line. The impetus here is to think about what objectivity is or could be if we reject the value-free ideal. One highly important account of the objectivity of science, due to Helen Longino, has become the focus of much of the philosophical effort regarding scientific objectivity and so deserves a brief exposition here. Longino's account of the objectivity of science begins with a specific location where, on her analysis, the value-free ideal must fail—the problem of the underdetermination of theory by evidence (Longino 1990, chs. 2–3; 2002, ch. 6). The underdetermination problem is a longstanding methodological problem that philosophy of science has investigated: on logical grounds alone, no scientific theory can be the unique theory confirmed by empirical evidence, even if we imagine we have gathered all possible evidence.[9] Since any theory presupposes various background beliefs and assumptions—including norms regarding what counts as evidence—the theory makes claims that go beyond what can be established by observational evidence alone. Given this evidential gap, Longino argues that values must be invoked to choose to develop or accept one theory rather than the other. Hence, all theory acceptance—since the underdetermination problem is entirely general—relies ultimately on values.

With the value-free ideal impossible in principle, Longino sets about articulating a new *social* account of objectivity in which there is no pretense that individuals and institutions in which they work lack commitment to values and in which values are not seen as inherently biasing (cf. Antony 1993). According to Longino, recognition of the social nature of knowledge demands a framework that acknowledges the necessity of a plurality of theories, which presuppose divergent background beliefs and values. In this framework, the route to objectivity occurs through a collective social process, wherein the clashing and intermeshing of alternative theories provides a means for critically assessing the background beliefs and values of one another.[10] Scientific objectivity is thus constituted by the fact that scientific knowledge must be presented in a *public domain where it must face criticism*. In this regard, Longino articulates a set of norms for the

[9]The classic texts on underdetermination are Duhem (1906/1954) and Quine (1951/1980). For a more comprehensive and critical discussion of the underdetermination thesis, see Harding (1976), Newton-Smith (1980), Laudan (1990), Laudan and Leplin (1991), Earman (1993), Leplin and Laudan (1993), Kukla (1993), Hoefer and Rosenberg (1994), Gillies (1993, ch. 5), Stanford (2001, 2006), and Intemann (2005).

[10]Longino's advocacy of theoretical pluralism is intended to address the inherently value-laden nature of scientific knowledge, but it is also motivated to address a more general problem, viz., the situated and contextual nature of knowledge. The situatedness of knowledge is, of course, a longstanding feminist concern and the motivation for the standpoint theories of Harding (1986, 1991) and others.

critical uptake of scientific claims among interested individuals. These norms—which include norms for publication venues, uptake of criticism, transparency of epistemic standards, and tempered equality of intellectual authority—then constitute the conditions under which a community can be said to be an objective knowledge-producing community (Longino 1990, 76–79; 2002, 128–135). As these norms are not meant to be descriptive of any actual community, they can be used to criticize extant scientific communities if those communities are, say, gender biased (violating tempered equality) or insulated from criticism (violating the norms relating to uptake and to publicity of standards). As is evident in the essays in this volume, Longino's normative epistemology forms a large part of the background for much contemporary philosophical work on objectivity.

The second theme that has brought objectivity to the forefront of contemporary science and technology studies is the rise of what Lorraine Daston has dubbed "historical epistemology." In this tradition, "historical epistemology" refers to the historical development of key concepts of epistemology. Rather than taking, for example, the notion of "matter of fact" or "experience" for granted and theorizing the transhistorical role of matters of fact or experience in knowledge, historical epistemology looks into the historical development of concepts of matter of fact and experience.[11] Largely through the work of Lorraine Daston and Peter Galison, one of the concepts most rigourously and extensively examined within this literature is objectivity. Their monumental volume *Objectivity* (Daston and Galison 2007) traces the development of concepts of objectivity as these concepts informed the practices of visual representation in science in the nineteenth and twentieth centuries. The volume exhibits how conceptions of objectivity arise from specific and changing concerns about the nature of the knowing subject, yield regimes for attempting to secure objective representation so conceived, interact with technological developments for the production of representations, and yield new epistemological problems that in turn can yield new conceptions of objectivity. These conceptions and the corresponding regimes for securing objective representation do not supersede one another without remainder; rather they yield a complex, layered, and polysemous set of concepts and practices that are deployed in the scientific representation of the world.

Daston and Galison's work sets the frame for many of the essays in this volume. It provides not merely specific claims to be elaborated or argued with, but also a research project that has vast potential to inform science and technology studies. In a set of interconnected case studies, Daston and Galison have argued that there is much illumination to be gained by looking at changes in the meanings of high-order epistemological terms like "objectivity" in relation to specific and changing concerns about the subjective obstacles to knowledge and to the elaboration of social, methodological, technical, and other regimes for securing objectivity so conceived.

[11] For discussion of "historical epistemology," see Daston (1994). On "matters of fact" from this perspective, see Daston (1991, 1993) and Poovey (1998). On "experience," see Dear (1995) and Jay (2005).

They also provide a model of the sort of detailed work that goes into establishing the existence and development of such conceptual-cum-material epistemological regimes.

While the work of Longino and of Daston and Galison form much of the problem space and the theoretical frameworks for the essays in this volume, there is a third feature of the contemporary scene, moving well beyond science and technology studies, that informs the current interest in objectivity within science and technology studies. This is the science under siege that one senses in contemporary disputes in areas of research such as climate science and pharmaceutical research. These controversies, in part, stem from the perception that government agencies are not treating scientific expertise with integrity and that the government is no longer properly relying on the evidence offered by the best sciences. The meddling with or ignoring of scientific expertise or evidence is not exclusively an activity of governments seeking partisan political gain. It is also a large part of pharmaceutical and other health research, much of which has been privatized and is, thus, beholden to the special interests and bottom line of the corporations that produce that research. Famous failures of oversight such as the case of Vioxx and other anti-inflammatory drugs, the ghost writing of medical articles to try to wrap them in the impartiality of public science, the setting up of industry research groups specifically to call non-industry research into question, and similar activities have eroded public confidence in the objectivity of science and have rendered it increasingly difficult for modern societies to recognize expertise and to know whom to trust on matters of evidence.[12]

The erosion of trust in science has at times been blamed at least partially on scholarship done within the science and technology studies community. This view is far less warranted than the critics make it out to be, but the issue of blame is not the most pressing. No one who is a scholar of any sort can genuinely believe that evidence is entirely arbitrary or demands for objectivity are nothing more than a move in a game of power. Indeed, the intermeshing of knowledge and power without the reduction of one to the other would seem to be one of the most crucial themes in work done by science and technology studies scholars. Steven Shapin (2010) has written: “The place of science in the modern world is just the problem of *describing* the way we live now: what to believe, who[m] to trust, what to do” (391). Many of the essays in this volume can profitably be read as attempts to delineate and to offer aid in solving precisely this problem: they offer perspectives on what objectivity can mean for us here and now, and how we might achieve objectivity both in our processes of knowledge production and in our regimes of policy construction.

[12]Some high-profile instances of concern about the pharmaceutical industry and its relations to medical research can be found in Healy and Cattell (2003), Angell (2004), and Elliott (2010). For population health more generally, two recent exposés by prominent historians of science are Oreskes and Conway (2010) and Proctor (2012).

1.1 The Essays

The essays in this volume are divided into three sections: (1) Positions on Objectivity in Contemporary Science and Technology Studies, (2) Objectivity as a Topic in Historical Epistemology, and (3) Securing Objectivity in Scientific Communities.

The first section of the volume features three contemporary analyses on scientific objectivity by Ian Hacking, Sandra Harding, and Peter Galison. Ian Hacking's provocative paper, "Let's Not Talk about Objectivity," urges researchers in science and technology studies to stop discussing objectivity *in the abstract*. Hacking distinguishes between two different kinds of questions about objectivity:

1. *Ground-level questions*: specific questions about particular cases that have some bearing on the objectivity of science (e.g., "can we trust medical research when it is funded by pharmaceutical companies?")
2. *Second-story questions*: general questions *about* objectivity that assume that objectivity is a stable epistemic ideal (e.g., "what is scientific objectivity?," "does research in climate science meet the standards of scientific objectivity?")

Hacking's injunction not to talk about objectivity is a recommendation to only ask ground-level questions, and not second-story questions, about objectivity. This is nicely encapsulated by Hacking's imperative: "let's get down to work on cases, not generalities," which is motivated by two principles. The main principle (derived from the Oxford ordinary language philosophers J. L. Austin and Gilbert Ryle) suggests that a word's ordinary usage is its meaning, and hence, we ought to study objectivity in its various sites. To discuss objectivity in the abstract, Hacking suggests, is a fruitless exercise that functions to reify an inherently unstable concept into a stable thing (cf. Hacking 1999, 22–24). A second related principle endorsed by Hacking is the idea that when we conceptualize objectivity, we should not think of it as a noun, but as an adjective ("objective") that indicates different ways in which science *fails* to be objective. In this framework, what it means to be objective will vary in different contexts, such as not allowing one's interests to guide one's research, not ignoring evidence, or not ignoring criticism.

Sandra Harding's paper, "Objectivity for Sciences from Below," offers a response to Hacking's strong injunction by emphasizing the political dimensions of objectivity. Drawing on her pioneering work in feminist standpoint theory (Harding 1986, 1991, 1992, 1993), Harding articulates and defends the "strong objectivity" program, which she subsequently tests against recent discussions of objectivity and against postcolonialist science and technology studies. Strong objectivity starts with an examination of the experiences of individuals, such as women and minorities, who have traditionally been excluded from knowledge production in order to criticize prevailing standards of objectivity—especially the "weak objectivity" of allegedly value-neutral science—and to articulate stronger standards of objectivity. This stance assumes that by adopting a standpoint outside of a discipline, one can achieve the distance required to critically assess the values, interests, and assumptions of that discipline. As Harding makes clear, this program is not merely interested in criticizing dominant research practices to improve the reliability and validity of science, but it is an inherently social and political project aimed at articulating standards that can produce science *for social justice movements*. After identifying various ways in which the strong objectivity program is consonant with major themes in recent science and technology studies research (e.g., historical work on the concept of objectivity, the dynamic co-evolution of science and society, theoretical pluralism), Harding responds to some potential objections to strong objectivity (e.g., accusations of relativism). By way of conclusion, Harding outlines her vision of a "new unification of multiple sciences" as a possible fruit of the strong objectivity program, wherein the global sciences are unified and harmonized by common goals of obtaining knowledge and developing more socially responsible societies.

In "The Journalist, the Scientist, and Objectivity," Peter Galison extends and enriches the discussion developed in *Objectivity* concerning the variety of concepts and regimes of objectivity at play in different fields of scientific representation. Galison opens up a novel perspective by relating a history of objectivity in science to that in journalism. These two histories intersect on more than one occasion, but it is especially their demand to discipline the self and its dangerous subjectivities that is common to both. As Galison shows in his investigation, the emphasis put on epistemic conditions such as impartiality, detachment, and balance in nineteenth-century journalism was not precisely congruous with the specific inclination towards objectivity informing scientific practice in the same period. As Galison puts it, while "the scientists were after a collective empiricism, a codification of shared knowledge that would give them the basic working objects of their fields, … the journalists were after a mobile discursive medium that could appeal to a much wider range of audience and advertisers, formalized in pyramidical, unemotional text and instantiated in the penny press." It is after World War I that the epistemic conditions of journalistic objectivity moved away from their original concerns and in the direction of a procedural-ethical ideal much closer to the one characterizing the sciences, both progressively reoriented and molded by commercial pressure. Galison concludes his paper with a stimulating attempt towards a common understanding of contemporary debates involving the objectivity of the digital image in science as well as in the world of the print and post-print media, again emphasizing how technical innovations influence epistemic regimes, further entwining the histories of journalistic and scientific objectivity.

The second section of the volume features four case studies that focus on historical aspects of scientific objectivity, ranging from Kant to Freud and Poincaré. In *Objectivity*, Daston and Galison emphasize the impact of Kant's newly articulated distinction between the categories of the objective and the subjective on the emergence of this notion and the scientific self in the nineteenth century. Kant opposed the objective validity of propositions, the necessary and universal pre-conditions of knowledge that properly are the subject matter of epistemology, to the merely subjective validity of the specific but contingent relations among the contents or order of the sensations for a specific person (Hume, so Kant argued, failed to distinguish these two notions, rendering his philosophy a hodge-podge of psychological theorizing and genuine epistemology). Nineteenth-century philosophy changed and remodelled this distinction into one between the objective, meaning in "relation to an external object," and the subjective as indicating something "personal, inner, inhering in us" (Daston and Galison 2007, 30). In her contribution, "The Ethos of Critique in German Idealism," Joan Steigerwald reexamines the history of objectivity and subjectivity in the works of Kant, Fichte, and Schelling, and she suggests that Kant's use of these categories did not symmetrically support or oppose the later nineteenth-century one, but rather complicated the boundaries between the objective and the subjective. By using Daston and Galison's notion of epistemic virtue to characterize Kant's philosophical project, Steigerwald shows how Kant's critical philosophy did not abstain from including values in epistemology. On the contrary, it made values fundamental to its practice by placing the ethos of critique at its core. Fichte and Schelling not only extended Kant's critique, but also introduced a meta-critical level in their reflections by investigating critically our cognitive acts as well as the transcendental reflections upon them. German idealism can thus be regarded as bearing the distinctive mark of critique as a reflection upon the limits of knowledge, a prevailing theme of that age.

For Kant, the sense organs, on their own, were the source of merely subjectively valid streams of sensation. In the nineteenth century, new scientific theories empirically investigated the physiology of the sense organs in ways that seemed to offer new epistemological insights. Among these new physiological theories, Johannes Müller's natural-scientific theory was bound to become most influential and variously inspire the construction of philosophical theories of objective knowledge, especially due to its conclusion that the quality of our sensations is determined by the physical structure of our nerves and does not depend on the external stimuli causing those sensations. Scott Edgar's paper, "The Physiology of the Sense Organs and Early Neo-Kantianism," analyzes the reception of this doctrine among neo-Kantians such as Helmholtz, Lange, and Liebmann. Edgar illustrates how, following Müller yet with interesting philosophical nuances, and not always linearly, they all rejected the view that the objectivity of our knowledge is determined by, or can be traced back to, mind-independent objects. Edgar's paper is a case study in how issues of objectivity can be—at various places and in various philosophical communities—disentangled from issues of realism.

Sense organs also play a crucial role in the essay by Paolo Savoia, "Seeing and Hearing," albeit in a different context, that of psychoanalysis. Savoia explores the different modalities of supporting evidence for an objective state of hysteria in the works of Charcot and Freud respectively. While the Parisian puts images (thus "seeing") as objective records of specific patients' gestures at the center of his practice, the Viennese is committed to a different form of investigation, one in which the psychoanalyst concentrates on hearing the patients' stories, but is also forced to put into play his own unconscious to interpret those stories. While the unconscious might seem to be something incommunicable by its own nature, Freud's strategy is to try to make it communicable (and hence objective) by individuating the structural similarities characterizing the human mind. The techniques underlying these two types of objectivity rely on different sets of epistemic norms and regimes of scientific observation, which identify and stabilize a new scientific object: the psychoanalytic self. Thus, the context of the emergence of psychoanalysis provides a fascinating case epitomizing what Daston and Galison refer to as the passage from mechanical objectivity, which came to prominence in the second half of the nineteenth century, to structural objectivity, which begins to take hold around the beginning of the twentieth century. This passage paradigmatically embodies restriction in scientific claims about incommunicable experience, in order to limit the idiosyncrasies of the scientific self.

While the previous essays in this section focus on different conceptions of the knowing self and how it could overcome its own subjectivity, Alex Csiszar's paper, "Objectivities in Print," examines the emergence of a new *communal* conception of scientific objectivity in the late nineteenth century. Csiszar presents a study in which the late nineteenth-century conception of knowledge as objective and the epistemic virtues associated with objectivity become distinctive traits of collectives instead of individuals. As he underlines, rather than stemming from internal needs of scientific practitioners to provide a stabilized notion of objectivity, this social conception of objectivity derives from wider concerns about the place that the scientific enterprise ought to occupy among the political and social institutions of which the scientists were part. Csiszar considers two fundamental moments in the development of a form of communitarian objectivity that have received insufficient attention from historians, but that are crucial in tying the processes of scientific publishing with normative commitments about scientific objectivity: the origin of the practice of peer review in 1830s England and the progressive stabilization of specialised periodicals as the hub encompassing and rationally coordinating collective scientific opinions. The epistemic and social motives that drove both moments find exemplary expression in Henri Poincaré's editorial activities and epistemological reflection.

The final section of the volume features three contributions more specifically dealing with a type of objectivity that trains its concerns upon the epistemic practices of communities. In an ambitious and programmatic paper, "Objectivity, Intellectual Virtue, and Community," Moira Howes advocates a novel approach for addressing the issue of objectivity. Howes rejects individualistic conceptions of objectivity, arguing that objectivity is best understood as a community-wide *intellectual virtue*, viz., an enduring commitment to salient and accurate information about reality. From this perspective, she discusses the social dimensions of intellectual virtues (e.g., how virtues are shaped by social context) as well as the relationship between objectivity and epistemic trustworthiness. Howes maintains that conceptualizing objectivity as a community-wide intellectual virtue allows us to better appreciate what failures of objectivity—such as epistemic failures due to implicit bias—amount to and how they might be avoided. The resulting analysis provides a strong case for utilizing resources from virtue epistemology to contribute to our understanding of what objectivity is and how it can be facilitated.

The final two essays of the volume exemplify Ian Hacking's favored particularist approach to objectivity insofar as they draw conclusions about the objectivity of scientific communities through detailed analyses of particular cases. Alison Wylie's essay, "A Plurality of Pluralisms: Collaborative Practice in Archaeology," engages with Helen Longino's influential work that emphasizes the importance of pluralism as a procedural means for ensuring the objectivity of science. Through a close examination of collaborative practices in archaeology, Wylie articulates different ways in which methodological pluralism can serve as an effective means for improving the objectivity of science. In contrast to narratives that place archaeologists and indigenous communities—with their divergent values and interests—in a relationship of inherent conflict, Wylie documents ways in which collaborations between archaeologists and indigenous communities have served to enhance archaeological research. For instance, she discusses how the interaction between conventional archaeological evidence and evidence gained from indigenous oral traditions can address (and sometimes reframe) focal archaeological questions in beneficial ways. The kinds of collaborations discussed in Wylie's chapter demonstrate concretely how the pluralism advocated by Longino and others can facilitate *transformative criticism*, i.e., criticism that can transform background values and assumptions. More generally, her work shows how resources from standpoint theory can be deployed within the framework of Longino's feminist empiricism.

Judy Segal's concluding essay, "The View from Here and There," examines how her own objectivity as a researcher on breast-cancer narratives changed after she was diagnosed with breast-cancer. A common trope in the medical humanities is to try to foster a form of patient autonomy by claiming that while doctors are objective experts of disease, patients are experts in the subjective experience of illness. Segal argues that this trope fails to illuminate the actual changes in her research as her perspective shifted from the objectivity of the scholarly stance—a neutral and disinterested observer who was removed from her object of study—to a new perspective that included her role as a patient. Far from being a position of pure subjectivity, Segal argues that the patient-perspective is an additional scholarly resource that "aspires to a higher objectivity: a nearer view, not aperspectival, but with standpoint." In articulating this argument, Segal draws upon Sandra Harding's standpoint theory, suggesting that her objectivity as a researcher of breast-cancer narratives was enhanced after her diagnosis since she was able to occupy a standpoint inside the specific medical-institutional position at issue and this, in turn, allowed her to view the position (and its assumptions) more critically. In addition to demonstrating that a simplistic objective-subjective divide is unhelpful for understanding the epistemic status of patient narratives, Segal presents a strong case for adopting a science and technology studies approach for studying these narratives.

Taken individually, the essays in this volume each supply new tools for theorizing what is valuable in the pursuit of objective knowledge and new ways to investigate its historical career. We see invitations to comparative projects in the history of journalism and science, or the history of science and philosophy, or the history of scientific publication. We see deployment of theoretical frameworks from philosophy to illuminate the proper standards of objective knowledge in medicine and archaeology, or the position of the theorist with respect to her own object of study. The volume offers many starting places and many avenues of research. More than that, taken collectively, the essays exemplify the very virtues of objectivity that they theorize—in reading them together one can sense various worries about the dangerously subjective in our age and the past, locate commonalities of concern as well as differences of approach, and see objects of intense scrutiny and begin to discern areas that have not yet received the attention they deserve. By putting together work that is not easily combined, the volume offers an expansive vision of a research community seeking a communal understanding of its own methods and its own epistemic anxieties, struggling to enunciate the key problems of knowledge of our time, and offering insight into how to overcome them.

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References

Alcoff, Linda, and Elizabeth Potter, eds. 1993. *Feminist epistemologies*. New York: Routledge.

Angell, Marcia. 2004. *The truth about the drug companies: How they deceive us and what to do about it*. New York: Random House.

Antony, Louise M. 1993. Quine as feminist: The radical import of naturalized epistemology. In *A mind of one's own: Feminist essays on reason and objectivity*, eds. Louise M. Antony and Charlotte E. Witt, 185–226. Boulder: Westview Press.

Ceci, Stephen J., and Wendy M. Williams. 2011. Understanding current causes of women's underrepresentation in science. *Proceedings of the National Academy of Science* 108: 3157–3162.

Chandler, James, Arnold I. Davidson, and Harry D. Harootunian, eds. 1994. *Questions of evidence: Proof, practice, and persuasion across the disciplines*. Chicago: University of Chicago Press.

Code, Lorraine. 1991. *What can she know? Feminist theory and the construction of knowledge.* Ithaca: Cornell University Press.

Collins, Harry M., and Trevor Pinch. 1993. *The Golem: What everyone needs to know about science.* Cambridge: Cambridge University Press.
Daston, Lorraine. 1991. Baconian facts, academic civility, and the prehistory of objectivity. *Annals of Scholarship* 8: 337–364.
Daston, Lorraine. 1993. Marvelous facts and miraculous evidence in early modern Europe. In ed. James Chandler et al. 1993, 243–274.
Daston, Lorraine. 1994. Historical epistemology. In *Questions of evidence: Proof, practice, and persuasion across the disciplines*, ed. James Chandler, Arnold I. Davidson, and Harry D. Harootunian, 282–289. Chicago: University of Chicago Press.
Daston, Lorraine, and Peter Galison. 1992. The image of objectivity. *Representations* 40: 81–128.
Daston, Lorraine, and Peter Galison. 2007/2010. *Objectivity*. New York: Zone Books.
Dear, Peter. 1995. *Discipline and experience: The mathematical way in the scientific revolution*. Chicago: University of Chicago Press.
Douglas, Heather E. 2009. *Science, policy, and the value-free ideal*. Pittsburgh: University of Pittsburgh Press.
Duhem, Pierre. 1906/1954. *The Aim and Structure of Physical Theory*. Trans. Philip P. Wiener. Princeton: Princeton University Press.
Earman, John. 1993. Underdetermination, realism, and reason. *Midwest Studies in Philosophy* 18: 19–38.
Elliott, Carl. 2010. *White coat, black hat: Adventures on the dark side of medicine*. Boston: Beacon Press.
Gillies, Donald. 1993. *Philosophy of science in the twentieth century: Four central themes*. Oxford: Blackwell.
Gross, Paul R., and Norman Levitt. 1994. *Higher superstition: The academic left and its quarrels with science*. Baltimore: Johns Hopkins University Press.
Hacking, Ian. 1999. *The social construction of what?* Cambridge, MA: Harvard University Press.
Haraway, Donna. 1988. Situated knowledges: The science question in feminism and the privilege of partial perspective. *Feminist Studies* 14: 575–599.
Harding, Sandra, ed. 1976. *Can theories be refuted? Essays on the Duhem-Quine thesis*. Dordrecht: D. Reidel.
Harding, Sandra. 1986. *The science question in feminism.* Ithaca: Cornell University Press.
Harding, Sandra. 1991. *Whose science? Whose knowledge? Thinking from women's lives*. Ithaca: Cornell University Press.
Harding, Sandra. 1992. After the neutrality ideal: Science, politics, and 'Strong Objectivity'. *Social Research* 59: 567–587.
Harding, Sandra. 1993. Rethinking standpoint epistemology: 'What is Strong Objectivity'? In *Feminist epistemologies*, eds. Linda Alcoff and Elizabeth Potter, 49–82. New York: Routledge.
Healy, David, and Dinah Cattell. 2003. The interface between authorship, industry, and science in the domain of therapeutics. *British Journal of Psychiatry* 182: 22–27.
Hempel, Carl G. 1983. Kuhn and Salmon on rationality and theory choice. *Journal of Philosophy* 80: 570–572.
Hoefer, Carl, and Alexander Rosenberg. 1994. Empirical equivalence, underdetermination, and systems of the world. *Philosophy of Science* 61: 592–607.
Intemann, Kristen. 2005. Feminism, underdetermination, and values in science. *Philosophy of Science* 72: 1001–1012.
Jay, Martin. 2005. *Songs of experience: Modern American and European variations on a universal theme*. Berkeley/Los Angeles: University of California Press.
Keller, Evelyn Fox. 1985. *Reflections on gender and science*. New Haven: Yale University Press.
Kincaid, Harold, John Dupré, and Alison Wylie, eds. 2007. *Value-free science? Ideals and illusions.* New York: Oxford University Press.
Kitcher, Philip. 1995. *The advancement of science: Science without legend, objectivity without illusions*. Oxford: Oxford University Press.
Kitcher, Philip. 2001. *Science, truth, and democracy*. New York: Oxford University Press.

Koertge, Noretta, ed. 2000. *A house built on sand: Exposing postmodernist myths about science*. Oxford: Oxford University Press.

Kuhn, Thomas S. 1977. Objectivity, value judgment, and theory choice. In *The essential tension: Selected studies in scientific tradition and change*, 320–339. Chicago: University of Chicago Press.

Kukla, André. 1993. Laudan, Leplin, empirical equivalence, and underdetermination. *Analysis* 53: 1–7.

Lacey, Hugh. 1999. *Is science value-free? Values and scientific understanding*. London: Routledge.

Latour, Bruno. 1999. *Pandora's hope: Essays in the reality of science studies*. Cambridge, MA: Harvard University Press.

Latour, Bruno, and Steve Woolgar. 1979. *Laboratory life: The construction of scientific facts*. Beverly Hills: Sage.

Laudan, Larry. 1984. *Science and values: The aims of science and their role in scientific debate*. Berkeley: University of California Press.

Laudan, Larry. 1990. Demystifying underdetermination. In ed. Wade C. Savage, 1990, 267–297.

Laudan, Larry, and Jarrett Leplin. 1991. Empirical equivalence and underdetermination. *Journal of Philosophy* 88: 449–472.

Laudan, Larry, and Jarrett Leplin. 1993. Determination undeterred: Reply to Kukla. *Analysis* 53: 8–16.

Leiserowitz, Anthony A., Edward W. Maibach, Connie Roser-Renouf, Nicholas Smith, and Erica Dawson. 2013. Climategate, public opinion, and the loss of trust. *American Behavioral Scientist* 57: 818–837.

Lloyd, Elisabeth. 1996. Science and anti-science: Objectivity and its real enemies. In *Feminism, science and the philosophy of science*, eds. Lynn Hankinson Nelson and Jack Nelson, 217–259. Dordrecht: Springer.

Longino, Helen E. 1988. Review essay: Science, objectivity, and feminist values. *Feminist Studies* 14: 561–574.

Longino, Helen E. 1990. *Science as social knowledge: Values and objectivity in scientific inquiry*. Princeton: Princeton University Press.

Longino, Helen E. 1993. Subjects, power, and knowledge: Description and prescription in feminist philosophies of science. In *Feminist epistemologies*, eds. Linda Alcoff and Elizabeth Potter, 101–120. New York: Routledge.

Longino, Helen E. 2002. *The fate of knowledge*. Princeton: Princeton University Press.

Machamer, Peter, and Heather Douglas. 1998. How values are in science. *Critical Quarterly* 40: 29–43.

Machamer, Peter K., and Gereon Wolters, eds. 2004. *Science, values, and objectivity*. Pittsburgh: Pittsburgh University Press.

McMullin, Ernan, ed. 1988. *Construction and constraint: The shaping of scientific rationality*. Notre Dame: University of Notre Dame Press.

McMullin, Ernan. 1993. Rationality and paradigm change in science. In *World changes: Thomas Kuhn and the nature of science*, ed. Paul Horwich, 55–78. Cambridge, MA: MIT Press.

Montford, A.W. 2010. *The hockey stick illusion: Climategate and the corruption of science*. London: Stacey International.

Newton-Smith, William. 1980. The underdetermination of theory by data. In *Rationality of science: Studies in the foundations of science and ethics*, ed. Risto Hilpinen, 91–110. Dordrecht: D. Reidel.

Okruhlik, Kathleen. 1994. Gender and the biological sciences. *Canadian Journal of Philosophy* 20 (suppl): 21–42.

Oreskes, Naomi, and Erik M. Conway. 2010. *Merchants of doubt: How a handful of scientists obscured the truth on issues from tobacco smoke to global warming*. New York: Bloomsbury Press.

Poovey, Mary. 1998. *A history of the modern fact: Problem of knowledge in the sciences of wealth and society*. Chicago: University of Chicago Press.

Popper, Karl R. 1972. *Objective knowledge: An evolutionary approach*. Oxford: Clarendon Press.

Porter, Theodore M. 1995. *Trust in numbers: The pursuit of objectivity in science and public life*. Princeton: Princeton University Press.

Proctor, Robert N. 1991. *Value-free science? Purity and power in modern knowledge.* Cambridge: Harvard University Press.

Proctor, Robert N. 2012. *Golden holocaust: Origins of the cigarette catastrophe and the case for abolition*. Chicago: University of Chicago Press.

Quine, Willard V. 1951/1980. Two dogmas of Empiricism. *The Philosophical Review* 60: 20–43. Repr. in *From a logical point of view*, 2nd ed., 20–46. Cambridge, MA: Harvard University Press.

Ruse, Michael, and Robert J. Richards, eds. 2008. *The Cambridge companion to Darwin*. Cambridge: Cambridge University Press.

Ryghaug, Marianne, and Tomas Moe Skjølsvold. 2010. The global warming of climate science: Climategate and the construction of scientific facts. *International Studies in the Philosophy of Science* 24: 287–307.

Salmon, Wesley C. 1980. Rationality and objectivity in science or Tom Kuhn meets Tom Bayes. In *Scientific theories. Minnesota studies in the philosophy of science 14*, ed. Wade C. Savage, 175–204. Minneapolis: University of Minnesota Press.

Savage, Wade C. ed. 1980. *Scientific theories. Minnesota studies in the philosophy of science* 14. Minneapolis: University of Minnesota Press.

Shapin, Steven. 1995. Here and everywhere: Sociology of scientific knowledge. *Annual Review of Sociology* 21: 289–321.

Shapin, Steven. 2010. Science and the modern world. In *Never pure: Historical studies of science as if it was produced by people with bodies, situated in time, space, culture, and society, and struggling for credibility and authority*, 377–391. Baltimore: The Johns Hopkins University Press.

Solomon, Miriam, and Alan Richardson. 2005. Essay review: A critical context for Longino's critical contextual empiricism. *Studies in History and Philosophy of Science* 36: 211–222.

Sonnert, Gerhard, and Gerald J. Holton. 1995. *Who succeeds in science? The gender dimension*. New Brunswick: Rutgers University Press.

Stanford, Kyle P. 2001. Refusing the devil's Bargain: What kind of underdetermination should we take seriously? *Philosophy of Science* 68: S1–S12.

Stanford, Kyle P. 2006. *Exceeding our grasp: Science, history, and the problem of unconceived alternatives*. Oxford: Oxford University Press.

Tuana, Nancy, ed. 1989. *Feminism & science*. Bloomington: Indiana University Press.

Part I
Positions on Objectivity in Contemporary Science and Technology Studies

Chapter 2
Let's Not Talk About Objectivity

Ian Hacking

The first landmark event in twenty-first-century thinking about objectivity was, as is well known, Lorraine Daston and Peter Galison's *Objectivity* (2007). It is a magisterial historical study of an epistemological concept, namely "objectivity". Hence I call it a contribution to what I call (if it wants a name) meta-epistemology, although other prefer the less apt but better sounding historical epistemology. You will find more food for thought, not to mention headaches, in their book, than in any other body of work about objectivity.

It will seem that *Objectivity* rejects the injunction stated in my title, for what is the book about, but objectivity? The authors are talking about objectivity if anyone is! Analytic philosophers like myself make what others regard as too many distinctions, and I shall illustrate that here.

Objectivity is about the concept of objectivity, its past uses, and the practices associated with it. For me, a concept is a word in its sites (Hacking 1984). In this context, that means the sites in which words cognate with "objective" were used over the past three centuries, the practices within which they were deployed, who had authority when using them, the actual modes of inscription, which in this case is closely associated with the use of pictures and other types of images. For me, as for a builder, a site is a rich field of activity to be described from many points of view, almost innumerable perspectives. *Objectivity* is a triumph of that type of analysis; it is not talking about objectivity but about the concept of objectivity (a distinction we most clearly owe to Gottlob Frege writing about number). Hence, to finesse the issue in a deplorably dishonest way, it does not violate my injunction, not to talk about objectivity. It talks about the concept. To counterbalance that reading, Galison's own view of what he and Daston were doing is lucidly presented in his contribution to this volume.

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Now I shall try to explain what I mean by my injunction not to talk about objectivity. I shall do so by expanding the abstract. The abstract is not the usual rather hasty summary of what one has said, but (including the title) a sequence of ten assertions or injunctions, each of which I shall explain. Hence there are ten numbered sections below. I shall conclude with Sect. 2.11, an empirical test case proposed by the editors of the present book.

2.1 Title: Let's Not Talk About Objectivity

We are often offered specific questions that fall under the heading of objectivity. Two used in an announcement which motivated this book are: "Can we trust medical research when it is funded by pharmaceutical companies?" And, "whose research in climate science meets the standards of scientific objectivity?"

The first question is an excellent one. It does not mention objectivity. It is a ground-level question. The second question is a second-story question (European first-story), couched in terms of "standards of scientific objectivity." In *The Social Construction of What?* I spoke of "elevator words" (Hacking 1999: 22). They are words used for what Quine called semantic ascent, words such as "true" and "real". Instead of saying that the cat is on the mat, we move up a story and say that it is true that the cat is on the mat. That is a statement about a statement. "Objective" is an elevator word. My title amounts to this: Let us stick to ground-level questions. Ascending to the second story and posing a question in terms of scientific objectivity does nothing to help us with a ground-level question, such as one about research in climate science.

Notice that in this typology, the book by Daston and Galison is a *third*-story investigation. It is the study of a concept, which, as stated earlier, for me means the study of a word in its sites, in this case, of an elevator word in its sites.

My title was in part a response to a version of an advertisement for a conference held in the summer of 2010. I recall reading:

OBJECTIVITY IN SCIENCE
WHAT IS IT?
WHY DOES IT MATTER?

The question, "what is it?", invites us to suppose there is an "it" there, something the philosopher or other analyst can contemplate and define. We have been down that road over and over again. The *locus classicus* is Socrates, who asks Euthyphro,

And what is piety, and what is impiety?

Playing only slightly with published translations, you can get Socrates to ask, "Piety: What is it? Why does it matter?" It is widely taught in freshman philosophy courses that you should not pose "it" questions like this, for they "reify" piety. It is also widely taught in slightly more advanced courses, that Socrates demonstrates that the search for a definition by necessary and sufficient conditions is wrong-headed;

piety is a "family-resemblance concept". I mention these matters not because I agree (or not) with that common wisdom, but to remind ourselves how close to the shoals we come, just in stating the topic of this volume. There are many important issues on the agenda for this book. I propose that almost all of them are better addressed without even mentioning objectivity, and hence, not asking what "it" is.

2.2 The Trajectory of Objectivity, as an Idea, Is the Triumph of Bumbling Public Good Sense Over Great but Bad European Philosophy (Descartes, Kant)

I will divide this assertion in two, first the philosophers, and then the public good sense.

2.2.1 (a) The Philosophers

It has become well known that "objective" and "subjective" almost entirely reversed their meanings between the time of Duns Scotus and the middle of the nineteenth century. Descartes was the last of the old guard, when he wrote of "objective reality." Alone among translators, Anscombe and Geach said that they did not even try "to translate the scholastic terms literally; they had degenerated to mere jargon by Descartes's time, and literal translation would be nonsense to modern readers." (Descartes 1954/1970: 81, footnote.) We might speak less of a reversal than of a loss of meaning,

It is often said that Kant effected the reversal. In fact, "It is to Baumgarten that we owe the modern distinction between 'subjective' and 'objective,' which had the opposite meanings to their present ones as late as Descartes." (Beck 1969: 284) Alexander Gottlieb Baumgarten (1714–1762) was the most distinguished pupil of Christian Wolff (1679–1754), who in turn was the most illustrious German philosopher between Leibniz and Kant. Kant used Baumgarten's *Metaphysics* as the set text on which he based his lectures for decades. Baumgarten is remembered for having given the word "aesthetic" its modern sense, and perhaps to have founded aesthetics as a discipline. Kant notoriously used that word in two very different ways in the first and third *Critique*. It is less often noticed that Kant likewise used objective/subjective differently in the first two *Critiques*. This has made for a lot of confusion about the use of the words.

Baumgarten was concerned with the difference between subjective and objective judgements of taste and value, for which he created the new terminology. Kant followed Baumgarten in the *Critique of Practical Reason* (1788/1949: Part I, Book I, Chapter 1, §1): "Practical principles [. . .] are subjective, or maxims, when the condition is regarded by the subject as valid only for his own will. They are objective, or practical, when they are recognized as objective, i.e. as valid for the will of every rational being." We are at once led to imperatives (and thus to the Categorical Imperative), which are objective, and obtain for all rational agents, as opposed to maxims, which are subjective, and apply only within an agent's system of beliefs and desires. "Objective" here means inter-subjective, which is one of its meanings today. Kant did up the ante, because for him objectivity meant super-duper obligatory inter-subjectivity. The term is clearly to be used in moral valuation. There is no ground for confusion here. Kant said what he meant.

It is far less clear what Kant meant by "objective" in the first *Critique*. It is not obvious to me that it is used in an evaluative way, but many readers find it so. The word seems to be used in a number of ways, all different from that of the second *Critique*. If in purgatory one were assigned the task of re-translating *The Critique of Pure Reason*, one might follow Geach and Anscombe's lead, and avoid literal translation (viz. replication) of that word. But when it came to the *Critique of Practical Reason*, one would do what everyone does, and translate the German word by its English homonym.

I maintain (Dear et al. 2012) that in English, (I do not say in German), objective/subjective were neutral, and not evaluative, terms, until around 1850. That is when Daston and Galison urge that what they call mechanical objectivity emerged. It is as if the words were there awaiting a rhetorical use, first as expressing, "the insistent drive to repress the wilful intervention of the artist-author, and to put in its stead a set of procedures that would, as it were, move nature to the page through a strict protocol, if not automatically." (Daston and Galison 2007: 121) Objective came to mean: no intervention of the subject, the artist-author. It did not denote a positive quality, but rather the absence of one that was deemed to be negative. And by extension (I argue) it picked up the rejection of other vices, such as deliberate bias, not listening to criticism. That result is summed up in assertion Sect. 2.6 below, that objectivity is not a virtue but the absence of various types of vice.

But the writings of the philosophers, and the adoption of their word by the scientists of 1850, does not cover the waterfront. I think Daston and Galison were right to emphasize the way that objectivity pursued its own career in the sciences, but there is more to the story than that. That leads me to the first part of assertion Sect. 2.2 of my abstract, the bumbling public.

2.2.2 (b) Bumbling Good Sense

To call the public bumbling is not, for me, to insult it. We Canadians make a sort of virtue of muddling through, a practice that we adopted from the British. We are unlike the French, Americans, and Soviets, who have or had great faith in reason. Unlike those rationalists, we have to get along with muddle, bit by bit. It is incomprehensible to foreigners that our Constitution (The Charter of Rights and Freedoms) has what we call a "notwithstanding" clause, that any province can opt out of any clause in the constitution, for a period of time, notwithstanding the fact that the clause is ordained in the constitution. I am not asking you to subscribe to this ethos of bumbledom, only explaining that I live with it, and think of it as a convenient device by which civic life can be made more civil.

Bumbledom is connected to an important part of the objectivity story that was well known to Daston and Galison, but to which they barely allude in footnotes. It is best learned from Theodore Porter's (1995) *Trust in Numbers: The Pursuit of Objectivity in Science and Public Life*. He addresses the important question of why we came, in the nineteenth and early twentieth century, to make such a great use of numbers when making decisions. To simplify his complex narrative immensely, it is because we think we can trust numbers when we cannot trust our fellows.

One can read into Porter what an anthropologist would call a functionalist explanation of our persistent reliance on quantification in all matters, public and private. I am not attributing functionalism to Porter, only using, or misusing, his work as evidence for a functionalist story. There are two sides to a functionalist explanation. (a) It explains the presence of a practice or custom in a society by arguing that the society would fall apart without that practice. (b) Members of the society do not give that explanation as a reason for following that practice.

Trust in Numbers invites an account of the role of quantification in democracy. In an (ideal) authoritarian state, decisions are by decree. In an (ideal) democratic organization, decisions are by consensus. The (ideal) democrat has to resort to facts in order to argue for policies. But what are the facts? They are trammelled only if there are protocols for determining them, and there are measures, comparisons, and quantities to hand. The democratic form of society needs numbers to trust; otherwise it would reduce to shouting or shooting.

This is a functionalist explanation, by criteria (a) and (b) above—even if a prudent historian of science like Porter would reject functionalist explanations. Relatively non-authoritarian polities in an increasingly technological world needed to resort to numbers in order to reach consensus without firearms. But (b) they did not know that was why they were doing it. Numbers, one might say, are the necessary form for a democracy in a technologically advanced and increasingly information-rich society such as was emerging from the industrial revolution. Numbers were a bumbling solution to a new problem—and it has done us a world of good, even though we did not know what we were doing.

It is not a final solution. We get to the next question in a sort of regress—whose numbers should we trust? That is always a serious question, one that goes behind the façade of so-called "objectivity". It is the topic of the research of Naomi Oreskes, who is a world expert on what could be called the "applied theory of objectivity." By that I mean public issues where the objectivity of research is called into question. At present, the most pressing example may be global climate change, on which she is something like *the* meta-expert, that is, not an expert on climate change, but an expert on experts about climate change. She does not talk about objectivity; she assesses the objectivity of experts (Oreskes and Conway 2010).

For a less practical but eminently philosophical discussion of trust in the public domain, see for example Scheman (2001). My test case, Sect. 2.11 below is one in which issues of trust importantly arise, as we shall see in due course.

My use, or abuse, of Porter's book explains why I put assertion Sect. 2.3 into my abstract.

2.3 The Public in Question Is Primarily That of Querulous Western Democracies as They Entered the Age of Technocracy, and It Did a Good If Unplanned Job of Dealing with Novelty

Now I pass to the fourth assertion, which does not require comment, given my preference for ground-level discourse, and distrust of elevator words that generate grandiose important-sounding but idle controversies.

2.4 It Is Often Hard to Be Objective in the Face of a Real-Life Debate, but There Is No Problem About Objectivity Itself—Except What Is Foisted on It By Highbrow Idealization and Misguided Polemics

2.5 The Adjective "Objective" Does the Work for the Abstract Noun, but in a Negative Way: In Any Single Situation, One or More of the Host of Ways to Fail to Be Objective Is What Matters

This idea revives a maxim of the Oxford linguistic philosophers of long ago, in particular, J. L. Austin and Gilbert Ryle. One should be wary of fancy words conceived in philosophical sin—rationality and reality, for example. Objectivity is among them. One Oxford maxim was to avoid the nouns and attend to the adjectives. Recall Austin's essay on Truth, which in turn begins by quoting Francis Bacon's famous essay on Truth: "What is truth, said jesting Pilate, and did not stay for an answer. Pilate [Austin continued] was a man ahead of his time." (Austin 1961: 117) The paragraph concludes with a pun with serious intent: "*In vino veritas*, perhaps, but in sober philosophical discussion, *verum*." Don't talk about truth, talk about the word "true" and its uses. Mutatis mutandis, we should attend to the adjective "objective", and not to objectivity.

A second maxim is to think of adjectives, such as "real" or "rational," in terms of what is *not* real or what is *not* rational. Daston and Galison described their book as about "objectivity in shirt sleeves," an agreeable old-fashioned image. In the old days, it was working men who took off their jackets and got down to work. The trope recalls Austin's own sartorial metaphor, whose sexism (of days gone by) is even more evident. In *Sense and Sensibilia* Austin (1962) called the adjective "real" a trouser word: in any particular case, it is failing to be real that "wears the trousers". In context what is *not* real determines the force of what is said. We do say that we were given real cream, but that is to say, in context, that it was *not* adulterated, or was *not* Coffeemate or some similar synthetic stuff, etc. A real duck, as Austin observed, is not a decoy, although for dinner one might be served real duck, as opposed to disguised turkey or something gamey like hare. Ryle taught that "rational" is also something of a trouser word. Except by way of a joke I would not say that my sensible aunt was a rational woman, but I would say that my elderly uncle was irrational when it came to his predilection for young ladies.

Likewise, consider a list of various connotations of objectivity: disinterestedness, emotional detachment, rule-governed procedures, quantitative methods, openness to criticism, responsiveness to evidence, or accountability to a mind-independent reality, among others. I take these from an unpublished talk by Joseph Rouse (2008). He grouped them under what he called "Epistemic Objectivity", as opposed to "Conceptual Objectivity". All but the first (disinterestedness) are positive. In my view, that gets things backwards. Let us accentuate the negative. In different contexts, to be objective is not to allow one's interests to intervene (be disinterested), not to be emotional, not to ignore evidence, not to proceed by whim, not to ignore criticism. Even the quantitative here had negative force, for it is not to be merely qualitative. Porter has of course told us why we want numbers—impartial numbers, those that are not partial to one side or the other.

Unlike so many contemporary authors I avoid discussions of "methodology" to the point that many find it difficult to figure out what I am doing. (It is striking that in a recent discussion of Daston and Galison's book (Dear et al. 2012), three historians discuss its methodology, whereas the philosopher—me—looks at the history!) Methodological purists will sneer that it is mere confusion to run Austin and Galison in the same stable. I shall briefly suggest how several strings are tied together.

Austin taught us how to study words in their sites, but despite his personal mastery of ancient philosophy, he examined a limited range of sites in the present. In the past 20 years or so "practice" has been the rallying trumpet of many invaluable contributions to science studies, so let it be noticed that Austin was there much earlier: *How to do things with words*. Unfortunately when Austin was picked up by Jacques Derrida and John Searle, the *doing* in Austin faded from notice. As has often been noticed, the advocacy of St. Paul was not a good thing for the truth of gospel, though it was brilliant public relations.

Michel Foucault taught us to make the past the history of the present, so that the sites in which a word is used extended to an endless collection of *énoncés* (things actually said, written, inscribed) as preserved in what he called the *archive*, which for him was not only a dusty archive of old papers, but also the *arché* in *archéologie*, namely the origin, source, or spring which embodied the organizing principles of the words in particular sites. For me, *Objectivity* and J.L. Austin are linked in the matrix of archaeology. I know that this is an eccentric way of thinking, and I would not urge it on others, but it seemed prudent to insert these words of explanation here.

The observations of this section, prior to this methodological aside, reduce to assertion Sect. 2.6, which states the cardinal thesis of this paper. I derived it, as stated in Sect. 2.2 above, from Daston and Galison's discussion of mechanical objectivity, but I am sure they do not make the same use of their analysis as I do with this global enunciation:

2.6 Objectivity Is Not a Virtue: It Is the Proclaimed Absence of This or That Vice

That thought leads immediately to a more specific observation:

2.7 When Public Virtues Compete—Evidence-Based Versus Clinical Medicine, for Example—We Need to Think Harder, Not More Objectively

On this specific example, Miriam Solomon's (2011a, b) work on evidence-based medicine sets exactly the right agenda. It is no good using the second-story argument, "We should use evidence-based medicine because it is objective." We need to argue the merits of evidence-based medicine. Richard Horton, the editor of *The Lancet*, the premier British medical journal, has long defended the virtues of clinical medicine over against the current demand for hegemonic evidence-based medicine. It is no good telling him he is not objective. I do not presume to judge whether he is right or wrong, but "subjectivity" is not among his failings.

Horton, incidentally, furnishes another example. On 14 October 2006, *Lancet* published a study by a group from Johns Hopkins estimating the number of deaths caused by the invasion of Iraq at more than 600,000 (Burnham et al. 2006).

Horton personally defended the paper and used it as the basis for an attack on American (and British) Iraq policies. This produced an immense outcry, accusations of all kinds of lack of objectivity. For a summary of reactions, and a transcript of one debate, see the *Washington Post* stories (last accessed 28 November 2011):

http://blog.washingtonpost.com/worldopinionroundup/2006/10/is_iraqs_civilian_death_toll_h.html

http://www.washingtonpost.com/wp-dyn/content/discussion/2006/10/18/DI2006101801279.html

The last mentioned is a fascinating ground-level debate, with the lead author of the paper, Gilbert Burnham, responding to a long series of questions. They are mostly about the nitty-gritty of the statistical analysis. He did make clear he has an interest in the question; he is worried about the number of lives lost. Horton, the editor, made plain that he is very interested, and is totally opposed to British involvement in the war against Iraq. But the ground-level questions are not usefully discussed in terms of objectivity, even if opponents chant "you are not being objective." It is Burnham's or Horton's numbers that should be challenged, not objectivity.

2.8 When Objectivity Is Declared to Be the Cardinal Virtue of Science, It at Once Gets Bashed (Rightly) —

Recall the culture wars, the science wars, of the mid-1990s. Some on the scientistic side maintained against their opponents that science is objective. Of course that did no good at all. Second-story defence never does. They should have been defending bits and pieces of the sciences, not invoking a pseudo-explanation of their value. It immediately prompted an equally misguided response, namely new accounts of what objectivity *is*. Daston and Galison have some sharp observations about why the radical new accounts of objectivity are working on the same plane as what they oppose.

Very interesting questions about the sciences do arise, but they should not be addressed in terms of objectivity. Thus we encounter, for example, "questions of objectivity in collaborative aboriginal research" (see Alison Wylie's essay in this volume). That is already loaded, for the collaboration is between aboriginal knowledge and the sciences; the objectivity of the sciences is not in question, but that of aboriginals is. And there are often matters of joint concern between commerce engendered by technoscience—salmon farming, to take the example of Sect. 2.11 below—and aboriginal interests—they have both traditional and legal rights to the salmon in question.

Before turning to salmon, I shall give another aquatic example concerning traditional knowledge, not fish but eels. When I worked in Cambridge, England, I had some colleagues who were trying to figure where on Earth a particular type of eel reproduced—the eels would swim to England from the Sargasso Sea, and months later reappear with a whole lot of young; then they would head out to mid-Atlantic again. I mentioned this to an old countrywoman in my village, who had grown up in intense poverty on the Essex marshes. "I know exactly where they breed," she said. I passed this on to the experts who thought it was a good joke, until finally at a time proposed by Vera I took two of them and her in my rattletrap car down to a particular spot where she had lived as a child—and before our eyes was a mass of slithery baby eels of just the right species. Note that "objectivity" is totally irrelevant to this anecdote.

Hence I suggest that a fitting topic for active reflection is collaboration between aboriginal and scientific modes of knowing, but not "questions of objectivity in collaborative aboriginal research."

2.9 When Objectivity Is Declared to Be the Cardinal Virtue of Science It at Once Gets Bashed—Or Else [I Continued] Abused (Deservedly), as in "NAOS: The National Association for Objectivity in Science"

Before I turn to NAOS, I wonder if anyone trolling the Internet found Objectivity, Incorporated? Objectivity, Inc. is a Silicon Valley company, that is, in Sunnyvale, California. When accessed in June 2010, http://www.objectivity.com/ declared that "We are the leader in scalable database management solutions for mission-critical, real-time and distributed applications." And what does that mean? For example, to quote again, it offers "an innovative new crime prevention and counter-terrorism tool for members of the intelligence community". The "flagship product" here "analyzes more than 100 million phone calls made by more than 1 million individuals to find the relationships between them, up to five degrees of separation - in less than 60 s on a standard laptop." What on earth has this to do with objectivity? Well perhaps it is Porter's trust in numbers taken to absurdity. At any rate you cannot imagine this corporation naming itself "Subjectivity, Inc."

In my opinion, the National Association for Objectivity in Science is a perfect illustration of the fact that the invocation of objectivity gets you nowhere. Objectivity, for NAOS, is about criticizing what it calls the theory of macroevolution, viz. what it defines as (1) "The first form of life came into existence through the random interaction of molecules in a 'primordial soup'" and (2) "All forms of life thereafter have come into existence through the operation of natural selection on randomly-produced genetic mutations." This theory, NAOS says, "has potentially disastrous social consequences," namely:

> most high school and university biology textbooks used in the United States do not present any scientific arguments against the theory of macroevolution. The failure to do so may lead a student to conclude that he or she is in fact the result of random, chance processes, and has not been created or designed for any special purpose. This in turn can have a devastating impact on the student, leading him or her to devalue human life and possibly engage in drug abuse, sexual promiscuity, or violence, or even commit suicide.

The primary purpose of NAOS, "is to promote objectivity in the teaching of the theory of macroevolution." In my opinion, if one wants to argue with these people, it would be a bad mistake to cavil about "objectivity". Instead, get some facts. Are there textbooks that preach this garbled primordial soup theory as fact? My son, who is a middle school principal in California, says that his biology teachers could find no such text in use by the public schools of that state.

To delve into detail, let's get the primordial soup and random mutation story right. If any schoolbook does preach exactly the theory as stated by NAOS, it had better be chucked. For it is not current science. I should record that I did recently encounter an Austrian student who thought exactly that theory was taught in Austrian schools. If that were true, we could invite NAOS to determine whether the introduction of such a text led to an increase in Austrian promiscuity, teen suicide, etc.

I have said enough. I hope I have made my title a little clearer, and we reach my final assertion Sect. 2.10:

2.10 So Let's Get Down to Work on Cases, Not Generalities

Happily, the editors of the present book, reviewing an earlier version of this essay, suggested an instructive case to examine in detail. It offers a fabulous opportunity to look at how real people in a hotly contested problem area use the word 'objectivity'. I do not claim that what follows will satisfy those who want to address not the word but what objectivity *really is* (G-d dammit). I say only the editors have pointed to 'objectivity' in a work site, where the men were probably in suit-and-tie, and the women in business attire, but were as good as in the "shirtsleeves" of Daston and Galison. As we get down to shirtsleeves, we will have to dabble in minutiae where the devil lives, and so lose our high-minded readers who aspire to major generalizations. I mean, this is, like, really boring.

2.11 A Test Case

I shall first set the scene in a personal way. The Fraser River, 1,400 km long, flows from the Rocky Mountains and empties into the Pacific Ocean, just south of Vancouver, British Columbia. It is dear to my own heart for I was always near the river for the first 20 years of my life, and in my seventeenth summer I worked my way through college on a survey party in a wilderness alongside the Fraser canyon in what we call "the interior".

For unknown millennia it has been a feature of life around the Fraser River—the river, its banks, its tributaries, and the sea into which it drains—that each summer myriad salmon go up the river from the ocean to spawn. The noblest of these, in both traditional and commercial lore, is the Sockeye Salmon (*Oncorhynchus nerka*). (So named as an English corruption of the Coast Salish word for "red fish".) The small fry hatch in streams or lakes that are part of the Fraser basin, swim down to the ocean, and return to their birth source after (usually) 4 years at sea. In my youth there was a time each year when the river was jammed with fish going up to spawn so that in lesser tributaries they were so plentiful one could pick them up by hand. No more. The decline in the salmon stock, including Sockeye, has been prodigious, but remained sufficient for a commercial fishery, a sports fishing industry, and indigenous peoples exercising their traditional rights of fishing for food. And then in 2009, the Sockeye run of young adults down to the sea did not occur. It was an ecological and commercial catastrophe.

Fingers pointed in all directions. An obvious suspect (by Mill's Methods of Difference, something different from the past few thousands of years) was the growth of salmon farming on the Pacific coast, which had imported Atlantic (non-indigenous) salmon. That was because the Atlantic salmon, accustomed to the rigours of the North Atlantic, grow much more quickly in the comparatively balmy waters of the North Pacific. The Atlantic salmon brought with them an Atlantic parasite, a louse, bearer of various pathogens, a louse that also prospered in the warm Japanese Current. Was this the story of the small pox all over again? Just as Europeans brought a parasite that decimated the human population of the Western Hemisphere, so, late in the day, commercial interests brought a new pathogen from the East to decimate the fish populations of the West.

In late 2009 the Government of Canada established a Commission of Inquiry into the Decline of Sockeye Salmon in the Fraser River. It was presided over by Mr Justice Cohen, and is known as the Cohen Commission. The entire proceedings are available online at www.cohencommission.ca, last accessed 28th November 2011, but the work of the Commission has been extended at least through December 2011. It was determined that 20 parties representing different interests would be granted standing, and almost all have, at the time of writing, now had their say. There are many conflicting interests. They include: The salmon farmers. The commercial (wild) fishery. Sports fisherman, and therefore the tourist industry, because rich people fly in from the ends of the earth to catch salmon on a fly. Aboriginal peoples, who have rights to the fish for food and tradition outside of fishing season. Conservationists.

It will be a long time before the conclusions of the Commission are published. Enough of stage setting, except to note a further complexity. As I have indicated, the spawning is cyclic, with roughly four cohorts, each of which is at sea for 4 years and then returns. The Sockeye run in 2010 was prodigious, contrary to the expectations of those who feared doom, though perhaps not so astonishing to those well aware of the different cohorts.

The editors posed an excellent challenge to my injunction that we should not talk about objectivity, but get on with the job. They found that many witnesses before the Commission spoke of objectivity! Should I reprimand these witnesses? Here is the exact query posed to me in a mail of 12th September 2011:

> The injunction not to cavil about objectivity but to get on with doing better research re the salmon fishery seems not to have been taken up by a number of witnesses before the Cohen Commission, for whom the ability of the Department of Fisheries and Oceans to foster objective science, for example, (and, thus, whether the DFO should be the oversight body for the fishery) is a crucial question. A Google search of the Commission website yields about a score of examples of 'objectivity' being used by witnesses before the Commission. What precisely do these witnesses not understand?

Excellent. So let's take a look.

There are thousands of pages on the Commission's website, which would have been a life's work to read a couple of decades ago, but now Google makes it all too easy to search. I have done so. I will not identify each document that I cite, for that would produce a rebarbative list of URLs, but the search function makes them easy to re-identify.

I did not look up all the occurrences of the word 'objective', since almost always, in this site, it is the noun that is used, meaning the aim or target of this or that, and not the adjective, which would concern us.

A first result is that the commissioners and the witnesses hardly ever use the word 'objectivity'. When accessed 28 November 2011, only 14 documents included the word 'objectivity', although a few hours later there were 16.

Two of the 14 documents in which the word occurs did not use it (to use Quine's famous distinction), but only mentioned it. In their footnotes each of these two cited a learned article in which the word 'objectivity' occurs. And each of these learned articles is by a philosopher! The philosopher was talking about objectivity—which of course is what I say we should not do. But the witnesses do not talk about objectivity; they merely refer to philosophers talking about objectivity.

As we get down to brass tacks, we may notice exhibit 1718D, a 3 March 2009 working draft of the Salmon Aquaculture Dialogue Working Group Report on Salmon Disease. The group consists of five chief scientists, two from Canada, one from Maine, one from Norway, and one from Chile (for the entire Austral region). In 174 pages, our word 'objectivity' occurs once. Information from Chile is presented, and it is observed that: "The accredited Chilean diagnostic laboratories making official diagnosis have a very good technological level, precision, objectivity and consistency as well as trained personnel." A pedant will notice the problem that the author of this part of the report will have been expressing 'objetividad', but leave that aside. We have a list of virtues that we hope are possessed by good laboratory workers, one of which is the absence of a bunch of vices. But he is certainly not talking *about* objectivity; he is praising his colleagues.

I shall choose two of the remaining eleven occurrences of the word 'objectivity'. One is in a report by the Salmon Farmers, and the other is uttered by counsel for a conservation Coalition.

In the Salmon Farmers Association Final Submissions, our word occurs on page 78 of 144 pages. Their preferred expert is Dr D. J. Noakes, with distinguished credentials as an academic scientist and administrator, whose expertise is in engineering systems design. The expert whom they challenge is Dr L. M. Dill, an equally distinguished academic scientist and administrator, BSc Zoology, MSc Fisheries and PhD Ecology, and in 2009 Director of a Behavioural Ecology Research Group in Vancouver.

Below a section headed, "Selective Quotations and Speculative Reasoning" they challenge Dr Dill, and argue that Dr Noakes got it right. In particular "The BCFSA [the aforementioned salmon farmers association] says Dr Noakes demonstrates his objectivity in his more thorough analysis of the escapes issue which included consideration of escapees as potential vectors for disease."

Objectivity is not a virtue (I have claimed), it is the proclaimed absence of this or that vice. Here we have a confirmation. The Salmon Farmers explicitly assert that their man, Dr Noakes, does not suffer from the vice of “Selective Quotations and Speculative Reasoning”, which (they assert) characterizes Dr Dill’s work. I am not agreeing with what they say; I say only that what they say perfectly accords with our analysis of objectivity.

Our final material comes from testimony on the afternoon of 17 March 2011. The witness is Dr Laura Richards, Regional Director of Science for the Department of Fisheries and Oceans (DFO). At the start (p. 2) it is made plain that the afternoon will be dedicated to what philosophers can call meta-issues, or what a lawyer called

> process issues relating to science and the management at DFO and, in particular, with respect to a science workshop that occurred in September 2009, and subsequent advice that came from that.
>
> I would emphasize this is not intended to get into the substance of that advice. Those issues go directly to the scientific questions that are before you, and they have been and will continue to be dealt with in the substantive hearings on each of those issues.

So substantive science was not the issue that afternoon, but, for example, the production, use, and dissemination of knowledge and evidence. The record of proceedings occupies 101 pages; in addition there are 25 exhibits including reports, curriculum vitae and e-mails.

The word ‘objectivity’ occurs once, on page 89. Dr Richards is being cross-examined about an e-mail. She is being questioned by Tim Leadem, Q.C. (sic), counsel for the Conservation Coalition.[1] He is worried that she seems preoccupied by dealing with the press rather than directing science research. Indeed he suggests that she may have gone as far as “spinning that [an e-mail] in terms of how that is going to be portrayed in the media.” She responds, in part, “I do want to emphasize that it’s important that for our perspective that Science is seen to be objective, and we try to maintain that objectivity.” (Recall that this exchange is spoken dialogue, not polished prose. The capital “S” on “Science” seems to be the work of the Commission stenographer, and is used *passim.*)

Suppose learned counsel had asked Dr Richards, “And what do you mean by objectivity?” I have no idea how she might have responded. Had *I* been *her* counsel, I would have said that she meant that it is important that “Science” be seen to be free of all those vices (bias etc.), and that is important to maintain that perception.

To give a sense of the hearing, I shall just mention the next counsel, Brenda Gaertner, for the First Nations Coalition.[2] She wants to know the extent to which

[1] Coastal Alliance for Aquaculture Reform, the Fraser Riverkeeper Society, the Georgia Strait Alliance, Raincoast Conservation Foundation, Watershed Watch Salmon Society, Mr Otto Langer, and the David Suzuki Foundation. (David Suzuki, a biology professor at the University of British Columbia, is the Canadian ecobiologist with the widest name recognition in Canada, thanks not only to his activism but also to his much admired national television productions.)

[2] First Nations Fisheries Council, Aboriginal Caucus of the Fraser River, Aboriginal Fisheries Secretariat, Fraser Valley Aboriginal Fisheries Society, Northern Shuswap Tribal Council, Chehalis Indian Band, Secwepemc Fisheries Commission of the Shuswap Nation Tribal Council, Upper

her research team made their results available to First Nations. Dr Richards' answer seems to me to be rather flustered. Remember that this damp afternoon of February, public servants are trying to sort things out in their bumbling way. They get on with it, and do so without talking about objectivity.

I have just been reading too much lawyering: "I rest my case."

References

Austin, John L. 1961. *Philosophical papers*. Oxford: Clarendon.

Austin, John L. 1962. *Sense and sensibilia*. Oxford: Clarendon.

Beck, Lewis White. 1969. *Early German philosophy: Kant and his predecessors*. Cambridge, MA: Harvard.

Burnham, Gilbert, Riyadh Lafta, Shannon Doocy, and Les Roberts. 2006. Mortality after the 2003 invasion of Iraq: A cross-sectional cluster sample survey. *The Lancet* 368(9545): 1421–1428.

Daston, Lorraine, and Peter Galison. 2007. *Objectivity*. New York: Zone Books.

Dear, Peter, Ian Hacking, Matthew L. Jones, Lorraine Daston, and Peter Galison. 2012. Objectivity in historical perspective. *Metascience* 21(1): 11–39.

Descartes, René. 1954/1970. *Descartes: Philosophical Writings*. Ed. and Trans. Elizabeth Anscombe, Peter Thomas Geach, London: Nelson.

Hacking, Ian. 1984. Five parables. In *Philosophy in context*, ed. Richard Rorty, Jerry Schneewind, and Quentin Skinner, 103–124. Cambridge: Cambridge University Press.

Hacking, Ian. 1999. *The social construction of what?* Cambridge, MA: Harvard University Press.

Kant, Immanuel. 1788/1949. *Critique of practical reason and other writings in moral philosophy*. Ed. and Trans. Lewis White Beck. Chicago: University of Chicago Press.

Oreskes, Naomi, and Erik M. Conway. 2010. *Merchants of doubt: How a handful of scientists obscured the truth on issues from tobacco smoke to global warming*. New York: Bloomsbury Press.

Porter, Theodore M. 1995. *Trust in numbers: The pursuit of objectivity in science and public life*. Princeton: Princeton University Press.

Rouse, Joseph. 2008. *Two concepts of objectivity*. Lecture at the "Reclaiming the world" conference, Toronto, May 28. http://jrouse.blogs.wesleyan.edu/static-page/work-in-progress

Scheman, Naomi. 2001. Epistemology resuscitated: Objectivity and trustworthiness. In *Engendering rationalities*, ed. Nancy Tuana and Sandra Morgen, 41–42. Albany: SUNY Press.

Solomon, Miriam. 2011a. Just a paradigm: Evidence based medicine in epistemological context. *European Journal for Philosophy of Science* 1: 451–466.

Solomon, Miriam. 2011b. Group judgment and the medical consensus conference. In *Philosophy of medicine*, ed. Fred Gifford, 239–254. Oxford: Elsevier.

Fraser Fisheries Conservation Alliance, Other Douglas Treaty First Nations who applied together (the Snuneymuxw, Tsartlip and Tsawout), Adams Lake Indian Band, Carrier Sekani Tribal Council, and the Council of Haida Nation.

Chapter 3
Objectivity for Sciences from Below

Sandra Harding

A distinctive standard for maximizing objectivity in research emerged from feminist discussions of the 1970s and 1980s.[1] This standard had to be stronger than the prevailing ones since the latter had permitted sexist and androcentric assumptions and practices to shape some of the very best research in biology and the social sciences. Of course one could expect social values and interests to influence the results of research projects that failed to insist on the most rigorous methods. But this kind of "bad science" was not the target of criticism here. The offending projects did already meet the prevailing research standards in their disciplines, whether quantitative or qualitative. Instead, the problem seemed to be that "good science" lacked the methodological resources to detect widely-held sexist and androcentric assumptions and practices that had shaped these results of research.[2]

Remedies for this situation have been debated for several decades. Here the focus will be on one set of principles for maximizing objectivity, referred to as "strong objectivity," that originated in reflections on practices of the new feminist biology and social science research.[3] These principles were articulated as standpoint

[1]Another version of this essay, directed to a different readership, appears as "Chapter 2: Stronger Objectivity for Sciences From Below" in Harding 2015.

[2]For examples of this kind of claim in early feminist research, see Gilligan 1982; Harding and Hintikka 1983; Hubbard et al. 1992; Kelly-Gadol 1976; Millman and Kanter 1975; Reiter 1975.

[3]The language of "strong" objectivity and the call for symmetrical accounts of the objects and subjects of research – "locating the researcher in the same critical plane as the overt subject matter" – (Harding 1987, p. 8) will remind some science studies scholars of David Bloor's "strong programme" for the sociology of science (Bloor 1976). Of course Bloor's conception of the "good science" that should be used to critically examine the researcher and his commitments was

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epistemology or methodology beginning in Dorothy Smith's work in the early 1970s.[4] Standpoint theories explained how assumptions about the superiority of men and inferiority of women, whether justified in biological or social terms, were so widely accepted in the sciences and their surrounding societies that the prevailing standards and associated practices for good method in each discipline could not detect them. That is, such assumptions are best conceptualized not as idiosyncratic or as ones held by individuals, as the prevailing philosophies of science assumed, but rather as ones shared by groups. Similarly, assumptions such as those of male supremacy, white supremacy, and Eurocentrism were held virtually society-wide. Consequently the repetition of observational procedures by subsequent researchers who shared the same assumptions could not enable their identification. The strong objectivity program then "operationalized" its more effective standards in methodological directives to start off research from outside the conceptual frameworks of the disciplines – for example, from the daily lives of women. Such politically and economically vulnerable groups received fewer of the benefits and bore more of the costs of the conceptual frameworks and everyday practices of dominant social institutions, including research disciplines. From the standpoint of their lives, the assumptions and practices of those who most benefitted from such institutions might well look different.[5] This was the insight that had generated so many of the early feminist criticisms of the law, government practices, the economy, the health care system, education systems, and dominant models of the ideal family. Standpoint theorists brought such perspectives into an examination of the production of scientific knowledge.

Here I first set in context some of the some main sources of the controversiality of the very idea of such a program as this one. Section 3.2 identifies further flaws with the neutrality ideal that standpoint theory and its strong objectivity have been constituted to meet and explains further the strong objectivity standard and practices recommended to improve both the reliability of sciences and their politics. Section 3.3 notes a number of ways in which this way of thinking about maximally objective research aligns with claims and projects of the social studies

precisely the one that is the target of criticism in the present paper. Ethnographers will be reminded of the reflexivity debates in their field of the 1980s and 1990s (See, e.g. Elam and Juhlin 1998). All of these related concerns were "in the air" and no doubt shaped my thinking when I first began to formulate these issues in the mid-1980s. I recollect that at the time my immediate concern was to capture the concept of objectivity that was already informally in use on behalf of a feminism that was persistently accused of abandoning objectivity, rationality, and good method. For better or worse, I intended to do so with as macho language as possible.

[4]Smith 1987, 1990. Smith always insisted on "the standpoint of women" in order to emphasize its origins in women's everyday lives rather than in feminist theory. See also Collins 1991, Haraway 1988, Harding 1986, Hartsock 1983, Jaggar 1988, Rose 1983. These and other subsequent essays developing and criticizing standpoint theory are collected in Harding ed. 2004.

[5]I first developed the notion of strong objectivity in my 1991 and 1993. Evelyn Fox Keller (1983), Karen Barad (2007) and Elisabeth Lloyd (1996) provide examples of three other valuable but quite different critical approaches to the "weak objectivity" question. Only my strong objectivity project is conjoined to standpoint epistemology/methodology.

of science and technology (SSST). That is, a good deal of the criticism of the strong objectivity program can be accounted for in terms of the "critics" ignorance of or resistance to the accounts produced by SSST in its almost five decades of research and analysis. The final section briefly identifies and responds to some of the most common misreadings and resistances to strong objectivity.

3.1 Sources of Controversy

This project was controversial from the beginning and remains so today. Of course it was initially perceived by many as arrogant and beyond the borders of reason to be challenging the standards that had produced some of the very best of Western sciences. The strong objectivity program seemed to such critics to be abandoning truth, reason, and probably even Western civilization! It was even more offensive to most of such critics to claim that there was something to be learned by starting off from women's lives to identify just what was wrong with such standards. Such reactions appeared from both the left and the right in the "science wars" of the 1980s and early 1990s.

Reflecting for a moment on how the strong objectivity project differs from the kind of "talking about objectivity" that Ian Hacking has so effectively criticized can lead to recognition of the philosophic context that has made the strong objectivity project and its standpoint theory so controversial to many and so valuable to many others. Citing a maxim of J. L. Austin, Gilbert Ryle, and other Oxford linguistic philosophers, Hacking says "one should be wary of fancy words conceived in philosophical sin – rationality and reality for example. Objectivity is among them" (Chap. 2, in this collection). Instead of invoking such "elevator words" intended to increase the authority of a claim, we should simply talk about the concrete "objective facts" about the issue. For example we can "look at the numbers, their authors, their methods, their interests, etc. Always, I say, work on the ground floor" (Hacking 1999). Hacking is certainly right about the vacuity of far too many invocations of objectivity in support of scientific claims.

Yet I think that my discussion here escapes these charges in several ways. First, it fits under Hacking's account of the virtues of Lorraine Daston and Peter Galison's *Objectivity* (2007), "a masterful historical study of an epistemological concept" (Chap. 4, in this collection). He says that "this falls under historical meta-epistemology . . . " which " . . . is a good thing".[6] "*Objectivity* is about the concept of objectivity, its past uses, and the practices associated with it" (Ibid.). Such meta-epistemological uses do not fall under his general charge against invoking objectivity-in-the-abstract. Daston and Galison document the changing standards for achieving objectivity in the representations in atlases of plants, animals,

[6] I certainly am not claiming that the work of strong objectivity and standpoint theorists is in the same category as the truly magisterial historical study that Daston and Galison provide.

constellations and other natural phenomena. Similarly, my discussion here is also about history, but in this case it is history-in-the making. It is about how the standards for maximizing objective research around the globe are already shifting in response to identifiable changes in social relations. It, too is about the past uses and the practices associated with the term, "…the sites within which they were deployed, who had authority when using them, the actual modes of inscription" (Ibid.).

This last phrase draws attention to how Daston and Galison's account is about changes in the *methodologies* for achieving "right sight" in the atlases. Thus the introduction of photography and, more recently, nano-technologies each generated new practices for accurately representing natural phenomena. So, too, my discussion focuses on changes in the methodologies for maximizing more accurate representations of nature (and social relations). Here "strong objectivity" specifies particular research practices necessary to improve the reliability and scope of scientific claims.

Finally, the strong objectivity project is in the service of more effectively aligning scientific practices with democratic social and political goals. This epistemology/methodology emerges from and is explicitly intended to advance social, political, and ethical projects: these new standards and practices are intended to produce sciences *for* social justice movements, not just "right sight" for its own sake.[7] Here, too, my project aligns with Daston and Galison's. They invoke the phrase "right sight" to capture the ethical as well as epistemic dimensions of shifts in the concept of objectivity. New standards cast an ethical shadow over older standards as they reveal the flaws in the latter. The high quality and value of scientists' older work can no more be assumed. In the case examined here, these new standards and practices were initially invoked by groups that showed how they had been harmed by the prevailing incompetent standards and practices for maximizing objectivity. Thus the adequacy of these new standards and practices was from their beginnings to be judged on not only epistemological/scientific grounds but also social/political/ethical ones.

Indeed, the most emotionally charged criticisms of the strong objectivity project have arisen because it challenges the particular epistemological/methodological solution that earlier generations settled on in their attempts to align scientific research more effectively with liberal democratic politics, and even with socialist democratic politics. Researchers and theorists between the two World Wars and at mid-century saw commitments to value-neutral research as the only reasonable standard of fairness capable of countering the tendency of fascistic and totalitarian ideologies masquerading as scientific to gain immense political power.[8] That kind

[7] Of course some elevator uses of "objectivity" may have this goal while yet remaining methodologically vacuous.

[8] See, for example, Richardson 2003 and 2006, and Hollinger 1996. The value-neutrality principle was invoked earlier by Max Weber, of course, and even by Galileo for socially progressive purposes. I discuss George Reisch's (2005) monumental analysis of these issues in Chapter 5 of Harding 2015.

of concern is certainly still relevant today, though we live in a more complex ethical and political environment.

Indeed, the threats to standards for good research in that era have been exacerbated by new challenges that were hardly imaginable then. The world has changed. Theorists in many democratic revolutions since the 1960s have argued that – paradoxical though it may appear – the supposedly value- and interest-neutrality goals and practices of modern Western sciences are not themselves value-neutral at all. Rather, in modern Western bureaucracies already structured by liberal political principles, appeals to neutrality tend to reinforce the institutional power of elite authority against the equality-seeking claims of economically and politically vulnerable groups. Such claims have been made not only against state and corporate economic and political practices, but also against the natural and social sciences that serve such elite institutions, whether or not intentionally. Furthermore, the emergence of the social studies of science and technology has been producing more and more details showing how the very best Western sciences have "an integrity with their era," as Thomas S. Kuhn famously put the point (1962, p. 1).

Finally, the increasing presence of Muslims and of Islam in the West has raised issues about how much "tolerance," how much pluralism and multiculturalism, prevailing Western Liberal democracies can and are willing to accommodate.[9] This kind of challenge emerges in the sciences and science studies also in reevaluations of indigenous knowledge traditions. It turns out that challenges to prevailing standards of objectivity in science seem to require rethinking the principles of Western Liberal democracies. Yet this is a task greater than most scientists, philosophers of science or science studies scholars ever imagined they should have to take on. They are not trained in economic, social and political theory, let alone in the history of non-Western knowledge traditions. And the still-powerful legacy of the between-the-wars generation of philosophers of science directs them to create only "scientific philosophy," that is, philosophy that does not align itself with particular political positions.[10]

Thus the terrain on which defenders of the strong objectivity program find themselves engaged includes an array of suspicious agents, such as funding agencies, tenure committees, the economically and politically vulnerable groups to whom they want to remain accountable, other social justice movements, and also defenders of the powerful epistemological and methodological legacy of social progressives in earlier generations.

[9]Calhoun et al. 2007, Jakobsen and Pellegrini 2008, Levey and Modood 2009. See Chapter 6 of Harding 2015 for further discussion of this issue.

[10]Though Richardson (2003, 2006) argues that that generation of philosophers of science was much more flexible in strategizing how to develop standards that advanced both the reliability and social progressiveness of the sciences than is suggested by the rigidly "positivist" positions usually attributed to them today.

3.2 The Logic of Standpoint Epistemology and Strong Objectivity

First, there is no single, fixed, eternal meaning of the term objectivity. Indeed, historians have shown how it is an essentially contested concept. In modern societies it remains a persistent site for controversies over conflicting knowledge claims. "[F]undamental ideals of Western society such as rationality and progress are grounded in certain conceptions of science. So when the value freedom of science is questioned, a fundamental institution in our lives is being challenged" (Kincaid et al. 2007, p. 4). Historian Robert Proctor points out how claims to objectivity sometimes are used to advance and sometimes to retard the growth of knowledge. Moreover, such claims have been made both on behalf of and against democratic research tendencies. He writes of objectivity being used in different historical contexts as "myth, mask, shield, and sword" (1991, p. 262).

In addition to its shifting meanings, the term also lacks a fixed referent. Objectivity – or the incapacity for it – has been attributed to individuals or groups of them, such as in uncomplimentary dismissals of women, African Americans, or the indigenous knowers of non-Western cultures as subjective and incapable of producing the reliable knowledge claims that supposedly can men, whites, Westerners, or some other elite group. In another usage on which historian of science Thomas S. Kuhn (1962) focused, it has been attributed to the particular kinds of inquiry communities characteristic of contemporary modern science. Trained to hold a skeptical attitude toward received beliefs, such communities must also develop principles of mutual respect and trust if such skeptics are not to suffer for articulating their critical perceptions and ideas. Supposedly, in such communities the lowest level graduate student is encouraged to think critically about dominant assumptions and claims, including those of his Nobel Prize-winning lab director. In a third usage, sometimes the term refers to the results of research. Yet we can wonder what this use of the term adds to assertions that these research results are highly confirmed. Here "objective" seems to be a substitute for "true" or truth-like. Finally, in actual research contexts the term is often used to refer to research methodology; this is the focus here (cf. Megill 1994).

The introduction explained how familiar standards and their associated methodology for maximizing objective research do not have the self-critical resources to detect widely-shared social commitments. This only "weak objectivity" is not competent to produce the "view from nowhere" that conventional philosophies of science have demanded. These days, because research tends to be expensive, only the perspectives of those already-advantaged groups who can access funding tend to prevail. Consequently it is their economic, political, and cultural commitments that tend to shape results of most research.

As indicated earlier, starting off research from outside a discipline can enable the detection of those dominant values, interests, and assumptions that make widely prevalent ways of thinking appear reasonable and even natural. Of course one can never get completely outside one's social location to float freely above one's culture and history, as the conventional philosophies of science have imagined to be possible. But finding or creating even just a little distance from prevailing social commitments can be sufficient to enable new critical perspectives to illuminate issues in new ways. How can this critical difference be identified and used to maximize the objectivity of research?

One important way to do so has been to create missing diversity in research communities. "Affirmative action" can turn out to provide scientific and political benefits for communities as well as for the individuals newly joining them. Another strategy has been to form alternative research communities. All of the recent democratic social movements have also pursued this project. These two strategies have often combined in the institutionalized structure of U.S. disciplinary organizations. Thus women and "minority" philosophers have formed their own professional organizations which meet alongside the mainstream philosophy conferences, and as groups and individuals they also participate in the mainstream governance and programming. The standpoints of poor people, of racial and ethnic "minorities," of people in other cultures, of women, of sexual minorities, and of disabled people are perhaps the most widely-used diversity standpoints from which dominant knowledge claims in every discipline have begun to be reevaluated. Such groups have not been the ones who designed and maintain the dominant institutional policies and practices that turn out to disadvantage them. Such institutions do not provide disadvantaged groups with the knowledge and power they need in order to manage their own lives in their own terms. Consequently, like "the stranger" in the classic sociological narratives, whose perspective can identify things invisible to "the natives," researchers "from below" can highlight features of the dominant economic, political, legal, educational, ethical, and family institutions that the dominant groups either can not or refuse to recognize (Collins 1991).[11] These days, many of the deep but only implicit cultural commitments of the modern West in its sciences and their philosophies are also finally becoming visible in the West as we begin to learn how to respect the critical perspectives on the West that arise from the daily lives and the legacies of non-Western cultures (Harding 2011b).

However, it is not enough simply to be able to identify culture-wide assumptions that shape our own research projects. Strong objectivity demands interrogation also of just which cultural commitments can advance growth of the knowledge a particular community desires. It cannot be that all useful knowledge humans might want could be produced by sciences funded primarily by profit-making corporations, militaries, and imperial governments! If sustainable environments, the eradication of poverty world-wide, and the elimination of social inequality were actually the values and interests of the dominant groups, not just what they claimed to believe important when caught in practices that deteriorate such goals, threats to those resources for human flourishing would have been eliminated long ago. Societies with different values and interests have in the past, do now, and will

[11]The language of "from below" originates in thinking of a society as structured in the form of a pyramid in which the small "top" rules the huge "bottom" of a hierarchical social system.

continue to produce reliable knowledge claims that conflict with ones emerging from dominant Western interests and values. Particular kinds of societies are "co-produced" with the particular kinds of sciences they want: each enables and limits the other.

This observation takes us to a number of ways in which standpoint methodology and its strong objectivity project are aligned with recent findings in the field of the social studies of science and technology (SSST).

3.3 Alignments with Social Studies of Science and Technology

I say "align with" since until recently the science studies work has only rarely identified neoliberal economic and political ideals as a problem or raised issues about the implications for science projects of pro-feminist, multicultural or postcolonial political and scientific goals.[12] In 2006 appeared the *East Asian Science, Technology and Society: An International Journal.* In 2012 an African SSST network was launched, and in 2014 the annual meeting of the main U.S. disciplinary organization in this field, the Society for the Social Studies of Science (4S), met in Buenos Aires jointly with The Sociedad Latinoamericana de Estudios Sociales de la Ciencia y la Technologia (ESOCITE).[13] Such "alignments" are clearly becoming sturdier. Here I can only briefly identify these SSST arguments and direct readers to ongoing discussions and debates which are much more complex than I can here represent.

3.3.1 Objectivity Has a History

One such alignment can be found in the evidence that objectivity ideals and favored strategies for achieving them have social histories; that is, they change in response to shifts in scientific goals as well as to shifts in processes in and pressures from society.[14] For example, as mentioned earlier, Daston and Galison's study shows how standards for objectivity shifted as new technologies of observation were introduced

[12] Anderson 2009 identifies several kinds of "alignments" between the postcolonial theory of Franz Fanon, Edward Said and others that has become institutionalized in U.S. English, French, and cultural studies departments and SSST. However, my focus is on alignments between advocacy of "strong objectivity," on the one hand – which, I argue, appears in all recent democratic liberation struggles – and, on the other hand, mainstream SSST.

[13] See http://sts-africa.org and the report of the 2014 conference co-sponsored with the Sociedad Latinoamericana de Estudios Sociales de la Ciencia y la Technologia (ESOCITE) at http://www.4sonline.org.

[14] Daston and Galison 2007, Jasanoff 2005, Novick 1988, Porter 1995, Proctor 1991.

in the production of scientific atlases over the last several centuries.[15] Moreover, Daston and Galison argue that scientists' senses of themselves as engaged in the highest moral pursuits are repeatedly challenged with each new objectivity practice. So, too, is the reputation of their work as honorable. Thus challenges to the "right sight" of scientific practices are perceived as challenges to the moral integrity of the scientist and his profession. In this account objectivity thus becomes one more feature of research ideals to lose its aura of universal validity and become located in particular historical contexts.[16] Thus a shift to strong objectivity in the context of increasing demands on states and their sciences for accountability to the needs and desires of social justice movements can be contextualized as just one more such moment in the history of this research ideal.

3.3.2 Sciences and Their Societies Are Co-produced or Co-constituted

Steve Shapin and Simon Schaffer (1985) introduced to SSST the image of the coconstitution or coproduction of sciences and their societies. They did so with their study of the correspondence between Hobbes and Boyle as these figures struggled to bring into existence distinctively modern democracies and sciences. Subsequently, Sheila Jasanoff (2004, 2005) demonstrated how different national anxieties and political cultures required different strategies to secure the objectivity of biotechnology decisions in Germany, England, the U.S., and the European Union. The scientific institutions and practices of different societies can exhibit different standards for maximizing objectivity.

This language of coconstitution or coproduction of sciences and their societies was a welcome shift from the earlier language of the "social construction" of science, which had emerged in the early days of the development of SSST (See Hacking 1999). The coconstruction language had even a better fit with Thomas S. Kuhn's demonstration five decades ago that the very best sciences exhibited an "integrity" with their historical era; they made the kinds of assumptions and focused on the kinds of problems characteristic of their particular social moment, but not necessarily of earlier or later ones (Hollinger 1996; Kuhn 1962). Such sciences might be "autonomous" from their societies in the sense that no social authority was explicitly directing their agendas. But they shared the values, interests, anxieties, and, one could say, the distinctive forms of curiosity of the era. However, ensuing critics of the social construction of the very best scientific knowledge sometimes

[15] And objectivity became detached from "true to nature" with the introduction a century and a half ago of photography and other mechanical transcribers of nature's regularities. Daston and Galison refer to this new ideal as mechanical objectivity.

[16] Cf. Shapin 1994 on truth; Schuster and Yeo 1986 on scientific method; Lloyd 1984 and Prakash 1999, among others, on rationality.

misleadingly suggested that for the social constructionists, nature played no role in scientific research. Yet no scholar ever made such a silly claim. Other critics worried that the social construction idea misleadingly suggested that "the social" somehow existed outside of and prior to scientific projects, which would be counter to the intentions of the social constructivists (cf. Latour and Woolgar 1979).

Yet simultaneous with the Shapin and Schaffer account and even earlier, feminist, anti-racist, and postcolonial SSST were already arguing that discriminatory and less than maximally reliable results of research that supported inequity were the logical outcome of sciences designed by societies invested in inequity. They insisted that it would take changes in these discriminatory social orders to legitimate sciences that were more accurate and that better aligned with democratic social relations. Moreover, changes in the latter would help to transform the former. So the co-constitution/co-production understanding of how change occurs simultaneously in sciences and their societies has been aligned with standpoint methodology and its strong objectivity ideal from the early days of the social justice movements. Unfortunately, with important exceptions, much of this early work, and especially the postcolonial argument, has remained mostly under the radar of mainstream Western SSST.[17]

This co-production work showed the internal relations between how we live and what we can know – between being and knowing. It challenged the older understanding of the history of scientific achievements as about either the internal "logic of science" or how external social, economic, and political forces had effects on scientific practices. In these newer accounts, the social reaches deeply into what were thought of as the foundations of our knowledge of the world, a point to which I return. Because of this dynamic nature of sciences, their borders continually shift. What counts as nature or as "real science" in one era frequently is at odds with the commitments of another era. Of course the same is true for what counts as a multicultural democratic society.

3.3.3 *Expanding Expertise*

Harry Collins and his colleagues, among others, argue that recognition of scientific expertise has been far too narrowly restricted. It tends to exclude many non-professionals whose *experience* enables them to "know what they are talking about" (Collins and Evans 2007). Relatedly, Ulrich Beck (1997) has argued that today the production of scientific knowledge is being demonopolized from the control of recognized scientists. Non-scientists increasingly are participating in the production of the kinds of sciences they want. They ask new kinds of questions and recruit

[17]For just a few examples of influential postcolonial writings, see Adas 1989, Brockway 1979, Goonatilake 1984, Haraway 1989, Headrick 1981, McClellan 1992, Moraze 1979, Nandy 1990, Petitjean et al. 1992, Sachs 1992, Sardar 1988. See also Harding ed. 2011a.

official scientists to research them. Moreover, when we have urgent health or environmental concerns and scientific accounts provide conflicting results, we are forced to conduct our own research. David Hess (2007) and Karin Backstrand (2003) have in different ways charted the importance of many kinds of "civic science" and "citizen science," in which ordinary citizens organize in various ways to make contributions to the agendas and practices of scientific research through their investigations of their environments, of patterns of disease, or of risks in and from scientific and technological research and its consequences that they regard as insufficiently appreciated. In these projects they often recruit scientists or engineers to work with them. Standpoint methodology and its strong objectivity are intended to enable the participation in many phases of scientific research of groups whose concerns are underrepresented in the design and management of scientific projects. From such perspectives standpoint methodology and its strong objectivity can be recognized as a kind of citizen science or participatory action research.

3.3.4 Intervening in Nature Can Be a Criterion of Good (and "Real") Science

Several philosophers have argued that Western philosophy of science has tended to overvalue the importance of representing nature's order and undervalue the importance of intervening in it (Hacking 1983; Rouse 1996). And sociologists have argued that since industrial research can often itself produce new understandings of nature's order, scientific and technical research are not as cleanly divisible as customarily assumed. These critics undermine also the claimed superiority of theoretical accounts over pragmatic ones, of "knowing that" over "knowing how," and thus of scientific over technical research (Nowotny et al. 2001; Shapin 2008). These insights legitimate the perception that starting off research from the concrete technical activities of economically and politically vulnerable groups can in some cases lead to recognition of how these contribute to the growth of scientific knowledge. This point is particularly salient to the reevaluations of indigenous knowledge that have been underway for some four decades (Selin ed. 2008). Conventionally evaluated by Westerners as only technologies, or only speculations (ie theories) lacking empirical support, indigenous knowledge is now increasingly recognized as valuable systematic knowledge about parts of nature and social relations about which Western sciences have often been ignorant.[18]

[18]Consider, for example, legal struggles between Western pharmaceutical corporations and indigenous groups over who should have rights and benefits from the Western appropriation of indigenous pharmacologies and agricultural products (See, for example, Brush and Stabinsky 1996, Hayden 2005). See also Schiebinger's work on colonial botany as the "big science" of its era. It required that the colonists and explorers extract plant materials and knowledge of their uses from the indigenes to turn them into products Europeans could sell (Schiebinger 2004, Schiebinger and Swan 2004, Brockway 1979, Harding 2015).

3.3.5 *Nature Is Disordered, Modern Western Sciences Are Disunified and Plural*

Philosophers and historians have argued for the disunity and pluralism within MWS (Galison and Stump 1996; Kellert et al. 2006), and for recognition of the necessary "disorder of nature" (Dupré 1993). As Kellert, Longino and Waters point out, such multiplicity has a number of sources, including the diversity of human goals, the indeterminacy of certain regularities of nature, and the complexity of so many natural phenomena (p. xi). The diversity of human interests and goals alone justifies standpoint methodology's production of perspectives on nature and social relations that often conflict with those of dominant groups. The widely recognized complexity and indeterminacy of social relations provide additional reasons to value scientific pluralism. For Helen Longino and some other feminist philosophers, the benefit of a plurality of views is that it provides criticisms and alternatives to other views that cannot be achieved in other ways.[19] This is not to say that research shaped by racist, sexist, and other socially iniquitous assumptions or goals should be equally welcomed into the diversity of human knowledge claims. It is a fact that these exist, but not all existing human assumptions and goals need be regarded as equally desirable or correct. Standpoint theory is not committed to a pernicious relativism. We return to this point below.

Yet one can wonder how "deep" such multiplicity and plurality must go into scientific world views. Is the transformation of sciences and their philosophies called for by social justice movements just a matter of adding missing facts about nature and social relations? Could the epistemologies and ontologies of the world's sciences be unified even if the routes to such unity varied? This is too complex an issue to go into here. However, we can at least note that if nature itself is too indeterminate and complex to be captured in a unified "theory of everything," and if human interests are so diverse that they will continue to explore new phenomena and new ways of knowing, then there is good reason to think that the pluralism of science "goes all the way down" through its methodologies, epistemologies and metaphysics. Kellert et al. (2006) insisted that their pluralism of sciences is a program and a matter of empirical evidence, not a manifesto. Yet we can wonder why it should not be a manifesto in today's context of ever-expanding market economies that systematically disrespect biological and cultural diversity (Harding 2015).

[19] Yet see Kristen Intemann's (2011) discussion of this kind of assessment of the value of pluralism, shared with the views of John Stuart Mill, in which the commitments to pluralism or diversity should not satisfy feminist agendas.

3.3.6 *Modernity/Tradition Contrast Misleading*

Finally, some science studies scholars have suggested that the solution to the diverse dissatisfactions with modernity, its sciences and their philosophies, is not to abandon modernity, but rather more rigorously and comprehensively to attain it. Contemporary Western philosophies of science in fact have only been partially modernized, the argument goes, since they still have not developed the conceptual resources effectively to examine critically their own cultural locations. They are still too traditional in their lack of a comprehensively critical program (Beck 1997; Nowotny et al. 2001, See also Harding 2011b). They are epistemologically underdeveloped in such respects. These philosophies are still invested in pre-modern tendencies to universalize the desirability of the beliefs and practices of one's own tribe or culture.[20] Standpoint methodologies and their strong objectivity can here contribute to such analyses. They require a thoroughly modern reflexivity – a "robust reflexivity" – such that one learns to see as reasonable others' conflicting perspectives on oneself.[21]

3.4 Criticisms and Challenges

This notion of strong objectivity and its standpoint methodology have disseminated across disciplines and also independently emerged wherever social justice movements claim authority for the distinctive ways that they see the world. In the West, both its fans and critics have sometimes tried to fit it into methodological practices and epistemological positions already familiar to them, in the course of which its strengths and limitations often are misread. A number of such criticisms that emerged in its early years are rarely still raised since they have been shown to be misunderstandings of its claims or grounded in precisely the older philosophies of science to which strong objectivity objects. Yet these and others also often raise interesting questions that cannot yet be settled.[22] Here I will summarize main criticisms and responses to them.

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[20]This project is aligned with Latour's (1993) famous argument that "we have never been modern," though it is not his solution to that situation.

[21]See Elam and Juhlin 1998, Harding 1998 (Chap. 11).

[22]Two collections of essays are addressed respectively to Dorothy Smith's and Nancy Hartsock's particular formulations of standpoint theory (Campbell and Manicom 1995; Kenney and Kinsella 1997). Two extended analyses and critiques of standpoint theory by distinguished feminist theorists appeared in *Signs: Journal of Women in Culture and Society*, each with responses by some of the original standpoint theorists (Hekman 1997, Walby 2001). A recent collection of essays brings together the original standpoint essays plus a number of diverse readings and criticisms of it (Harding ed. 2004). Additional analyses and criticisms can be found in book reviews of the work of the standpoint theorists.

3.4.1 Does the Strong Objectivity Program Introduce Politics into Otherwise Value-Neutral Sciences?

No. It identifies how prevailing politics have already directed research projects and left its fingerprints on the results of research. And it shows how some kinds of politics (anti-sexist, anti-racist, and others) can in fact advance the growth of knowledge.

3.4.2 Does the Strong Objectivity Program Advance an "Identity Politics" Claiming Privileged Knowledge for Oppressed Peoples?

No. Men have productively and with ethical sensitivity started off research from issues arising in women's lives, whites from back lives, Westerners from colonized lives, and so forth. Moreover, no knowledge claims can gain automatic assent. Standpoint claims are as corrigible as any others. But the strong objectivity project does argue that seeking out the perspectives of excluded, politically and economically vulnerable groups can be an important source of resources for enlarging bodies of knowledge and increasing the reliability of the results of research. And when such a group itself takes on a project of collectively articulating its needs and desires, it can become a group "for itself" rather than only "in itself", that is rather than a group constituted only by others as an object of their knowledge and policy.

3.4.3 Don't the Natural Sciences Already Have Adequate Safeguards Against Social Biases? Can Strong Objectivity Be Relevant to Them?

Such critics presume that eventually the social is always winnowed out from results of research in the natural sciences thereby leaving pure facts and value-free explanations of them in the resounding successes of physics, chemistry, and biology. However, research in biology, medicine, environmental studies, engineering, and even physics and chemistry have shown how these knowledge systems, too, are co-constituted with their social orders and will share distinctive social features with them. To be sure, one should not expect to find the kinds of now-obvious social features in the more abstract sciences. Yet the latter, too, are co-constituted with their social orders and can benefit from questions arising "elsewhere," as critiques by later generations and from other cultures have compellingly demonstrated. Social justice movements cannot wait for the large-scale social transformations that will more easily enable the detection of widely held erroneous assumptions in the natural sciences, too – ones that support what are now powerful inequities. Rather, they hold that such transformations can themselves be hastened by challenges to false and oppressive knowledge claims. That is, the "co-production" of sciences and their societies can be an agent's category for social intervention in the natural sciences, too, not just a descriptive category for non-interventionist observers.[23]

3.4.4 Is Strong Objectivity Too Modern? Is It Too Postmodern?

Does strong objectivity retain too much of the Enlightenment, or positivist, or logical empiricist conceptual framework? Or, alternatively, does it abandon concerns for truth and the reliability of scientific knowledge claims? The prevalence of both criticisms reveals that standpoint methodology is doing something different from the principles of both camps in these kinds of "science wars." It does not give up Enlightenment, positivist, and logical empiricist concerns that research should be fair to the empirical evidence, to its strongest critics, and to the highest ethical principles and the goals of social justice (See Novick 1988). Of course what counts as each of these has differed from generation to generation and culture to culture. Such struggles are vividly depicted in recent histories of science, as indicated earlier (Daston and Galison 2008; Jasanoff 2004, 2005; Richardson 2003, 2006). Standpoint projects importantly advance Enlightenment goals as these make sense for our world today. As I have argued elsewhere, postmodern critics often themselves make kinds of modernist assumptions that standpoint projects challenge. For example, in their rejection of philosophies of "science" they, too, assume that there can be one and only one set of institutions and practices to which that term can apply. They are unfamiliar with the postcolonial SSST discussions (Harding 1988), as well as, I would now add, the proposals of Western scientific pluralism.

3.4.5 Does Strong Objectivity Embrace or Fall into Relativism?

Does strong objectivity endorse the position that every man is his own best historian, as Novick (1988) put the point? Does this practice abandon the importance of truth, value-neutrality, and universally valid claims and practices about nature and social relations? In my opinion, there are two acceptable ways to answer this question. One is to argue, as I have above, that strong objectivity standards simply recognize facts about nature and social research practice that could not be detected in earlier eras. For example, there is no "view from nowhere" possible from which one can see every social and natural reality past, present, and future. As indicated earlier,

[23]Sheila Jasanoff has suggested this role for co-production as an agent's category in the introduction to her 2004.

new human desires for knowledge are forever emerging, and the world is too indeterminate and too complex to permit such a totalizing understanding of nature and social relations. So such new apparent truths require new kinds of scientific standards and practices.

But at this point one could use the term "principled relativism" to refer to standpoint theory and its strong objectivity, as did Frederic Jameson (1988, p. 144). Strong objectivity is committed not to all knowledge claims being equally valid, to "anything goes" in the results of research. It is committed rather to "situated knowledge," in Donna Haraway's (1988) words. That is, it is committed to the inevitability of deeply conflicting knowledge claims, each with impeccable evidence for such in the eyes of its claimant. The situations of such knowers always both enable and limit what they can know. Finally, we can recollect that almost all research in the natural sciences is "mission directed" to improve health, generate greater profit, produce effective weapons, defeat global warming, and so forth. This is so whether or not the individual scientist is motivated purely by his curiosity. Yet no one thinks the results of such research invalid simply because such projects were undertaken for such human purposes. So strong objectivity is always relative to its purposes. Of course we can and should continue to debate just what are good places in the social order from which to start thinking scientifically.

3.4.6 Is Strong Objectivity Too Western? Is It Too White?

This epistemology has itself been produced at a particular time and place for specific purposes and within the discourses available to its creators and users. It is like all others in this respect. Philosopher Uma Narayan (1989) points out that the validation of women's experience on which Western feminists insist cannot carry the critical edge in a society where it is already validated, such as in Hindu society where the genders are conceptualized as having complimentary rather than hierarchical relations. Of course such societies can oppress and exploit women no less than in societies with hierarchically organized gender. Yet some other epistemological/methodological strategy is needed for those circumstances. Moreover, she notes that standpoint theory and strong objectivity were developed in opposition to positivist tendencies in research. Yet positivism has not had the hegemonic official status in other societies, such as India, that it has had in many Western societies. Indian feminists face other serious problems with their local research establishments and need different epistemic/methodological tools for their projects. Chela Sandoval (1991) has developed a form of standpoint epistemology/methodology that she finds more useful for U.S. women of color, and Patricia Hill Collins (1991) and Bell Hooks (1983) have given it distinctive transformations to serve their needs as Black feminist theorists. Walter Mignolo (1995) began by claiming Gloria Anzaldua's (1987) "borderlands" version of a standpoint theory as the grounds for his own arguments for a distinctive Latin

American standpoint on neocolonialism and colonial diasporas today, but later developed his notion as the "colonial difference" (Mignolo 2000).[24]

Indeed, it is clear that there are a number of other distinctive and possibly problematic cultural assumptions that shape much Western feminist work. For example, few feminists have critically examined the distinctively Christian and Protestant religious and spiritual commitments that have been identified as embedded in a Western secularism that is also a foundational commitment of Western sciences and their philosophies and methodologies.[25] This is too complex an issue to pursue here, but we can note that one of its effects is a resistance to countenancing culturally embedded indigenous knowledge projects as "real science" regardless of the empirical evidence presented in support of them.

3.5 A New Harmonizing of Multiple Sciences?

What kinds of sciences do we want for today's multicultural, democratic societies? What kind do we want for a West that is already encountering repeated decentering in today's global political economy? These are not the issues faced by the influential philosophers of science two and three generations ago. Yet many of us share with the latter commitments to developing more fair and socially responsible societies and the kinds of sciences that can serve such goals. We share the desire to work cooperatively in local and international contexts. We share valuing knowledge of how our worlds actually work – of what are their regularities and underlying causal tendencies (Richardson 2003, 2006). We can commit ourselves to a new kind of "unification" of global sciences (if one wants to consider resuscitating that term) through strategizing how to maximize and harmonize the scientific and political benefits of multiple scientific questions, conceptualized from multiple social perspectives, with a multiplicity of useful methods. Our challenges here for sciences and their philosophies are those that face international relations more generally these days. This kind of harmonization will have to be created through negotiation and compromise, as always already occurs within the practices of successful Western sciences themselves (Galison and Stump 1996).

Just how we could succeed at such goals in today's world requires public discussion in local and global contexts. Unfamiliar terms and concepts can become comprehensible through public discussion of their benefits and limitations (Think

[24]I am not claiming that hooks, Anzaldua, and other authors who do not explicitly refer to standpoint theory or strong objectivity in fact are merely tweaking the arguments developed by the feminist standpoint theorists cited earlier. Rather, as indicated earlier, I propose that the strong objectivity and standpoint positions tend to emerge whenever new groups organized on their own behalf ("for themselves") critically evaluate the inadequacies of dominant views, policies, and practices. The strong objectivity program and its standpoint theory are organic "logics of scientific inquiry" for creating critical "sciences from below."

[25]See, for example, Sands 2008 and Sullivan 2010.

of the history of such terms as genes, techtonic plates, biodiversity, ozone holes, and black holes in space.). Since we now can see that sciences and their societies are co-constituted, we further justify the importance of starting off from the society side of the co-constitution in today's social justice movements to identify research ideals and strategies that address progressive, though multiple and often conflicting, scientific and political goals. The co-constituting of societies and their sciences can be human agents' projects, not just a description of events and processes only passively witnessed by individuals and their societies. Such projects raise puzzling questions, but those are the relevant ones on which we could focus. Strong objectivity and its standpoint theory provide one useful way to begin such projects.

References

Adas, Michael. 1989. *Machines as the measure of man*. Ithaca: Cornell University Press.

Anderson, Warwick. 2009. From subjugated knowledge to conjugated subjects: Science and globalization, or postcolonial studies of science? *Postcolonial Studies* 12(4): 389–400.

Anzaldua, Gloria. 1987. *Borderlands/La Frontera*. San Francisco: Spinsters/Aunt Lute.

Backstrand, Karin. 2003. Civic science for sustainability: Reframing the role of experts, policy-makers, and citizens in environmental governance. *Global Environmental Politics* 3(4): 24–41. (Reprinted in Harding ed. 2011)

Barad, Karen. 2007. *Meeting the universe halfway: Quantum physics and the entanglement of matter and meaning*. Durham: Duke University Press.

Beck, Ulrich. 1997. *The reinvention of politics: Rethinking modernity in the global social order*. Cambridge: Polity Press.

Bloor, David. 1976. *Knowledge and social imagery*. London/Boston: Routledge & K. Paul.

Brockway, Lucile H. 1979. *Science and colonial expansion: The role of the British royal botanical gardens*. New York: Academic.

Brush, Stephen B., and Doreen Stabinsky (eds.). 1996. *Valuing local knowledge: Indigenous people and intellectual property rights*. Washington, DC: Island Press.

Calhoun, Craig, Michael Warner, and Jonathan Van Antwerpen (eds.). 2007. *Varieties of secularism in a secular age*. Cambridge: Harvard University Press.

Campbell, Marie, and Ann Manicom. 1995. *Knowledge, experience, and ruling relations*. Toronto: University of Toronto Press.

Collins, Patricia Hill. 1991. *Black feminist thought: Knowledge, consciousness, and the politics of empowerment.* New York: Routledge. (Reprinted in Harding ed. 2004)

Collins, Harry, and Robert Evans. 2007. *Rethinking expertise*. Chicago: University of Chicago Press.

Daston, Lorraine, and Peter Galison. 2007. *Objectivity*. New York: Zone Books.

Daston, Lorraine, and Peter Galison. 2008. Objectivity and its critics. *Victorian Studies* 50: 666–677.

Dupré, John. 1993. *The disorder of things: Metaphysical foundations for the disunity of science*. Cambridge: Harvard University Press.

Elam, Mark, and Oscar Juhlin. 1998. When Harry met Sandra: Or using a feminist standpoint to get the right perspective on the sociology of scientific knowledge. *Science as Culture* 7(1): 95–109.

Galison, Peter, and David J. Stump. 1996. *The disunity of science*. Palo Alto: Stanford University Press.

Gilligan, Carol. 1982. *In a different voice*. Cambridge: Harvard University Press.

Goonatilake, Susantha. 1984. *Aborted discovery: Science and creativity in the third world*. London: Zed Books.

Hacking, Ian. 1983. *Representing and intervening*. Cambridge: Cambridge University Press.
Hacking, Ian. 1999. *The social construction of what?* Cambridge, MA: Harvard University Press.
Haraway, Donna. 1988. Situated knowledges: *The science question in feminism* and the privilege of partial perspective. *Feminist Studies* 14(3): 575–599. (Reprinted in Harding ed. 2004)
Haraway, Donna. 1989. *Primate visions: Gender, race, and nature in the world of modern science*. New York: Routledge.
Harding, Sandra. 1986. *The science question in feminism*. Ithaca: Cornell University Press.
Harding, Sandra. 1988. Feminism, science, and the anti-enlightenment critiques. In *Feminism/postmodernism*, ed. Linda Nicholson, 83–106. New York: Methuen/Routledge & Kegan Paul.
Harding, Sandra. 1998. *Is science multicultural? Postcolonialisms, feminisms, and epistemologies*. Bloomington: Indiana University Press.
Harding, Sandra (ed.). 2004. *The feminist standpoint theory reader*. New York: Routledge.
Harding, Sandra (ed.). 2011a. *The postcolonial science and technology studies reader*. Durham: Duke University Press.
Harding, Sandra. 2011b. Interrogating the modernity vs. tradition contrast: Whose science and technology for whose social progress? In Grasswick ed. 2011, 85–108.
Harding, Sandra. 2015. *Objectivity and diversity: Another logic of scientific research*. Chicago: University of Chicago Press.
Harding, Sandra, and Merrill Hintikka, eds. 1983/2003. *Discovering reality: Feminist perspectives on epistemology, metaphysics, methodology and philosophy of science.* Dordrecht: Reidel.
Hartsock, Nancy. 1983. The feminist standpoint. In Harding et al. 1983/2003, 283–310. (Reprinted in Harding ed. 2004)
Hayden, Cori. 2005. Bioprospecting's representational dilemma. *Science as Culture* 14(2): 185–200.
Headrick, Daniel R. 1981. *The tools of empire: Technology and European imperialism in the nineteenth century*. New York: Oxford University Press.
Hekman, Mary. 1997. Truth and method: Feminist standpoint theory revisited. *Signs: Journal of Women in Culture and Society* 22(2): 341–365. (Reprinted in Harding ed. 2004)
Hess, David J. 2007. Science in an era of globalization: Alternative pathways. In *Alternative pathways in science and industry: Activism, innovation, and the environment in an era of globalization*, Chap. 2. Cambridge: MIT Press. (Abridged in Harding ed. 2011, 419–438)
Hollinger, David A. 1996. *Science, Jews, and secular culture*. Princeton: Princeton University Press.
Hooks, Bell. 1983. *Feminist theory from margin to center*. Boston: South End Press.
Hubbard, Ruth, Mary S. Henifin, and Barbara Fried (eds.). 1992. *Biological woman: The convenient myth*. Cambridge, MA: Schenkman.
Intemann, Kristen. 2011. Diversity and dissent in science: Does democracy always serve feminist aims? In Grasswick ed. 2011, 111–132.
Jaggar, Alison. 1988. Feminist politics and epistemology. In *Feminist politics and human nature*, ed. Alison Jaggar, 353–394. Totowa: Rowman and Littlefield. (Reprinted in Harding ed. 2004)
Jakobsen, Janet R., and Ann Pellegrini (eds.). 2008. *Secularisms*. Durham: Duke University Press.
Jameson, Fredric. 1988. 'History and class consciousness' as an unfinished project. *Rethinking Marxism* I: 49–72. (Reprinted in Harding ed. 2004)
Jasanoff, Sheila (ed.). 2004. *States of knowledge: The co-production of science and social order*. New York: Routledge.
Jasanoff, Sheila. 2005. *Designs on nature: Science and democracy in Europe and the United States*. Princeton: Princeton University Press.
Keller, Evelyn Fox. 1983. Gender and science. In Harding et al. 1983/2003, 187–206.
Kellert, Stephen H., Helen Longino, and C. Kenneth Waters (eds.). 2006. *Scientific pluralism*. Minneapolis: University of Minnesota Press.
Kelly-Gadol, Joan. 1976. The social relations of the sexes: Methodological implications of women's history. *Signs: Journal of Women in Culture and Society* 1(4): 810–823.

Kenney, Sally, and Helen Kinsella (eds.). 1997. *Politics and feminist standpoint theories*. New York: The Haworth Press.
Kincaid, Harold, John Dupré, and Alison Wylie (eds.). 2007. *Value-free science? Ideals and illusions*. New York: Oxford University Press.
Kuhn, Thomas S. 1962. *The structure of scientific revolutions*, 2nd ed. Chicago: University of Chicago Press.
Latour, Bruno. 1993. *We Have Never Been Modern.* Trans. Catherine Porter. Cambridge: Harvard University Press.
Latour, Bruno, and Steve Woolgar. 1979. *Laboratory life: The construction of scientific facts*. Beverly Hills: Sage.
Levey, Geoffrey B., and Tariq Modood (eds.). 2009. *Secularism, religion and multicultural citizenship*. New York: Cambridge University Press.
Lloyd, Genevieve. 1984. *The man of reason: "Male" and "female" in Western philosophy*. Minneapolis: University of Minnesota Press.
Lloyd, Elisabeth. 1996. Science and anti-science: Objectivity and its real enemies. In *Feminism, science and the philosophy of science*, ed. Lynn Hankinson Nelson and Jack Nelson, 217–259. Dordrecht: Springer.
McClellan, James E. 1992. *Colonialism and science: Saint Domingue in the old regime*. Baltimore: Johns Hopkins University Press.
Megill, Allan, ed. 1994. *Rethinking objectivity*. Durham: Duke University Press. (Originally published in *Annals of Scholarship* 8(3–4) and 9(1–2), 1991/1992)
Mignolo, Walter. 1995. *The darker side of the renaissance: Literacy, territoriality and colonization*. Ann Arbor: University of Minnesota Press.
Mignolo, Walter. 2000. *Local histories/global designs: Coloniality, subaltern knowledges, and border thinking*. Princeton: Princeton University Press.
Millman, Marcia, and Rosabeth Moss Kanter (eds.). 1975. *Another voice: Feminist perspectives on social life and social science*. New York: Doubleday Anchor.
Moraze, Charles (ed.). 1979. *Science and the factors of inequality*. Paris: UNESCO.
Nandy, Ashis (ed.). 1990. *Science, hegemony, and violence: A requiem for modernity*. Delhi: Oxford University Press.
Narayan, Uma. 1989. The project of a feminist epistemology: Perspectives from a non-western feminist. In *Gender, body, knowledge*, ed. Susan Bordo and Alison Jaggar. New Brunswick: Rutgers University Press.
Novick, Peter. 1988. *That noble dream: The "objectivity question" and the American historical profession*. Cambridge: Cambridge University Press.
Nowotny, Helga, Michael Gibbons, and Peter Scott. 2001. *Re-thinking science: Knowledge and the public in an age of uncertainty*. Cambridge: Polity Press.
Petitjean, Patrick, Cathérine Jami, and Anne-Marie Moulin (eds.). 1992. *Science and empires: Historical studies about scientific development and European expansion*. Dordrecht: Kluwer.
Porter, Theodore M. 1995. *Trust in numbers: The pursuit of objectivity in science and public life*. Princeton: Princeton University Press.
Prakash, Gyan. 1999. *Another reason: Science and the imagination of modern India*. Princeton: Princeton University Press.
Proctor, Robert N. 1991. *Value-free science? Purity and power in modern knowledge*. Cambridge: Harvard University Press.
Reisch, George A. 2005. *How the cold war transformed science: To the icy slopes of logic*. Cambridge: Cambridge University Press.
Reiter, Rayna R. (ed.). 1975. *Toward an anthropology of women*. New York: Monthly Review Press.
Richardson, Alan W. 2003. Tolerance, internationalism, and scientific community in philosophy. In *Philosophy of science and politics*, ed. Friedrich Stadler and Michael Heidelberger, 65–89. Vienna: Springer.
Richardson, Alan W. 2006. The many unities of science. Politics, semantics, and ontology. In Kellert et al. 2006, 1–25.

Rose, Hilary. 1983. Hand, brain and heart: A feminist epistemology for the natural sciences. *Signs: Journal of Women in Culture and Society* 9(1): 73–90. (Reprinted in Harding ed. 2004)
Rouse, Joseph. 1996. *Engaging science: How to understand its practices philosophically*. New York: Cornell University Press.
Sachs, Wolfgang (ed.). 1992. *The development dictionary: A guide to knowledge as power*. London: Zed Press.
Sandoval, Chela. 1991. U.S. third world feminism: The theory and method of oppositional consciousness in the postmodern world. *Genders* 10: 1–24.
Sands, Kathleen. 2008. Feminisms and secularisms. In Jakobsen et al. 2008, 308–329.
Sardar, Ziaddun (ed.). 1988. *The revenge of Athena*. London: Mansell.
Schiebinger, Londa. 2004. *Plants and empire: Colonial bioprospecting in the Atlantic world*. Cambridge, MA: Harvard University Press.
Schiebinger, Londa, and Claudia Swan (eds.). 2004. *Colonial botany: Science, commerce, and politics in the early modern world*. Philadelphia: University of Pennsylvania Press.
Schuster, John A., and Richard R. Yeo (eds.). 1986. *The politics and rhetoric of scientific method: Historical studies*. Dordrecht: Reidel.
Selin, Helaine (ed.). 2008. *Encyclopedia of the history of science, technology, and medicine in non-western cultures*, 2nd ed. Dordrecht: Kluwer.
Shapin, Steven. 1994. *A social history of truth*. Chicago: University of Chicago Press.
Shapin, Steven. 2008. *The scientific life: A moral history of a late modern vocation*. Chicago: University of Chicago Press.
Shapin, Steven, and Simon Schaffer. 1985/1989. *Leviathan and the air pump. Hobbes, Boyle and the experimental life*. Princeton: Princeton University Press.
Smith, Dorothy E. 1987. *The everyday world as problematic: A sociology for women*. Toronto: Toronto University Press.
Smith, Dorothy E. 1990. *The conceptual practices of power: A feminist sociology of knowledge*. Toronto: Toronto University Press.
Sullivan, Shannon. 2010. The secularity of philosophy: Race, religion, and the silence of exclusion. In *The center must not hold: White women philosophers on the whiteness of philosophy*, ed. George Yancey. Lanham: Lexington Books.
Walby, Sylvia. 2001. Against epistemological chasms: The science question in feminism revisited. *Signs: Journal of Women in Culture and Society* 26(2): 485–509.

Chapter 4
The Journalist, the Scientist, and Objectivity

Peter Galison

4.1 Facts and Objectivity

Beginning in the mid-nineteenth century, a novel form of right depiction emerged in the sciences, crossing disciplines from anatomy to astronomy, and reshaping pictures of galaxies, plants, skulls, clouds, and fossils. This transformation in scientific practice developed together with an alteration in what it meant to be a scientist—a reformation of the scientific self. In *Objectivity*, Lorraine Daston and I tracked this change for the sciences through images because the compendia of scientific atlases offered a large, bounded, self-referential, and quasi-continuous body of work that reached across our range of interest both in disciplines and chronologically (from the eighteenth century through to the current day). By bracketing the question of what objectivity might mean in a myriad of other fields, we hoped to get some clarity in the domain of scientific representation. We promised ourselves we would return to see how practices of objectivity played out in other endeavors. Later, we told ourselves, we could see how the history of objectivity did—or did not—map to these other regimes. History, politics, literature, documentary film, journalism—each of these and others too have had their own objectivities. "Later" now being upon us, this is a first gesture toward relating a history of objectivity in science to that in journalism, ending with a first gesture toward a common understanding of contemporary debates about the objectivity of the digital image in science and in the world of print and post-print media.

Objectivity: a capsule summary. The core argument is this. The history of objectivity cannot be understood without a history of subjectivity any more than the concept of left can be elucidated without right, or up without down. By focusing on

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the shifting historical boundary between what is of us and what is of the world, we found that the epistemic and ethical issues were irreducibly intertwined. Managing the contours of the self and managing the contours of a world outside us are one in the same. The shoreline defines simultaneously the edge of a continent and the beginning of the sea. Objectivity, we found, was one epistemic virtue among others—sometimes pedagogical utility, precision, reproducibility, accuracy, even truth could pull against the ambition to hold oneself back and let nature write itself to the page.

Oversimplifying, to understand the history of objectivity, Daston and I found it helpful to grasp three starting points. First, an era starting in the eighteenth century and in some ways continuing to the present ("truth-to-nature") in which the objects depicted were not particulars but universals; not your or my skeleton, but the human skeleton in its perfection. The right *form of sight* here is idealization, undistracted by the particulars of a cracked rib or a caterpillar-eaten leaf. And the right kind of scientist, a genius or sage, with the capacity to see behind the curtain of appearances. Second, an era beginning in the mid-nineteenth century that supplemented but did not eliminate the first. "Mechanical Objectivity," as we called it, was characterized by a cultivated will to will-lessness—a quieting of our desires and aims, and a hunt for esthetic perfection. The mechanical site of this form of high objectivity in the sciences made a virtue of attending to particulars, and a vice of idealization—a form of sight that saw what, in the limit case, was given independently of us. Third, an era of trained judgment, in which the right kind of observer was an expert, not by inherent constitution (no genius), but instead by long and careful training that allowed the researcher to effectively re-identify patterns, eliminate artifacts of the apparatus, and categorize the world.

Concretely: by the 1850s, it was already acceptable for the Leipzig physiologist Otto Funke, the first to crystallize hemoglobin, to produce a reference volume of images, an atlas, in which he compiled drawings of what he observed through the ocular lens, even when the scientist himself *knew* some things were depicted in ways that departed from nature. A yellowing around the edges, refractions of corners that were clearly distorted, these were artifacts of the optics and sample—but Funke *nonetheless* drew what he saw not what he knew—a *moral* as well as an epistemic necessity. Funke was not alone. All across Europe and the United States, the old form of scientific atlases began taking on a new form, minimal in interpretation, long-lasting in physical production, faithful to observation rather than ultimate truth, trilingual for accessibility, and printed to last for the ages. Other scientists, too, from Berlin to Boston, began to hold themselves back, even when everything they believed spoke against such self-abnegation and for corrective "improvement."

Journalism, like science, was on the move in the 1830s. In Jacksonian America, a new kind of newspaper began to emerge, not the six-penny party newspaper, but the single penny paper, drawing on a mass distribution, affiliated with no political party, promiscuous in its advertisements. This was a paper that profited not from group allegiance, but precisely from the expanded audience brought by *non*-alignment.[1] Some years later, during the American Civil War, reporters began standardizing a form of writing that reinforced this non-affiliation in the form of the now canonical inverted pyramid: begin with the who-what-when-where and the widest construal of events, and then, gradually, paragraph by paragraph, bring the focus on ever greater detail. Finally, during the 1890s, "news" and "opinion" came to occupy separate sections of the paper with differing writing styles. For one historian of journalism, "[t]he 1890s is a good place to end a history of 'objectivity' because it is one of the first decades when 'objectivity' was a recognized ethic in journalism, but also one of the last in which 'objectivity' goes basically unquestioned."[2]

In journalism as in science, objectivity was a conjointly moral and epistemic task. Historian Michael Schudson put it this way: "the belief in objectivity in journalism, as in other professions, is not just a claim about what kind of knowledge is reliable. It is also a moral philosophy, a declaration of what kind of thinking one should engage in, in making moral decisions."[3] Also emphasizing the ethical dimension, Stephen J. A. Ward invites us to empathize with those reporters who clung to aspects of objectivity in the few decades after the first World War: "Buffeted by controversy and powerful crosscurrents in society, these journalists looked for a way forward for their profession. They invented objectivity as an ethical signpost in troubled times."[4] Agreed. Though journalistic arguments over objectivity engage more directly with the politics of the moment, and the scientific disputes more with the production of knowledge, the two histories of objectivity (scientific and journalistic) cross at many points. Now that we have a start in understanding both, it is worth stepping back to make the comparison (and contrast) explicit.

Begin with facts. Facts were the shared fragments of these decades from 1830 to 1890. Facts were the residue left over after separation from party, loyalty, payment, value, interpretation, and emotion. Fact-based news was mobile, saleable, communicable. *Facts* became the journalists' common coin—not as a procedure but as modules of information.[5] The insistent nineteenth-century hunt for "Who, What, When, Where" became the byword, posted in newsrooms; these bits were where the news story began. For some time now, historians have fought over how to explain the turn to facticity—sometimes demeaning the attention of those years as "naïve empiricism": they have attributed the result to new printing technologies, to the expansion of literacy, to newly dominant political ideologies, to the "natural evolutionary history" of reporting that any society must pass through. But these accounts, however we assess them, all speak to the rise of the fact—rather than an objectivity constituted by a will-to-will-lessness. That came later.

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[1] Schudson (1978). Dan Schiller modifies the Schudson account by emphasizing that the democratic, middle-class audience exists more as an ideal than a reality Schiller (1981).

[2] Mindich (1998, 114).

[3] Schudson (1978, 8).

[4] Ward (2004, 257). Ward offers a passionate and persuasive argument for a new "pragmatic objectivity" to come after the limits of "traditional objectivity" have become all too clear.

[5] On the history of the scientific fact, see for example Daston (1991) and Poovey (1998).

4.2 Journalistic Objectivity

Walter Lippmann was one of the first journalists to re-think the factual, not simply in antitheses (fact/opinion, fact/interest), but in a larger, more conceptual frame. He was interested in what made understanding possible; in this sense a shift to a more Kantian problematic (he had studied philosophy at Harvard with William James and George Santayana). The war too created a new set of conditions under which knowledge of events had to be understood—the United States under President Wilson had, like France and Germany, instituted dramatic wartime censorship structures. While not denying that censorship was necessary, Lippmann balked at its rampant use.

Lippmann's framed his notion of journalistic objectivity in the years following World War I as a condition of visibility. Certain conditions about the world must obtain for something even to be a candidate for the news:

> [T]he news is not a mirror of social conditions, but the report of an aspect that has obtruded itself. The news does not tell you how the seed is germinating in the ground, but it may tell you when the first sprout breaks through the surface. It may even tell you what somebody says is happening to the seed under ground. It may tell you that the sprout did not come up at the time it was expected. The more points, then, at which any happening can be fixed, **objectified,** measured, named, the more points there are at which news can occur.[6]

The question then arises: what are the conditions under which "happenings" can be fixed, objectified and measured? What makes these so? Some events present themselves, more or less evidently, as "obtrusive." So Lippmann argued. A report on a county Clerk's desk shows that John Smith is bankrupt. But take even a step or two beyond the filing and you are in a murkier world: "The story of why John Smith failed, his human frailties, the analysis of the economic conditions on which he was shipwrecked, all of this can be told in a hundred different ways. There is no discipline in applied psychology, as there is a discipline in medicine, engineering, or even law, which has authority to direct the journalist's mind when he passes from the news to the vague realm of truth. There are no canons to direct his own mind, and no canons that coerce the reader's judgment or the publisher's." One journalist's "version of the truth" is but one such take, and journalism, like fiction writing, can never lay claim to a universal, omniscient vision. Sinclair Lewis, Lippmann writes, can never show that he has the full and definitive truth of Main Street.[7] So it is for journalists. Newspaper writers aware of their own "weaknesses" know all too well that that they stand somewhere, enframed by interests and accepted ways of seeing. Lippmann: "there is no **objective** test, [the newspaper writer's] own opinion is in some vital measure constructed out of his own stereotypes, according to his own code, and by the urgency of his own interest. He knows that he is seeing the world

[6]Lippmann (1922, 341), emphasis added. On Lippmann's invocation of a "scientific naturalism" and a wide search of the literature showing the paucity of references to journalistic objectivity before Lippmann see e.g. Richard Streckfuss (1990).

[7]Lippmann (1922, 360).

through **subjective** lenses. He cannot deny that he too is, as Shelley remarked, a dome of many-colored glass which stains the white radiance of eternity."[8]

This dependence of events on the writer meant that, for Lippmann, there was a sociality to the ontology of news—what counts as an event must be of a certain kind. Otherwise, it ends up utterly dependent on the situation not just of the writer, but of the writer's surround: "Unless the event is capable of being named, measured, given shape, made specific, it either fails to take on the character of news, or it is subject to the accidents and prejudices of observation." So news truly is a form of *collective* empiricism, an empiricism that relies, fundamentally on the nature of that collectivity's institutions.

> The quality of the news about modern society is an index of its social organization. The better the institutions, the more all interests concerned are formally represented, the more issues are disentangled, the more **objective criteria** are introduced, the more perfectly an affair can be presented as news. At its best the press is a servant and guardian of institutions; at its worst it is a means by which a few exploit social disorganization to their own ends. In the degree to which institutions fail to function, the unscrupulous journalist can fish in troubled waters, and the conscientious one must gamble with uncertainties.[9]

News, Lippmann implies, cannot make a society democratic; in some sense news must grow up with democratic institutions. True, the press could stand as a corrective to governmental abuse and inform the citizens. But without the institutional structures that cast a "searchlight" on certain events, events themselves lose any rigidity of structure. Rendered malleable, the whole of our reality shifts and distends in the hands of unconstrained and often unscrupulous journalists.

"The study of error," Lippmann insisted, "is not only in the highest degree prophylactic, but it serves as a stimulating introduction to the study of truth. As our minds become more deeply aware of their own **subjectivism**, we find a zest in objective method that is not otherwise there." Here is the kind of writing about journalism that makes it into a form of epistemology—a study of journalistic error that forms just the kind of probe that the epistemologist of science Alexandre Koyré found tracking errors in the study of science. Lippmann: "We see vividly, as normally we should not, the enormous mischief and casual cruelty of our prejudices. And the destruction of a prejudice, though painful at first, because of its connection with our self-respect, gives an immense relief and a fine pride when it is successfully done."[10] Removal of prejudice is a kind of cultivation of self, a re-making of who we are that alters our perception and therefore our assessment of our world:

> There is a radical enlargement of the range of attention. As the current categories dissolve, a hard, simple version of the world breaks up. The scene turns vivid and full. There follows an emotional incentive to hearty appreciation of scientific method, which otherwise it is not easy to arouse, and is impossible to sustain. Prejudices are so much easier and more interesting. For if you teach the principles of science as if they had always been accepted,

[8]Lippmann (1922, 360), emphasis added.

[9]Lippmann (1922, 363), emphasis added.

[10]Lippmann (1922, 409–410), emphasis added.

> their chief virtue as a discipline, which is **objectivity,** will make them dull. But teach them at first as victories over the superstitions of the mind, and the exhilaration of the chase and of the conquest may carry the pupil over that hard transition from his own self-bound experience to the phase where his curiosity has matured, and his reason has acquired passion.[11]

Here is the kind of cultivation of self, the extirpation of subjectivity that the German physiologist Rudolf Virchow was after in his 1877 address to the Versammlung Deutscher Naturforscher und Ärzte,

> I have been teaching my science for more than thirty years, and . . . in these thirty years I have honestly worked on myself, to do away with ever more of my subjective being and to steer myself ever more into objective waters. Nonetheless, I must openly confess that it has not been possible for me to desubjectivize myself entirely. With each year, I recognize yet again that in those places where I thought myself wholly objective I have still held onto a large element of subjective views.[12]

For Virchow the moral-epistemic battle was pitched against the subversive subjectivities of the scientific self—"my opinions, my representations, my theory, my speculation."[13] It demanded patience and more: a cultivation of the scientific self through skill and art (*Geschick und Kunst*)—it was just this working on the scientific self that was so central for us in *Objectivity*. For Lippmann in 1922, the analogous self-struggle aimed to rein in the "prejudices," "codes," "superstitions," and "self-bound experience" that over-simplified the world. In both cases—Lippmann's journalistic objectivity and Virchow's scientific objectivity—the aim was to create a self open, *attentive* to the world, one that was, indeed, more like science. The difference was that Virchow's struggle was alone; Lippmann's demanded a self-analysis that was always already social. But individual or collective, the self-cultivation was a precondition for both science and journalism, and scientific objectivity hovered nearby as a model for journalistic self-conditioning.

Leading historians of journalism disagree about when to date the beginning of objectivity as an ideal, some identify the concept with the fact-hunting of 1830–1870, others with the more explicitly nonpartisan papers of the 1880s and 1890s. But the strongest arguments locate the objective in the 1920s and 1930s—when the term "objectivity" enters explicitly.[14] Intriguingly, this occurs around the time when (so we have argued) mechanical objectivity comes under pressure—and interpretation,

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[11] Lippmann (1922, 410), emphasis added.

[12] Virchow (1877, 74).

[13] Virchow (1877, 74).

[14] Schudson (1978, 120): [By the 1920s,] "People came to see even the findings of facts as interested, even memory and dreams as selective, even rationality itself a front for interest or will or prejudice. This influenced journalism in the 1920s and 1930s and gave rise to the ideal of objectivity as we know it." Or again, p. 122, only after World War I, "when the worth of the democratic market society was itself radically questioned and its internal logic laid bare, did leaders in journalism and other fields, like the social sciences, fully experience the doubting and skepticism democracy and the market encouraged. Only then did the ideal of objectivity as consensually validated statements about the world, predicated on a radical separation of facts and values, arise."

indeed *subjective* interpretation comes to be seen as a necessary part of scientific inquiry. And here the historical epistemology of the two domains, journalism and science, begins to cross. Because in both, objectivity remains both an ideal and a target: we want objectivity, we despise it.

On 27 May 1939, the press baron, editor of TIME, Henry Luce, came to the Buckwood Inn, in Shawnee-on-the-Delaware, to address his salesmen—with a blast against objective journalism. TIME, he argued, is certainly not simply "impartial"; it never was and never will be. The magazine "is attacked with equal or slightly varying bitterness for being pro and con the same thing. What is most of all amazing about this reputation is that never, at least with my knowledge and consent, did TIME ever claim impartiality. TIME's charter is that TIME will tell—will tell the truth about what happened, the truth as it sees it. Impartiality is often an impediment to truth. TIME will not allow the stuffed dummy of impartiality to stand in the way of telling the truth as it sees it."[15]

One might think that impartiality was only a particular facet of objectivity, that objectivity itself would be safe from attack by one of the most establishment, conservative American magazines—at the height of the Cold War. But, if anything, the Cold War redoubled Luce's cynicism about the concept. On 27 March 1950, TIME excoriated *The New York Times* for its blind adherence to objective reporting when *a*symmetry was in order. *The Times* had published an article about forced labor camps in the Soviet Union and then one the next day on the abuse of "wetbacks" in American camps. Here is TIME:

> Last week, the New Leader's William E. Bohn read The New York Times a forceful lesson in the dangers of ***mechanical objectivity*** . . . [the Times'] two headlines equate the system of contract labor in the U.S., which sends a few hundreds of thousands of workers across the country under admittedly evil conditions, with the Soviet system of concentration camp slavery which means deaths to millions . . . . [Mexicans] swam the Rio Grande . . . But there is no record of anyone crossing any body of water to reach a Russian concentration camp. To pretend that the two evils are at all comparable is to perpetrate an enormous and dangerous falsehood . . .[16]

In 1952, Luce let the TIME editors know in no uncertain terms how limiting objectivity was: "We are for objectivity because there is objective truth, truth in the universal, scientific truth, moral truth, which is quite independent of what anyone of us or all of think at any given time. Majorities do not make truth. Intellectual fashions do not make truth. Individual prophets come nearer to it—Amos or John the Baptist or Walt Whitman." Luce went on to distinguish two meanings of journalistic objectivity. One was tonal: flat, voided of emotion. That aspect of objectivity was optional. But cross the line to another, to the search for a journalism that had no presuppositions involving value or interpretation, and there one courted nonsense.

Not extension of naïve empiricism, but reaction against skepticism, "a method designed for a world in which even facts could not be trusted."

[15]Luce (1969, 56–57).

[16]TIME 55, no. 13 (1950), p. 30, emphasis added.

Such antiseptic journalism was, for Luce, impossible. "*That* is a modern usage and that is strictly a phony. That I had to renounce—and denounce. When we say 'the hell with objectivity,' this is what we are talking about. It is both theoretically and practically impossible to select, recognize or organize facts without using value judgments."[17]

Luce's anti-objectivity hit politics precisely when Cold War reporting symmetrized what for him was anything but a symmetric situation. Balance—on Luce's view—was nothing but apologia. In a 1959 editorial, "Objectivity Rampant," TIME blew a gasket at the way that the media wrote about Russian "tourist" [Anastas Ivanovich] Mikoyan, Krushchev's primary emissary, as he made his way around the United States. First, TIME insisted, the accounts of *The New York Times* and *Minneapolis Tribune* were simply "unbalanced," having "leaned over backward to preserve the 'objectivity' in which the U.S. press takes inordinate pride. Most stories ran as straightforward accounts of the rubberneck tour, without qualifications, without reservations, without showing cautious awareness of the other Mikoyan, the calculating Russian emissary, who followed Tourist Mikoyan everywhere he went." That said, when too *un*-balanced (like the *Daily News*) the reporting was so negative as to create sympathy for Mikoyan. Overall, though "The US press did not buy Salesman Mikoyan's wares, but in the name of objectivity it made them look pretty good." Nor surprisingly, the Russians themselves had a fine gloss—one that illustrated just how dangerous "parallels" could be: the Russian Moscow Literary Gazette "dredged U.S. history for a parallel to Mikoyan's visit, recalled how good-will Ambassador Ben Franklin soothed monarchist France's prejudices and suspicions, successfully sold himself and the infant U.S. republic."[18]

But attacks on objectivity from the Cold War right were soon drowned by ripostes from the left. Over the course of the 1960s, no end of blasts was aimed at objectivity—advocacy journalism, "new journalism," – and not least the "gonzo journalism" of Hunter S. Thompson. Many at *The New York Times* were offended by these inroads into the hard-won ethos of objectivity. In October 1972, Lester Markel, the retired editor of *The New York Times*, lamented that "the effort for objectivity has been made tougher by the advent of two loudly-trumpeted techniques: 'advocacy' and 'new' journalism." Of course reporters were human, of course their prejudices should be made clear. But the application of "techniques of fiction" to nonfiction carried grave dangers. Composite characters, for example, offered a slippery slope away from reality, and he recognized that "it is often possible for facts to get in the way of real truth." Still, the *Times* editor judged that the solution to problems

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[17]Nov 14, 1952, Henry Luce to TIME editors, in Luce (1969, 70–71); or just a bit later, 4 May 1953: "The Fetish of Objectivity," TIME 61, no. 18, p. 51. TIME quotes the Denver Post: "The reporter was told his first paragraph . . . should tell the 'who, what, when, where and why'—and no more . . . . The pure factual objectivity has often been a will-l'-the-wisp . . . Newspapers should continue to strive for as much objectivity as possible, but should have no taboos against 'interpretation' when [it] is necessary to an understanding of any happening . . . The trend will be toward more 'interpretation . . . '."

[18]TIME 73, no. 4 (1959), p. 58.

associated with classical objectivity lay not in the directions of new techniques, but rather in the extension and refinement of "old journalism."[19]

Historians on the left decried the way that turn-of-the-century "objective" reporting on lynchings had carefully followed the who/what/when/where line and simply narrated the killing of African-American men, women and children as they were murdered. The articles even offered "reasons" for the killing—both sides, balance, and for the critics of the 1960s and 1970s, an ethical violation of the first order. Vietnam brought these issues into an embattled present, no place more vividly than in the way the Gulf of Tonkin Incident—the putative casus belli of the war—was reported. Reporters just repeated the government version—the North Vietnamese had, unprovoked, fired on an American ship. When, later, long after even high government officials admitted the evidence of any shots at the ships was probably an illusion, the reporter who broke the story was asked about it. He replied that if the President says black is white, you write, "the president says that black is white." Historian Daniel C. Hallin concluded that here, as in so many other instances during the divisive Vietnam War, "The effect of 'objectivity' was not to free the news of political influence but to open wide the channel through which official influence flowed."[20]

If we want to make commensurable the histories of journalistic and scientific objectivity, we also need to know the guiding rules—how objectivity-as-practice is taught and regulated morally. In this respect, it is worth attending to the long string of "Codes of Ethics" adopted by the Society of Professional Journalists—going back decades and (after 1973) revised every 10 or 15 years: 1926, 1973, 1984, 1987, 1996.[21]

As late as 1987, the preamble to the Code of Ethics put objectivity front and center. Here is the preamble, underscoring objectivity:

> SOCIETY of Professional Journalists, believes the duty of journalists is to serve the truth.
>
> We BELIEVE the agencies of mass communication are carriers of public discussion and information, acting on their Constitutional mandate and freedom to learn and report the facts.

[19]Markel (1972). On the writing genre of "new journalism" (focusing on Tom Wolfe), see Hanson (1997). Hanson follows Tom Wolfe in seeing "new journalism" as an alternative or even successor to "objective" ("old journalism"), and then goes on to characterize the formal aspects of this new narrative style.

[20]Calcutt and Hammond (2011, 102); Hallin (1989, 25 and 70–71). Gaye Tuchman's 1972 article "Objectivity as Strategic Ritual: An Examination of Newsmen's Notions of Objectivity" attacked the idea of objectivity differently: by arguing that "'objectivity' may be seen as a strategic ritual protecting newspapermen from the risks of their trade." Here ritual is defined "as a routine procedure which has relatively little or only tangential relevance to the end sought." It is "compulsive" and "strategic." Tuchman (1972, 660, 661).

[21]"The present version of the code was adopted by the 1996 SPJ National Convention, after months of study and debate among the Society's members. Sigma Delta Chi's first Code of Ethics was borrowed from the American Society of Newspaper Editors in 1926. In 1973, Sigma Delta Chi wrote its own code, which was revised in 1984, 1987 and 1996." From "Why Doesn't the SPJ Enforce its Code of Ethics." Accessed July 4, 2012. http://www.spj.org/ethicsfaq.asp

> We BELIEVE in public enlightenment as the forerunner of justice, and in our Constitutional role to seek the truth as part of the public's right to know the truth.
>
> We BELIEVE those responsibilities carry obligations that require journalists to perform with intelligence, **objectivity**, accuracy, and fairness.[22]

The Code then goes on to devote the entirety of section IV to "ACCURACY AND OBJECTIVITY," under which falls:

Good faith with the public is the foundation of all worthy journalism.

1. Truth is our ultimate goal.
2. **Objectivity** in reporting the news is another goal that serves as the mark of an experienced professional. It is a standard of performance toward which we strive. We honor those who achieve it.[23]

Interestingly, though truth is the "ultimate goal," objectivity is clearly distinct from it ("another goal") that marks, as a practice, the standard of performance. While truth may be the unattainable asymptote toward which the journalist strives, there are those who "achieve" objectivity. Truth is a thing—a platonic one perhaps, but objectivity is a process that can be followed and even reached, honorably, in this, our sublunary world.

By 1996, the Society of Professional Journalists had reconsidered their position, and the revised code of that year eliminated objectivity in every instance. Do no harm, said the Code, act independently, be accountable, treat subjects as human beings—all this and more. But objectivity had vanished from sight.[24]

But not for long. In 1997, Sandra S. Nelson, a journalist on the education beat, and, in her non-working life, a political activist, came head to head with her employer, McClatchy Newspapers, Inc. The *News Tribune*, in Washington State, was not happy with her citizen role, organizing rallies, picketing, lobbying for a ballot initiative, among other things, and transferred her to copy-editing. She brought suit, to which the paper rejoined, "Nelson's activities violated the [newspaper's] ethics code and raised concern about TNT's appearance of objectivity." Or in another formulation, this one to the United States Supreme Court, the McClatchy Company insisted, "The editorial standards at issue in this case are fundamental to the First Amendment's guarantee of a free press. Objectivity lies at the heart of *The News Tribune's* presentation of the news, and *The News Tribune's* requirement that reporters refrain from political activism directly supports the newspaper's actual and perceived objectivity." In the end, The U.S. Supreme Court let the Washington ruling stand: the right to a free press, said the court, included the right to impose a kind of political abstinence on its reporters—and this trumped the reporters' rights

[22] http://ethics.iit.edu/ecodes/node/4340, emphasis added.

[23] http://ethics.iit.edu/ecodes/node/4340, emphasis added.

[24] http://www.spj.org/ethicscode.asp

of free expression.[25] Other states, commentators worried, could well follow the state of Washington's move toward legally binding objectivity.

News of the death of journalistic objectivity is, it seems, premature. What is clear from this history is that journalistic objectivity, like that of scientific objectivity, demands, above all, a conditioning of the self toward different modes of self-restraint—from avoiding the imposition of a pet theory to stripping an election sticker off your car. As of January 2012, *The New York Times* let its reporters know that it continues to demand a curtailing of citizen life to maintain the life of reporter. Reporters should not have too much friendship with news sources, spend too much time with them, become romantically involved, should not pay their sources or receive gifts from them, or cooperate in ventures. "The people of our company are family members and responsible citizens as well as journalists. Nothing in this policy is intended to abridge their right to live private lives—to educate their children, to worship and take part in community affairs. But like other dedicated professionals, we knowingly accept disciplines—in our case, with the goal of ethical and impartial journalism." No sporting of political buttons on the job, says the *Times*. Indeed, no political insignia of any kind. Nor did the long arm of objectivity halt at the edge of work: Staff members may not give money to candidates. They may not seek public office. They may not march or rally. At the limit, a certain degree of local involvement might be acceptable, but not wider. Reporters may not report about spouses or close relatives—and the list goes on.[26]

Former CNN assignment editor David T. Z. Mindich has argued that "objectivity" should be replaced by more specific characteristics: detachment, nonpartisanship, inverted pyramid writing, reverence for facts, and balance.[27] I am inclined to agree that a more analytic assessment of objectivity is indeed required, but would classify the characteristics differently. Reverence for facts, for example, was a preoccupation for journalists in the nineteenth century, long before objectivity came into the picture—the focus on facts was associated, as others like Schudson have argued, with the penny press, grounded in an economic model premised on a separation from a particular party or group in its origin or in its destination

[25]See Calvert (1998–1999, 23 and 31–32). Content downloaded/printed from HeinOnline (http://heinonline.org), January 17, 2012, 18:33:30 201232.

[26]The New York Times Company, "B1. Participation in Public Life," in Policy on Ethics in Journalism. http://www.nytco.com/press/ethics.html#keeping. In 2010, *Daedalus* published a full issue on journalistic ethics—and though it raised the deep challenges to objectivity in the digital, online age, it concluded that the benefits of objectivity outweighed its costs. Jane B. Singer: "In a networked environment, interaction with audience members has become integral to the journalistic process. Consider again that notion of objectivity. One of the most hotly debated issues in the industry today is whether objectivity remains valuable (or even plausible) or whether it is being superseded by an ethical zeitgeist better suited to the rise of a relativistic medium. An emerging consensus seems to suggest that journalistic credibility in an unfettered information environment remains crucial and rests to a significant extent on independence from partisan or factional interests." Singer (2010, 95).

[27]Mindich (1978).

audience. The inverted pyramid writing structure seems even more particular—useful as it may be, it is one among many forms of exposition. Detachment and balance, different as they are, seem more appositely associated with objectivity in journalism.

What we have in the history of objectivity is a continuing, forever unfinished construction of professional selves—through practices of self-abnegation. What is it to be a scientist? What is it to be a journalist? Perhaps thinking of the period self as passing through layers, finding specific forms of sight alongside a shifting status of the image (or news or history) offers us a productive way to think the skilled self more generally.

Three intermediate conclusions: (1) the nineteenth-century journalistic engagement with impartiality, independence, and balance was not of a piece with the nineteenth-century scientific orientation toward objectivity as a form of consummate self-restraint, a "will to will-lessness." (2) After World War I, the key epistemic conditions of journalism moved beyond an all-out effort to find impartiality, independence, and explicitly embraced a more procedural-ethical ideal that was closer, explicitly closer, to the sciences. But this occurred at just the moment when objectivity in science itself was coming under revision by scientists. (3) The science-inflected objectivity was contested in journalism, from the moment it was introduced and that contestation has never ceased.

4.3 The Manipulated Image

Journalists have never stopped contesting objectivity in journalism—in every generation since 1920, you can find writers lamenting and celebrating the imminent death of the objective. Gonzo journalism, historical fiction, infotainment, talk radio, blogs; each new format has re-ignited the debate. But however complex the borrowing, overlapping, and renunciation of scientific objectivity, there is one domain where the scientific and journalistic have developed not just in parallel, but almost as a single entity: in the ethics and epistemology of digital manipulation.

Of course the manipulation of photographic images goes back as far as the photograph. Burning, dodging, and cropping brought objects in and out of visibility. So could adding or reducing contrast, modifying exposure time, positioning the camera, air-brushing elements, or staging the scene. Conventions about the "unretouched" photograph came late to photojournalism—*Life* magazine only began grappling with these issues in the 1930s.[28] But one change, more than any other, has brought

[28]See for example Hicks (1952, 42): "During its experimental period [1934–1936] *Life* enunciated for itself and adopted as part of its working philosophy the principle that the photograph should not be retouched except in the rarest circumstances. The day of the intervention of drawing between camera reporter and reader was over, yet most newspapers and some other magazines, primarily for mechanical but also for 'artistic' reasons, had carried retouching to a point where, in many instances, the printed picture was a combination of a photograph and hand 'art' work."

modification to the masses: Photoshop. That one program had a greater effect on scientific, journalistic, and advertising images than just about any innovation in cameras, film, or printing in the last 50 years. Created by John Kroll, a University of Michigan graduate student in 1987, the first Photoshop came out in 1990, and within a few years was being used across the world in millions of authorized and, by most guesses, an equal number of pirated copies.[29]

What is striking is that the very same techniques that are used to improve models' bodies have been deployed to tidy up scientific data and rearrange photojournalists' productions. In the wave of anxiety that has crashed over newspapers, magazines, and scientific journals, a new specialization has emerged: digital forensics. One of the most prominent of the new breed of investigators is Hany Farid, who is very clear that the modification of images goes back a very long way, at least as far as Mathew Brady's civil war images. What is different is the ease of such endeavors: "In today's world, anyone with a digital camera, a PC, Photoshop and hour's worth of time can make fairly compelling digital forgeries." According to Farid, the rise in fraud allegations about images has skyrocketed. In 1990, the Federal Office of Research Integrity reported that less than 3 % of scientific fraud charges were leveled against images. A little more than a decade later, that number was 26 % and by 2007, it was over 44 %.[30]

So similar are the issues faced, that Farid and his colleagues constantly track back and forth among the triad of science, fashion, and news. On the science side, the problem is so endemic that just about every major scientific publication has issued ethical guidelines for the use of digital images. Mike Rossner, managing editor of the *Journal of Cell Biology*, and his co-author and editor Kenneth M. Yamada, put it this way in the lead article "What's in a Picture? The Temptation of Image Manipulation" from 2004:

> It's all so easy with Photoshop. In the days before imaging software became so widely available, making adjustments to image data in the darkroom required considerable effort and/or expertise. It is now very simple, and thus tempting, to adjust or modify digital image files. Many such manipulations, however, constitute inappropriate changes to your original data, and making such changes can be classified as scientific misconduct. Skilled editorial staff can spot such manipulations using features in the imaging software, so manipulation is also a risky proposition (Fig. 4.1).[31]

In short: it is wrong and we will catch you.

In the 1980s, I was on a National Academy of Sciences committee looking into fraud, fabrication, and plagiarism in science. Image manipulation was a very minor

[29]See, for example, the following histories of Photoshop: http://creativeoverflow.net/history-of-photoshop-journey-from-photoshop-1-0-to-photoshop-cs5/ and http://en.wikipedia.org/wiki/Adobe_Photoshop_version_history. Accessed June 10, 2012.

[30]Dreifus (2007).

[31]Rossner and Yamada (2004). Published July 6, 2004. Accessed June 10, 2012. http://jcb.rupress.org/content/166/1/11.full

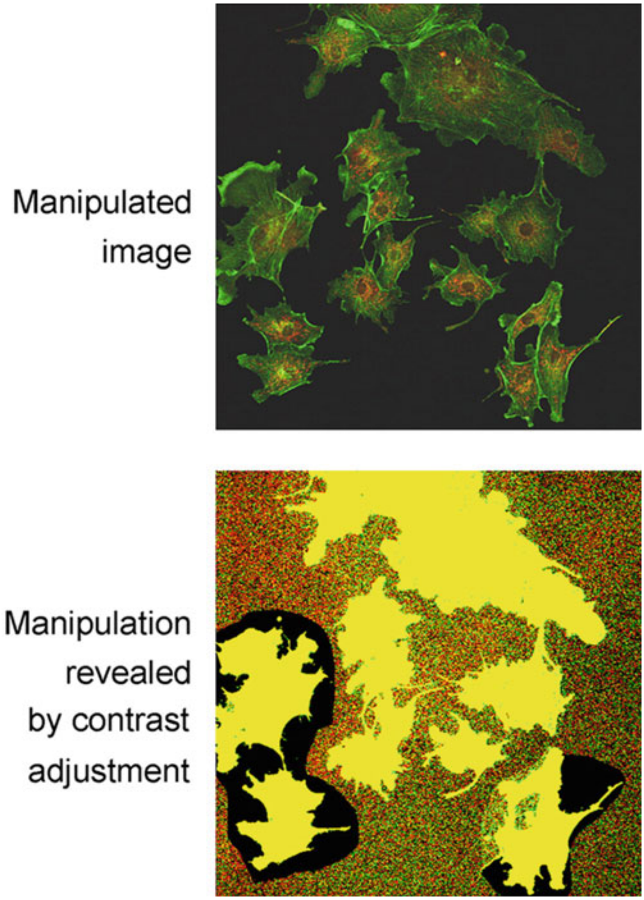

Fig. 4.1 Editors Rossner and Yamada gave an example of image manipulation in this instance: The *top* (manipulated image) appears to be a single microscopic view, while the *bottom* (exposed by a high contrast adjustment) reveals that a variety of cell images have been combined to present the illusion of a single image (Reproduced from Rossner and Yamada 2004)

consideration, and digital manipulation not even a ghost of a threat. Now *Science* has very explicit strictures about what constitutes allowable and forbidden changes to the image:

> *Science* does not allow certain electronic enhancements or manipulations of micrographs, gels, or other digital images. Figures assembled from multiple photographs or images, or non-concurrent portions of the same image, must indicate the separate parts with lines between them. Linear adjustment of contrast, brightness, or color must be applied to an entire image or plate equally.Nonlinear adjustments must be specified in the figure legend.

> Selective enhancement or alteration of one part of an image is not acceptable. In addition, *Science* may ask authors of papers returned for revision to provide additional documentation of their primary data.[32]

Here is the digital residue of objectivity: Otto Funke may have been tempted to remove the yellow chromatic distortion at the side of his microscopic lens, he may have wanted more than anything to fix the strange appearance of crystal edges refracted out of their physical positions. But his ambition was to draw what he saw, not what he knew. The equivalent here is the demand by *Science* that its authors refrain from assembling images from a variety of images, make manifest the breaks in parts of the image put up against one another, and apply shifts in picture quality to the whole, not selective parts of the image. Here and in other strictures was digital objectivity.

By the early 2000s, destabilizing image distortion in the sciences was arriving, and even being published at a clip that worried the editors of journals all the way up to the National Academy of Sciences. The *Journal of Cell Biology* reported systematically on the kinds of distortion they wanted to block: Electrophoresis gels entered into print with selectively "cleaned up" bands using the clone stamp. Cells were rearranged on a microscopic image by cut and paste. Immunogold data were enhanced in images and other dots excised to tidy up the image.

According to one group of editors, what drove this worrisome, expanding use of selective manipulation came down to a desire by authors to "beautify" their data. Here is *Nature Cell Biology*:

> By far and away the most prominent problem is that scientists do not take the time to understand complex data-acquisition tools and occasionally seem to be duped by the ease of use of image-processing programmes to manipulate data in a manner that amounts to misrepresentation. The intention is usually not to deceive but to make the story more striking by presenting clear-cut, selected or simplified data—an approach we have dubbed 'data beautification'. The *Journal of Cell Biology* has looked at the problem systematically and estimates that up to 20 % of accepted papers contain some questionable data, a rate that has not decreased since the journal instituted an editorial data-screening process.[33]

A generation ago, one of the main issues faced by journals was the too-easy use of statistical packages, with researchers running through a variety of statistical tests (one tailed, two-tailed, etc.) until they found one that gave them the best p-values. In response, the best funded of the journals (including the *New England Journal of Medicine*) armed themselves with sophisticated statistical staffs to reproduce the analysis made by each author group and to evaluate the appropriateness of the test deployed. But by then, the "beautification" had become the order of the day.

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[32]*Science*, "About the Journal, Information for Authors." Accessed January 17, 2012, 5:44 PM. http://www.sciencemag.org/site/feature/contribinfo/prep/prep_subfigs.xhtml

[33]The journal here cites *The Journal of Cell Biology* 166: 11–15 (2004); *Nature* 434: 952–953 (2005). From *Nature Cell Biology* 8: 101–102 (2006), on 101. Accessed June 10, 2012. http://www.nature.com/ncb/journal/v8/n2/pdf/ncb0206-101.pdf

For the 9 August 2007 issue of *Paris Match,* it seems that Nicolas Sarkozy needed even more beautification than immunogold as he canoed with his son in Lake Winnipesaukee (New Hampshire). What diet or exercise could not do, Photoshop could. Love handles vanished. Advertisements went much farther. In one photo, Twiggy's face was so utterly transformed in an Olay ad that the United Kingdom's Advertising Standards Authority banned the photo as deceptive. Another advertisement featuring the model Filippa Hamilton shrunk her waist to such a degree that it became a matter of public dispute. In response, Hany Farid and his team set to work to develop a metric that would be calibrated against human judgment and then proceed algorithmically, using geometric and photometric criteria, to establish a degree of distortion. "This metric," Eric Kee and Hany Farid, asserted, "correlates well with perceptual judgments of photo retouching and can be used to *objectively* judge by how much a retouched photo has strayed from reality."[34] Here we have it: an objective standard to measure departures from objectivity.

Precisely the kind of Photoshop manipulation that plagued the objective image in science journals, models and politicians, afflicted more mainstream journalism. "Beautification," again, only this time not the improvement of electrophoresis gels, love handles, wrinkles, or waistlines, but often the violent events that landed an image on the front-page of the world's newspapers. On Monday 31 March 2003, *The Los Angeles Times* printed a dramatic photograph by Brian Walsky on page one. The image showed a British soldier warning, gripping his gun, signaling to Iraqi civilians to duck under Iraqi fire just outside Basra. Two days later, *The L.A. Times* explained:

> After publication, it was noticed that several civilians in the background appear twice. The photographer, Brian Walski, reached by telephone in southern Iraq, acknowledged that he had used his computer to combine elements of two photographs [one with the soldier in a dramatic stance while a father cowered unobtrusively in the background, the other with the same soldier in an unassuming position, this time with the father running front and center toward the camera while he clutched the child], taken moments apart, in order to improve the composition. Times policy forbids altering the content of news photographs. Because of the violation, Walski, a Times photographer since 1998, has been dismissed from the staff. The altered photo, along with the two photos that were used to produce it, is published today on A6.[35]

Detecting these and other manipulations have involved forensic researchers on both sides of the science/journalism divide as an overlapping set of statistical analyses and image processing techniques moves smoothly back and forth. Farid and his company, for example, have become investigators and counter-fraud researchers for both journalism and science.

[34]Kee and Farid (2011, 19907), emphasis added. Accessed June 10, 2012. http://www.pnas.org/content/early/2011/11/21/1110747108.abstract

[35]*Los Angeles Times,* 2 April 2003. Accessed June 10, 2012. http://articles.latimes.com/2003/apr/02/news/war-1walski2

Even the measures recommended by scientists and journalists have begun to overlap. In both cases, it has become a commonplace for both sides to enforce a very similar ethics of the digital image. Both have regularly begun to require the submission of "raw" digital imagery, both militate against "selective" modification within the image, both require specification of what has been done to the image and with what programs. In the endless spiral of beautification and self-restraint, there will no doubt always remain a back and forth. But in the hunt for the truly raw "raw" image, in the countervailing drive to self- and outward policing, we see the very contemporary residue of a very old debate in the history of objectivity.

4.4 Conclusion

Objectivity has not only a history; it has histories. On the side of science, this particular epistemic virtue is quite different from truth or accuracy, precision or quantification. Instead, it rises in the mid-nineteenth century focused around the scientists' aspiration to hold themselves back, and, insofar as possible, to allow a kind of raw and particular nature to inscribe itself on the page. This mechanical objectivity does not form a central goal of journalism. Yes, there is a nineteenth century journalistic fascination with facts—who/what/when/where—but the compilation of itemized facts is quite different from the *procedures* of tracing, photographing, and inking that riveted the natural scientists. The scientists were after a collective empiricism, a codification of shared knowledge that would give them the basic working objects of their fields (clouds, elementary particles, skulls), while the journalists were after a mobile discursive medium that could appeal to a much wider range of audience and advertisers, formalized in pyramidical, unemotional text and instantiated in the penny press.

Scientific and journalistic objectivity did, however, come much closer to convergence in the years after World War I. For the scientists, more numerous, better resourced, and no longer so epistemically defensive, this was a time when they could frankly embrace trained judgment as a needed supplement to pure procedure. As scientists themselves put it, they were no longer willing to sacrifice accuracy on the altar of objectivity. That is, they would no longer trade a good, drawn image of a moon crater for a blurry black and white telescopic photograph. Conversely, if getting a shared, repeatable diagnosis from an encephalogram meant using a practiced eye, then so be it. On the journalists' side, far from bolstering their professional self-confidence, World War I had subjected reporters to tremendous pressure to follow government bulletins, propaganda, and censorship. Emerging from the war, newspaper men and women looked to science for a model of objectivity at just the moment the journalists were most shaken in their faith that it could be achieved. Put shortly: after the Great War, scientists began to supplement mechanical objectivity with trained judgment; journalists simultaneously entered the discourse of objectivity *and* launched a drive that has never ceased to guard a place for interpretation. Journalistic objectivity has, for its entire history, been always already disputed.

Starting in the late twentieth century, the two objectivities began to share a discourse of objectivity around the manipulable image. Here was a resource that radically facilitated the acquisition, processing, transmission, and reproduction of images. But at the same time, the numerical image offered a far greater vulnerability both to intentional and inadvertent misuse than ever before. More remarkable yet, it was the *same* set of Photoshop vulnerabilities and forensic diagnoses that launched this period of objectivity-anxiety on both sides of the science/journalism divide. This is a story that has only just begun. Like electronic warfare, measures, counter-measures, and counter-counter-measures form a continuing chain. Injunctions follow one after the other to the scientists and journalists: provide raw images, do not alter the meaning of the image, eschew the use of cloning tools, avoid excessive changes of contrast. But the twenty-first century is stable neither for scientists nor journalists. Commercial pressures alter both laboratory and newsroom, entrepreneurial scientists, infotainment journalists leave no consensus at all about what it means to be a scientist or journalist. And with these shifts, the ethical-epistemology of each is in flux.

Even for the past history of journalistic objectivity, we still have much to learn. We need a truly comparative history of objectivity in journalism, one that looks at strong journalistic traditions, for example, in Russia, France, Britain, and elsewhere that would analyze and periodize shifts in rhetorical style, the role of images, the shifting attention to facts, and the ambition to embrace or defy a procedural form of objectivity drawn from the sciences. Though not an easy task, it would be one that could do much to help us understand the ethical-epistemological disjunctions that have so shaped the last 150 years.

References

Calcutt, Andrew, and Philip Hammond. 2011. *Journalism studies: A critical introduction*. Oxford: Routledge.

Calvert, Clay. 1998–1999. The law of objectivity: Sacrificing individual expression for journalism norms. *Gonzaga Law Review* 34: 19.

Daston, Lorraine. 1991. Baconian facts, academic civility, and the prehistory of objectivity. *Annals of Scholarship* 8: 337–364.

Daston, Lorraine, and Peter Galison. 2007/2010. *Objectivity*. New York: Zone Books.

Dreifus, Claudia. 2007. A conversation with Hany Farid, proving that seeing shouldn't always be believing. *New York Times*, October 2. Accessed 10 June 2012. http://www.nytimes.com/2007/10/02/science/02conv.html?_r=1&oref=slogin

Hallin, Daniel C. 1989. *The uncensored war: The media and Vietnam*. Oxford: Oxford University Press.

Hanson, Ralph E. 1997. Objectivity and narrative in cotemporary reporting: A formal analysis. *Symbolic Interaction* 20: 385–396.

Hicks, Wilson. 1952. *Words and pictures: An introduction to photojournalism*. New York: Harper & Brothers.

Kee, Eric, and Hany Farid. 2011. A perceptual metric for photo retouching. *Proceedings of the National Academy of Sciences of the United States of America* 108(50): 19907–19912.

Lippmann, Walter. 1922. *Public opinion*. New York: Harcourt, Brace and Company.

Luce, Henry Robinson. 1969. In *Ideas of Henry Luce*, ed. John K. Jessup. New York: Atheneum.
Markel, Lester. 1972. Bias in the news. *New York Times*, October 31, p. 45.
Mindich, David T.Z. 1998. *Just the facts: How objectivity came to define American journalism*. New York: New York University Press.
Poovey, Mary. 1998. *A history of the modern fact: Problem of knowledge in the sciences of wealth and society*. Chicago: University of Chicago Press.
Rossner, Mike, and Kenneth M. Yamada. 2004. What's in a picture? The temptation of image manipulation. *The Journal of Cell Biology* 166(1): 11–15.
Schiller, Dan. 1981. *Objectivity and the news: The public and the rise of commercial journalism*. Philadelphia: University of Pennsylvania Press.
Schudson, Michael. 1978. *Discovering the news: A social history of American newspapers*. New York: Basic Books.
Singer, Jane B. 2010. Journalism ethics amid structural change. *Daedalus* 189: 89–99.
Streckfuss, Richard. 1990. Objectivity in journalism: A search and a reassessment. *Journalism Quarterly* 67: 973–983.
Tuchman, Gaye. 1972. Objectivity as strategic ritual: An examination of Newsmen's notions of objectivity. *American Journal of Sociology* 77: 660–679.
Virchow, Rudolf. 1877. Die Freiheit der Wissenschaften im modernen Staatsleben. *Amtlicher Bericht über die Versammlung Deutscher Naturforscher und Aertzte* 50: 65–77.
Ward, Stephen J.A. 2004. *The invention of journalism ethics*. Montreal: McGill-Queens University Press.

Part II
Objectivity as a Topic in Historical Epistemology

Chapter 5
The Ethos of Critique in German Idealism

Joan Steigerwald

The ambition of Lorraine Daston's and Peter Galison's *Objectivity* is considerable—not only do they offer a rethinking of our notion of scientific objectivity by historicizing it, but they also suggest a rethinking of our histories of science by doing it differently. They argue that scientific objectivity is a nineteenth-century phenomena, emerging through techniques and mechanisms of image making that aimed at a blind sight of nature in its particulars, unmarked by the prejudices, skills or judgments of the subject. Contrasting this mechanical objectivity to the ideal of truth-to-nature that preceded it and the trained judgment that followed it, they offer a "mesoscopic history" that traces the history of ways of seeing and technologies of scientific images across disciplinary and geographic borders. Although examining concrete practices, they eschew seeking specific hidden causes or philosophical frameworks to explain these changes, preferring to follow surface ramifications as they track the uses of scientific atlases across diverse scientific communities. Most singularly they offer an ethico-epistemic history, which argues objectivity is an epistemic virtue that is fused with a certain kind of scientific self (2008, 677). Daston's and Galison's history of objectivity is thus also a history of subjectivity. They contend that the nineteenth-century turn to mechanical objectivity was the result of the rejection of eighteenth-century emphases upon observational genius and discerning judgment in extracting a true image of nature from the mass of passively received sensation. A new emphasis upon self-restraint was perceived as necessary to discipline the overactive self of a previous generation. Scientific objectivity and its attendant scientific self, then, formed a new epistemic virtue, an ethos wedded to epistemology in the pursuit of scientific knowledge.

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Daston and Galison find the reception of Immanuel Kant's philosophy in the nineteenth century provided its vocabulary of objectivity and subjectivity. Kant argued that the transcendental unity of self-consciousness provides the necessary conditions for objective validity and universal knowledge. In contrast, empirical sensations provide only a subjective validity. Objective knowledge is thus determined by the subject's intellectual contributions, by the a priori forms of the understanding, rather than by perception. Daston and Galison emphasize that in the early nineteenth century Kant was variously appropriated and refracted through a range of traditions. By mid-nineteenth century, if the terminology of subjectivity and objectivity was retained, objectivity was redefined as a relation to an external object and subjectivity as inhering in the subject. The acquisition of objective knowledge was now seen as requiring the suppression of subjectivity and in effect a battle of the will against itself. A new anxiety over subjective intrusions into knowledge of nature resulted in a characterization of earlier periods, of the Enlightenment, of idealism and Romanticism, as excessively subjective, and as valuing a self that was not only speculative but also autocratic.

Given the significance of Kant, and post-Kantian German idealism, to this account of the emergence of scientific objectivity and the scientific self in the nineteenth century, it is worth pausing to examine their notions of objectivity and subjectivity more fully. Although Daston and Galison are primarily concerned with the reception of Kant and post-Kantian philosophy in the nineteenth century, rather than a close reading of Kant, they offer an admirable characterization of Kant's transcendental idealism in a few pages (2007, 205–10). But they also gather Kant into a prevalent philosophical position, as holding that epistemology is incompatible with ethos, and that epistemology belongs to the realm of objective validity and therefore stands opposed to subjectivity in all its forms (2008, 671). Their notion of epistemic virtue, however, can be effectively enlisted for a different characterization of Kant's philosophical project, one placing an ethos of critique at its centre. Indeed, rather than eschewing values in epistemology, his critical project can be regarded as making values fundamental to its proper practice. Kant presented his *Critique of Pure Reason* as responding to the demand of his age, "that reason should take on the most difficult of all tasks, namely, that of self-knowledge, and to institute a court of justice, by which reason may secure its rightful claims, while dismissing all groundless pretensions, not through decree, but in accord with its own eternal and immutable laws" (Axi).[1] Kant's legal language is often interpreted as demanding the imposition of rational rules onto our cognitive acts. But he is better understood as introducing his critique as a tribunal for the investigation of the validity of reason's claims and the warrant by which it acquires its laws. Kant critiqued as inadequate empirical philosophies that attempted to derive all knowledge from the senses. But more centrally his critique put the excesses of rational metaphysics on trial. His

[1]In citing the *Critique of Pure Reason*, standard references are used to A and B, the first edition (1781) and second edition (1787), found in volumes III and IV of the Akademie edition (1902–1983), respectfully.

transcendental idealism argued for the discursivity of human cognition, in which the a priori concepts of the understanding provide the form of experience and sensibility provides its content. A "mature and adult power of judgment," he contended, should accept how we ought to reason after reflection upon what human cognition can rightfully claim (A761/B789). The epistemic virtue Kant's *Critique of Pure Reason* validated and valued was the restriction of human cognition to appearances. His ethos of critique made a virtue of epistemic modesty (Axiv; A739/B767).

Kant's transcendental idealism has been critiqued for its rationalist austerity in its insistence upon the necessary laws of cognition, but his critique of pure reason also introduced a different form of austerity in its insistence upon a reconciliation of reason to the world of appearances. Post-Kantian German idealism is often characterized as abandoning Kant's critical strictures and unleashing reason for speculative flights. The nineteenth-century scientists documented by Daston and Galison worried over subjectivitist philosophies that abandoned the tethering of cognition to experience, and that imposed the ideas of reason onto the natural world and constructed willful, imaginary metaphysical systems. Yet two of Kant's most prominent successors, Johann Gottfried Fichte and Friedrich Wilhelm Joseph Schelling, deemed themselves as carrying further the Kantian critical project. They began with a critique of Kant's critique, a meta-critique, which interrogated the assumptions underlying transcendental idealism and the elements Kant excluded from critical reflection. If at times excessively reflexive and abstract, their meta-critical philosophies attempted to prevent the sedimentation of our conceptions of objectivity and subjectivity. In questioning Kant's settlement with appearances, they did not then claim a hypostatization of self-consciousness or speculative powers of nature, but interrogated the subjective and objective contributions to our cognition even more comprehensively than Kant. Indeed, Fichte and Schelling can be regarded as furthering the values introduced through critical reasoning, by emphasizing a philosophical reflection that lifted thought out of its unconscious habits, and stimulated thinking as well as moral action to be freely self-determining. In this sense, they can be regarded as extending Kantian critique as an epistemic virtue.

5.1 Kant's Critical Project: Critique as an Epistemic Virtue

Daston and Galison contend not only that the "modern sense of 'objectivity'" (as a "relation to an external object") and its opposition to subjectivity (as "personal, inner") are legacies of the nineteenth century, but also that these definitions are a reaction against and an inversion of Kant's definitions of objectivity and subjectivity (2007, 30–31). They emphasize how, in the nineteenth century, the "act of repeatedly distinguishing between objective image and subjective interpretation for image after image created the phenomena it was meant to enforce: the sharp boundary between objective image and subjective interpretation" (2008, 668). Yet Kant's uses of the terms objectivity and subjectivity are not as symmetrically opposed to the nineteenth-century uses as Daston and Galison suggest, and indeed he can be regarded as complicating the boundary between objectivity and subjectivity. Kant recognized that his notion of objectivity was revolutionary for his time, in relinquishing the common assumption that "our cognition must conform to objects" and instead "assuming the objects must conform to our cognition" (Bxvi). His model, however, was the new experimental natural science, as exemplified by Galileo, which "comprehended that reason only has insight into what it itself produces after a design of its own; that it must take the lead with principles for its judgments according to constant laws and compel nature to answer its questions" (Bxiii). Kant concluded that the laws of experience are the a priori forms of thought, with the objective validity of these laws ultimately founded in the transcendental unity of self-consciousness. But these a priori laws are only half the story of Kant's transcendental idealism. Empirical sensations only have subjective validity, nevertheless "the condition of the objective use of our concepts of the understanding is merely the manner of our sensible intuition, through which objects are given to us" (A286/B342). Critical reflection upon the validity of our cognitive claims led Kant to accept its boundedness to objectivity in both formal and empirical senses, and to make a virtue of being reconciled to these limits. Much of Kant's transcendental idealism concerns the synthetic relation of the two stems of human cognition, sensibility and understanding, and the mediating work of the imagination as well as laws in effecting these relations. Moreover, he contended that judgments of particular empirical laws require the projection of an order of nature that can only be a subjective regulative idea. Kant's critical project involved not only self-reflection, but also self-cultivation as epistemic virtues. Rather than sharpening the boundary between objectivity and subjectivity, then, Kant provided an analysis of the several ways and different layers in which subjective and objective contributions are made to our cognitive experience.[2]

Although human reason has a natural tendency to exceed the bounds of experience, Kant's critical philosophy set out to reign in such metaphysical flights, through reflection upon both the a priori forms and empirical matter of objective knowledge. Kant introduced his *Critique of Pure Reason* in 1781 as a court of justice to adjudicate the sources and boundaries of reason, and thus its rightful claims to cognition. He acknowledged that the utility of such a critique was largely negative, serving to purify reason from its metaphysical excesses (A11/B25).

[2]Kant's terms *Objekt* and *objektive*, and *Subjekt* and *subjektive*, are readily translated into English. Kant also used the term *Gegenstand*, commonly translated as object. Some scholars have argued for a systematic difference in Kant's uses of the terms *Objekt* and *Gegenstand*. Henry E. Allison, for example, organized his analysis of the two parts of Transcendental Deduction in the *Critique of Pure Reason* around the distinction between the objective validity of the categories with respect to objects [*Objekte*] in a logical sense, and the objective reality of categories with respect to objects [*Gegenstände*] understood in their applicability to human experience. Although some scholars have taken up Allison's distinction, it has also been widely criticized on philological grounds, and Allison himself has subsequently admitted that the distinction in Kant's use of the terms is misleading (Allison 2004, 476 n. 11). The Guyer and Wood translation renders both German terms as object.

Rei Terada, however, emphasizes the positive work of Kant's critique, its project of reconciliation to the world. She suggests that Kant's language of rights and boundaries "makes room for the odd notion of a right to appearance (2009, 84–85)" That we have a right to no more than appearance may come as a relief—in being able to have no more, in being supposed to do no more, we are free to do no more. Kant's critique suggested that to conclude that the limits to reason are inevitable already constitutes an endorsement of them; things that cannot be otherwise require our endorsement. This endorsement completes our obligation. We are obligated to accept the world of appearances, to accept the character of human cognition and the bounds of experience. We are also obligated to do no more; the right to claim no more is also the right to be free from guilt that there are questions that reason cannot answer. To accept Kant's settlement is to accept that it would not be desirable to possess any knowledge other than the knowledge we do possess.

Terada contends that to be satisfied with these necessary limits and thus to be reconciled to our world constitutes a minimal value, drawing attention to the austerity of the Kantian settlement. She argues that Kant, however, added to this minimal satisfaction in his 1790 *Critique of the Power of Judgment*, by introducing further powers of judgment. She highlights Kant's notion of objective liking (the feeling generated through judgment of the relative perfection of an object) as well as aesthetic judgments of taste (the feeling of pleasure in the apprehension of an object and the judgment it is beautiful), both of which enhance our satisfaction with our given world (2009, 73–87). But Terada sustains the common philosophical distinction of fact and value, in giving minimum value to Kant's acceptance of the restriction of human cognition to appearances, and in regarding value as subjective additions to fact perception through feelings of satisfaction. Daston and Galison, however, make a compelling case for our perceptions of fact being intimately entangled with full-bodied values. They do not restrict values to subjective liking, but give them both moral and epistemic significance. Kant's critique can be regarded as an ethos in their sense, as introducing a moral demand to accept how we ought to reason based upon a critical awareness of the limits of human cognition. His critical modesty, his reconciliation to appearances, then, is an epistemic virtue that is fully valued.

Kant granted that only a mature and adult power of judgment would practice such epistemic virtue. Yet he also declared his age was a "genuine age of criticism, to which everything must submit" (Axi). These arguments of the first *Critique* were developed by Kant in his 1784 essay "What is Enlightenment?" In this essay critique is given a larger public role, not only in producing individuals as rational beings but also in contributing to the formation of a rational society. Michel Foucault sees the significance of Kant's questioning in its reflection upon the present and upon the status of his own critical project. He sees Kant's critique as an ethos with a larger social significance, in which the critique of what we are is at the same time an analysis of the historical limitations imposed upon us and an experiment with the possibility of going beyond them (49–50). The critical project thus began with a critique of the present age, with Kant engaging in late eighteenth-century public debates over religion, education, politics, history and anthropology. But what makes Kant's project relevant to Daston's and Galison's history of objectivity is that for Kant critique was first and foremost a reflection upon the epistemic claims and pretensions of his time. Critical self-knowledge, as a reflection upon the rightful claims of human cognition and an analysis of its distinct elements, also involved a critique of contemporary philosophical positions. Indeed, Kant's posture of epistemic modesty can only be fully appreciated in relationship to the philosophical traditions against which he positioned it.

Transcendental reflection provided Kant with the perspective "through which [he could] make the comparison of representations in general with the cognitive power in which they belong, and through which [he could] distinguish whether they are to be compared to one another as belonging to the pure understanding or to sensible intuition" (A261/B317). Transcendental reflection also provided him with the perspective from which he could critique the amphiboly of the concepts and cognitive powers discriminated in reflection, confusions common among prominent philosophers. He was critical of John Locke, for example, who, lacking such a transcendental perspective and thus deceived by the amphiboly of the concepts of reflection, "sensitivized the concepts of understanding" (A271/B327). Daston and Galison effectively position Kant's transcendental idealism against Enlightenment empiricist philosophies, of Locke and his successors, which derived all knowledge from sensations, even knowledge of the self. Kant dismissed sensations as the basis for knowledge, arguing that they were subjective artifacts of the construction of sense organs that varied between individuals. He contended that only the a priori forms of cognition could provide coherent experience and universal concepts of objects (2007, 208). Importantly, however, Kant was also critical of Gottfried Wilhelm Leibniz's rationalist philosophy, which, similarly deceived by the amphiboly of the concepts of reflection, "intellectualized the appearances" (A271/B327). Leibniz believed he could know the inner nature of things only through the abstract concepts of the understanding. Kant insisted, *contra* Leibniz, it is impossible for us to know things through pure concepts without sensibility, and since we only know things through the forms of human sensory intuition, we cannot know things as they are in themselves but only as they appear to us. Kant's transcendental idealism was a response to British empiricism and its skeptical consequences, as Daston and Galison rightly emphasize; but it was also a response to the rational philosophical tradition in Germany with its pretentions to knowledge beyond the conditions of our sensibility, which Daston and Galison do not acknowledge.[3] Arguably, the larger preoccupation of the *Critique of Pure Reason* is its critique of rational metaphysics and its epistemic immodesty.

Kant's Amphiboly of the Concepts of Reflection appears at the end of his Transcendental Analytic, as he is about to leave the secure domain of cognition, the terra firma of phenomena, for the stormy seas of transcendental illusion. The

[3]Daston and Galison acknowledge that Kant's opposition to empiricist philosophy as merely subjective did not lead him to claim reason reveals the essence of things in themselves (2007, 208). But they do not recognize the significance of Kant's critique of rational metaphysics.

Transcendental Dialectic forms the substantive part of Kant's *Critique of Pure Reason*, the critical examination of the illusions to which pure reason is subject when it severs its ties to sensory intuition—the adventures of reason without end, its deceptions and empty hopes. It was primarily to counter these excesses of traditional metaphysics that Kant introduced his critical tribunal. But before embarking upon this larger task, Kant cast a glance back at the map of the island of cognition, and asked whether we could not be satisfied with what it contains (A235-36/B294-95). Terada reminds us of the two aspects to this question. In laying bare that there is nowhere else to go, no other land upon which we can settle, Kant's critique places us under an obligation to be satisfied with where we are; our obligation is tied to exigencies. Despite the temptations of speculation lying before it, critical reflection reminds us of the virtues of epistemic modesty by exposing the empty pretensions of purely formal reason unmoored from the matter of phenomena. But it is difficult to be satisfied with this domain unless we comprehend by what title we possess it; Kant's critique also emphasized that our rights to the domain is tied to the right to appearances and to no more than appearances (2009, 87–88).

Kant's critical examination of the title by which we possess the domain of experience was not, however, restricted to a reflection upon the boundaries of that domain. He also sought to justify that title by validating the cognitions we can rightfully affirm. As Foucault notes, Kant's critical project involved not only reflection upon the pretensions and confusions of his contemporaries, but also upon his own epistemic claims (49–50). Reflection upon the modalities of cognition, as a mode of thinking about thinking, analyzes the sources of cognition in both understanding and intuition. It is also a method or medium by which philosophy grounds itself, through an analysis of the conditions that warrant cognition (Gasché 1986, 13–22). One of the central claims of the *Critique of Pure Reason* is that a priori concepts of the understanding are the necessary conditions of human cognition and experience, and that the objective validity of these concepts is determined by the transcendental unity of self-consciousness. But these formal conditions of cognition only acquire their objective use through their necessary connection to sensory intuition. In the Transcendental Deduction, Kant sought to legitimate the relation of the a priori concepts of understanding, the categories, to the objects of cognition by demonstrating that they are the epistemic conditions necessary for any thought of an object in general: "The objective validity of the categories, as a priori concepts, rests upon the fact that through them alone experience (as far as the form of thought is concerned) is possible" (A93/B126). He argued that the ground for this objective validity is established through connection with the transcendental unity of apperception, which generates the "*I think* [that] must *be able* to accompany all my representations" (B131-32). Since the unity of representations of an object in a category requires a unity of consciousness, and consciousness of that unity, the unity of apperception provides the ground for the relation of representations to objects and hence for the objective validity of the categories. The transcendental deduction of the categories is only complete, however, when the relation of the categories is established not only to the cognition of an object in general through the unity of self-consciousness, but also to what is given under the forms of human sensibility. The "condition of the objective use of all our concepts of the understanding is merely the mode of our sensible intuition" (A286/B342); a priori laws must be determined through sensory intuitions, otherwise they would be merely empty logical forms. Kant's claim was thus not that the categories are true and necessarily conform to objects, but rather that they are capable of truth or falsity in specific judgments (Allison 2004, 173–78, 87–88). The objective unity of apperception grounding the pure concepts of understanding must be able to be related to the subjective unity of the synthesis of apprehension in empirical consciousness in judgments. Kant contended that the origin of the a priori concepts of understanding, the categories, is established through their coincidence with the logical functions of thinking. In the Clue to the Discovery of all Pure Concepts of the Understanding, he argued that the categories are acquired through reflection upon the functions and forms of judgment—activities of comparison, reflection, and abstraction. But the a priori concepts of experience, the pure concepts required for our cognition of objects, are derived from those forms of judgment that are needed for thinking about the unity of our sensory intuitions (Longuenesse 1998, 72–80). If the objective validity of our cognitive experience is warranted by the unity of self-consciousness, objective validity is also bounded by the objects of our senses (A286/B342-43); both are necessary conditions of the title to objective knowledge.

With Kant's emphasis upon the formal conditions of our cognition, it is easy to lose sight of his insistence upon the import of its material conditions, embedded as they are deep within the apparatus of thinking. Indeed, Daston and Galison offer a fair assessment of Kant, in arguing his idealism prioritizes the subject's intellectual contributions to cognition. An effect of the specular nature of philosophical reflection is that it has difficulty in inscribing what is outside it other than through appropriating a negative image of it (Derrida 1981, 33). Nevertheless Kant distinguished his transcendental idealism from general logic, in emphasizing it is the form of thought about empirical objects. He insisted that the pure concepts of the understanding must be reconciled with appearances for a rightful claim to cognition. Kant's transcendental philosophy might thus be better described as a doubling, rather than an inversion, of scientific notions of objectivity, insisting upon the objective validity of a priori concepts as well as the objective apply of concepts to sensory intuitions.

Moreover, Kant's discrimination of two distinct stems of human cognition—sensibility and understanding—did not produce a clear boundary between the subjective and objective contributions to experience. Quite the contrary, his analysis of the disparate sources of cognition lead him to reflect upon the series of synthetic acts and mediating apparatus needed to bring them into relationship. On the one hand, the material of sensation must be presented in way suitable for ordering by the understanding. Objects given to us by mean of sensibility are ordered through the pure forms of intuition, the a priori forms of space and time. The given manifold of sensation is then taken up into empirical consciousness through a series of synthetic acts—first the apprehension of a manifold, then the reproduction and combination of these appearances, and finally a consciousness of their belonging to a unified act of synthesis. The imagination plays a central role here, reproducing, associating and synthesizing the manifold of apprehension into a unified representation. Kant characterized such empirical representations as subjective associations, but they form appearances that can be recognized in a concept to engender objective cognition. On the other hand, the a priori concepts through which we order our experience must be prepared for relationship to appearances. The pure concepts of the understanding, the categories, are acquired from reflection upon the activities of discursive thinking, and their objectivity is established through their grounding in the unity of self-consciousness. Principles facilitate their application to intuition by providing rules for the cognition of an objective temporal order, as the formal condition of inner intuition. The imagination, acting now in a productive capacity, generates schemata to provide determinations of appearances within inner intuition. Kant argued that the elements of judgment thus meet in inner intuition and its a priori form, time, as the one whole in which all our representations are contained. Judgment for Kant, then, is a complex series of acts of synthesis involving heterogeneous sensory and intellectual contributions to cognition; to mediate between them in specific judgments Kant introduced the instruments of imagination and productivity, schemata and principles. If concepts provide a rule by which we can order our intuitions in general, the act of relating concepts to intuitions in particular empirical judgments nevertheless remains without a rule. Despite Kant's elaborate mediating apparatus, he concedes judgment remains largely a matter of wit, a talent for enacting complex syntheses in singular instances without determinate warrant. For all his attempts to provide objective grounds for cognition, its objective validity warranted by the unity of self-consciousness and bounded by the objects of our senses, he admitted actual acts of cognition involved various subjective processes of synthesis.

To have a critical perspective upon the island of cognition, and upon the title to specific empirical judgments, also suggests a view of the whole of its domain. In the Appendix to the Transcendental Dialectic Kant allowed reason to extend beyond the firm terrain of experience to postulate a system of nature as a whole. He was not proposing we could know the objective order of our world, grounded in a first or final cause of the world, as in speculative metaphysics. Rather he was proposing that we could project the unity of nature as a regulative idea and subjective guide for our reflection upon nature in the diversity of its empirical laws. Kant extended his reflections upon the projected system of nature in the Introductions to his *Critique of the Power of Judgment*, introducing a principle of purposiveness to guide our reflection upon the unity of the diverse laws of nature (XX: 208–21; V: 181–88). This principle of purposiveness might seem to suggest that we can regard nature as if it is designed with our cognitive needs in mind. It might lead us to regard nature as if it favors human beings in the distribution of intelligible and beautiful forms, and to support the realization of the moral purpose of humankind. Indeed, many have read Kant as suggesting a supersensible ground underlying nature as the basis for this apparent purposiveness of nature for our intellect (see Guyer 2003). But Kant insisted that the principle of purposiveness is purely a subjective principle and thus plays a strictly epistemic function in our reflective judgments; it is not nature but our judgments that are purposive. The principle of purposiveness reflects the form of the subject's judgment, in which the unity of empirical laws becomes the purpose of the activity of judging (Steigerwald 2013). Empirical laws extend beyond the a priori concepts necessary for the possibility of cognition in general and that structure determinate judgments of objects. Reflective judgments must discern the unity in diversity and synthesize empirical particulars into a law. Such purposive judging is future orientated, enabling us to anticipate what we do not yet know, and to project a systematically unified whole onto the diverse, contingent and empirically given (Zuckert 2007, 1–86; Longuenesse 2005, 211–35). But if such projective judging is necessary to form the idea of a unity and uniformity of nature as the background for objective cognition, Kant recognized that it aims at an indeterminate end, that its validity is strictly subjective, and thus that its claims are limited.

The cultivation of human reason is the larger project of Kant's critical philosophy. In fostering participation in critique within individuals and more generally within culture, Kant saw the prospect of progressive enlightenment and of reason organically generating or cultivating itself. The *Critique of Pure Reason*, in presenting philosophy as the idea of a possible science or system of knowledge [*Wissenschaft*], pointed to the failed methods of the past and made a claim for critical philosophy as offering a way forward. Kant thus held out the prospect that we can learn to philosophize and to exercise our talent for reasoning in accordance with valid principles, but he also insisted that it is reason itself that must recognize its principles. Reason cannot establish a science unless it has an idea to base it upon which, but reason can recognize it idea only when it has become actual (Shell 1996, 178–81). Kant's critical philosophy, in instituting reason's self-examination, sought to foster its development by establishing its rightful claims, both those with objective and those with subjective validity, and both the productive activity of our cognitive powers and their boundaries. In "What is Enlightenment" Kant also stressed the importance of reason governing itself. To be enlightened is to be autonomous, to think for oneself and to engage judiciously with the percepts of established authority, and in acting and thinking for oneself to take responsibility for one's own affairs. Now, however, Kant enlisted the learned public as a critical tribunal, arguing that the scholar should have both the freedom and the responsibility to examine critically authority not only in philosophical traditions, but also in its civic, political and religious forms (VIII: 35–42). Foucault finds in this essay a powerful combination of a reflection upon our cognition, a reflection upon our historical development, and a reflection upon our present that he identifies as the attitude of modernity. To be elements and agents of a process of enlightenment requires taking responsibility for this process. For Foucault, Kant's significance lay in his recognition that precisely at the present moment critique is necessary to define the conditions under which the use of reason is legitimate, and thus to determine what is obligatory and what is arbitrary. He thus characterizes Kant's philosophical ethos as a limit-attitude—critique as reflecting upon limits (1984, 45–46).

5.2 Meta-critical Projects: After Kant

In making critical reflection upon the bounds of human reason a virtue, Kant's transcendental philosophy can be regarded as having adopted a position of relative epistemic modesty. But Kant's works were not accepted uncritically by the next generation of German philosophers. Realists claimed to find a thing in itself lurking within Kant's account of sensibility. Empirical skeptics rejected the fundamental argument of the first *Critique* that purely formal concepts could apply to what was given in experience. A focus of these critiques was Kant's purported demonstration of the objective validity of a priori concepts by grounding them in the transcendental unity of apperception. Kant himself had struggled to clarify his conception of self-consciousness, in the end simply maintaining that we possess an immediate awareness of this pure "I think" without further justification. In a particularly biting 1793 review, Gottlob Ernst Schulze argued that Kant's restrictions upon cognition should also apply to its transcendental conditions, and that Kant was guilty of hypostatizing a subject as a thing in itself as the basis of cognition in violation of his own critical strictures (Beiser 2002, 240–71; Frank 1987, 96–111). Fichte's and Schelling's readings of Kant were shaped by this critical reception, and their own philosophical systems were developed to resolve the problems that both critics and supporters foregrounded with Kant's transcendental idealism. Neither accepted the terms of the Kantian settlement, the title to the domain of appearances he claimed through determining the validity of our subjective and objective contributions to cognition. Fichte sought a more rigorous understanding of our subjective contributions to cognition, through reflection upon the conditions of self-consciousness and striving for free self-determination in thinking as well as acting. Schelling supplemented Fichte's critical idealism with a philosophy of nature, which sought to extend Kant's critical analysis of contemporary concepts of nature by investigating the boundary conditions of natural phenomena in the endless becoming of nature. In questioning the validity of Kant's claim of a right to appearance, Fichte and Schelling also questioned the concomitant obligation to be satisfied with appearances. Indeed, both, in different ways, opened philosophy to the insatiable prospect of an endless task. Fichte and Schelling retained the Kantian sense of critique as an epistemic virtue, but brought into critical reflection the concepts of subjectivity and objectivity used by and against Kant. Their philosophical projects can be characterized as meta-critical, in that they critically examined not only subjective and objective contributions to cognitive acts but also transcendental reflections upon those contributions. In contrast to Kant, the limits to their philosophical projects were not the boundaries of experience, but the boundaries of philosophy, and the unsettling obligation to accepts its necessary incompletion.

Fichte developed his *Wissenschaftslehre* in a series of texts between 1794 and 1799 as an extension of the Kantian investigation of the transcendental conditions of experience, by subjecting the facts of consciousness Kant took as his starting point to further analysis. He would claim, rather immodestly, to provide a better defense of Kantian philosophy than Kant himself and in ways more consistent with the principles of critical philosophy. Fichte objected to Kant taking for granted the division of cognition into passive sensibility and active understanding, and thus posing the problem of their relation. He especially objected to Kant resting the validity of a priori concepts of the understanding upon pure self-awareness, without providing the conditions for the "I think" accompanying all cognitive consciousness or inquiring into how the "I" could be immediately conscious of itself and of itself as thinking. He also objected to Kant resting practical reason upon freedom as a fact of consciousness, and for providing no common foundation for theoretical and practical reason. Instead, in keeping with the spirit of critical idealism, Fichte sought to inquire into the transcendental genesis of subject, not by proposing the subject as a metaphysical entity, but by examining in philosophical reflection the activity of the I that underlies all acts of thinking or doing, including those that Kant left as assumptions. Contrary to the claims of his nineteenth-century critics, Fichte did not abandon Kant's tethering of cognition to the world, and continued to insist upon the finitude of human subjects thinking and acting in the world. He did, however, introduce new values into critical reflection, by insisting that free self-determination should form the basis of cognitive and not only moral reasoning. Indeed, he placed freedom at the centre of his philosophy, arguing that all activity of the I should be grounded in freedom. He insisted even one's philosophical position is a free choice, and thus also an ethical position. Critical reflection both upon the activity of the I and upon the philosophical analysis of that activity is key to making this ethical choice. In naming his philosophical system a *Wissenschaftslehre*, Fichte made explicit the critical ethos informing it. If philosophy is a science [*Wissenschaft*] of knowledge [*Wissen*], a search for a knowledge of knowledge, the *Wissenschaftslehre* is a theory [*Lehre*] of that science, a reflection upon the philosophical reflection upon knowledge (Zöller 1998, 16–17). Fichte's philosophical system can be regarded as fitting Foucault's characterization of critical philosophy, perhaps even more so than Kant, in not only interrogating epistemic claims, but also interrogating itself.

The founding principle of Fichte's philosophical system is the self-positing activity of the self or what he termed "the I [*das Ich*]." He was not proposing a substantive being, a soul or spirit, as the basis of human subjectivity. Rather he sought to investigate the subjective warrant for our cognitions, the activity of the I behind unified self-consciousness. He contended that the I posits itself [*Das Ich setzt sich selbst*]; this pure self-positing is the ground of all activity of the human mind, and constitutes what the "I is [*Ich ist*]" or what "I am [*Ich bin*]." Fichte emphasized that the I is not a fact [*Tatsache*], like some thing, but an act [*Thathandlung*]. The self-positing act [*Thathandlung*] of the I constitutes the identity between its action [*Handlung*] and the deed [*That*] that is its product. The subject and the object of this act, its form and content, are identical, making the I a subject-object. Fichte adopted this unusual terminology in an attempt to capture his unique perspective of the I as a pure act. If the terms and technical details of his argument are difficult to follow, his conclusion is clear; the "I is that which it posits itself to be," and the I posits itself as its own pure activity (1971, I/1982, 91–98). The pure self-positing activity of the I underlies all cognitive and practical activity of the finite human subject.

Although every act of consciousness involves the self-positing activity of the I, this activity remains pre-reflective, indeterminate and without predication. To become reflectively self-conscious requires that the pure indeterminate activity of the I become determined, for the "I am" to become "I am this or that." It is as conscious beings in the world interacting with objects and other subjects that we lose the immediacy of pure self-positing and become aware of our finitude. It is only as a finite subject engaged with the world of appearances that the I can appear to itself. The feeling of the limitation of its own activity prompts the subject to posit an external world and itself as a finite embodied being in the world. Feeling its pure spontaneity limited, the I posits something opposed to itself, something that is not itself, or what Fichte terms the "not-I [*nicht-Ich*]." The I posits [*setzt*] itself as counter-posited or op-posited [*entgegensetzt*] by the not-I. The not-I acts as a check [*Anstoß*] upon the I's pure indeterminate activity and prompts the I to reflect upon its activity and to become self-consciousness of its acts. This check upon the I's activity prompts the I reflexively to determine itself as well as to determine an object external to itself. Fichte's formulation, of a not-I is counter-posited to the I, has sometimes been read as suggesting the external world is but a projection of the mind. Fichte did deny any meaning to a thing in itself, contending that the world can only have meaning in relation to the I's cognitive and practical activity. And even more than Kant, his examination in philosophical reflection of the subjective activity in cognition inscribes what is outside of it as a negative image of itself, as a not-I. But Fichte insisted his *Wissenschaftslehre* was a critical idealism, not a dogmatic idealism; the check of a not-I external to the I is necessary to the determinate knowledge of the I, even if that not-I can only be determined or known through the I (1971, I/1982, 210–11, 227–31, 250–53). Rather than rejecting the strictures of Kant's critical philosophy, Fichte used Kant's method of reflection, applying it to the activity of the I and its limitation through the not-I, and thus analyzing the subjective and objective contributions to our cognition.

Fichte represented his philosophical method as proceeding as an experiment, in which the philosopher observes and investigates the activity of the I. Through reflexive distance the "philosophical eye" observes the "I," retracing ideally the real activity of the I. As he demanded of his students: "Think yourself ... and observe how this occurs" (Fichte 1971, II/1988a, 439–50; see Zöller 1998, 26–39). But Fichte did not stop at the transcendental reflection that Kant used to inquire after the conditions of cognition and to analyze cognition into separate powers of intuition and understanding. He also used philosophical reflection to inquire after the conditions of the "I think" that Kant claimed founded the pure concepts of the understanding. By making self-consciousness itself an object of consciousness, Fichte contended that the philosopher is able to apprehend how the I becomes aware of itself in its encounter with the not-I and to reconstruct the I's reflexive self-construction. He claimed philosophical reflection could also unify Kant's separation of cognition into two distinct faculties, sensory intuition and understanding, by tracing the pre-reflective activities of sensation and imagination that Kant hurried over in his works. He brought into philosophical reflection the feeling of an encounter of the I with something alien to it, and the positing an intuited external object as its condition. He also brought into philosophical reflection the activity of the imagination in relating the matter and form of experience, its wavering between intuition and its possible conceptualization, until it is fixed in a concept of the understanding through judgment. Fichte purported even the I's original pre-reflective self-positing could be made evident in philosophical reflection (1971, I/1982, 217–35, 291–97). In tracing the construction of self-awareness and bringing unconscious syntheses into conscious reflection, the philosopher strives for an immediate intuition of all the self's activity, or what Fichte termed an intellectual intuition (1971, I/1982, 463–65).[4]

Through philosophical reflection the I strives for self-identity—for the unity of the real and ideal activities of the I, and of intuition and understanding. Yet the role Fichte gave to reflection seems instead to introduce a duplicity into the self. The experiment of philosophical reflection appears to divide the self from itself—to make it at once subject and object of itself. Fichte claimed that this apparent contradiction is appeased by attending to the work of philosophical reflection in effecting free self-determination. Reflection is tasked with lifting the activity of the I out of the sphere of giveness and blind habits of thought, out of both unthinking empirical consciousness and rigid philosophical thought, and making the I conscious of its own activity. Philosophical reflection thus should not only retrace the activity of the I, but also ensure that the I acts freely. Indeed, he demanded of his students not only that they "Think yourself ... and observe how this occurs," but also that they think for themselves and thus think freely. This emphasis upon free activity of the I has led many to read Fichte as privileging practical reason over theoretical reason.[5] Yet he argued that both cognitive and practical activities require free self-determination. In his *Wissenschaftslehre* thinking and willing are each implicated in the other. Thinking depends upon willing, in that we ought to think freely, even freely choosing how to philosophize, and we ought to determine ourselves in both our cognitive and practical activities. Similarly, willing depends upon thinking, in that our willing must be thought to have meaning for the I, and needs the concept of the end willed to give form to willing (1988b, 260). This entanglement of thinking and willing shows that the ethos of freedom at the center of Fichte's critical idealism is not only the basis of moral practice but also an epistemic virtue. But it also shows that all free acts of the I are limited by particular determinations of will. His idealism had bold ambitions for the ends of free self-determination—the self-identity of the I as real and ideal, object and subject; an intellectual intuition of the I's original activity; and a coincidence of what the self ought to be with what the self wills itself to be. But he also insisted that such ends

[4]Fichte's conception of intellectual intuition thus contrasts with Kant's idea of an archetypal intellect, for which whatever it thinks exists.

[5]Beiser goes so far as to categorize Fichte as a pragmatic idealist (Beiser 2002, 218). Zöller, who highlights the duplicity of thinking and willing in Fichte's philosophical system, nevertheless argues that Fichte foregrounds willing as the primary activity of the I, especially in the later formulations of his Jena *Wissenschaftslehre* (Zöller 1998, 4, 71–82).

can only be striven towards, endlessly, and never actually attained. In exploring all the activity of the I, not only reflecting upon the subjective contributions to consciousness but also examining the conditions of reflection itself, Fichte was faced with the limits of the method of critical philosophy. Immediate self-awareness or intellectual intuition remains an ideal that eludes realization in finite human consciousness, an ideal that drives reflective activity forward but that also makes explicit the shortcomings of that activity. Unsatisfied with Kant's presentation of the unity of self-consciousness, Fichte opened philosophy to the unsettling prospect of unending critical reflection and necessary incompletion.

Fichte's emphases upon the activity of the I, upon freedom of will in our cognitive acts and representing the external world as a not-I determined through the I, ensured his reputation as a preeminent philosopher of subjectivity. Yet his critical idealism recognized that much remains constrained in human existence, intellectually as well as physically and socially. Our self-determination is inevitably constituted through a tension between our finitude and our ideal ends, between what is fixed and given, and what is open and yet to be realized. Like Kant, Fichte nevertheless held that critical reflection and the cultivation of reason is an ethical demand, and he gave the philosopher a privileged role in teaching us to think and act autonomously. Importantly, Fichte's idealism was not private and individualistic, but like Kant's premised upon intersubjectivity. But unlike Kant, Fichte was insistent that it as subjects acting in the world that we become aware of our freedom. In his 1796 *Foundations of Natural Law*, he argued that the discovery of moral consciousness depends upon the check of a not-I in the form of a summons [*Aufforderung*] of others that is at once a demand and a request, an incitement and an invitation. The summons of an intersubjective encounter implies mutual recognition and obligation, and is the reason for the development of individual self-consciousness and consciousness of freedom. The recognition of ourselves as free is dependent upon our recognition of others as free, with our rights to freedom theoretically, practically and socially conditioned by the demands and rights of others for freedom. The ideal end Fichte strove towards, then, was not just self-identity and an ethical demand to improve oneself, but also through self-improvement to improve society and to strive for social harmony. Even more than Kant, he appealed to the learned public as a critical tribunal, and went far beyond Kant in questioning traditional authorities, advocating revolutions in political and social structures as well as revolutions in philosophy (Fichte 1971, VI: 289–346/1987; 1971, III: 1–389/2000; La Vopa 2001). But again Fichte's radicalness did not only introduce critical reflection upon the limits of contemporary social structures and obligations, but also opened the prospect of endless change.

Fichte's *Wissenschaftslehre* thus interrogated not only the conditions of cognition, but also the conditions of transcendental idealism. His meta-critical philosophy focused upon the subjective contributions to cognition, examining what Kant assumed as facts of conscious and attempting to bring into philosophical reflection the activity of the I, the active deed [*Tathandlung*], constituting those facts. Schelling instead turned his attention to the objective side of cognition, critiquing both Kant's and Fichte's transcendental idealisms as inadequate in their representations of the natural world, and arguing for the need for an independent philosophy of nature. Yet despite Schelling's insistent moves away from transcendental idealism, he repeatedly returned to its analysis of cognition. Indeed, in his various works he moved between the discourses of transcendental idealism and the philosophy of nature, making differing and often conflicting statements regarding their relationship. In his 1797 *Ideas towards a Philosophy of Nature*, he argued that "nature is only the visible organism of our understanding," after the method of transcendental philosophy, conceiving the real in terms of the ideal. But he also claimed that "*the ideal must arise out of the real and be explained from it*," giving priority to the philosophy of nature (1856–1861, II: 55–56/1976, V: 106–7/1988, 41–42).[6] In his 1800 *System of Transcendental Idealism* he conceded that "neither transcendental philosophy nor the philosophy of nature alone" is adequate; rather both are required, although thus "the two must be forever opposed, and can never merge into one" (1856–1861, III/1976, IX/1978, 331–32). Schelling's critical ethos was meta-critical, but in a different sense than Fichte's. He used transcendental philosophy and the philosophy of nature as tools to interrogate each other, each acting as at once the foundation and critique of the other. He drew analogies between both, arguing that the activities of nature and the activities of cognition offer reflections of each other. But after the method of Fichte's *Wissenschaftslehre*, he held that transcendental philosophy prevents the philosophy of nature from completion by continually questioning the conditions of determinate knowledge of natural powers. The philosophy of nature in turn marks the limit of transcendental philosophy, by drawing attention to a dark presence in cognition of the real that defies conceptual analysis. Schelling concluded that it is not possible to stand outside both or to decide between them, but only to examine critically the one through the lens of the other.

Schelling's philosophy of nature supplemented Fichte's *Wissenschaftslehre* and the focus of transcendental idealism upon the subjective contributions to cognition, by giving activity and life to the natural world without reducing it to abstract conception or metaphysical postulates. Fichte's attention upon the activity of the I meant that he gave nature a strictly negative character as a restraint upon subjective consciousness, the not-I as a check or op-positing of the activity of the I. Focusing upon subjective warrant for our cognitive claims, nature is cast solely as something other, a dead objectivity (Hegel 1969–1989, IV: 42, 51). Kant similarly reduced the material contributions to cognition to a mere something [*etwas*] or thinghood [*Sachheit*] lying beyond the boundary of sensation (A92-93/B125; A143/B182). If Kant allowed that the findings of natural science could be made determinate through philosophical analysis, he insisted that that analysis must begin with empirical phenomena and not speculations upon the inner nature of things. Schelling argued for pushing beyond Kant's settlement with appearances. While insisting that we know nothing at all except through experience, Schelling

[6]Page numbers for Schelling's works are from the *Sämmtliche Werke* (1856–1861) when included in the editions cited; when an edition does not reference the *Sämmtliche Werke*, its pagination is given separately.

contended that empirical science is concerned only with the "*surface* of nature;" if directed to what is "*objective*" in nature, it only "views its object in *being*," as a finished product. Schelling instead sought to bring into philosophical analysis the activity or productivity of nature, what is "non-objective in nature;" to regard "its object in *becoming*" (1856–1861, III/1976, VIII/2004, 274–83). He insisted his inquiry was accordingly necessarily speculative. Schelling's *Naturphilosophie* might seem to be an extravagant project for the turn of the nineteenth century. It might seem to defy the critical philosophies that challenged traditional metaphysics, questioned epistemic claims and pretensions, and reflected upon the historical limitations of contemporary philosophical systems. Yet Schelling no more hypostatized metaphysical powers constituting nature than Fichte hypostatized the subject as a metaphysical entity. He became deeply engaged with the material and contingent processes of the natural world, following closely the concrete investigations of contemporary natural science. He proposed a philosophy of nature, however, not a natural science. As a critical philosophy it interrogated the conclusions of those sciences, and pointed to an insurmountable irresolution in the determination of natural processes. In not settling with appearances, with what is objective in nature, and yet not then claiming access to the fundamental powers of nature, to simplest or final essences, Schelling's philosophy of nature speculated critically upon the endless becoming of nature, upon the non-objective, in which each power or form could be subject to further investigation.

An emphasis upon the critical character of Schelling's philosophy of nature suggests it was engaged in largely negative work, questioning the representation of the natural world of both transcendental idealism and natural science. But it also offered positive contributions to the investigation of nature through the notion of boundary concepts. In general terms, Schelling portrayed natural products as the relative equilibrium of opposed processes in the ongoing becoming of nature. Highlighting the free spontaneity and animation as well as the necessary limitations of the activity of the world, he contended that the interplay of productivity and constraint finds resolution in natural products, but that this resolution is only temporary as each product is continually subject to annihilation and renewed production. Schelling gave these general principles substance by introducing boundary concepts as tools of analysis for concrete contexts. Drawing upon contemporary natural science Schelling conceived the physical world through an opposition of gravity and light, and matter through an opposition of attractive and repulsive powers. He conceived living being as preserved through an opposition of inward inversion and receptivity to the stimulus of its surrounding environment, an opposition of the individuation of matter and outward formation. Schelling's contention was that the natural products taking specific material forms at specific junctures in the activity of nature can be investigated and comprehended through specific "boundary concepts of empirical natural science [*Grenzebegriffe der empirischen Naturlehre*]" (1856–1861, II: 386/1976, VI: 81–82). But he was insistent that such boundary concepts are not fundamental natural powers; indeed, gravity and light, attractive and repulsive powers, each inorganic and organic power might be subject to further analysis. Nature, as the middle factor in an endless becoming, is only apparent in particular materialized forms, but each phenomenon is but a relative equilibrium of higher and lower processes. Different products at different degrees of organization and activity can be investigated through distinct methods and boundary conditions, without postulating those conditions as elemental. Thus Schelling's speculative philosophy of nature, as concerned with what is "non-objective in nature," the object in "its *becoming*," restricted itself to the quite modest activity of conceptualizing the boundary conditions of particular kinds of phenomena.

Schelling argued that the oppositions in our concepts of nature have their analogy in those of our mind. The boundary conditions marking stages in the dynamic becoming of the world are limited to our conceptions. The concepts in the philosophy of nature, of natural processes taking material form at the boundaries of opposed processes, reflect the processes of concept formation in transcendental idealism, as the activity of the I is constrained through encounters with the world. The I intuits itself as sensing and becomes conscious of the opposition between itself and things as the first step towards intelligence. To raise itself above intuition to reflection, the I produces a new opposition between the syntheses of outer sense and the syntheses or representations of inner sense. Judgment [*Urteil*] separates and compares intuition and conception, so that they can be related reflectively and freely, but a border [*Grenze*] and opposition [*Gegensatz*] is thus generated that must be traversed with a band [*Band*] or mediating link [*Mittelglied*]. For Schelling, transcendental idealism thus reveals the dialectic of the mind, with each act of cognition taking place at a boundary between spontaneity and limitation, opposition and synthesis. Each concept is a product of this dialectic, the "boundary concepts of empirical natural science" not only expressing the interplay between activity and constraint in nature, but also in turn the product of such an interplay in the mind. Critically reflecting upon the dialectic in every act of judgment and enfolded in every concept, Schelling concluded that the oppositions in nature reproduce those of cognition, with nature and mind subject to the same processes (1856–1861, III/1976, IX/1978, 389–530). The dialectical form of our concepts of nature reflects our embeddedness in nature as thinking and living beings engaged with the world. Schelling claimed that the true representation of science is "that it is the development of a living actual being that presents itself within it" (1856–1861, VIII/2000, 199). We are necessarily in a world of our own thinking and acting, even as we as thinking and acting beings are constrained and produced by that world.

In drawing analogies between transcendental idealism and the philosophy of nature Schelling did not give priority to one in our understanding of the world, but rather indicated the limitations of each. That our concepts reflect the processes of nature shows our inability to transcend the natural processes from which our mind develops. That the activity of nature is rendered in terms of the processes of the mind shows our inability to know nature objectively independently of subjective thinking. Schelling did not follow Fichte in striving to overcome the dialectic of the I's activity, to progress towards self-identity and self-determination, even if only as an ideal. He accepted that our embeddedness in nature is fundamental, and not a constraint we should strive to overcome. Yet he did not then accept Kant's settlement, his reconciliation to appearances. He rejected Kant's depiction of the terra firma of phenomena and the determinate concepts of science, finding in its stead an ever-shifting terrain. Situated in the midst of the world in endless becoming, Schelling was unsettled by the flux, by the contingencies and tensions of both mind and nature. His meta-critical ethos made unease inevitable. But if unable to accept any determinate concept of subjectivity or objectivity, even as an ideal end or necessary limit, Schelling argued for the value of boundary concepts as epistemic tools for exploring the life of the world and the mind. His meta-critique exposed the lack of objective or subjective grounding for our epistemic claims, and made a virtue of acknowledging our place within the world.

The writings of Schelling and Fichte can appear impenetrable when considered from outside the tradition of critical idealism. Part of that impenetrability is due to the technical vocabulary of the post-Kantian tradition in which they were written, and their introduction of new and unusual terms. They both also have a tendency to reflexive excess. It is thus not surprising that mid-nineteenth century scientists viewing German idealism from a distance found an overactive subjectivity that warranted disciplining. But Daston's and Galison's work has shown us that we should not uncritically accept the nineteenth-century's notion of mechanical objectivity and its correspondent notion of an ascetic scientific subject, and similarly we should not uncritically accept its hasty dismals of German idealism. German idealism was informed by an epistemic virtue of critique, an ethos taken up in the Enlightenment and rigorously applied in Kant's philosophy, and given meta-critical force by figures like Fichte and Schelling. The philosophies of Kant, and even Fichte and Schelling, were concerned to reflect critically upon our conceptions of objectivity and subjectivity. Kant's transcendental idealism argued for an ethos of epistemic modesty, through its critique of pure reason and reconciliation of cognition to appearances, and its argument that mature judgment must be aware of both the rightful claims and limitations of our capacity for self-determination. Fichte and Schelling rejected some of the constraints Kant placed upon philosophy, but by extending his critique to interrogate some of the assumptions Kant excluded from reflection. They thus introduced a meta-critique that reflected not only upon our cognitive acts but also upon Kant's transcendental reflections upon those cognitive acts, pushing even further than Kant the problematization of notions of objectivity and subjectivity. Fichte's *Wissenschaftslehre* sought to bring into reflection the activities of the I Kant left unanalyzed, interrogating the transcendental warrants he introduced for our cognitive claims and arguing for freedom in thinking as well as in acting. Schelling supplemented Fichte's transcendental idealism with a philosophy of nature that sought to investigate the boundary conditions of natural phenomena in the dynamic life of the world, conceiving these boundary conditions in analogy with the dialectic of thinking. But whereas Kant argued that the limits of critical philosophy lay in the right and obligation to be satisfied with appearances, for Fichte and Schelling it lay in recognizing that our philosophical inquiries into subjective and objective activity are inevitably incomplete. In questioning the terms of the Kantian settlement with appearances, they did not do so to warrant metaphysical postulates, but instead retained the ethos of critique as a limit-attitude; yet their focus was on the limits of philosophy and the unsettling prospect of critical reflection as an endless task. Thus while the nineteenth-century critiques of the excesses of German idealism have some warrant, their simplistic reading of complex meta-critical texts have produced a particular and skewed history of objectivity and subjectivity. The significant contribution of Daston's and Galison's work is its stimulus for us to continue to reexamine that history.

References

Allison, Henry E. 2004. *Kant's transcendental idealism.* Revised and enlarged edition. New Haven: Yale University Press.

Beiser, Frederick C. 2002. *German idealism: The struggle against subjectivism, 1781–1801*. Cambridge: Harvard University Press.

Daston, Lorraine, and Peter Galison. 2007. *Objectivity*. New York: Zone Books.

Daston, Lorraine, and Peter Galison. 2008. Objectivity and its critics. *Victorian Studies* 50: 666–677.

Derrida, Jacques. 1981. *Dissemination*. Trans. Barbara Johnson. Chicago: University of Chicago Press.

Fichte, Johann Gottlieb. 1971. *Fichtes Werke*. Ed. Immanuel H. Fichte, 11 vols. Berlin: de Gruter. Reprint of Fichte, Johann Gottlieb. 1845–1846. *Johann Gottlieb Fichtes sämmtliche Werke*, 8 vols. Berlin: Veit.

Fichte, Johann Gottlieb. 1982. *The science of knowledge.* Ed. and Trans. Peter Heath and John Lachs. Cambridge: Cambridge University Press.

Fichte, Johann Gottlieb. 1987. Some lectures concerning the scholar's vocation. Trans. Daniel Breazeale. In *Philosophy of German Idealism Fichte, Jacobi and Schelling*, ed. Ernst Behler, 1–38. New York: Continuum.

Fichte, Johann Gottlieb. 1988a. *Early philosophical writings*. Ed. and Trans. Daniel Breazeale. Ithaca: Cornell University Press.

Fichte, Johann Gottlieb. 1988b. *Foundations of transcendental philosophy (Wissenschaftslehre) nova methodo (1796/99).* Ed. and Trans. Daniel Breazeale. Ithaca: Cornell University Press.

Fichte, Johann Gottlieb. 2000. *Foundations of natural right: According to the principles of the Wissenschaftslehre*. Trans. Michael Bauer. Cambridge: Cambridge University Press.

Foucault, Michel. 1984. What is enlightenment? In *The Foucault reader*, ed. Paul Rabinow, 32–50. New York: Pantheon Books.

Frank, Manfred. 1987. 'Intellektuale Anschauung'. Drie Stellungnahmen zu einem Deutungsversuch von Selbstbewuβtsein: Kant, Fichte, Hölderlin, Novalis. In *Die Aktualität der Fruhromantik*, ed. E. Behler and J. Hrisch, 96–126. München: Ferdinand.

Gasché, Rodolphe. 1986. *The tain of the mirror: Derrida and the philosophy of reflection*. Cambridge: Harvard University Press.

Guyer, Paul. 2003. Kant's principles of reflecting judgment. In *Kant's critique of the power of judgment: Critical essays*, ed. Paul Guyer, 1–62. New York: Rowan and Littlefield.

Hegel, Georg Wilhelm Friedrich. 1969–1989. *Differenz des Fichte*'*schen und Schelling*'*schen Systems der Philosophie*. In *Gesammelte Werke*, ed. Otto Pöggeler, 21 vols., IV: 12–51. Hamburg: Felix Meiner Verlag.

Kant, Immanuel. 1784/1983. An answer to the question: What is enlightenment? In *Perpetual Peace and Other Essays*. Ed. and Trans. Ted Humphrey, 41–48. Indianapolis: Hackett Publishing.

Kant, Immanuel. 1787/1997. *Critique of pure reason.* Trans. Paul Guyer and Allen W. Wood. Cambridge: Cambridge University Press.

Kant, Immanuel. 1790/2000. *Critique of the power of judgment.* Trans. Paul Guyer and Allen W. Wood. Cambridge: Cambridge University Press.

Kant, Immanuel. 1902–1983. *Kants Gesammelte Schriften*, ed. Königlich Preussisichen Akademie der Wissenschaften, 20 vols. Berlin: Walter de Gruyter.

La Vopa, Anthony. 2001. *Fichte: The self and the calling of philosophy, 1762–1799*. Cambridge: Cambridge University Press.

Longuenesse, Beatrice. 1998. *Kant and the capacity to judge: Sensibility and discursivity in the Transcendental analytic of the Critique of pure reason*. Trans. Charles T. Wolfe. Princeton: Princeton University Press.

Longuenesse, Beatrice. 2005. *Kant and the human standpoint*. Cambridge: Cambridge University Press.

Schelling, Friedrich Wilhelm Joseph. 1856–1861. *Schellings sämtliche Werke*. Ed. K.F.A. Schelling, 14 vols. Stuttgart: Cotta.

Schelling, Friedrich Wilhelm Joseph. 1976. *Historisch-kritische Ausgabe.* Ed. Jörg Jantzen et al. Stuttgart: Frommann-Holzboog.

Schelling, Friedrich Wilhelm Joseph. 1978. *System of transcendental idealism*. Trans. Peter Heath. Charlottesville: University of Virginia Press.

Schelling, Friedrich Wilhelm Joseph. 1988. *Ideas for a philosophy of nature*. Trans. Errol E. Harris and Peter Heath. Cambridge: Cambridge University Press.

Schelling, Friedrich Wilhelm Joseph. 2000. *The ages of the world*. Trans. Jason M. Wirth. New York: SUNY.

Schelling, Friedrich Wilhelm Joseph. 2004. *First outlines of a system of the philosophy of nature.* Trans. Keith R. Peterson. New York: SUNY.

Shell, Susan. 1996. *The embodiment of reason: Kant on spirit, generation, and community*. Chicago: University of Chicago Press.

Steigerwald, Joan. 2013. The antinomy of the teleological power of judgment and its significance for the critical project. In *Objectivity after Kant: Its meaning, its limitations, its fateful omissions*, ed. B. Demarest and G. Van de Vijver, 83–97. Hildesheim: Olm Press.

Terada, Rei. 2009. *Looking away: Phenomenology and dissatisfaction, Kant to Adorno*. Cambridge, MA: Harvard University Press.

Zöller, Günter. 1998. *Kant's transcendental idealism: The original duplicity of intelligence and will*. Cambridge: Cambridge University Press.

Zuckert, Rachel. 2007. *Kant on beauty and biology: An interpretation of the critique of judgment*. Cambridge: Cambridge University Press.

Chapter 6
The Physiology of the Sense Organs and Early Neo-Kantian Conceptions of Objectivity: Helmholtz, Lange, Liebmann

Scott Edgar

6.1 Introduction

"We finally come," wrote the philosopher Otto Liebmann in 1869, "to Johannes Müller's doctrine of specific nerve energies, the importance of which for philosophy should not be understated" (Liebmann 1869, 30–1). Müller's doctrine was a theory about the physiology of the sense organs that he defended most fully in his 1833–1840 *Handbook of Human Physiology*. Müller wanted to explain the fact that the sensations associated with the five human senses have their own characteristic qualities (or "energies" in Müller's archaic use of the word). Thus the quality of visual sensations differs from the quality of auditory sensations, which differ from the quality of tactile sensations, and so on. He amassed a collection of experimental results demonstrating that this difference could not be explained by differences in the external stimuli that cause the sensations, because, for example, one and the same stimuli – say, sunlight – causes both sensations of light and of warmth, depending on which nerves it stimulates. He posited instead that the sensory nerves associated with each of our five senses have their own specific physiological structure, and that these structures, rather than any properties of external stimuli, determine the different specific qualities of our sensations. Over the next several decades, philosophers like Liebmann would take Müller's doctrine to have far-reaching consequences for their conceptions of knowledge and objectivity.

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In fact, when we trace the reception of Müller's doctrine among neo-Kantian philosophers in the 1850–1870s, we find a striking example of philosophers and philosophically-minded scientists taking an empirical, natural-scientific theory of the knowing subject, and constructing philosophical theories of knowledge in response to that scientific theory.[1] In particular, they constructed philosophical theories of what the objectivity of knowledge must consists in, if it is to be available to subjects as conceived on Müller's theory. For neo-Kantians like Liebmann and F.A. Lange, and for the philosopher-scientist who partly inspired their Kantianism, Hermann von Helmholtz,[2] the central epistemological insight of Müller's doctrine was this: if the character of a representation is determined by the nature of the subject's sensory or cognitive apparatus, rather than by the properties of mind-independent objects, then just because of that fact, the representation will not resemble mind-independent objects.[3] Helmholtz, Lange, and Liebmann all take this insight to entail a striking philosophical conclusion: on its basis, they all argue that the objectivity of human knowledge cannot consist in its having any relation to a mind-independent world. They thus stake out views of objectivity that are starkly at odds with the views of some of their most high-profile contemporaries, perhaps most significantly, scientific materialists such as Ludwig Büchner, Karl Vogt, and Jacob Moleschott.

But apart from their philosophical interest, these neo-Kantians' arguments are interesting for a more general historical reason. They illustrate an important feature of the intellectual landscape of the German-speaking world following the collapse of Hegelian idealism, and during and after the materialism dispute of the 1850s, a long-running and at times vitriolic controversy about whether advances in natural science were leading to materialism and atheism. The neo-Kantians' arguments illustrate how during that period natural science's broader cultural authority was increasing relative to philosophy's, and philosophy's authority was diminishing relative to natural science's – a situation that provoked no small amount of anxiety among

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[1]My account thus contrasts with that of Daston and Galison (2010), who identify ways that *scientists*' conceptions of objectivity changed in the second half of the nineteenth century in response to *philosophical* conceptions of the subject and subjectivity. I take my account to complement theirs, rather than contradict it, since the history of post-Kantian theories of objectivity is complicated and clearly contains contrasting trajectories of ideas.

[2]Helmholtz, unlike Lange and Liebmann, did not identify himself unambiguously as a neo-Kantian. I treat him here as a neo-Kantian partly because (as we will see in Sect. 6.3) he was at pains to emphasize the Kantian dimensions of his philosophy, and partly because his efforts to articulate a Kantian vision of philosophy set an agenda for philosophers like Lange, who did self-identify as neo-Kantian.

[3]I do not intend to endorse the sweeping epistemological generalization that Helmholtz, Lange, and Liebmann see as the epistemological insight of Müller's doctrine, nor will I attempt much in the way of a defence of their view that Müller's doctrine provides evidence for it. I here accept the generalization provisionally, only in order to uncover and evaluate Helmholtz's, Lange's, and Liebmann's arguments about the consequences it would have, if true, for the concept of objectivity.

philosophers. We can see precisely that situation manifested very concretely in the arguments of philosophers who appropriated empirical results and theories from natural science for use as evidence in philosophical disputes.

I aim to explain how reflection on Müller's doctrine led these neo-Kantians to reject the view that our knowledge's objectivity consists in resembling or being determined by mind-independent objects. To this end, I begin in Sect. 6.2 with a foil for the neo-Kantians, the scientific materialist Ludwig Büchner. Then Sect. 6.3 takes up Helmholtz in the 1850s. There is significant overlap between Büchner's and Helmholtz's views in the 1850s: they agree that natural science is a paradigm of knowledge and that philosophical doctrines must derive some or all of their justification from them; they also agree that the content of our objective knowledge consists in images that resemble spatially-arrayed matter and causal forces in the external world. The important difference between them, for my purposes, is Helmholtz's concern for Müller's doctrine: while Büchner and Helmholtz agree that our sensations of secondary qualities like colour, tone, or smell are subjective, only Helmholtz appeals explicitly to Müller's doctrine to establish this claim. He argues that since on Müller's doctrine the character of our sensations of secondary qualities is determined by our sensory nerves, those sensations do not resemble the external objects that occasion them. Further, since for Helmholtz in this period, representations are objective only when they resemble the external objects that occasion them, it follows for him that our sensations of secondary qualities are subjective.

However, while Müller's doctrine itself concerned only sensations of secondary qualities, it provided a model that Helmholtz, Lange, and Liebmann use to extend its central epistemological insight to other classes of representations. If they could show that other classes of representations are, like sensations of secondary qualities, determined by the subject's sensory or cognitive apparatus, it would follow that those classes of representation do not resemble mind-independent objects. Thus in the 1860s, Helmholtz appeals to Müller's doctrine to argue that representations of spatial structure are not, after all, images of any real spatial structure among external objects (Sect. 6.4). Lange (Sect. 6.5) and Liebmann (Sect. 6.6) make similar arguments not just for representations of spatial structure, but also extend their arguments to our representations of causal structure as well. Thus, Lange and Liebmann argue, since not even representations of primary qualities resemble mind-independent objects, *none* of our representations afford us information about mind-independent objects. Consequently, for Lange and Liebmann, if objective knowledge is to be available to humans, its objectivity can have nothing to do with a mind-independent world. Helmholtz, too, eventually (Sect. 6.7) arrives at the same conclusion. I conclude in Sect. 6.8 by considering briefly why these neo-Kantians' appeals to Müller's doctrine would have made for such powerful arguments against rival post-Hegelian conceptions of objectivity such as Büchner's.

6.2 A Materialist Conception of Objectivity: Ludwig Büchner

As the dominance of Hegel's speculative idealism ebbed in the 1840s and 1850s, a wave of philosophy claiming the authority of natural science constituted a major backlash against it. Scientific materialists like Büchner, Vogt, and Moleschott made both methodological and metaphysical criticisms of Hegel's speculative idealism.[4] Methodologically, they insisted against Hegel that knowledge derives ultimately from the senses, and thus that natural science, the method of which they took to be systematic empirical observation, is the paradigm of knowledge. Metaphysically, they insisted that what is real is inanimate, purposeless matter, and not any ideal or rational substance that develops teleologically according to its own natural purposes.

Büchner's *Force and Matter* offered a popular, non-technical expression of these views. First appearing in 1855, and aimed at popular and scientific audiences, rather than at professors of philosophy, it went through four editions in two years. In it, Büchner argues that the world ultimately consists of matter and a small number of forces (such as heat, electricity, magnetism, and mechanical force) that inhere in matter (Büchner 1855, Ch. 1/1864, Ch. 1).[5] Matter is spatially extended and force exists in space. On Büchner's view, force and matter are both explanatorily and ontologically basic. That is, natural science, and especially physics, reveals that our best explanations of natural phenomena are explanations that appeal only to the size, shape, and motion of matter, as well as to the forces that inhere in it. Thus for Büchner, natural science provides the authority for the philosophical claim that matter and force are the basic constituents of the world.

Büchner calls the world that consists of matter and force "the objective world" (Büchner 1855, 174, 183/1864, 168, 178–179). Thus for Büchner, "objective" is in the first instance a term that describes the world, and not knowledge. In particular, "objective" refers to the metaphysical fact about the world that it exists independently of our experience of it or our attempts to know it. Büchner thus uses the phrases "objective world" and "external world" interchangeably. (See for example Büchner 1855, 174/1864, 168.)

However, Büchner also uses the term "objective" in the context of *Force and Matter*'s central epistemological argument. Büchner's epistemology is a blunt empiricism, according to which all of our representations derive from the senses. He claims that the senses establish a "determinate relation" to the "external" or "objective" world, and that this relation is the source of all knowledge (Büchner 1855, 163, 174/1864, 159, 168). Consequently, Büchner insists that we have no a priori knowledge. He argues that even our most abstract representations – for example, our purportedly universal ethical and aesthetic ideals – ultimately derive

[4] For a detailed historical account of scientific materialism, see Gregory 1977.

[5] See Büchner 1855, 1–4/1864, 1–4 especially for his arguments that there is no matter without force inhering in it, and no force that does not in here in matter.

from the senses. (These representations seem not to derive from the senses only because the human race has acquired them through an empirical learning process so long that it began in prehistory.) Büchner claims that because these representations derive from the "determinate relation" to "the objective world" that the senses establish for us, they therefore have "objective form" (Büchner 1855, 174/1864, 168). He thus reveals that he thinks representations are objective when they are derived, by means of the senses, from the objective world.

Büchner offers only a very crude account of the sensory and cognitive processes that he thinks gives rise to objective representations, but it nevertheless makes clear what he thinks the contents of those objective representations are. First, he claims that the use of our senses provides us with "external stimulus," and that the senses "conduct external impressions to the brain, which receives digests, and reproduces them." Büchner then claims that as this sensory process continues, it gradually produces in us an "internal image of the external world" (Büchner 1855, 172/1864, 166). Büchner's use of "image" [*Bild*] is significant: it suggests that, on his view, when we successfully represent the objective world, that representational relation is paradigmatically pictorial or visual. That is, our knowledge represents the external world in something like the same way that a portrait represents the person it is a portrait *of*. Thus Büchner thinks that when we have objective knowledge, that knowledge consists in an "internal picture" that resembles the objective world. But further, since that objective world consists fundamentally of matter and force arrayed in space, our paradigmatically objective representations will be images of matter and force arrayed in space. The spatial structures pictured in those images will resemble, or correspond to spatial structures in the external world the way the spatial relations between different parts of a face in a portrait correspond to the spatial relations between different parts of the face of the portrait's subject.

6.3 Müller's Doctrine and Objectivity: Hermann von Helmholtz

The same year that Büchner's *Force and Matter* appeared, Helmholtz gave a popular lecture on the occasion of a memorial of Kant. His topic was the physiology of the sense organs and he called the talk "On Human Vision." He opens by addressing Hegel's view that pure, speculative reason could answer even questions about the natural world, and what Helmholtz sees as Hegel's opposition to natural scientific principles conceived as the ground of a theory of nature, an opposition expressed most clearly in Hegelian criticisms of Newton (Helmholtz 1855/1884, 369). Helmholtz laments the fact that these views persist in the philosophy of the 1850s, at least to the extent that some philosophers see an opposition between philosophy and natural science.

Helmholtz wants to reconcile philosophy and natural science, and he proposes physiology of the sense organs as the starting point for that task. He argues that it offers the philosopher a natural-scientific means of investigating what Kant had called the subjective conditions of knowledge. Helmholtz thus expresses two ideas that would shape the dominant neo-Kantianism of the following two decades: first, that the proper project of philosophy is Kant's project of investigating the subjective conditions that give rise to knowledge; and second, that physiology of the sense organs and experimental psychology offer a natural scientific (and thus the best) way to carry this project out. Consequently, in his Kant lecture, Helmholtz takes himself to have ultimately philosophical motivations for his concern with the physiology of vision.

Central to Helmholtz's lecture is a long discussion of Müller's doctrine of specific nerve energies and related theories, as well as the experimental results he takes to be evidence for them. Helmholtz recounts to his audience that Müller wanted to explain the fact that the five senses have sensations with fundamentally different qualities.[6] While sensations of blue and yellow are different, we can transform one into the other by modifying it continuously: blue blends into green, and green blends into yellow. But we cannot similarly transform our sensation of blue into a sensation of concert A. The qualities of visual and auditory sensations are somehow just different.

Both Müller and Helmholtz think the "old" way to explain these differences is to suppose that different qualities of external stimuli explain the different qualities of our sensations. On this old view, visual sensations are caused by specifically visual stimuli, auditory sensations are caused by specifically auditory stimuli, and so on.

However, Müller amassed a set of experimental results (which Helmholtz confirmed and expanded in his own research) that show this old view is untenable. The results fall into two classes. First, there are results showing how a single kind of external stimulus causes different qualities of sensations, depending on which nerves it stimulates. Helmholtz gives his audience a quotidian example. When "aether vibrations" in the form of sunlight strike the retina, we experience sensations of light. But when they strike the skin, we experience sensations of warmth. The visual and tactile sensations are not caused by different, specifically visual and tactile stimuli (Helmholtz 1855/1884, 377). Second, there are results showing how different kinds of external stimuli cause sensations of the same quality. For example, we have the sensation of a flash of light when we are exposed to a flash of light in the external world – say, lightening, or a bright light hidden behind an aperture that is opened and closed quickly. But as Helmholtz discusses at length in his Kant talk, we also have the sensation of a flash of light if the corner of our eye is struck at just the right spot, or if we have electrodes attached to our forehead and cheek, passing an electric current over our optic nerve. Thus, Müller and Helmholtz argue, there is no one specifically visual quality of stimulus that causes our visual sensations (Helmholtz 1855/1885, 380).

[6]For a more detailed account of Müller and his doctrine of specific nerve energies, see Boring 1929/1957: Ch. 5.

In contrast with the "old" theory that different qualities of sensations are caused by different qualities of external stimuli, Müller posited that each of the five sense modalities has nerves with its own physical structure. The different physical structures of our nerves thus explain the different qualities of the different sense modalities' sensations. (Helmholtz extended Müller's theory by positing that the different sense modalities also had nerve fibres with their own specific physical structure.) Müller concluded that the quality of our sensations is determined by the physical structure of our nerves, and not by the external stimuli that cause them. Here, for Helmholtz, is the real epistemological insight of Müller's doctrine: since the quality of our sensations is determined by the physiological structure of our sense organs, then, precisely in virtue of that fact, our sensations do not resemble the external stimuli that cause them. Rather, our sensations are merely "symbols" that serve as causal indicators of those stimuli's presence (Helmholtz 1852/1883, 608 and 1853/1884, 19).

For Helmholtz, Müller's doctrine has enormous epistemological significance. Throughout his Kant lecture, Helmholtz uses the term "object" to refer to things in the external world that stimulate our sensory nerves. Further, during this period, he takes our representations to be objective only when they picture or resemble those external objects. But since Müller's doctrine states that representations do not resemble the external stimuli that cause them when their character is determined by the subject's sense organs, for Helmholtz it follows that those representations are subjective. Thus at least since the early 1850s, he used the terms "subjective" to refer to the features of our representations that are determined by the physiological structure of our sensory apparatus, and "objective" to refer to the features of our representations that are determined by properties of external objects. (See for example Helmholtz 1852/1883, 602, 607.)[7] Here, for Helmholtz, is the real contribution that the physiology of the sense organs can make to philosophy. It discovers, for example, how much of our "image of the external world is also determined by the structure of the physical part of our eye" (Helmholtz 1855/1884, 374), and thereby discovers how much of our visual representations are subjective. Ultimately, the physiology of the sense organs separates out our representations' subjective content from their objective content.

Helmholtz's Kant lecture and other writings on physiology from the 1850s reveal what he takes the objective content of our knowledge to be. Conspicuously, during this period when Helmholtz discusses Müller's view that the quality of our sensations is determined by the physical structure of our sensory apparatus, all of his examples are about sensations such as light, tone, and warmth – that is, sensations that he understands to be of secondary qualities. Thus when in the 1850s Helmholtz talks about what he takes to be the subjective element of our representations, he leaves representations of space entirely out of his discussion. It is fitting that he does

[7]Helmholtz is not alone among neo-Kantians who, prior to the mid-1860s, took the objective elements of our representations to be those determined by properties of external objects. See, for example, Zeller 1862/1877, 492.

so, because he conceives of the physical stimuli that cause our sensations as spatially arrayed arrangements of matter in motion (for example, "aether vibrations") *and* as "objects" in the "external world" (where for Helmholtz that phrase refers to a world that is independent of our minds). Thus when he asserts that the physicist achieves a representation of "invisible atoms, motions, and forces" (Helmholtz 1853/1884, 18), he means to assert that physical theories offer us more than mere symbols of the external world, but accurately picture the objects in it.[8]

In fact, the conclusion of Helmholtz's Kant lecture reveals especially clearly what he takes the content of our objective knowledge to be. He says,

> In what way do we first pass outward from the world of sensations to the world of reality? Obviously only through an inference: we must presuppose the presence of objects as the causes of our nerves' excitation, since there can be no effect without a cause . . . . We see now that we need this principle [that every effect has a cause] before we can have any acquaintance with the things of the external world, we need it in order to gain any cognition that objects are given to us in space, between which objects a relation of cause and effect can obtain. (Helmholtz 1855/1884, 395)

Here, Helmholtz identifies "things in the external world" with objects arrayed in space and subject to causal forces, and he argues that we have knowledge of them precisely because those objects cause us to have representations of them. Since our representations of those external objects are determined by the objects themselves, Müller's doctrine gives us no reason to deny that our representations resemble them. Thus by Helmholtz's lights those representations are objective. Hence he maintains that our objective representations are images that resemble an external world consisting of matter and force arrayed in space – an account of the content of objective knowledge that is identical to Büchner's.

6.4 Helmholtz and the Subjectivity of Spatial Representation

However, just over a decade later, in the 1866 third part of Helmholtz's *Physiological Optics*, he makes significant revisions to his account of the content of our objective knowledge.[9] He rejects his earlier view that our objective representations are images of external objects arrayed in space, and he does so with an argument that, as he presents it, invokes what he takes to be the central epistemological insight of Müller's doctrine.

The argument begins with Helmholtz rehearsing his earlier claims about sensations of secondary qualities. Sensations are the effects that external objects have on us, but the nature of an effect is determined not only by the nature of its cause, but also by the nature of "the person on whom the effect is produced" (Helmholtz 1867/1925, 19). Thus the quality of our sensations is determined by the nature of

[8]Thanks to Gary Hatfield for extremely helpful discussion on these points.

[9]My account in this section of the evolution of Helmholtz's views owes a great deal to Hatfield 1990 and 2011.

our sensory apparatus and, Helmholtz infers, our sensations do not resemble the properties of external objects that caused them. They are not, in that sense, images, but merely "symbols". But now Helmholtz suggests that this argument applies generally to all "[o]ur human representations, ... and all representations of any conceivable intelligent creature" (Helmholtz 1867/1925, 19; translation amended). For example, the argument applies to our representations of the shape or spatial structure of a table, just as much as to our representation of its colour. Helmholtz suggests that since our spatial representations are determined by our own sensory or cognitive apparatuses, then precisely in virtue of that fact, our spatial representations do not resemble the spatial structure of external objects, and so by his lights are subjective.

In fact, this argument is too quick, and cannot be Helmholtz's whole story about why spatial representations are subjective. For Helmholtz, spatial representations are not individual sensations, and thus not individual effects on us of external objects. Rather, spatial representations are interpretations of sensations. They are constellations of sensations assembled by our minds by means of unconscious inductive inferences. (Helmholtz thus calls them spatial *perceptions* to distinguish them from sensations.) Consequently, our spatial representations are not conditioned by our sense organs in the same way our sensations of secondary qualities are. One might think it were possible that while our individual sensations do not resemble the secondary qualities of objects that cause them, our mind nevertheless assembles the sensations into constellations that are spatially isomorphic to arrays of objects in the external world.

Helmholtz rejects this position. The problem is that, while our spatial representations might not be conditioned by our sense organs, they are nevertheless conditioned by our mind's inductive processes. In the third part of the *Physiological Optics*, Helmholtz explains our inductive inferences as an "urge" of the understanding that he conceives on analogy to the biological function of an organ such as an eye. Thus for Helmholtz, we are biologically disposed to make these inferences, and they are valid because we have no other means of comprehending nature (Helmholtz 1867/1925, 34–5). But now Helmholtz can invoke the epistemological insight he takes from Müller's doctrine. Inductive inferences and (at their root) our concept of causality are simply our human way of comprehending nature, and thus part of our cognitive apparatus. Since our representations of spatial structure are determined by this cognitive apparatus, they do not resemble any real spatial structure in the external world.

Helmholtz thus gives up his earlier position that our representations of matter and force arrayed in space are images of the external world – that is, that they resemble real spatially-arrayed matter and forces in the external world. He argues instead for a view of our spatial representations that is both more austere and more pragmatic. On his revised view, our representations, including our spatial representations, have only "practical truth" (Helmholtz 1867/1925, 19). Even if those representations do not resemble external objects, they are at least symbols that function as reliable causal indicators of external objects. We can thus use them to make predictions about and "to regulate our movements and actions among" those external objects (Helmholtz 1867/1925, 19).

Thus while our representations are not images that resemble external objects, they nevertheless afford us information about the causal structure of external objects in the following way. Both unconsciously and consciously, we make inductive inferences over sensations, and come to represent lawlike regularities among them. Some of those regularities turn out not to be subject to our will (where Helmholtz understands our will to be the innervation of muscles that bring about movement). For example, every time we have sensations of lightening, sensations of thunder follow it, and there is nothing we can do to make that regularity fail. In that sense, the regularity is a fact that we cannot alter at will. Helmholtz argues that such regularities indicate or provide evidence for the existence of "something independent of our will and imagination, that is, an external cause of our sensations" (Helmholtz 1867/1925, 32). More specifically, Helmholtz maintains that we can infer "from the changing sensations that external objects are the causes of this change" (Helmholtz 1867/1925, 32).

To be clear, Helmholtz is claiming that each of our sensations stands in two different sets of causal relations. First, as we have already seen, within experience we represent lawlike regularities *between* sensations. Thus when we identify sufficiently robust regularities within experience, we say we have identified the cause of the phenomenon in question. But second, Helmholtz is claiming we also recognize that each change in our sensations is the effect on us of an external object. To be sure, neither our sensations of secondary qualities nor the spatial relations among them resemble external objects. But still, for Helmholtz merely the fact that changes in our sensations are effects on us of external objects allows us, in a very limited way and only for lawlike changes in sensation, to "emerge from the world of sensation to the apperception of an external world" (Helmholtz 1867/1926, 32). So within experience we identify robust causal structures in how our sensations change. But since changes in our sensations are caused by changes in external objects, we can infer the existence of causal structures among external objects that are isomorphic to the causal structures we have identified within experience. Thus Helmholtz maintains that our representations afford us information about the causal structure of objects in the external world.[10]

In the *Physiological Optics*, Helmholtz sees a fundamental distinction between our representations of spatial structure and our representations of causal structure. He thinks the character of our spatial representations is determined by our own

[10]One might reasonably wonder why Helmholtz thinks he can infer that tokens of a single type of causal structure among sensations are all caused by tokens of a single type of causal structure among external objects: after all, the point of Müller's experiments was to show that a single type of pattern among sensations can be occasioned by multiple, different types of stimuli. Of course, Helmholtz has not forgotten this. Thus, for example, sensations of flashes of light might be occasioned by either a light behind an aperture or by an electric current passed over the optic nerve. But at the same time, the experience of the physiologist doing the experiment consists of representations in two, distinct causal structures: one with representations of her subject sitting in front of the light and the aperture; the other with representations of her subject sitting wired to a battery. Thanks to Alan Richardson for pressing me to clarify this point.

cognitive processes, but our representations of causal structure are determined by the causal structure of objects in the external world. Thus even though Helmholtz now denies that our representations of spatially-arrayed objects are images that resemble external objects, he thinks our representations of causal structure do afford us information about the causal structure of external objects. Consequently, he maintains that those representations of causal structure are our knowledge's only objective content.

To be sure, with this view Helmholtz retreats from his 1850s account of the content of our objective knowledge, but he does not retreat far enough. The problem is Helmholtz's claim to know that each change in our sensations is caused by a change in objects in the external world. What is the basis for his claim to know that causal correlation? He tries to argue that we could know it, because we can infer the existence of mind-independent objects as the causes of our sensations' changes. But his own account of causality and inductive inference rules out the possibility of the required inference. On his view, we make causal or inductive inferences because of an "urge" of our understanding to make our representations comprehensible. Thus for Helmholtz, our causal inferences are warranted simply because they are expressions of this urge. But if the inferences are warranted by our understanding's urge to make our representations comprehensible, those inferences' valid application does not extend beyond the sphere of our representations. Consequently, we cannot use inductive, causal inferences to infer the existence of any mind-independent objects external to or, as it were, behind our representations. Yet that is just how Helmholtz proposes that we infer that they exist.

Given the epistemological insight that Helmholtz takes from Müller's doctrine, it should hardly surprise us that he cannot ultimately maintain that our representations afford us information about the causal structure of the external world. To say that our inductive inferences are determined by an "urge" of the understanding is to say that they are determined by the nature of our cognitive apparatus. But then, invoking the insight of Müller's doctrine, the representations of causal structure that our inductive inferences provide us do not resemble any real causal structures among external objects. So by Helmholtz's own lights, and despite his own claims to the contrary, we cannot know anything about the causal structure of the external world.

Lange would not make this mistake in his account of objectivity.

6.5 Objectivity for Humanity: F.A. Lange

Lange's *History of Materialism* appeared in the 1866. The book is in the first instance a critical, if also sympathetic, review of materialist philosophy from the ancient period to Lange's own time. But Lange also articulates positive views of knowledge and the philosophical investigation of it that echo the vision of neo-Kantian philosophy that Helmholtz expressed in his 1855 Kant talk. In fact, Lange defends views remarkably similar to Helmholtz's view in the *Physiological Optics*, despite the fact that Lange's book appeared the same year as the *Physiological Optics* and was based on lectures he gave years earlier. Yet one difference between Helmholtz and Lange is the latter's pessimism about the possibility of any knowledge of a mind-independent world. He ultimately argues that the physiology of the sense organs confirms Kant's distinction between appearances and things in themselves. That is, he thinks the physiology of the sense organs forces us to take seriously

> the hypothesis that the whole system ... into which we bring our sense-perceptions – in a word, our whole experience – is conditioned by an intellectual organization that compels us to feel as we do feel, to think as we do think, while to another organization the very same objects may appear quite different, and the thing in itself cannot be pictured by any finite being. (Lange 1866, 235–6/1873–1875, 2:4–5/1925, 2:158)

For Lange, this has important consequences for how philosophers should conceive of knowledge's objectivity.

Like Helmholtz in the same year, Lange argues that our representations of space are not images that resemble external objects. And like Helmholtz, Lange draws this conclusion from an argument that invokes what he takes to be the central epistemological insight of Müller's doctrine. He begins with a sketch of Müller's and Helmholtz's argument that the quality of our sensations of secondary qualities is determined by the nature of our sense organs, and not by the external stimuli that cause our sensations. Thus the "fact that certain vibrations of the air or the aether may leave me completely unmoved, that nevertheless others elicit in me the sensations of light, or shade, etc. lies in an organization that precedes experience ... " (Lange 1866, 255–6). But if the quality of our sensations of, say, tone "is conditioned through our organism," and is not determined by the external stimuli that cause them, then our tone sensations do not resemble those external stimuli. The vibrations in the air caused by a tuning fork "must first come into contact with the auditory nerves of a human or similar being in order to produce tone sensations in consciousness" (Lange 1866, 255–6).

Then Lange immediately extends this argument to spatial representations. He argues that the same reasoning must apply to our representations of the sound waves that cause our tone sensations:

> Here one would shrink shamefully away from the importance of these considerations, if one wanted to take the vibration, which is visible or measurable through sound, to be the thing in itself; since the whole representation of waves and oscillations in parts of the air is through and through as dependent on the conditions of our sense of sight and sense of touch as the sensations of sound is dependent on our sense of hearing. (Lange 1866, 256)

According to Lange our representations of spatially-arrayed matter ("waves and oscillations" in the air) depend no less on our sensory apparatus than our sensations of secondary qualities like tone do, and therefore our representations of spatially-arrayed matter do not resemble things in the external world. With this claim, Lange commits himself to the empirical hypothesis that some physiological or psychological processes determine the character of our spatial representations. Just as with Helmholtz in the *Physiological Optics*, Lange cannot claim that our sense organs determine the character of our spatial representations in exactly the same way they do for our sensations of secondary qualities. Like Helmholtz, Lange thinks our spatial representations are complex products of sensory and cognitive processes, as opposed to individual sensations, which are simple.[11] (Unlike Helmholtz, who in the *Physiological Optics* has a well worked out account of how our spatial representations are produced by unconscious inductive inferences, in the first edition of *History of Materialism* Lange has no detailed hypothesis about the specific nature of physiological or psychological process that produces spatial representations.)

Since for Lange our spatial representations are complex products of sensory and cognitive processes, if a creature had sufficiently different sensory and cognitive processes, it would represent space differently than we do.[12] Thus for Lange, since our spatial representations are determined by our sensory and cognitive processes, they do not resemble the spatial structure of external objects. Or as he puts it in his Kantian jargon, our spatial representations are only appearances, and do not resemble things in themselves.

This much of Lange's account, at least in its outlines, is consistent with the account of the content of objective knowledge that Helmholtz gives in the *Physiological Optics*. Also similar to Helmholtz, Lange thinks our causal inferences are warranted because we are biologically disposed to make them. On Lange's view, "the concept of cause is rooted in our organization ..." (Lange 1866, 263/1873–1875, 2:45/1925, 2:212), that is, we are physiologically disposed to structure our representations as causes and effects. Lange sees this account of causality as squarely within the vision of Kantian philosophy that Helmholtz expressed in his 1855 Kant lecture. Lange thinks that his account of causal inference follows Kant's in that both explain the warrant for our causal inferences by appeal to a concept of causality that is required for any possible experience and that is, in that sense, a priori. Also, Lange thinks it will ultimately be physiology that provides the full account of this concept of causality and its operations:

> Perhaps some day the basis of the concept of cause may be found in the mechanism of reflex action and sympathetic excitation; we should then have translated Kant's pure reason into physiology and so made is more easily conceivable. (Lange 1866, 263/1873–1875, 2:44/1925, 2:211)[13]

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[11]In the first edition of the *History of Materialism*, this view of our spatial representations becomes clear only in Lange's argument against the crude nativist hypothesis that our representation of space is a "ready-made form" that we fill with sensations. Lange insists to the contrary that our representations of space are produced and shaped by physiological and psychological processes (Lange 1866, 254).

[12]This is an argument that Lange repeats and expands significantly in the second edition of *History of Materialism*. See Lange 1873–1875, 2:429/1925, 3:226. There he argues that, for example, the fact that our (human) space has three dimensions need not hold for other, differently constituted beings.

[13]I note without pursuing it that Lange, here and elsewhere, explicitly commits himself to a vision of Kantian theory of knowledge that is thoroughly naturalistic. Thus while there is a circularity involved in pointing to causal processes to explain the epistemological basis of causal inferences, he is committed to thinking that it is a benign circle.

Further, Lange recognizes that this account of the warrant for our causal inferences has an important consequence. If our causal inferences are determined by a physiological disposition to structure our representations as causes and effects, then our causal inferences are warranted only within the domain of our representations, and our representations of causal structure do not afford us information about causal structures among external objects. Nor can we use causal inferences to infer the existence of external objects as the causes of our sensations, and thus Lange thinks we cannot ultimately know that those objects exist. (Lange thinks the concept of causality can furnish us with the *concept of* mind-independent objects that cause our sensations. But his stated view is that for all we can know, our concept of those objects might be empty.[14]) Here is an important point of disagreement between Helmholtz and Lange. In the *Physiological Optics*, Helmholtz wants to claim that at least our representations of causal structure are determined by the causal structure of external objects, and thus that we can know that external causal structure. But for Lange, since we cannot infer that our sensations are caused by external objects, we cannot claim that the causal structure we represent is determined by the causal structure of external objects. Thus we cannot claim that our representations of causal structure afford us knowledge of the causal structure of external objects.

This view of knowledge has significant consequences for Lange's account of objectivity. Helmholtz's criterion of objectivity is that the objective representations are those that resemble, or at least afford us information about, properties of objects in the external world. While Helmholtz thinks that by those lights only our representations of causal structure are objective, Lange denies even that. He concludes that no part of our knowledge affords us any information about the external world. By Helmholtz's lights, Lange has whittled the content of our objective knowledge down to nothing. Consequently, Lange must deny either that we have any objective knowledge, or that the objective elements of our knowledge are those that afford us information about external objects.

Lange takes the second route. He calls the idea that objective knowledge represents the external world "absolute objectivity" (Lange 1866, 234/1873–1875, 2:3/1925, 2:156), and he argues that the physiology of the sense organs forces philosophers to give that idea up (Lange 1866, 235–6/1873–1875, 2:4–5/1925, 2:158). However, Lange reasons that even if all of our representations – "in a word, our whole experience" – are determined by our physiological and psychological organization, some elements of our representations will at least be common to all humans, precisely in virtue of their common physiological and psychological structures. On Lange's view these common physiological and psychological structures ensure that at least some elements of our representations will be universally valid in Kant's sense, that is, intersubjective. Since this universal validity is a consequence

[14]It is not clear that Lange consistently maintains his own stated view that he cannot (causally) infer the existence of mind-independent objects. See Edgar (2013) for a more detailed account of Lange on these points. However, whatever ambiguities his views have on these points, they do not appear in his discussions of objectivity, so I ignore them here.

of human beings' shared physiological and psychological structures, it is universal only for humans (and sufficiently similar beings). Lange thus conceives of this universal validity as biologically conditioned and species-specific.[15] Lange calls it "objectivity for humanity", and he thinks it is the only objectivity available to us.

Lange's concept of objectivity thus constitutes a decisive break from Helmholtz's. No longer is the criterion of objectivity that objective elements of our representations resemble or afford us information about properties of external objects, since on Lange's view there are no such elements. He denies even that any elements of our representations are determined by properties of external objects. Thus for Lange, objectivity simply has nothing to do with the mind-independent world.

6.6 Nature as a Phenomenon of Consciousness: Otto Liebmann

During the same period, Liebmann defended a similar view of objectivity. In his 1865 *Kant and his Epigones* and 1869 *On the Objective Viewpoint*, he argues for an idealism according to which all of nature is nothing more than a phenomenon of consciousness. In *The Objective Viewpoint*, he wants to show how Müller's doctrine demonstrates one of the premises he needs to establish that idealism, thereby providing natural scientific support for the view. But at the same time, his principal aim in *The Objective Viewpoint* is to develop a "critical" account of objective sight as visual awareness of matter arrayed spatially in the external world – that is, to show how this account of objective sight is consistent with his idealism (Liebmann 1869, iv).

To begin, Liebmann thinks Müller's doctrine confirms Locke's thesis that sensory qualities do not resemble mind-independent objects (Liebmann 1869, 6–7, 32, 130). But Liebmann argues that Müller's doctrine leads to a further conclusion, namely, to the idealist thesis that nature is a phenomenon of consciousness and therefore that our representations of nature afford us no information about a mind-independent world. He argues that Müller's conclusion that sensory qualities do not resemble objects in the external world must be extended to the physiologist's picture of sensory processes themselves. The physiologist appeals to external stimuli exciting sensory nerves that are connected to a nervous system and a brain. But, Liebmann argues, our representation of that physical and physiological system is itself composed of sensory qualities – for example, the set of tactile feels included in the content of our representation of external matter, and the patterns of light and

[15]For Lange's explicit discussion of the species-relative nature of our objective knowledge, see 1872–1875, 2:539–40/1925, 3:336.

colour that constitute our visual representation of the brain.[16] On Liebmann's view, Müller's doctrine entails that these representations too do not resemble any mind-independent objects (Liebmann 1869, 134–6):

> Accordingly, it is actually a wholly biased, mistaken view if a man believes that he inhabits an illuminated, coloured, noisesome world; rather, it lives in him, in his consciousness, and in the consciousness of all subjects that are, like him, sentient and understanding. (Liebmann 1869, 140)[17]

Thus, Liebmann wants to conclude, none of our representations are determined by or resemble a world that exists beyond them.

In fact, this argument provides only partial support for Liebmann's idealism. As Helmholtz and Lange understand, Müller's doctrine on its own does not entail that our representations of spatial and causal structure do not resemble real spatial and causal structure of mind-independent objects. That is why they both develop further arguments modelled on Müller's for their denials that our representation of spatial structure resembles the real spatial structure of external objects. Liebmann's argument, as he states it, neglects the possibility that our representations of external stimuli exciting nerves and sending signals to the brain might resemble the real spatial or causal structure of that process, even if the pinkish-grey hue of our image of the brain does not correspond to anything beyond our representations. Thus despite Liebmann's apparent suggestion that his idealism is nothing but the epistemological consequences of Müller's doctrine worked out consistently, that cannot ultimately be all there is to his argument.

Indeed, it is not all there is to his argument. Liebmann does deny that our representations of spatial and causal structure resemble any real spatial or causal structures in the mind-independent world, but his reasons for these denials are fundamentally different than Helmholtz's and Lange's. In particular, Liebmann's reasons do not depend on positing physiological or empirical-psychological processes that determine our representations of spatial and causal structure. For Liebmann, our representations of space, time, and causality are Kantian "forms of knowledge", and he argues that they are ordering relations that the mind (he typically says "spirit", "intellect", or "understanding") uses to interpret sensations (Liebmann 1869, 108). He argues further that, as ordering relations the mind uses to interpret sensations, they cannot themselves be derived from sensations (Liebmann 1869, 109). (He thus maintains that our spatial representations are innate in a way

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[16] Lange suggests a nearly identical argument a few years later (Lange 1873–1875, 2:423/1925, 3:219).

[17] Also:

> But the whole is and remains a sensible phenomenon within our consciousness, constituted out of subjective sensations, disciplined, interpreted, spatially arrayed, and objectified by irrefutable rules of our understanding, which we obey without knowing why. It thus has no absolute, but only a relative being; it exists only on the presupposition of our sensibility, in virtue of our intellectuality in our consciousness. (Liebmann 1869, 140–1)

that both Helmholtz and Lange are at pains to deny in the 1860s.) For Liebmann, these forms of knowledge are rules of the mind without which we could have no empirical knowledge at all.

Liebmann's conception of these forms of knowledge as rules without which no empirical knowledge would be possible has two important consequences. First, the forms of knowledge are unexplained explainers. While (as we will see below), they explain the possibility of objective representation for Liebmann, he insists that they admit of no explanation themselves. Rather, they are "the final, ultimate explanatory ground" of objective representation (Liebmann 1869, 108). It would be consistent with this view to argue against Helmholtz and Lange that physiology of the sense organs cannot provide any explanation of these forms of knowledge, since as an empirical science, physiology presupposes and depends on just those forms. Liebmann thus maintains that philosophers cannot investigate or explain these forms empirically.

Second, Liebmann thinks that his conception of our representations of space, time, and causality entails that we cannot claim that they resemble any features of a mind-independent world. Since, on his account, these forms of knowledge are merely rules of the mind without which empirical knowledge would not be possible, Liebmann thinks we must restrict their valid application to the sphere of our representations. Thus they do not validly apply to a mind-independent world (Liebmann 1869, 140–1).[18] Consequently, Liebmann takes himself to rule out the possibility that our representations of external stimuli exciting our sensory nerves and sending signals to our brain resemble any real spatial or causal structures in the mind-independent world.

Because Liebmann thinks we cannot explain our representations of space, time, and causality by physiological or any other natural scientific means, he cannot fully accept Helmholtz's view that physiology of the sense organs provides natural scientific means of carrying out Kant's project of investigating the subjective conditions of knowledge. But he nevertheless maintains that Müller's doctrine of specific nerve energies is significant for philosophy precisely because it demonstrates one of the premises he takes himself to need in order to establish his thesis that all of nature is nothing but a phenomenon of consciousness: namely, that the sensory qualities our mind orders according to spatial, temporal, and causal relations do not resemble mind-independent objects. For Liebmann, Müller's doctrine thus provides a measure of natural scientific support for his denial that any element of our representations is determined by or resembles properties of a mind-independent world (Liebmann 1869, 31–2, 130).

[18]Further, like Lange, Liebmann recognizes that if our causal reasoning is valid only within the sphere of our representations, we cannot validly claim that our sensations are the effects on us of mind-independent objects. Consequently, Liebmann argues that most we can conceive of the relation of our sensations to mind-independent objects is that an unknowable X (the mind-independent object, the Kantian thing in itself) stands in an unknowable relation to our mind. He calls that unknowable relation the "transcendental factor" in experience (Liebmann 1869, 152–3).

Yet in addition to this idealism, Liebmann also takes “objective sight” to mean visual awareness of matter arrayed in space in an external world. His principal aim in *The Objective Viewpoint* is thus to develop an account of objectivity that is consistent with both that view of objective sight and his idealism’s denial that our representations afford us information about mind-independent objects. Central to that account of objectivity is Liebmann’s treatment of the concept of an *external world* as itself a spatial representation, and thus a representation that admits of explanation by appeal to our ability to represent space (and time and causality). On this view, “without the a priority of the spatial forms of intuition, the subject could never come to have a representation of anything external” (Liebmann 1869, 109).

On Liebmann’s account, we start with sensations that Müller’s doctrine tells us are subjective in the sense that their qualities are determined by the nature of our sense organs and not by properties of external objects. He argues that the forms of our knowledge – our representations of space, time, and causality – are responsible for transforming those sensations into objective sight. Liebmann explicates his view that the forms of knowledge are ordering relations for sensations by arguing that they constitute a spatio-temporal-causal array onto which our mind (necessarily, without voluntary control) projects our sensations of secondary qualities. The array consists in part of three spatial dimensions. On Liebmann’s view, when our mind projects sensory qualities onto determinate points on this spatial array at a determinate point in time, and then represents those qualities’ locations changing over time according to necessary causal laws, we thereby represent concrete material (that is, extended) objects interacting causally with one another in space. For Liebmann, these objects are “external” just in virtue of the fact that they are arrayed in a three-dimensional space (Liebmann 1869, 18–20).

Since for Liebmann our representations are objective when they are of objects arrayed in space in the external world, he does not, like Lange, define objectivity as universal validity or intersubjectivity. Still, he thinks the universal validity of objective representations is a direct consequence of his account of objectivity. Representations of concrete material objects arrayed in space in an external world will be shared by “all subjects that are … sentient and understanding” (Liebmann 1869, 140). That is, they will be universally valid for humans and any other beings with relevantly similar forms of knowledge.

Finally, while Liebmann, like Helmholtz, understands representations to be *objective* when they are of objects in the external world, the similarity between their views is superficial. Helmholtz in the 1850s and 1860s identifies the external world with a mind-independent world, a world beyond our representations. But Liebmann rejects exactly that identification. For him, the external world is the spatial (and temporal and causal) world, a world represented in consciousness in virtue of the fact that our mind projects our sensations onto an array that itself is nothing but a form of knowledge. In contrast, for Liebmann the mind-independent world is the world of things in themselves, which on his idealism is completely unknowable. Consequently, like Lange and in contrast with Helmholtz in the 1850s and 1860s, Liebmann severs any connection between the concepts of objectivity and a mind-independent world.

6.7 The Laws of the Actual: Helmholtz's Mature Conception of Objectivity

However, Helmholtz would follow suit within a decade. In an 1878 address called "The Facts in Perception," he withdraws his claim that any part of our representations, including our representations of causal structure, is determined by or affords us any information about a mind-independent world. He no longer maintains the argument – which was inconsistent with his own account of the basis of causal inference – that we can infer the existence of a world beyond our representations as the cause of our sensations. Consequently, he demotes the claim that such a world exists to the status of hypothesis. Helmholtz prefers that hypothesis to the alternative hypothesis that there is no external world, but he sees no way to disprove that alternative, and so acknowledges that his preference never amounts to knowledge (Helmholtz 1878/1977, 137).[19]

However, while Helmholtz no longer thinks we can infer the existence of external objects as the causes of our sensations, he still maintains that we can represent causal relations *within* experience. Indeed, he maintains that these causal relations are not mere hypotheses, but that they constitute the content of our knowledge (Helmholtz 1878/1977, 138). He argues that lawlike relations between representations, just because they are repeated often enough, are reinforced in our memory, while idiosyncratic, nonlawful changes in our representations are washed away. In this way, we come to have an image of the lawlike in experience (Helmholtz 1878/1977, 131).

Echoing the discussion of laws from his *Physiological Optics*, Helmholtz suggests that we can formulate some laws with such generality and completeness that we cannot, by means of our will, bring it about that the laws fail. (There is nothing we can do about the fact that thunder follows lightening.) But in the *Physiological Optics*, Helmholtz took these lawlike regularities to indicate, or provide evidence for, the existence of mind-independent objects that cause our sensations, and he took the objective content of our knowledge to consist in information about the causal structure of those mind-independent objects. Here, lawlike regularities that we cannot alter at will constitute the objective content of our knowledge – but they do so *just because* we cannot alter them at will. That is, Helmholtz no longer thinks that objective representations are those that are determined by or afford us

[19] In fact, as Liesbet de Kock has recently shown, Helmholtz first clearly articulates his view that our belief in an external world is a mere hypothesis several months before his address "The Facts in Perception" in response to a criticism from J.P.N. Land. Although I cannot here give de Kock's interpretation its due, I note that she gives an account of the development of Helmholtz's views that contrasts sharply with the one I am offering. On her account, Helmholtz's view in 1878 that our belief in an external world is merely a "hypothesis" that can never amount to knowledge does not constitute a substantive break from his earlier views. Rather, on her interpretation, Helmholtz was pushed in 1878 to articulate clearly a pessimism about our knowledge of the external world that he had maintained implicitly at least since the *Physiological Optics* if not before (de Kock 2014, 15–21).

information about mind-independent objects, since he no longer thinks there *are* any such representations. Rather, he now maintains that objective representations are those that are not subject to our will.[20] For Helmholtz, laws that we cannot alter at will constitute "the actual." He calls these laws the "objectum," and identifies them with Fichte's "not-I", which he knows is a term Fichte used for the objective content of knowledge (Helmholtz 1878/1977, 126, 140).[21] Thus for Helmholtz in "The Facts in Perception," the laws of the actual constitute the objective content of knowledge (Helmholtz 1878/1977, 140).

This conception of objectivity, according to which the objective elements of our knowledge are those that are not subject to our will, is by no means identical to Lange's or Liebmann's. Yet Helmholtz follows them at least in severing any connection between his conceptions of objectivity and a mind-independent external world.

6.8 Conclusion

I have argued that reflection on Müller's doctrine and its epistemological consequences led Lange, Liebmann, and eventually Helmholtz all to reject the view that objective knowledge affords us information about or is determined by mind-independent objects. For these neo-Kantians, Müller's doctrine thus provides evidence for conceptions of objectivity that have nothing to do with the mind-independent world. Their accounts of objectivity thus stand as examples of how philosophers and philosophically-minded scientists appropriated results and theories from natural science, and marshalled those results and theories as evidence in philosophical disputes. Their accounts of objectivity thus illustrate how, in the context of the post-Hegelian German-language intellectual landscape, the increased authority of natural science relative to philosophy was manifested in concrete argumentative contexts. I conclude by considering briefly why their appeals to natural science would have made for such powerful epistemological arguments in that post-Hegelian context.

Hegel had argued that truths about nature and humanity's place in it were known by speculative reason, but by the 1850s the backlash against Hegel was in full effect. Both Büchner, the scientific materialist, and Helmholtz, the neo-Kantian, are at pains to emphasize the role they thought natural science should play in philosophical theorizing, and both were at pains to emphasize the anti-Hegelian

[20] Lorraine Daston and Peter Galison emphasize this conception of objectivity. See Daston and Galison 2007/2010, especially Chs. 4–5. I note here without pursuing it that Helmholtz's mature conception of objective representations as those that are not subject to our will appears to be just one of several, and not a conception of singular significance or influence – at least among neo-Kantians in the second half of the nineteenth century.

[21] I am indebted to Robert Brain for helpful discussion on these points.

thrust of these views. Later, when Lange and Liebmann followed Helmholtz in appealing to Müller's doctrine to inform their Kantian accounts of knowledge, they similarly emphasized that Müller's was a natural scientific doctrine supported by a growing body of experimental evidence. Thus to the extent that the epistemological consequences of Müller's doctrine resemble philosophical doctrines going back to Kant (and even to Locke), these neo-Kantians presented Müller as providing natural scientific confirmation for ideas that in the seventeenth and eighteenth centuries could only be considered hypotheses.[22]

Lange, more explicitly than Helmholtz or Liebmann, fully exploits the purportedly natural scientific warrant for his conception of objectivity in an argument against rival post-Hegelian conceptions – specifically, those of scientific materialists like Büchner. In the expanded second edition of *History of Materialism*, Lange argues explicitly that the physiology of the sense organs makes the scientific materialist conception of objectivity untenable. He opens his argument with a sweeping account of the philosophical significance of physiology of the sense organs:

> We have hitherto seen in every department that it is the scientific, the physical study of phenomena, which is able to throw upon man and his intellectual nature the light of real knowledge, though it may be at first a few scattered rays. Now we come to the department of human inquiry in which the empirical method has celebrated its highest triumph, and in which, at the same time, it leads us to the very limits of our knowledge, and betrays to us at least so much of the sphere beyond it as to convince us of its existence. This is the physiology of the sense organs. (Lange 1873–1875, 3:408/1925, 3:202–3)

Lange goes on to expand his argument from the first edition that Müller's doctrine and further arguments modelled on it entail that no part of our knowledge is determined by or affords us information about a mind-independent world. But here he is loudly calling attention to what he takes to be the source of his argument's authority: "the scientific, the physical study of phenomena", that is, "the empirical method", which when it is applied to the human knowing subject, "celebrate[s] its highest triumph." Lange wants to make clear, especially to the scientific materialist, that (he thinks) his account of objectivity does not depend on any merely speculative, a priori philosophical commitments, but on the materialist's own paradigm of knowledge, that is, natural science. He thus proposes to take full account of Müller's doctrine precisely in order to "see how much of materialism may be retained" in light of it (Lange 1873–1875, 3:410/1925, 3:204). While some materialist doctrines can be retained, Lange thinks, the materialist conception of objectivity cannot be. Since for Lange – as for Helmholtz and, to a degree, Liebmann as well – the physiology of the sense organs, and thus natural science itself, ultimately reveals why the objectivity of our knowledge can have nothing to do with a mind-independent world.

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[22]Helmholtz 1855/1884, 379; 1878/1977, 118–9; Lange 1873–1875, 2:409/1925, 3:202–3; Liebmann 1869, 20.

References

Boring, Edwin G. 1929/1957. *A history of experimental psychology*. New York: Appleton-Century-Crofts.

Büchner, Ludwig. 1855/1864. *Force and matter*. Trans. J. Frederick Collingwood. London: Trübner & Co.

Daston, Lorraine, and Peter Galison. 2007/2010. *Objectivity*. New York: Zone Books.

de Kock, Liesbet. 2014. In the beginning was the act: A historical and systematic analysis of Hermann von Helmholtz's psychology of the object. Dissertation, University of Ghent.

Edgar, Scott. 2013. The limits of experience and explanation: F.A. Lange and Ernst mach on things in themselves. *British Journal for the History of Philosophy* 21(1): 100–121.

Gregory, Frederick. 1977. *Scientific materialism in the nineteenth century*. Dordrecht: Reidel.

Hatfield, Gary. 1990. *The natural and the normative: Theories of spatial perception from Kant to Helmholtz*. Cambridge: MIT Press.

Hatfield, Gary. 2011. Kant and Helmholtz on primary and secondary qualities. In *Primary and secondary qualities: The historical and ongoing debate*, ed. Lawrence Nolan. Oxford: Oxford University Press.

Helmholtz, Hermann von. 1852/1883. Über die Natur der menschlichen Sinnesempfindungen. In *Wissenschaftlichen Abhandlungen*, vol. 2, 591–609. Leipzig: Johann Ambrosius Barth.

Helmholtz, Hermann von. 1853/1884. Über Goethe's naturwissenschafliche Arbeiten. *Vorträge und Reden*, vol. 1, 1–24. Braunschweig: Friedrich Vieweg und Sohn.

Helmholtz, Hermann von. 1855/1884. Über das Sehen des Menschen. *Vorträge und Reden*, vol. 1, 365–396. Braunschweig: Friedrich Vieweg und Sohn.

Helmholtz, Hermann von. 1867/1925. *Treatise on physiological optics*. Trans. James P. C. Southall. Menasha: The Optical Society of America.

Helmholtz, Hermann von. 1878/1977. The facts in perception. Trans. Malcolm F. Lowe. In *Epistemological writings*, ed. Robert S. Cohen and Yehuda Elkana, 115–163. Dordrecht: Reidel.

Lange, Friedrich Albert. 1866/1873–1875. *Geschichte des Materialismus und Kritik seiner Bedeutung in der Gegenwart*. Iserlohn: J. Baedeker. Trans. as: 1877–1881/1925. *History of materialism and criticism of its present importance*, ed. Ernest Chester Thomas. London: Routledge and Kegan Paul.

Liebmann, Otto. 1865. *Kant und die Epigones*. Stuttgart: Carl Schober.

Liebmann, Otto. 1869. *Über den objektiven Anblick*. Stuttgart: Carl Schober.

Müller, Johannes. 1833–1840. *Handbuch der Physiologie des Menschen*. Coblenz: J. Hölscher.

Zeller, Eduard. 1862/1877. Über Bedeutung und Aufgabe der Erkenntnistheorie. *Vorträge und Abhandlugnen*, 479–526. Leipzig: Fues.

Chapter 7
Seeing and Hearing: Charcot, Freud and the Objectivity of Hysteria

Paolo Savoia

7.1 Introduction

This essay takes its origin from a problem raised by the complex and much investigated relation between the French neurologist and alienist Jean-Martin Charcot (1825–1893), and the father of psychoanalysis, Sigmund Freud. Their works on hysteria have been crucial in the rise of the sciences of the mind as we know them and are written in a period – the fin-de-siècle and the turn of the century – that saw the formation of some of their most significant conceptual tools. One of them, as we will see, is the concept of *trauma*,[1] which allowed both physicians to conceive hysteria and, more generally, nervous diseases in a new way.

Charcot and Freud write in the same years about the same psychopathological phenomenon, hysteria. Why, then, do they give such a great importance to, respectively, seeing, or eye observation, and hearing, that is, listening to the patient's account?[2] How is it possible that Charcot's texts, lectures and therapeutic practices abound with images and photographs, while Freud's texts completely lack them?

I am grateful to Arnold I. Davidson for precious criticism and advice.

[1]On Charcot and trauma see Micale 2001; for a discussion of Freud's trauma and an overview of the immense literature on the topic see Leys 2000, 18–40.

[2]At least two texts which addressed this problem have helped me in isolating it: Gilman 1993; and de Marneffe 1991. Gilman explains the shift from seeing to hearing by giving an account of the scientific interpretations of Jewishness at the turn of the century; de Marneffe focuses on the different importance that Charcot and Freud gave to the patient's subjective content of their discourses on themselves. While these are certainly both instructive interpretations, the point I would like to make is a different one, although, I hope, not incompatible with them. An interesting

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I will try to answer these questions at an historical level, taking into account different forms and practices of scientific objectivity. Freud's approach to symptoms and neuroses is much better known than Charcot's, so I will focus on the latter and use Freud's early theory of hysteria in order to compare their alternative views of the objectivity of the inner life of the human being.[3]

Despite some critical differences that will emerge in the conclusion, the framework of my analysis is given by Daston and Galison's history of objectivity as entailing a commitment to a series of epistemic virtues, conceived as epistemic and ethical elements that merged into regulative ideals. The present essay is thus situated within the trend in the historiography of science that is generally known as "historical epistemology".[4] However, as a methodological premise, in my analysis:

1. I will not try to set up a causal explanation of the passage from the primacy Charcot accorded to sight to the one Freud accorded to hearing. Rather, my aim is to describe two structurally different sets of epistemic norms and regimes of scientific perception.
2. I will not make use of a sharp and clear distinction between observation and theory, or between clinical work and theoretical reflection. The reason for this is historical as well as theoretical. Charcot and Freud both favored a clinical approach to nervous diseases: Charcot opposed clinical practices of observation to medical "systems"; Freud intended to ground the scientificity of psychoanalysis on observation. Therefore, I will deal with issues such as how they conceived and practiced observation; what senses they made use of in order to observe; and how they conceptually made sense of their "data". Besides, in this paper observation and perception will not represent universal anthropological constants, but rather fully historical activities. My emphasis will thus be on the correlations between, on the one hand, epistemic virtues and norms, and, on the other hand, the ways in which scientific observation individuates and stabilizes scientific objects.[5] As we move forward into this exploration, we will realize that the object in question is no other, and no less, than the mind, or *the self*.

discussion and overview on the literature on the topic can be found in Cartwright 1995, 47–80. For a general account of photography and psychiatry in the 19th century see Gilman 1982, 164–213.

[3]On Charcot's life and works see Bonduelle et al. 1996.

[4]On historical epistemology, broadly conceived as a Franco-American tradition, see for example Daston 1998; 2001a; Hacking 1999; Davidson 2001, ix–xiv; Lecourt 2001; Braunstein 2002; Sturm and Feest 2009; Rheinberger 2010.

[5]I am referring here to Daston and Lunbeck 2011a, 1–6. On the history of scientific observation see also Singy 2006.

7.2 Seeing Hysteria: Charcot

With respect to the problem of the objectivity of hysteria and of the mind, we are concerned here by a relatively small part of Charcot's work: his demonstration of the existence of male hysteria. This analysis seeks to demonstrate two things: a shift of the meaning of the concept of trauma from the physical to the psychological (or "moral" as it was sometimes called) and the predominance of the clinical and scientific ideal of *visibility* in the works of Charcot and his school at the Salpêtrière, an ideal that was regarded as the main feature of what could be called *objective* in the field of the psyche.

In the first lecture of the third volume of his 1885 *Lectures on the Diseases of the Nervous System delivered at the Salpêtrière*, Charcot sketches what may be seen as his research programme. First of all, he praises the anatomo-clinical method, a glorious French medical tradition. Clinical observation must be the main guide for the science of the nervous system, which is also to be connected with the whole of the biological sciences. However, Charcot goes on, the specificity of neurology consists in the particular features of its study of lesions. Of course, in principle we can say that each singular symptomatology corresponds to a specific cerebral lesion, which in turn reveals the disorders of the functions of the cerebral regions involved. Even so, Charcot admits, "on the question of cerebral lesions much uncertainty exists". There are indeed cases of pathological states located in the nervous system, "which leave in the dead body no material trace that can be discovered" (Charcot 1887/1889, 12). Hysteria is the clearest example of this kind of diseases, called neuroses:

> These symptomatic combinations deprived of anatomical substratum, do not present themselves to the mind of the physician with that appearance of solidity, of *objectivity*, which belong to affections connected with an appreciable organic lesion (Charcot 1887/1889, 12).

An attentive, patient, and repeated activity of observation is the principal means for describing the regular type of hysterical phenomena. This is in fact what Charcot did with his famous characterization of the four phases – epileptoid, great movements, passionate attitudes, terminal delirium – of the hysterical attack (Fig. 7.1a, b).

Charcot's introductory lecture serves us well to see what is at stake in his treatment of hysteria. The problem here is the absence of the anatomical substratum of the symptoms: as he explicitly points out, symptoms lack solidity and *objectivity*. Charcot's approach is thus well representative of a common way of reasoning that was typical of late nineteenth-century psychiatry: the clinical understanding of neuroses – slippery illnesses whose anatomical seat cannot be identified – has to be complemented by ad hoc anatomical and physiological hypotheses. As far as the more controversial problem, characteristic of hysteria, of the simulation by patients making up imaginary symptoms, let us just recall that Charcot was convinced to solve it by means of the experimental tool of hypnosis.[6] And hypnosis

[6]Charcot presented a famous memoir on hypnotism at the Paris Academy of Science in 1882, fully supporting the scientific character of this otherwise suspicious practice (see Charcot 1882); on the history of hypnotism see Gauld 1992.

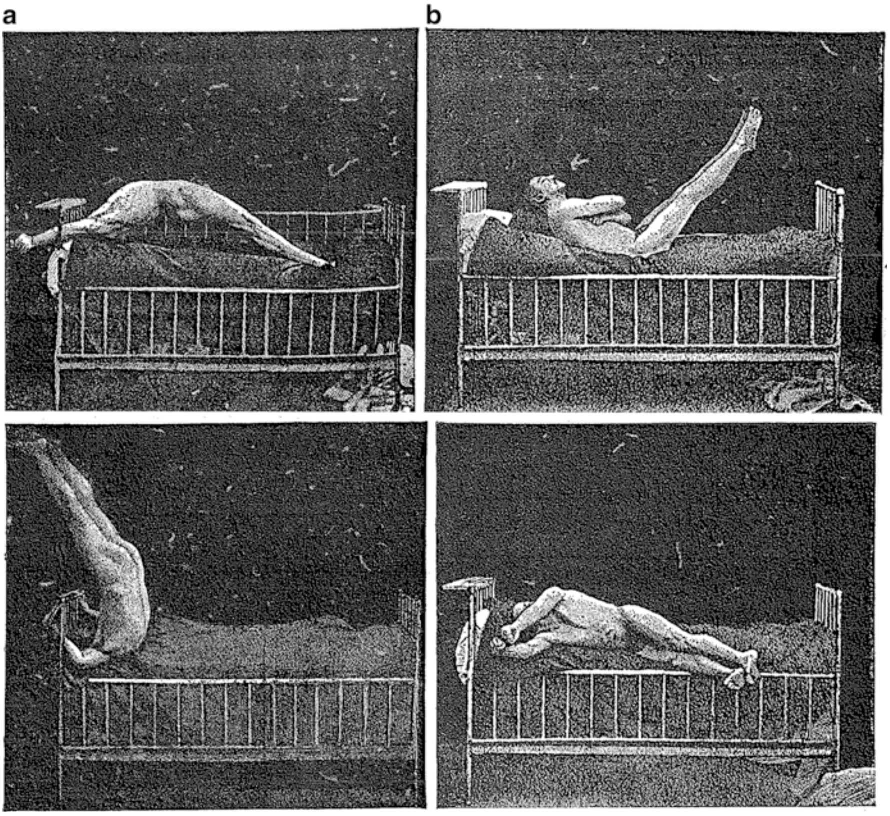


Fig. 7.1 (**a** and **b**) Hysterical attack in a male patient. J-.M. Charcot, *Leçons sur les maladies du système nerveux*, Paris: Delahaye 1887, vol. 3

for Charcot should produce *visible and recordable effects*. For example, if we hypnotize a subject and we graphically compare, through an apparatus that measures the movement of the chest, his movements' quality and regularity to those of a suspected malingerer, then we will easily be able to verify whether this suspicion is well founded or not. In the hypnotized subject's movements there will be no intervention of the will, for hypnosis is the abolition of conscious will, and therefore the lines traced by the apparatus will be regular. On the other hand, the image of the movements of the simulating subject will be irregular, marked by the presence of a more or less conscious will to simulate. In fact, the will of the malingerer is objectively represented by the irregular lines in the graph. This apparatus does not record anything, but it is graphically able to tell us whether we are dealing with a simulator or with a real hysteric (Charcot 1887/1889, 14–18) (Figs. 7.2, 7.3 and 7.4).

Let us now return to the issue of male hysteria. Charcot's aim is to make hysteria become objective by making it visible by means of experimentation and

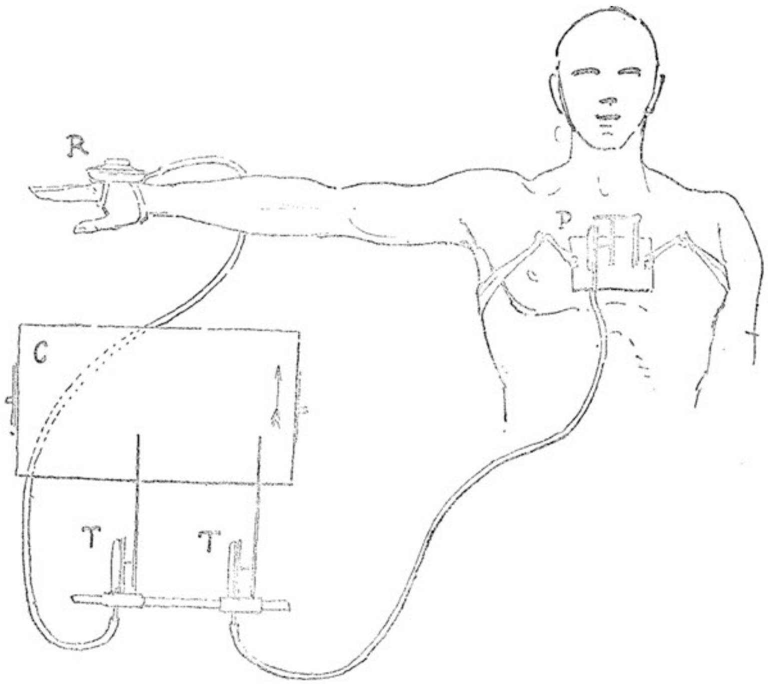


Fig. 7.2 The visualization of the simulator. This device, called pneumograph, was used by Charcot in order to identify and distinguish hysteric patients from simulators. Detectors of the movement of the chest of hypnotized patients are linked to a pen tracing lines on a piece of paper; depending on the regularity of these lines the physician will be able to see if there is an intervention of the will (when the line is irregular) or not (when they are regular). Given the fact that hypnotization is the abolition of the will, in the former case we are in presence of a malingerer who is only simulating the absence of the will. J-.M. Charcot, *Leçons sur les maladies du système nerveux*, Paris: Delahaye 1887, vol. 3

observation. In other words, for Charcot, demonstrating the existence of male hysteria is tantamount to demonstrating its objectivity.

We won't be able to discuss all the complex gender issues involved in the attribution of hysteria to male patients here,[7] so let us confine ourselves to the structure of Charcot's argument and proofs, characteristic of his powerful clinical

[7]On gender and the history of hysteria see Showalter 1993; King 1998, 205–246; Micale 1995; Edelman 2003; Goldstein 2009, 49–55. Charcot was interested in debunking the medical opinion according to which only weak and effeminate boys could be seized by hysteria. He builds on the works of some English and American physicians who diagnosed the so-called "railway spine", a nervous disorder that followed episodes of trauma caused by accidents that happened to strong and virile workers of the railways. These kind of male subjects served well Charcot's purpose of making hysteria a universal phenomenon. On this complicated history of traumatism see Micale and Lerner 2001a, b; Harrington 2001; Caplan 2001; Hacking 1995, 183–197; Leys 2000.

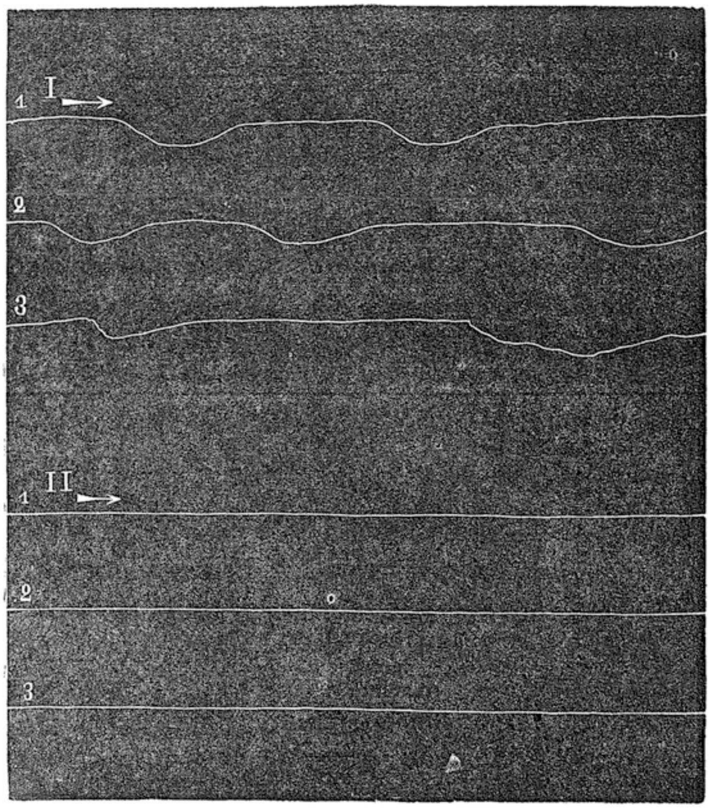


Fig. 7.3 The visualization of the simulator. Real hysteric: no voluntary movement traced, absence of the will; J-.M. Charcot, *Leçons sur les maladies du système nerveux*, Paris: Delahaye 1887, vol. 3

lectures. First, Charcot presents six cases of male patients, all of them characterized by two main features: they have classic and clear hysterical symptoms, and above all the typical forms of paralyses; their pathological state can be traced back to one or more episodes (such as accidents and assaults) that impressed on them a vivid emotion of fear, terror, etc., but left no appreciable material, organic lesion that could count as a cause for their paralyses. Charcot begins then to show the process that led him to recognize the reality of male hysterics.

The first step is a comparison between the cabdriver named "Porcz.", a difficult case of right brachial "monoplegia" (paralysis) mysteriously originated by a traumatic fall, and "Deb.", a second patient with a paralysis doubtlessly originating in an organic lesion of the peripheral nerves of the shoulder. Although the two paralyses seem identical, a careful clinical inspection supported by graphical representation reveals that the area indicating the distribution of the first patient's monoplegia is

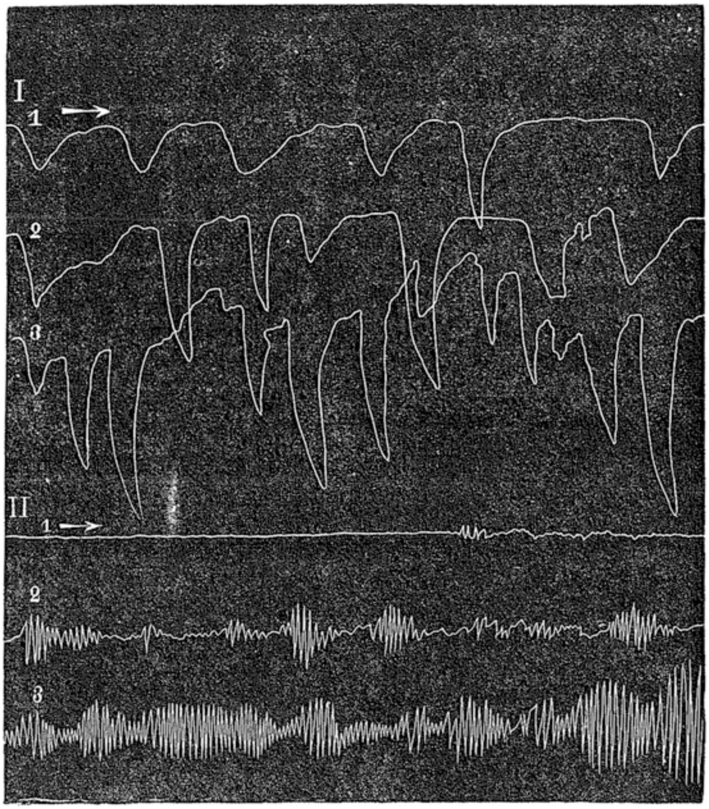


Fig. 7.4 The visualization of the simulator. Simulator: voluntary movement traced, presence of the will; J-.M. Charcot, *Leçons sur les maladies du système nerveux*, Paris: Delahaye 1887, vol. 3

completely different from the one observed in a case of organic lesion of the brachial plexus (Charcot 1887/1889, 270) (Fig. 7.5a, b, respectively). No doubt, Charcot goes on, we are dealing with a lesion of the nervous system, but:

> We have here unquestionably one of those lesions which escape our present means of anatomical investigation, and which, for want of a better term, we designate *dynamic* or *functional* lesions (Charcot 1887/1889, 278).

At this point, Charcot explicitly formulates the hypothesis of hysteria and starts looking for other symptoms that might confirm his earlier findings regarding cases of complete hysteria. Once the diagnosis is established, Charcot can start thinking about a therapy. However, another problem arises, since every therapeutic intervention – as Charcot points out, referring to Claude Bernard – should be based on physiological grounds, namely on the knowledge of the mechanism that

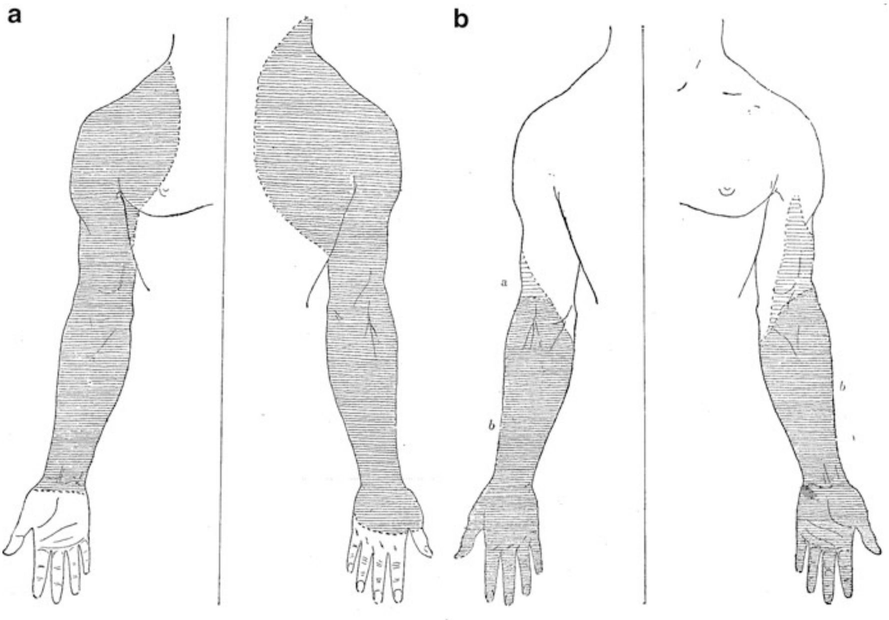


Fig. 7.5 (**a** and **b**) Graphic comparison between the bodily regions involved in a hysterical traumatic paralysis (Porcz.) and in a organic lesion of the peripheral nerves of the shoulder (Deb.). J-.M. Charcot, *Leçons sur les maladies du système nerveux*, Paris: Delahaye 1887, vol. 3

produced these traumatic hysterical paralyses in the first place (Charcot 1887/1889, 288).[8] This mechanism must be understood in order to undo its effects.

But how is it possible, then, to show that these *psychical paralyses* are "as objectively real as those depending on an organic lesion" (Charcot 1887/1889, 289)? Charcot's answer is based on the introduction of hypnosis as an experimental device that should lead us *to see* the objectivity of psychic paralyses, of psychic trauma, and *of the psyche itself*. Hypnosis enables the physician to induce by suggestion in the experimental subject the idea of a paralysis, thereby producing it as a visible phenomenon. Hypnosis experimentally produces the phenomenon that should be passively observed and recorded because it allows us to see what would have been otherwise invisible, namely the subjective process of the etiology of a hysterical paralysis. Charcot thus presents a new character on the scene, a hysterical girl; he hypnotizes her, and induces in her a paralysis identical to Porcz.'s, simply producing a small shock on her shoulder (Fig. 7.6). Since, as proved by another comparison,

[8]It has been often said that Charcot – unlike Freud – only looked for clinical descriptions and neglected etiological analyses, but there are plenty of references to Claude Bernard's model of experimental medicine to be found in Freud's works as well. The difference with Freud is thus less at the level of the opposition between clinic and etiology, than at the level of the one between physiological and psychological causes.

Fig. 7.6 Hypnotism. Desiré Malgloire Bourneville et Paul Regnard (sous la direction de), *Iconographie photographique de la Salpêtrière*, Delahaye, 1879–1880, vol. 3

there are people pathologically predisposed to trauma,[9] people who live in a sort of a state of constant hypnosis, and who do not need to be hypnotized in order to suffer the pathological consequences of a trauma, the demonstration is successfully concluded. So Charcot can finally provide his own definition of trauma:

> This nervous shock is produced by some strong emotion, a fright, a feeling of terror determined by an accident, especially when this accident menaces life ... On these occasions a peculiar mental condition is often developed ... which is very intimately connected, in my judgment, with the hypnotic state. In both of these conditions, in fact, the mental spontaneity, the will, the judgment, is more or less suppressed or obscured, and suggestions become easy (Charcot 1887/1889, 335).

According to Daston and Galison, the second half of the nineteenth century is the time in which a conception of mechanical objectivity emerged in connection with innovative modes of representation expressing new epistemic virtues, such as the

[9]Like all of his fellow physicians and alienists at the fin-de siècle, Charcot firmly believed that nervous and mental illnesses had a hereditary organic basis, and that trauma was just the episode that could trigger it. On this topic the most complete study is Coffin 2003.

ideal of the purity of observation. The techniques underlying mechanical objectivity consist in practices such as training the senses in scientific observation, keeping laboratory notes, monitoring one's hypotheses and opinions, the control over one's beliefs and fantasies, and so on. The typical scientific self pursuing mechanical objectivity is the one who works to eliminate all aesthetic and moral judgment, and all kind of preconception from his observational activities, and who tries to be, as much as possible, like a recording machine, a merely recording eye.

This is exactly the case with Charcot. Georges Canguilhem clearly showed that already in 1865 Claude Bernard thought that medicine became adult, that is to say that it became an experimental science (Canguilhem 1968, 127–141): the physician, the neurologist in our case, should be an active inquirer of nature, he has to actively ask questions to nature. But once he has done that, he has to observe, because experiments are nothing more than "provoked observations". For Claude Bernard,

> The observer must be the photographer of phenomena, his observations must represent nature in exact terms. One has to observe without any preconceived idea; the observer's mind must be passive, that is, it must be silent; he listens to nature and writes what it dictates (Bernard 1865/2008, 64).

This picture of the observer as the champion of mechanical objectivity is exactly the picture to which Charcot wanted to adjust himself, as he declared in a 1888 lecture: "But in truth I am nothing but a photographer; I register what I see" (Charcot 1892, 178). The wide use he made of photography and images of all sorts should not be considered as a mere technical device, but as a part of a complex epistemic and ethical attitude of the scientist.[10] Charcot and his collaborators talked about the camera *in ethical terms*. Albert Londe, the head of the photographic service of the Salpêtrière, wrote in his important book on medical photography that this device has the virtue of being "*sincere*" (Londe 1893, 4). In accordance with Charcot's ideal of seeing patients *and not letting them speak*, Londe recalls several important functions that photographs play: as devices for the training of the medical eye, as a means of mechanical reproduction of the whole of the observable, and as a valuable tool for the writing of the clinical cases (Fig. 7.7).

> During the study of certain nervous affections ... we encounter attitudes and essentially transient states. Here photography is useful, because it allows us to record the image of these too much fast phenomena ... Thanks to our photocronographic methods we will overcome the incapacities of the eye (Londe 1893, 4).

[10]The Salpêtrière had a well equipped photographic service, and between 1876 and 1880 were published, under Charcot's direction, the famous photographic atlases of hysteria, under the title of *Iconographie photographique de la Salpêtrière*; see Didi-Huberman 2003. For example, Charcot once wrote that "photographs are impartial documents, which place under the medical observer's eyes a faithful image of the investigated matter" (Londe 1893, viii).

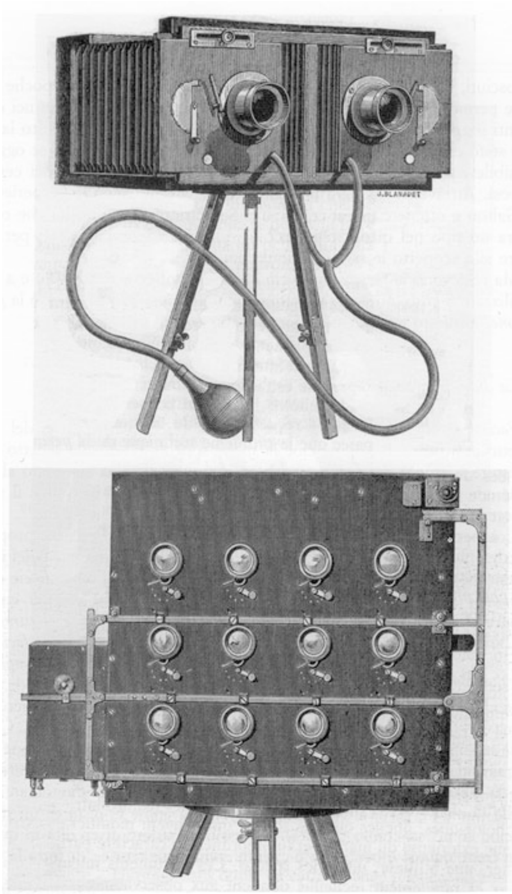


Fig. 7.7 Photographic devices; A. Londe, *La photographie médicale. Applications aux sciences médicales et physiologiques*, Paris: Gautier-Villars, 1893

The patients' bodies and behaviours should speak to the observer, to the expert and seeing physician. Only by eliminating the patients' subjective and ambiguous narratives can their psyche (i.e., their most subjective part) be made objective.[11]

[11]It is also worth noting that against the background given by mechanical objectivity, we can make a reinterpretation of the famous struggle on suggestion and the artificiality of hypnotic phenomena between the schools of Charcot and Hyppolite Bernheim in Nancy. Bernheim denied the status of experimental tool to hypnosis, and believed that there was no such thing as hysteria, given that all of these phenomena were to be reduced to the physician's "suggestions" over the patient. And that's why suggestion was for Bernheim a very effective therapeutic means. Their debate can be seen as the opposition between Bernheim's refusal to acknowledge the possibility of a mechanical objectivity of the psyche, and Charcot's vindication of it. For Bernheim there was simply no material to passively record, since states of mind were produced by the physician's suggestion. Therefore, no mechanical objectivity was possible. See Bernheim 1891; Nicolas 2004; Castel 1998.

7.3 Hearing Hysteria: Freud

The analysis below will focus primarily on the epistemic virtues and observational techniques associated with the emergence of psychoanalysis and will take a lot of things for granted about the Viennese neurologist. Freud takes up Charcot's concepts of dynamic lesion and psychic trauma, but, unlike the French physician, he considers them as fully psychological concepts, and he claims that one has to make use of hearing and listen to what the hysterical patients say about themselves. Moreover, the patients' accounts should be interpreted by the physician, because they are in a relation of symbolic expression with pathological somatic symptoms and can therefore reveal their etiology. The hysteric's somatic symptoms thus became *conversion*s of a psychological trauma.[12] The therapy will therefore have to discover the traumatic memory and free its pathological energy through complex techniques of hearing and talking. Here are some passages Freud wrote in the footnotes that he added to his translation of Charcot's *Tuesday Lectures* at the Salpêtrière:

> The core of a hysterical attack ... is a *memory*, the hallucinatory reliving of a scene which is significant for the onset of the illness. It is this event which manifests itself in a perceptible manner in the phase of '*attitudes passionnelles*' ... The *content of the memory* is as a rule either a psychical *trauma* ... or is an event which, owing to its occurrence at a particular moment, has become a trauma". Psychical trauma is now "an *accretion of excitation* in the nervous system, *which the latter has been unable to dispose of adequately by motor reaction* (Freud 1892–1894/1953–1974, 137).

Freud and Breuer's famous 1895 book entitled *Studies on Hysteria* achieved a complete psychologization of dynamic lesions, functional or traumatic they were, and extended them to the etiology of all the neuroses. Moreover, Freud's critique of the theories of the hereditary character of nervous pathologies left room for the elaboration of a psycho-sexual etiology rooted in the singular personal life of each subject. Finally, by putting together and linking the patients' confessions made in the hypnotic state and the memories of forgotten events in the patients' past, Freud and Breuer set up a new therapeutic technology that coincided in principle with experimentation and the collection of scientific data. According to Freud, it is the memory of the psychic trauma that "behaves like a foreign body", like "an infiltrate" from somewhere else (the unconscious), with respect to the psychological personality of the suffering subject (Breuer and Freud 1895/1953–1974, 255). Therefore, it is this strange object, namely a memory not remembered by its own subject, that is the direct cause of hysteria: "*Hysterics suffer mainly from reminiscences*" (Breuer and Freud 1895/1953–1974, 10). According to Freud, the

[12] I don't mean to claim that Freud's work is a "purely" psychological one, nor that he "discovers" a supposed realm of the psychological. I am referring here only to the psychologization of the concept of trauma (on Freud's biological background and claims see the classic Sulloway 1979).

Studies on Hysteria discovered that symptoms disappeared when the physicians succeeded in "*bringing clearly to light*" the memory of the traumatic event. This could happen only if the patient could *verbally* describe the event with the most accurate details, that is, by expressing through words his pathogenic past (Breuer and Freud 1895/1953–1974, 9). Freud would then start asking his patients to free themselves of their will and "to adopt an attitude of completely objective observation towards the psychical processes taking place in them" (Breuer and Freud 1895/1953–1974, 239). In other words, the patient becomes, all at once, the subject to be cured, the physician-scientist's assistant, and the experimental matter through which it is possible to gather and, at a later stage, organize knowledge.

If we turn to *The Interpretation of Dreams* we can focus on the web of techniques that both the analyst and the patient are required to apply to themselves, and we can better understand Freudian claims to objectivity.[13] After the *Studies on Hysteria*, Freud argues, psychoanalysis continued to develop certain techniques, which serve to psychologically prepare the patient for the analysis: the patient must concentrate her attention on her own inner psychic representations, and eliminate every kind of criticism from the account she gives of her ideas. According to Freud, this is what differentiates self-observation from simple reflection: saying one's own involuntary thoughts without criticism (Freud 1900/1953–1974, 100–02). The therapist has to *hear* this material and interpret it in order to use it both as a therapeutic tool and as experimental material offered by a collaborator. We can say that the virtue underlying mechanical objectivity is fully at work and informs Freud's procedures. Indeed, he asks the patients – who are at the same time his collaborators and his experimental subjects – to practise this epistemic virtue with respect to themselves through what Michel Foucault would have called a technique of power-knowledge.

But who is or has to be the analyst, and what does he have to do in order to become this kind of scientific self? At first glance, Freud claims that the psychoanalysts are the ones who engage in the work of self-observation, namely the ones who turn towards themselves the virtue of mechanical objectivity and neutrally record the material that comes from their interiority.

> The adoption of an attitude of uncritical self-observation is by no means difficult. Most of my patients achieve it after their first instructions. I myself can do so very completely, by the help of writing down my ideas as they occur to me (Freud 1900/1953–1974, 103).

And he goes on, significantly mentioning Claude Bernard:

> Anyone who seeks to do so [...] must [...] endeavour during the work to refrain from any criticism, any *parti pris*, and any emotional or intellectual bias. He must bear in mind Claude Bernard's advice to experimenters in a physiological laboratory: "*travailler comme une bête*" – he must work, that is, with as much persistence as an animal, and as much disregard of the result" (Freud 1900/1953–1974, 535).

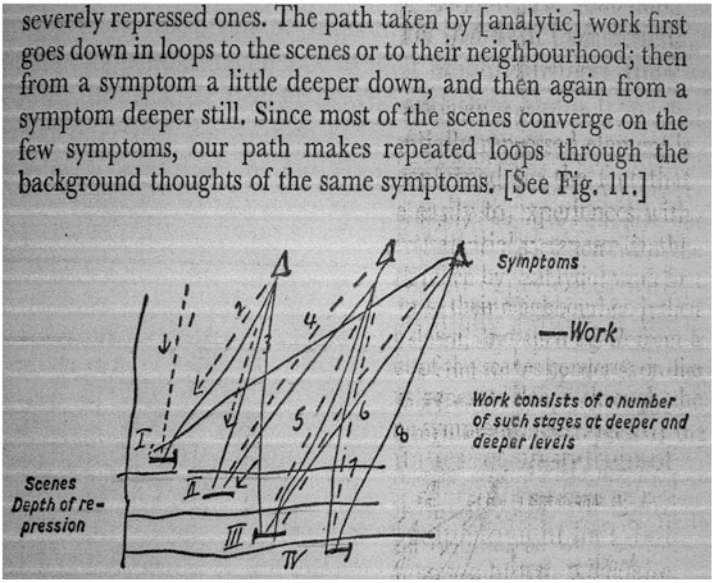
[13] I have started to explore this topic in another context (see Savoia 2010).

There are two obstacles to this apparently not difficult task of "uncritical self-observation":

1. Since the observers speak of themselves, the content of their speech is absolutely singular, subjective, hard to generalize; moreover, the part of themselves the scientists have to use for the sake of knowledge is a new emerging scientific object, *the unconscious*, which is, among other things, impossible to visually record.
2. Freud will soon recognize that self-observation is not simple at all and that the presence of someone who interprets and leads the process is necessary.

Already in the preface of the first edition of the dream-book, Freud mentioned a sort of epistemic embarrassment due to the fact that he was presenting such a personal and subjective material to the scientific community, more "than is necessary for any writer who is a man of science, and not a poet" (Freud 1900/1953–1974, xxiv). A scientist, one might expect, doesn't speak about himself, his life and subjectivity, but rather tries to suppress them in the search for objectivity. One of the major risks was that of compromising the reliability of one's own scientific enterprise. *As we can see, this is precisely Charcot's problem: how is it possible to make objective something that is subjective, something that is not immediately recordable with the eyes?* Daston and Galison described one of the differences between the epistemic virtues characterizing mechanical objectivity and *structural objectivity* on the grounds of the different aspects the scientific self has to fight against. In the latter case, what one has to fight against is a solipsistic self, a self-centered subjectivity incapable of communicating its own observations. We can say that one of Freud's major preoccupations was to express in an understandable and universal language the absolutely idiosyncratic content of the self, both his own self and his patient's self. The answer to this problem was to describe the *structure* of the psychical apparatus, which led to the so-called first topic, namely the description of the mind as divided in three parts or regions: the conscious, the pre-conscious, and the unconscious. Given the fact that the dynamics of psychological behaviour couldn't be represented by plastic images that recorded specific gestures, it had to be the result of the interpretation of the patient's stories. And these stories had to be made intelligible by hypothesizing a system of interrelated parts, a structure. Structural objectivity has nothing to do with the sight, the gaze, and images (Daston and Galison 2007, 256–57) (Figs. 7.8 and 7.9).

Freud overcame the second above-mentioned obstacle by claiming that the only condition to be an analyst is to be analysed, but this analysis can't be a self-analysis and will always have to be conducted by another, already trained, analyst (Freud 1910a/1953–1974, 144–45; 1910b/1953–1974, 226–27; 1912/1953–1974, 115–17). Of course, this argument leads to the paradox that the only one who has achieved and could ever achieve an auto-analysis is Freud himself. However, my main concern here is the concept of *interpretation* and its relations to structural objectivity. Interpretation, as the art of uncovering the deep and hidden meanings of the patient's ideas, is the bridge that connects, and makes coincide, therapy with analysis. Freud's art of interpretation has to be modulated on the individuality of


severely repressed ones. The path taken by [analytic] work first goes down in loops to the scenes or to their neighbourhood; then from a symptom a little deeper down, and then again from a symptom deeper still. Since most of the scenes converge on the few symptoms, our path makes repeated loops through the background thoughts of the same symptoms. [See Fig. 11.]

Figs. 7.8 and 7.9 Mechanical objectivity and structural objectivity of hysteria. Figure 8 is the mechanical reproduction of the phase of "passional attitudes" of a hysterical attack suffered by the famous Augustine, treated by Charcot. Figure 9 is the hand-written scheme of the deep structure of the psyche when a hysterical symptom occurs, drawn by Freud in 1897. Desiré Malgloire Bourneville et Paul Regnard (sous la direction de), *Iconographie photographique de la Salpêtrière*, Delahaye, 1878, vol. 2 and S. Freud, letter to Fliess, May 25th, 1897, in *The Standard Edition of the Complete Psychological Works of Sigmund Freud*, ed. J. Strachey, London: Hogarth Press 1953–1974, vol. 1

the patient, and has to deal with his unconscious; therefore, it requires particular intuitive abilities. The analyst has to put into play his own unconscious, and the techniques he must perform will also aim at acquiring a certain capacity to master and use his own most subjective part during the interpretation process.[14]

In 1910, Freud takes up again the comparison between the poet and the scientist: the creative writer, he claims, is always better at describing love life because he draws on a special quality, namely "the courage to let his own unconscious speak" (Freud 1910c/1953–1974, 11, 165). However, his purpose is to produce emotional effects, and aesthetic and intellectual pleasure. Therefore, he is not able to faithfully represent reality since the scientist is needed for this latter task, as he is the one who renounces pleasure and uses a different language in describing reality. But the courage to let one's own unconscious (and that of other people) speak seems to be a quality shared by both of them, while only modalities and goals are different. That is to say, the distinction between the poet and the scientist is not longer so rigidly articulated around the opposition between subjectivity (that the poet has to express regardless of objectivity) and objectivity (that the scientist has to obtain by suppressing her own subjectivity) as it was in the second half of the nineteenth century – a distinction that is beautifully expressed by Bernard's aphorism: "l'art c'est *moi*; la science, c'est *nous*" ['Art is *me*, science is *us*'] (Bernard 1865/2008, 96).[15]

Generally speaking, we can notice that in psychoanalysis: (1) *hearing* is privileged over sight; (2) an exchange is required between two selves, in which the physician uses his own subjectivity in order to acquire knowledge; (3) the aim is interpretation, which in turn is correlated with the description of structures and not to a passive recording of data.

The characterization of what has been called *trained judgment* (Daston and Galison 2007, 308–61) seems appropriate to describe Freud's work. However, there are also two important differences. First, we are dealing here with interpretations of discourses and not of images. Second, in Freud's early writings we can see the simultaneous emergence of, and a connection between, at least one epistemic virtue – structural objectivity – and one new clinical and therapeutic practice, based on a *trained* and subjective interpretation of people's discourses.

[14]If the physician wishes to interpret – as Freud writes in a 1912 technical paper – he "must turn his own unconscious like a receptive organ towards the transmitting unconscious of the patient" (Freud 1912/1953–1974, 12, 117).

[15]In the 1980s the psychoanalyst Heinz Kohut will express this idea of a new kind of objectivity in aphoristic fashion: analytic-depth psychology posited a "new kind of objectivity, namely a scientific objectivity which includes the subjective" (Kohut 1982, 399), quoted in Lunbeck 2011, 267.

7.4 Conclusion

We can say that the epistemic virtue entailed by mechanical objectivity was one of the conditions of possibility for Charcot's visual objectivity of the psyche, which was grounded in a solid set of epistemological practices and concepts. On the other hand, the sort of epistemological crisis Freud experienced forced him to build a science of the self without appealing to any form of mechanical objectivity. In this way, he had to take into account the specificity of the verbal relation between two human subjects, which is one of the main characteristics of psychoanalysis, and focus on structure and interpretation. A shift in the concept of the self would come as a result. If we follow the historian Jan Goldstein's terminology, we can say that while the self of late the nineteenth-century neurology was marked by a *horizontal fragmentation* (between the body, the brain and the mind), being a self whose roots lie in biology, in the Freudian meta-concept of subjectivity the self is characterized by *vertical fragmentation* (between consciousness and the unconscious), and becomes a much more "psychological" self (Goldstein 2005, 3–6). Peter Galison in an essay on Rorschach also indicated that there is a correlation between, on the one hand, mechanical objectivity and "aggregated self" and, on the other hand, judgmental objectivity and an "apperceptive self", suggesting the same kind of dynamic coupling between the object to describe and the subject who describes it (Galison 2004, 292).

These two epistemic models could also help us to historically understand the correlations between the well known late twentieth-century retreat of psychoanalysis-related theories, and the new avalanche of brain images and pyschopharmacology. It is now almost common sense to say that twentieth-century psychiatry moved from a state of "brainlessness" to one of "mindlessness", meaning that with the introduction of psychoactive drugs in the 1950s psychiatry progressively abandoned Freudian and para-Freudian assumptions on unconscious psychological conflicts that affected the mind, and began to extensively explore the material aspects of the physical brains supported by the impressive development of functional brain imaging technologies.[16]

I would like to finally argue that this historico-epistemological approach to the sickness of the mind and the self can be potentially fruitful even when applied to more contemporary issues. Let's take for example a relatively recent brief research paper published by a Oxford neuropsychology unit on "the functional anatomy of hysterical paralysis" (Marshall et al. 1997, B1). The authors deal with a case of "conversion disorder" (recorded by the DSM-IV) and soon point out that this kind of disorder has been and still is quite controversial. "Many physicians – they remark – still regard such disorders either as feigned or as a failure to find the responsible organic cause for the patient's symptoms". They present a woman with left-sided paralysis in whom no organic disease or structural lesion could be found.

[16]This kind of language has been used by Eisenberg 1986.

"By contrast, psychological trauma was associated with the onset and recurrent exacerbation of her hemiparalysis". So far, the description of the case could have been presented virtually in exactly the same terms both by Charcot and the early Freud. But at this point this article starts to diverge from both of them, and especially from Freud's account.

> We recorded brain activity when the patient prepared to move and tried to move the paralysed (left) leg and when she prepared to move and did move her good (right) leg. Preparing to move or moving her good leg, and also preparing to move her paralysed leg, activated motor and/or premotor areas previously described with movement preparation and execution. The attempt to move the paralysed leg failed to activate right primary motor cortex. Instead, the right orbito-frontal and right anterior cingulate cortex were significantly activated. We suggest that these two areas inhibit prefrontal (willed) effects on the right primary motor cortex when the patient tries to move her left leg (Marshall et al. 1997, B1).

Roughly speaking, this description could look like one by given Charcot with the addition of modern techniques of brain imaging. For sure, it would surely be misleading to say that we are back to mechanical objectivity, both because history never repeats itself and because brain images are all but "passive" recordings of mere data, but on the contrary these techniques and images blur the boundaries between the process of production of the image and the representative content of the image: what we see is not a representation, a product, but a visual elaboration of the actual process of representation[17] (Fig. 7.10). However, it doesn't seem misleading nor wrong to say that the new technologies of functional brain imaging gave an answer (one of the many possible answers) to an epistemological problem formulated by Charcot and others in the late nineteenth century. Current technologies enabling scientists to visualize the brain seem to represent a solution to a problem that arose in the nineteenth century, and create new forms of practical and ideal objectivities of the self that have just started to be explored.[18] In doing so, these technologies seem to bypass the "psychological trauma" mentioned even by the authors of the paper quoted above: physicians and researchers do not have to listen to patients and experimental subjects, but instead they have to "see" – no matter what seeing a functional brain image exactly means – their brains, and we can say, their *selves*.

We will need to examine the correlated changes of the techniques of inscription and stabilization of the very object of these sciences, namely the self. To study the relationships between the self as a scientific object, and the self as the target of objective knowledge, must be one of the major tasks of an historical ontology of the self.

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[17]For an accurate epistemological, technical, and cultural analysis of functional brain images see Dumit 2004.

[18]See for example Rose and Abi-Rached 2013.

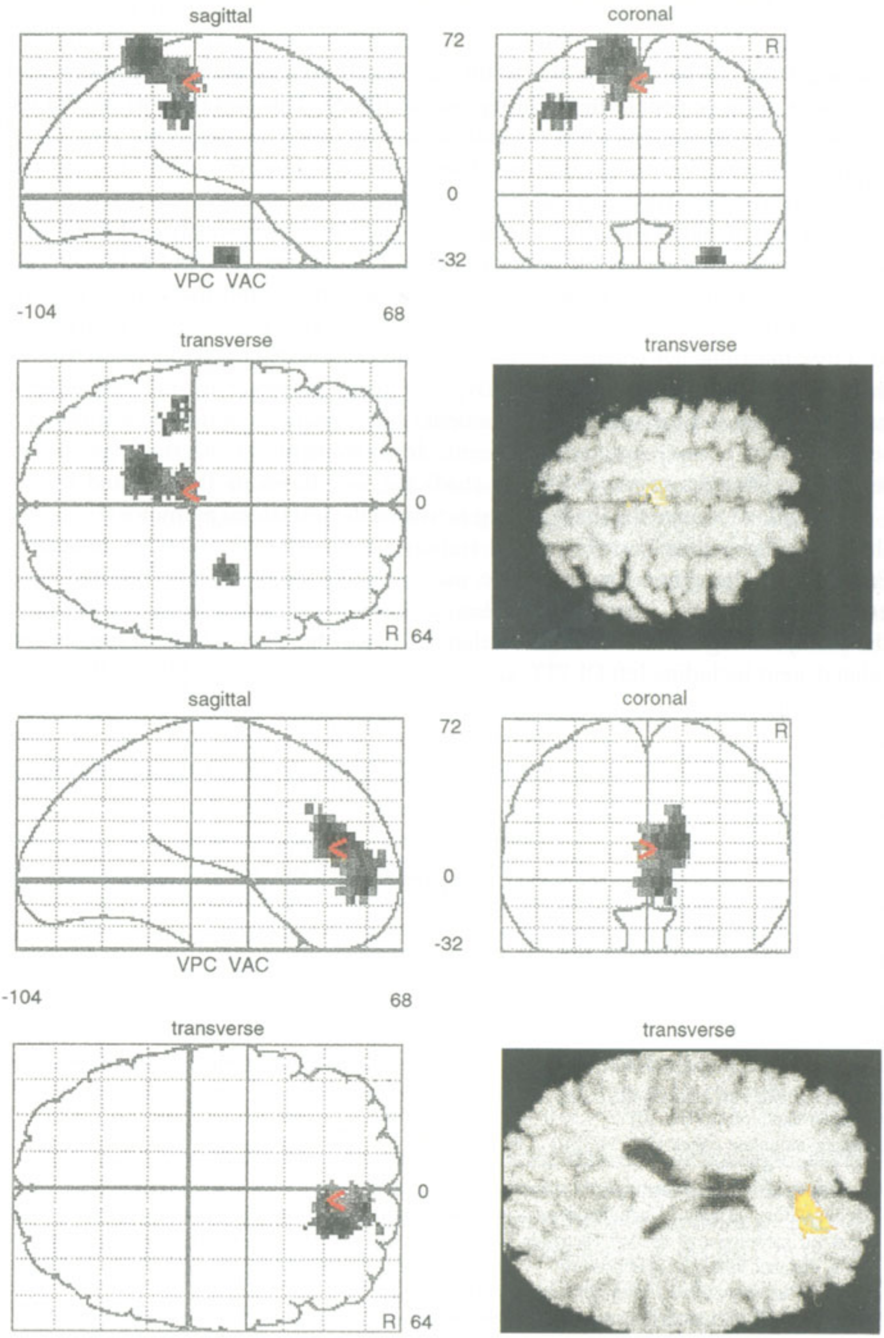

Fig. 7.10 Functional brain image of a hysterical paralysis. John C. Marshall, Peter W. Halligan, Gereon R. Fink, Derick T. Wade and Richard S.J. Frackowiak, "The Functional Anatomy of a Hysterical Paralysis", *Cognition*, 64 (1997)

References

Bernard, Claude. 1865/2008. *Introduction à l'étude de la médecine expérimentale*. Paris: Flammarion.

Bernheim, Hyppolite. 1891. *Hypnotisme, suggestion, psychothérapie*. Paris: Doin.

Bonduelle, Michel, Toby Gelfand, and Cristopher G. Goetz. 1996. *Charcot: un grand médecin dans son siècle*. Paris: Michalon.

Braunstein, Jean-François. 2002. Bachelard, Canguilhem, Foucault. "Le style française en épistemologie". In *Les philosophes et la science*, ed. Pierre Wagner, 920–963. Paris: Gallimard.

Breuer, Joseph, and Sigmund Freud. 1895/1953–1974. Studies on hysteria. In Freud 1953–1974, vol. 2.

Canguilhem, Georges. 1968. *Études d'histoire et de philosophie des sciences*. Paris: Vrin.

Caplan, Eric. 2001. Trains and trauma in the American gilded age. In Micale et al. 2001, 57–79.

Carl, Wolfgang, and Lorraine Daston (eds.). 1999. *Wahrheit und Geschichte*. Göttingen: Vandenhoeck & Ruprecht.

Cartwright, Lisa. 1995. *Screening the body: Tracing medicine's visual culture*. Minneapolis: University of Minnesota Press.

Castel, Pierre-Henri. 1998. *La querelle de l'hystérie: La formation du discours psychopathologique en France (1881–1913)*. Paris: Presses Universitaires de France.

Charcot, Jean-Martin. 1882. Sur les divers états nerveux déterminés par l'hypnotisation chez les hystériques. *Comptes-rendus Hebdomadaires des Séances de l'Académie des Sciences (Section de Médecine et de Chirurgie)* 94: 403–405.

Charcot, Jean-Martin. 1887/1889. *Lectures on the Diseases of the Nervous System*, vol. 3. Trans. Thomas Savill. London: The New Sydenham Society.

Charcot, Jean-Martin. 1892. *Leçons du mardi à la Salpêtrière*, vol. 1. Paris: Bureaux du Progrès Médical, E. Lecrosnier & Babe.

Coffin, Jean-Cristophe. 2003. *La transmission de la folie 1850–1914*. Paris: L'Harmattan.

Daston, Lorraine. 1998. Une histoire de l'objectivité scientifique. In Guesnerie et al. 1998, 115–126.

Daston, Lorraine. 2000a. The coming into being of scientific objects. In Daston ed. 2000, 1–14.

Daston, Lorraine (ed.). 2000b. *Biographies of scientific objects*. Chicago: University of Chicago Press.

Daston, Lorraine (ed.). 2004. *Things that talk: Object lessons from art and science*. New York: Zone Books.

Daston, Lorraine, and Peter Galison. 2007. *Objectivity*. New York: Zone Books.

Daston, Lorraine, and Elizabeth Lunbeck. 2011a. Introduction: Observation observed. In Daston et al. 2011, 1–9.

Daston, Lorraine, and Elizabeth Lunbeck (eds.). 2011b. *Histories of scientific observation*. Chicago: University of Chicago Press.

Davidson, Arnold I. 2001. *The emergence of sexuality: Historical epistemology and the formation of concepts*. Cambridge: Harvard University Press.

de Marneffe, Rebecca. 1991. Looking and listening: The construction of clinical knowledge in Charcot and Freud. *Signs* 17: 71–111.

Didi-Huberman, Georges. 2003. *Invention of Hysteria: Charcot and the Photographic Iconography of the Salpêtrière*. Trans. Alisa Hartz. Cambridge: MIT Press.

Dumit, Joseph. 2004. *Picturing personhood: Brain scans and biomedical identity*. Princeton: Princeton University Press.

Edelman, Nicole. 2003. *Les metamorphoses de l'hystérique*. Paris: La Découverte.

Eisenberg, Leon. 1986. Mindlessness and brainlessness in psychiatry. *The British Journal of Psychiatry* 148: 497–508.

Freud, Sigmund. 1892–1894/1953–1974. Preface and footnotes to the translation of Charcot's *Tuesday Lectures*. In Freud 1953–1974, vol. 1, 131–143.

Freud, Sigmund. 1900/1953–1974. The interpretation of dreams. In Freud 1953–1974, vols. 4–5.
Freud, Sigmund. 1910a/1953–1974. The future prospects of psycho-analytic therapy. In Freud 1953–1974, vol. 11, 139–152.
Freud, Sigmund. 1910b/1953–1974. 'Wild' psycho-analysis. In Freud 1953–1974, vol. 11, 219–228.
Freud, Sigmund. 1910c/1953–1974. A special type of choice of object made by men (contributions to the psychology of love, I). In Freud 1953–1974, vol. 11, 163–176.
Freud, Sigmund. 1912/1953–1974. Recommendations to the physicians practicing psycho-analysis. In Freud 1953–1974, vol. 12, 109–120.
Freud, Sigmund. 1953–1974. *The standard edition of the complete psychological works of Sigmund Freud*, ed. James Strachey. London: Hogarth Press.
Galison, Peter. 2004. Image of self. In Daston ed. 2004, 257–296.
Gauld, Alan. 1992. *A history of hypnotism*. Cambridge: Cambridge University Press.
Gilman, Sander L. 1982. *Seeing the insane*. New York: Wiley.
Gilman, Sander L. 1993. The image of the hysteric. In Gilman et al. 1993, 345–452.
Gilman, Sander L., Helen King, Roy Porter, G.S. Rousseau, and Elaine Showalter. 1993. *Hysteria beyond Freud*. Berkeley: University of California Press.
Goldstein, Jan. 2005. *The post-revolutionary self: Politics and psyche in France 1750–1850*. Cambridge: Harvard University Press.
Goldstein, Jan. 2009. *Hysteria complicated by ecstasy: The case of Nanette Leroux*. Princeton: Princeton University Press.
Guesnerie, Robert, and François Hartog (eds.). 1998. *Des sciences et des techniques: un débat*. Paris: Éditions de l'EHESS.
Hacking, Ian. 1995. *Rewriting the soul: Multiple personality and the sciences of memory*. Princeton: Princeton University Press.
Hacking, Ian. 1999. Historical meta-epistemology. In Carl et al. 1999, 53–77.
Harrington, Ralph. 2001. The railway accident: Trains, trauma, and technological crises in nineteenth-century Britain. In Micale et al. 2001, 31–56.
King, Helen. 1998. *Hyppocrates' woman: Reading the female body in ancient Greece*. London: Routledge.
Kohut, Heinz. 1982. Introspection, empathy, and the semi-circle of mental health. *International Journal of Psychoanalysis* 63: 395–407.
Lecourt, Dominique. 2001. *L'épistemologie historique de Gaston Bachelard*. Paris: Vrin.
Leys, Ruth. 2000. *Trauma: A genealogy*. Chicago: University of Chicago Press.
Londe, Albert. 1893. *La photographie médicale: Application aux sciences médicales et physiologiques*. Paris: Gauthier-Villars.
Lunbeck, Elizabeth. 2011. Empathy as a psychoanalytic mode of observation: Between sentiment and science. In Daston et al. 2011, 255–75.
Marshall, John C., Peter W. Halligan, Gereon R. Fink, Derick T. Wade, and Richard S.J. Frackowiak. 1997. The functional anatomy of a hysterical paralysis. *Cognition* 64: B1–B8.
Micale, Mark S. 1995. *Approaching hysteria: Disease and its interpretations*. Princeton: Princeton University Press.
Micale, Mark S. 2001. Jean-Martin Charcot and *les névroses traumatiques*: From medicine to culture in French trauma theory of the late nineteenth century. In Micale et al. 2001, 115–139.
Micale, Mark S., and Paul Lerner. 2001a. Trauma, psychiatry, and history: A conceptual and historiographical introduction. In Micale et al. 2001, 1–29.
Micale, Mark S., and Paul Lerner (eds.). 2001b. *Traumatic pasts. History, psychiatry, and trauma in the modern age, 1870–1930*. Cambridge: Cambridge University Press.
Nicolas, Serge. 2004. *L'hypnose: Charcot face à Bernheim*. Paris: L'Harmattan.
Rheinberger, Hans-Jorg. 2010. *On Historicizing Epistemology: An Essay.* Trans. David Fernbach. Stanford: Stanford University Press.
Rose, Nikolas, and Joelle Abi-Rached. 2013. *Neuro: The new brain sciences and the management of the mind*. Princeton: Princeton University Press.

Savoia, Paolo. 2010. Sexual science and self-narrative: Epistemology and narrative technologies of the self between Krafft-Ebing and Freud. *History of the Human Sciences* 23(5): 17–41.
Showalter, Elaine. 1993. Hysteria, feminism, and gender. In Gilman et al. 1993, 286–344.
Singy, Patrick. 2006. Huber's eyes: The art of scientific observation before the emergence of positivism. *Representations* 95: 54–75.
Sturm, Thomas, and Uljana Feest, eds. 2009. *What (good) is historical epistemology?* Berlin: Max Planck Institute for the History of Science, Preprint 386.

Chapter 8
Objectivities in Print

Alex Csiszar

Since the late nineteenth century, observers of science have recognized a close link between several of the practices associated with scientific objectivity and the apparatus of specialized scientific publishing. Customs and values concerning peer review, the adjudication of credit for knowledge claims, the accessibility of knowledge claims to public scrutiny, and the establishment of credentials in science are often said to have their locus in the publishing practices associated with periodicals. Indeed, commitments to the epistemic virtues that have been associated with print – its immutability, mobility, and its exemplary publicness – have even served as justification for granting agencies and tenure committees to use scientific papers as units of measurement in identifying and assessing scientific achievement.

So compelling has seemed the link between certain scientific genres and the objective character of modern science that some observers have suggested that it is of very long standing, and that the development of periodical publishing in the sciences was a precondition for the emergence of the normative structure of science itself. I will argue here that these views are both historically mistaken and philosophically misleading. The urge to associate conceptions of objectivity with periodical publishing in the sciences is a remarkably recent development, having arisen slowly over the course of the nineteenth century. Moreover, a survey of formative episodes during which this association began to command wide assent suggests we ought to be careful about ascribing any essential character to it. Practices and beliefs regarding periodical publishing in the sciences have varied to a far greater extent than is often recognized; when they arose, the epistemic virtues now associated with print were as much a rhetorical as they were a technological accomplishment. Finally, appeals to norms such as objectivity cannot always be understood in terms purely of epistemic virtue and vice; in the cases I will focus on

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below, such appeals have arisen in contexts where scientific practitioners have felt called upon to articulate the relationship the sciences ought to have with the wider social or political constituencies within which they are embedded.

I will focus on two formative moments during which the bond between modern normative commitments about science and scientific publishing were in the process of formation. The first concerns the birth of systems of refereeing in England, where I will emphasize the disparity between earlier and later views about what such practices are supposed to be for. The second concerns the late nineteenth-century consolidation of the periodical literature as the seat of collective scientific opinion at the same time that objectivity in science came more commonly to be viewed as inhering in the rational coordination of such collective opinions. This section focuses on the intersection of these concerns in the editorial activities and epistemological reflections of the French mathematician Henri Poincaré.

I begin the essay by outlining – in both the historical and philosophical literature – the epistemic virtues most at stake for the actors in these accounts, with a focus on virtues that are understood to be attributes of collectives rather than of individuals. Historians' recent focus on changing conceptions of the knowing self that might be revealed through the history of objectivity has put the focus on objectivity as an attribute of individuals. Conceptions of objectivity that are attributes of *groups* of knowers have been just as prominent since the later nineteenth century, but have received far less attention from historians.

While we ought to be careful not to read too much into origin stories, at least one feature of these ones continues to be pertinent to – and ought to be a part of – current debates about the efficacy of the apparatus of journal publishing. This is that they developed not simply in response to expert communities' perceived desire to achieve some historically-stable ideal of objective judgment, but rather in the context of concerns about how those communities might flourish as part of the broader political cultures in which they were participants.

8.1 Objectivity as Group Trait

Belief in an intimate link between scientific publishing and shared norms in science has led many to imagine that the modern sciences have always depended on periodical publishing in broadly similar ways. Thus, not only did the physicist and Mertonian observer of science John Ziman argue that an "article in a reputable journal does not merely represent the opinions of its author; it bears the imprimatur of scientific authenticity" (Ziman 1968, 111) but he went further, suggesting that the "invention of a mechanism for the systematic publication of fragments of scientific work may well have been the key event in the history of modern science" (Ziman 1969, 318). The philosopher of science David Hull said of the procedures inaugurated by Henry Oldenburg in 1665 when he founded the *Philosophical Transactions* that "this is the method that has come down to us for promoting individual ownership while allowing communal use" (Hull 1988, 323). In the same context, Robert Merton and Harriet Zuckerman – in a study of the sociology and history of referee systems – stated that "[p]rinting … provided a technological basis for the emergence of that component of the ethos of science which has been described as 'communism'" (Zuckerman and Merton 1971/1979, 115).

Recent work, however, which lies at the intersection of the history of science and book history, has shown just how precarious and divergent from our own were early modern uses of print in natural philosophy. Adrian Johns has argued in particular that during the early modern period much remained deeply uncertain about the status of print as a device for reliably transmitting knowledge claims, fixing them in a durable medium, and managing property rights. Furthermore, early modern periodicals associated with natural philosophy such as the *Philosophical Transactions* in Britain and the *Journal des sçavans* in France bore little resemblance in their procedures and functions to the modern scientific journal, which only came into being over the course of the nineteenth century (Johns 1998, 2000; Vittu 2001).

Not only has the publishing apparatus of science undergone substantial change over time, norms and epistemic virtues in the sciences have themselves changed in significant ways. Lorraine Daston and Peter Galison have shown that the various senses in which objectivity may be used to characterize scientific knowledge – and as an attribute of individuals who are engaged in producing it – only developed during the nineteenth century. Most centrally, the ideal of mechanical objectivity, which came to prominence in the mid-nineteenth century, sought to put restraints on an individual's interpretive will and make the scientific observer approximate a registration device (Daston and Galison 1992, 2007; Canales 2009). But this was only one, albeit prominent, version of the new objectivities of nineteenth-century science. The varieties of objectivity most similar to those at play in this essay have sometimes been grouped under the label of *communitarian* objectivity. Daston and Galison have focused on two specific kinds of late nineteenth-century concerns that might be brought under this rubric.[1] First, they have documented strictures against scientific claims that depend on private sensation or incommunicable experience. They label such strictures on the scientific self "structural objectivity," (Daston and Galison 2007) and they find them exemplified in the theories of science put forward by Henri Poincaré, Gottlob Frege, and Rudolf Carnap. Second, they have investigated the problem of research questions that can only be answered through large-scale direct cooperation of geographically-dispersed individuals (Galison and Daston 2008). Such projects – exemplified by international mapping ventures such as the late-century Carte du Ciel – normally required rigorous protocols and training to produce standardized observers, to the extent that precision and even accuracy were sometimes sacrificed in the pursuit of uniformity. Such projects require coordination across spaces, cultures, languages, and variously-trained observers.

[1] Daston developed the language of "communitarian objectivity" over the course of several essays up to the early 2000s (Daston 1999a, b, 2001). Her most recent publications on the subject co-written with Galison have however dropped the phrase in favour of the two more specific injunctions (that I discuss below), "structural objectivity" and "scientific coordination."

Daston and Galison's two varieties of communitarian objectivity are well chosen precisely because they represent limit cases in a wider field of concerns over the objectivity of shared knowledge. But they by no means exhaust the contexts in which commitments to scientific objectivity might be associated with knowledge-making communities. When C.S. Peirce characterized the real as the "definite opinion to which the mind of man is, on the whole and in the long run, tending" (Peirce 1871, 455) he had in mind not simply the U.S. Coast and Geodetic Survey on which he worked, nor the mathematical relations whose logic he studied, but processes of *making communal* through which all claims necessarily passed on their way to becoming knowledge. According to Peirce, all knowledge was necessarily collective belief.

The subjects of Daston and Galison's studies were determined in part by their interest in the parallel history of scientific subjectivities, of techniques of the self. In the specific cases of communitarian objectivity that they studied, their actors were interested in limiting individual idiosyncrasies. Consequently, they have emphasized conceptions of objectivity that were attributes of individuals rather than of collectives. This puts the focus on the plain fact of being a part of a collective rather than on what sorts of collectives – and what sorts of collective practices – might appropriately be labelled objective ones. This latter concern, however, has continued to be a major presence in scientists' and philosophers' accounts of objectivity since the turn of the twentieth century. Again, when Peirce spoke of science itself, he often had this irreducibly social sense in mind: "What I mean by a 'science' … is the life devoted to the pursuit of truth according to the best known methods on the part of a group of men who understand one another's ideas and works as no outsider can" (Peirce 1905).[2]

Scientists and observers concerned about the objectivity of knowledge in this stronger communitarian sense have thus been concerned not simply about whether a scientific claim is *in principle* communicable, but also about whether and how it – not to mention supporting or contrary evidence – has in fact been communicated or made a communal possession. Rather than focusing on techniques for standardizing observations and laboratory records, they have thus been concerned about the practices through which such circulation eventuates in the acceptance or rejection of the claim by groups of researchers, and even about simply what it might *mean* for a claim to achieve the status of knowledge with respect to such groups.

This more sweeping commitment to objectivity as characterizing the communal nature of scientific knowing has been particularly influential since the early twentieth century. The displacement of the seat of objectivity from individual moral character to collective norms lay at the heart of Robert K. Merton's insights that it is "a distinctive pattern of institutional control of a wide range of motives" that is responsible for scientific behaviour (Merton 1942/1968, 613). Karl Popper

[2]In one interpretation of Peirce's unorthodox Kantianism, the "transcendental unity of apperception" – a precondition for knowledge of objective reality – has essentially been recast as a *social* unity, as the possibility of community consensus (Apel 1998, chap. 3).

(1945/1963, 217), too, argued that "what we call 'scientific objectivity' is not a product of the individual scientist's impartiality, but a product of the social or public character of scientific method." He continued, "science and scientific objectivity do not (and cannot) result from the attempts of an individual scientist to be 'objective', but from the friendly-hostile co-operation of many scientists." Virtually identical observations have been made by more recent observer-practitioners such as David Hull (1988, 3–4), Ziman (1995, 34), and even some historians of science (Eamon 1985). The view of objectivity as an emergent property of collective action has been developed further by more recent philosophers. Philip Kitcher has argued, using a formal decision-theoretic model, that selfish behaviour among individuals can lead, within a properly-regulated scientific community, to productive outcomes and objective behaviour for the community as a whole: "particular kinds of social arrangements make good epistemic use of the grubbiest motives" (1993, chap. 8). Helen Longino (1990, chap. 4) has laid out with great clarity the distinctions between objectivity as an attribute of individual practice (or method) and as a product of social (critical) interaction, arguing strongly that only accounts based on the latter have any hope of plausibility. Finally, Miriam Solomon (2001) has taken a more naturalistic approach, and while her account does not particularly emphasize objectivity, she insists that any account of such epistemic virtues ought to be sought in the consequences of social rather than individual actions.

There is thus a significant disconnect between contemporary philosophical approaches to scientific objectivity and the historical perspectives initiated by Daston and Galison that have focused largely on individual practices. This disconnect is not a result of the novelty of the more recent philosophical accounts. That knowledge production is an irreducibly communal activity first became a commonplace in the later nineteenth century (around the same time that holistic conceptions of sociology were formulated). As Steven Shapin (2008, 21) has recently put it, "late modernity's most powerful knowers came to be portrayed as ordinary people." As this happened, the distinguishing feature of the scientific enterprise came commonly to be seen in its modes of collective action rather than personal virtue. The British physiologist Michael Foster – echoing T.H. Huxley's dictum that science was simply "trained and organised common sense" – argued that "men of science have no peculiar virtues, no special powers ... Though in themselves they are no stronger, no better than other men, they possess a strength which ... is not their own but is that of the science whose servants they are." The man of science, he argued, was "a joint in a great machine, and he can only work aright when he is in due touch with his fellow-workers ..." (1900, 19–20). Similarly, French commentators such as the politician and chemist Marcellin Berthelot – part of the same Third Republic political culture that influenced Emile Durkheim and his followers – emphasized *solidarisme* as constituting the preeminent feature of scientific inquiry (1897/1901, 7–9). The mathematician and activist Charles-Ange Laisant (1904, 348) suggested that when "the idea of solidarity combined with modern media of communication," then scientific relations among men became a model for political relations in general.

But there is more than one way to imagine scientific communities, and more than one way to conceive of their objectivity. As current debates over social epistemology make clear, objectivity as an attribute of collectives can refer to more than one set of traits or injunctions. And while actors and observers have disagreed about what allows scientific collectives to be sources of objective knowledge, virtually no one argues that objectivity simply reduces to intersubjectivity. Not just any kind of group will do.

In the rest of this essay I will focus on one of the ways in which the processes by which knowledge as a collective good is produced have been implicated in the problem of objectivity: the customs and procedures associated with scientific publishing. In the second half of the twentieth century, concern about this has been dominated especially by peer review. But this is only one way in which publishing norms and the objectivity of knowledge have been seen to be connected. By the late nineteenth century, before formal referee procedures were widespread, periodical publishing had become a deep concern among scientists concerned with the legitimacy and objectivity of the scientific enterprise. Over the course of that century, scientific journals had supplanted other platforms (face-to-face meetings, correspondence, and other print formats) to become the primary seat of public knowledge claims. The problem of how to optimize and rationalize the system of scientific publishing became a focus of endless debate. The means by which researchers dispersed over disciplines and distances might remain in touch with the scientific literature was of especial concern. Legitimate knowledge could only become a collective good – indeed could only become knowledge as such – if it circulated in reliable and predictable ways.

8.2 Print and Objective Judgment

During the first half of the nineteenth century, little was obvious about what role journals might play in the vetting of knowledge claims on behalf of expert communities. Scientific journals themselves were a new kind of object, and just what they were for remained up in the air. Journals dedicated to spreading news of scientific discoveries had begun to spread at the end of the eighteenth century, but they remained one of several mediums by which a knowledge claim might become public. In 1834, the German chemist, pioneer of the modern teaching laboratory, and prolific editor Justus von Liebig contrasted books with journals, suggesting that the latter better captured the dynamics of communal knowledge production: "in the former everything is determined by the opinion of a single individual and his judgment is without appeal, but journals allow for defence and justification; and since here there must be a balancing out of opinions, we approach nearer to communal aims" ([Liebig] 1834, 316; Volhard 1909, 324–59).

This comparison seems imbued with a thoroughly Mertonian spirit of communism and organized skepticism, a spirit that Liebig connects directly to the periodical press. But closer scrutiny of what Liebig had in mind suggests how different his assumptions were from later editors of such journals. He did emphasize the responsibility of editors to act as "sentinels for the purpose of signalling that which is good and that which is in error" (315). But they discharged this duty not by limiting the pages of the journal to those contributions that had received the approbation of the wider community; rather, in Liebig's model, they did so by publishing authoritative critiques backed by their own personal credibility. In the above phrase, Liebig was in fact defending his habit of publishing what he described as "ruthless, harsh reviews" of scientific work. These were motivated not – he assured his readers – by "a love of conflict or a desire to belittle others," but from the duty which the "trust that people have placed in him demands to be fulfilled" (315).

The establishment of the practices we associate with peer review as a general expectation in everyday scientific life did not occur until much later. In fact, despite the comparatively massive scale of German science by the late nineteenth century, such expectations were particularly slow to develop there. Charismatic, authoritative editors such as Liebig continued to dominate publishing well into the twentieth century. In 1936 Albert Einstein could react with surprise and indignation after an editor of the American journal *Physical Review* sent a paper of his – on the nonexistence of gravitational waves – to an outside referee. ("We ... had sent you our manuscript for publication and had not authorized you to show it to specialists before it is printed. I see no reason to address the—in any case erroneous—comments of your anonymous expert" [Kennefick 1999, 208–9; Schweber 2008, 9].) The disclosure of yet-to-be-published research to other specialists in his field was, in Einstein's view, a clear violation of editorial decorum as well as a shirking of editorial responsibility.

It was in Britain – at the time that Liebig was making his case for the rights and duties of the authoritative journal editor – that men of science cobbled together the procedures for prepublication refereeing that later evolved into what became known as editorial peer review during the Cold War. Rather than being a means of sorting the good knowledge from the bad, however, the impetus for the Royal Society's scheme for refereeing manuscripts for inclusion in the *Philosophical Transactions* was a concern to reform and improve the public standing of natural philosophy in England. Moreover, the authority of the system was to be based, like Liebig's, on the credibility of well-known, trusted individuals as reviewers, rather than on a concept of collective imprimatur derived from anonymous judgment.

In the late 1820s, Charles Babbage and David Brewster led a campaign to reverse what they viewed as the decline of British science by, first and foremost, making it possible for men of science to earn a living through their scientific accomplishments (Morrell and Thackray 1981; Miller 1981; Snyder 2011). Looking to France, with its endowed Academy of Sciences and its civil honours – an amalgamation of pre-Revolutionary privilege, Napoleonic patronage, and meritocratic zeal – they believed that what was needed were means by which true men of science could be identified and recognized, both by their peers and by the state. This required finding means of picking out what they perceived as *real* contributors to science from pretenders such as high-born hangers-on and low-born charlatans. Proposals to use membership in the Royal Society as an honorific had foundered due to dissension over proposed election reforms. (Being elected as a fellow of the Royal Society was a relatively simple matter as long as one knew the right people and was able to pay the annual dues.)

In *Reflections on the Decline of Science in England* (1830), Babbage sought some other distinguishing mark that would separate real men of science from the rabble. The criterion he arrived at was publication in the *Philosophical Transactions*. He suggested the Royal Society use authorship as a means of effecting "the division of the Society into two classes." This was by no means an obvious approach. One might have considered who had been credited with actually making certain important discoveries or inventions, who had taught others to do so, or even who had disseminated contributions to natural philosophical topics in other formats. Babbage, however, was among the most vehement partisans of the nineteenth-century cult of print as sign and symbol of progress. "Until the invention of printing," Babbage wrote, "the mass of mankind were in many respects almost the creatures of instinct." Print was a vehicle not only for the spread of knowledge but for social mobility as well, allowing useful knowledge to reach those not favoured by personal circumstances (1837, 51). Moreover, contributions to the *Philosophical Transactions* were easy to count. According to Babbage's statistics, of the 714 Fellows of the Royal Society, 109 Fellows had been published in the *Transactions*. A more exclusive class could be formed of those who had published at least *two* papers in the *Transactions*. This led to 72 names which, as Babbage noted, was about the size of the French Académie des Sciences. Giving these classes official status would help produce a meritocratic order and raise the standing of the Society as a whole, making membership and scientific authorship something truly to be sought after (Babbage 1830, 154–5).

The use of authorship as a marker of merit was not unprecedented, but in a culture in which authorship – especially for gentlemen – remained an ambivalent distinction, it was by no means uncontroversial (Johns 2003). The suggestion inspired others to subject to closer scrutiny the procedures used by the Royal Society for deciding what to publish. An Italian-born doctor and FRS, Augustus Bozzi Granville, set out to "dissect" the social body of the Society and its claim to represent real men of science ([Granville] 1830; Granville 1836). He did so by producing what was surely the first prosopography of science, analysing the Fellows of the Society according to their profession and rank in society, and placing "against each individual member his claim to the honour of having been admitted as such, based upon what he may have done in the way of 'improving natural knowledge.'" The measure he used to determine who had contributed to natural knowledge was – following Babbage – the number of papers published in the *Transactions*. (See Fig. 8.1 for Granville's first table on Fellows, dedicated to bishops.) Although there was indeed some variation according to social rank, the central point was clear: very few fellows had published anything in the one venue that mattered. Granville interpreted his findings as definitively establishing that precious few Fellows "had any claim to the title of a savant" (Granville 1874, 218).

AND NOW TO WORK.

DISSECTED LIST

OF THE

FELLOWS OF THE ROYAL SOCIETY

FOR

MDCCCXXX.

TABLE I.

Of Bishops, Fellows of the Royal Society, distinguishing those who have contributed to the Philosophical Transactions.

0	Law, Lord Bishop of Bath and Wells	
0	Howley, Lord Archbishop of Canterbury	
9	Brinkley, Lord Bishop of Cloyne	
0	Magee, Lord Bishop of Dublin	
0	Sparke, Lord Bishop of Ely	5
0	Huntingford, Lord Bishop of Hereford	
0	Webb, Lord Bishop of Limerick	
0	Kaye, Lord Bishop of Lincoln	
0	Marsh, Lord Bishop of Peterborough	
0	Burgess, Lord Bishop of Salisbury	10

TOTAL—9 contributions towards improving natural knowledge by 10 Spiritual Lords.

☞ The Numbers in the left-hand column refer to the Number of Papers written by the Fellows, and published in the Transactions.

Fig. 8.1 First table in A. B. Granville's *Science without a Head* (1830, 34) listing all Fellows of the Royal Society that are bishops alongside their "contributions towards improving natural knowledge," measured by the 'Number of Papers' each had published in the *Philosophical Transactions*

Granville recognized that such a method for measuring scientific credentials was potentially misleading, since "the Transactions do not exhibit a correct view of all the labours of the fellows." While it ought to be true that the "measure of the labours of the Royal Society may be said to be found in its Transactions," there was reason to be suspicious of the mechanisms the Society used to determine just what it chose to print. Noting that many of the labours of the fellows "have been rejected without assigning any ground," Granville decided "to go a little more behind the scenes" (1830, 52). That is, Granville went to the archives. He was shocked by what he found. Under close scrutiny, the Committee of Papers – the body that made all decisions about publication – appeared both incompetent and corrupt. This Committee – a subset of the Council – simply voted on whether to accept or reject papers. It rarely, Granville found, called on specialist help, and thus it regularly passed judgment on papers without anything like the necessary competence. “Assuredly it cannot be expected that a sculptor for instance, a painter, a secretary to the Admiralty, an astronomer royal, and a botanist, congregated together, should come to a right decision respecting the propriety of publishing a paper on physiology or internal anatomy! Yet such things have come to pass” (1830, 55). Granville went on to suggest that these committees of eminent men with a wide range of competencies were rife with corruption, sometimes rejecting papers of foreigners and giving a free pass to eminent friends of the Society. Although Granville’s shock at this state of affairs may seem natural to those of us brought up to love and fear the scientific referee, the situation was a great deal more complicated than he made it out to be. The implication that “a competent judge” would be less subject to prejudice than a ballot taken by committee consisting of accomplished individuals with a wide range of competencies was far from obvious. He himself admitted that the latter system was perhaps “vastly objectionable ... on many accounts” (1830, 54). Then as now, specialists in the same branch of knowledge might indeed know the subject best, but they very likely also knew the author through ties of friendship, mentorship, or (perhaps most intimate of all) rivalry. If this state of affairs later came to be viewed as a minor weakness in a fundamentally sound system it was a more serious objection at a time when the most trustworthy individuals were generally perceived to be those with general learning and gentlemanly virtue rather than narrow technical knowledge. The archetype of this kind of judge was not one who read in isolation but one who listened, perused, and discussed.

Babbage’s book was an especially shrill salvo in the clamour for reform then gripping not only British natural philosophy, but British society and the political status quo more generally. Though Babbage and Granville belonged to opposing scientific factions, they both urged the abandonment of a long-standing political order increasingly characterized as “Old Corruption.” Within science this oligarchy was seen as having been exemplified by the Royal Society under the presidency of Joseph Banks (MacLeod 1983; Drayton 2000, chap. 5; Foote 1951; Gascoigne 1998), who had died in 1820. More widely it was represented by the informal ties that bound together England’s patrician elite, whose legitimacy had depended on widely-held convictions about the intertwined religious and legal foundations of prosperous, stable government. As the old order ceased to command assent – beginning with Catholic Emancipation in 1829 and reaching its symbolic apex with the 1832 Reform Acts – new conceptual schemas were invented through which to articulate new political identities, including radicalism, liberalism, and socialism (Clark 2000). Perhaps most crucially, public opinion – inextricably coupled in the minds of many to the periodical press – was widely viewed as a crucial source for all claims to political legitimacy (Parry 1993, chap. 1; Jupp 1998, chap. 8; Barker 1999).

In 1831, following a bitterly-contested presidential election framed by the language of reform, the Royal Society's Council solicited recommendations for new statutes. On March 22 (Domestic Manuscripts, DM/1/30), the Cambridge professor and methodologist William Whewell responded at length. One of his key recommendations was that the Royal Society adopt the practice of the Paris Academy of Sciences "to refer most or all memoirs received by it to a committee of a few persons known to be acquainted with the subject in question and to require from these committees written reports." Whewell's idea had little to do with prepublication vetting of manuscripts for publication, however. Crucial to the Académie's system, he explained, was that their reports were regularly printed themselves. These reports were "often more interesting than the memoirs themselves, containing both good abstracts & judgments upon the subject by the best authorities ... The advantage of this proceeding is ... the encouragement to writers from the certainty of being appreciated and the facility of diffusion of scientific information by abstracts and critiques." For Whewell it was the *public* character of these reports that was crucial. He re-iterated this point in a letter on the aims and functions of the proposed Association for British Science (Morrell and Thackray 1984, 52), noting that "the bearing of what [British men of science] have done upon the present state of science has not been often clearly placed before the public."

The Society soon began to experiment with reports in just the way Whewell had recommended. No paper was to be printed in the Transactions "unless a written Report of its fitness shall have been previously made by one or more Members of the Council" (Sussex 1832–1833, 141). Some of these were published over the next few years in the new *Proceedings of the Royal Society*, a new periodical the Society inaugurated partially in an effort to become more in touch with new scientific publics. The year following its implementation, the Society's president, the Duke of Sussex, celebrated the success of the new arrangement:

> The decisions of men who are elevated by their character and reputation above the influence of personal feelings of rivalry or petty jealousy, possess an authority sufficient to establish at once the full importance of a discovery, to fix its relations to the existing mass of knowledge, and to define its probable effect upon the future progress of science. (1832–1833, 142)

In fact, however, Whewell's idea was quickly subjected to competing visions of this new personage, the referee. From the very first report, which Whewell had volunteered to write in collaboration with the young applied mathematician John William Lubbock, there was trouble. They were to write on a manuscript of celestial mechanics that had been submitted by their friend, the astronomer George Airy, called "On an Inequality of Long Period in the Motions of the Earth and Venus " (Airy 1830–1831). While Whewell wished to avoid matters of detail and of explicit criticism in favour of emphasizing what was new and important, Lubbock insisted that a referee ought to call out errors wherever they could be found so that authors were kept honest. In Whewell – an eminent generalist – and Lubbock – a younger specialist working on topics very near to those of Airy – were juxtaposed two images of who the referee ought to be, and what their role could be in British science.

Systems of reference were implemented by many other specialized scientific societies in Britain.[3] Over time, however, Lubbock's vision won out. Within 2 years, the Royal Society stopped publishing such reports and Whewell's imagined raison d'être for such reports – to produce synthetic reports on the state of knowledge by trusted authorities – faded. The new system underwent many alterations, but the idea that most dominated in subsequent decades was that the referee system was a means by which the Royal Society could reward active men of science for services rendered to science. As a young T.H. Huxley put it in 1851 (in a letter to his future wife Anne) "having a paper in the 'Transactions' was one of the best of qualifications" to become a member of the Society itself (Huxley 1900, 1:72).

The considerations that motivated the Royal Society's system of prepublication reports are not wholly distinct from latter-day concerns, but there are significant differences. Authorship in the *Philosophical Transactions* did come to be a marker of scientific identity, an honour that could be translatable into prestige and, ultimately, professional rewards. However, the original archetype of the referee was not the anonymous figure standing in for the collective judgment of the community but rather the well-known, trustworthy gentleman of science. Moreover, while the question of fairness of the means by which the Royal Society determined which papers to accept for publication was a consideration, there was little concern at first for maintain the reliability of the scientific literature. None of the early promoters of referee systems referred to a canon of authoritative publications as a body of reliable knowledge claims. The repurposing of referees as gatekeepers of knowledge only became dominant toward the end of the century, once scientists came to perceive the existence of a scientific literature in need of protection. Perhaps not coincidentally, it is also from this time on that we begin to find complaints that "the 'referee system' which prevails in some of the 'learned societies' has broken down" (Irving 1892).

Given later commitments to the function of expert refereeing in protecting science from untrustworthy claims, it may stretch credulity that a movement fixated on improving the standing of the scientific enterprise would have invented such a system with*out* much attention to the reliability of the finished product. At other moments in its history, the quality and consistency of the *Transactions* as a reflection of the character of the Royal Society *had* been a critical consideration, including in 1752 when the Royal Society first created a Committee of Papers to oversee its publication (Fraser 1994). But the years surrounding 1830 were different. Both radical and elite reformist circles were concerned more about governance, the nature of expertise, and the bounds of participation. Evidence for the decline of science was focused on the scope and visibility of the enterprise, as well as on rival sites – new institutions and venues for publishing science – that might be perceived as competing with the Royal Society as legitimate representatives of authoritative

[3]Two societies – the Geological and the Astronomical Societies of London – had experimented with referee systems prior to that of the Royal Society, but it was primarily through the Royal Society's elaborate system that the referee became a well-known personage in British science by mid-century.

science. In the light of those latter, emerging sites, censures of the *Philosophical Transactions* were most often procedural than consequentialist.

What motivated the actors who advocated or developed a system of prepublication reviewing was not, first and foremost, a concern for the reliability of knowledge, but rather for the standing of the scientific enterprise, and of scientific practitioners, in England. Views about the legitimacy of scientific institutions and groups depended on commitments about the foundations of good government more generally. It is therefore no accident that the Royal Society (among others) was re-imagining the workings and aims of its publishing apparatus just as the legislative and electoral apparatus of English Society was being refashioned.

To demonstrate that the original justifications and imagined functions of institutions are at variance with what they have later been taken to be by no means establishes that they could not have come to serve these functions later on. But it should encourage us to cast our nets wide in later normative accounts of such institutions. If we are to argue that there is a sense in which practices such as peer review are indispensable to objectivity in science, the justification cannot rest on historical claims that it has always been this way.

8.3 Print and the Coordination of Knowers

In 1881, the American doctor and bibliographer John Shaw Billings admonished editors of scientific periodicals who did "not seem to appreciate fully their responsibility for the articles which they accept for publication." In earlier ages it had always been safe to assume, as long as knowledge claims did not travel too widely, that authors would be "appreciated at their true value in their immediate neighbourhood." But these days, he pointed out, editors "should remember that a certain number of readers, and especially those in foreign countries, have no clue to the character of the author, beyond the fact that they find his works in good company." It ought to be possible, Billings implied, to rely on the editorial apparatus of journal publishing to weed out untrustworthy claimants to knowledge (1881, 67).

Billings's perspective on the duties of scientific journal editors is one feature of a larger transition that is said to have taken place from a communications regime based primarily on informal exchange – whether face-to-face or through correspondence – to a more formal, impersonal regime focused on the circulation of public (printed) documents, one that was accompanied by the replacement of personal credibility with system-trust. It would be a mistake, however, to accept this transition as reflecting a straight-forward reality. There is little reason to believe that correspondence, manuscript exchange, and other forms of personal contact – not to mention judgments about individual character in adjudicating the reliability of knowledge claims – ever ceased to be of crucial significance in scientific communication. What we can say, however, is that scientists' beliefs and commitments about the relative efficacy of these regimes of communication and of trust underwent important changes during this period.

In the latter half of the century, European scientific practitioners reflected constantly on the altered conditions in which they worked, concerned both about the increase in the number of researchers and of rampant disciplinary specialization. In both France and Britain, the Franco-Prussian War came to represent a symbolic turning point: it was widely believed that Prussia's crushing victory was a consequence of its superior development of science and industry in the previous decades. At first the principal focus was on educational reforms, but this broadened in subsequent decades to the systematic organization of the research community. Savants hoped to overcome older models of scientific fraternity based on small, privileged elites, whether these were the select professional assemblies exemplified by the Parisian Académie des Sciences, or the informal coteries exemplified by the gentleman amateurs that dominated British natural philosophy. More appropriate forms of organization for modern science would ideally take into account what appeared to be the vastly expanded and increasingly specialized nature of the enterprise. In 1874, the astronomer Hervé Faye observed in a speech to the Académie that it "is no longer a matter of a handful of illustrious competitors contending for a few rare laurels, but of an army of workers the likes of which the world has never before seen." Faye was arguing that the Académie's vast system of prizes (awarded for work already accomplished) ought to be replaced by a system of grants in aid of research yet to be done. "Everything has changed between these two epochs, not only the magnitude of your revenues, but the social conditions, the ideas, the needs, the interests, and above all science itself" (1874, 1529–30).

By the 1890s, Norman Lockyer pointed out that the very success of educational reforms had utterly changed the conditions of scientific sociability by producing a new class of scientific worker: "Students are turned out by the score who are not only capable of using ordinary laboratory instruments to good effect, but who have taken part in original research. Such persons constitute a class which has only lately come into existence":

> Whether or no they are to spend their lives in a dull routine of teaching or testing, ... or whether they are to aid or even to follow the advance of knowledge depends largely upon the facilities for acquiring information which are afforded to them. They leave the University, or the University College, with its well-stocked library, and forthwith their touch or want of touch with the outer world depends almost entirely on the periodic literature of the science to which they have devoted themselves. (1893, 241)

Lockyer himself had founded the magazine *Nature*, but this was geared more toward scientific news than to orderly distribution of information. "It follows naturally from the spread of scientific education that the results of scientific study must be made more accessible than heretofore." The periodical literature – properly arranged – would be the medium to keep the scientific investigator in "touch with his fellow-workers" (Foster 1900, 20).

Late nineteenth-century scientific conversation was rife with concern over the proliferation and disorganization of the scientific literature. The physicist William Ayrton warned in 1898 that "an investigator who is much engaged with research can hardly do more as regards scientific literature than read what he himself writes—

soon he will not have time to do even that" (1899, 768). In France, Charles Langlois wondered about the "risk that science itself might suffocate, like a badly-assembled fire, under the weight of exactly those materials designed to sustain it" (1900, 25). Periods of anxiety over information overload have arisen as far back as we care to look. But what distinguished this moment was the particular focus on the journal as the standard of what could be said to be known. This was more a qualitative than quantitative change. As the scientific journal came to hold a monopoly on knowledge claims it was increasingly the focus of large-scale efforts to rationalize and standardize the means by which articles were accepted for publication, printed, distributed, and archived.

This history of efforts to reform scientific communications is closely bound up with late nineteenth-century discourses of objectivity. As attention shifted away from the special character of individuals to that of the community of knowers, and the very nature of that community was understood to be in transformation, the practical organization of science was central to making good on claims about the special character of the knowledge of those communities. The spectre of incommunicability was experienced not simply as a problem of private psychology but of public disorder.

Scientists and observers – such as Charles S. Peirce, Ernst Mach, and Henri Poincaré – for whom the special character of scientific knowledge rested on the collective circumstances of its production often participated in these efforts in organizational reform. The most pressing concern was not simply the degree of trust that could be put in the contents scientific journals (although this *was*, unlike in earlier epochs, a real matter of concern). If authoritative knowledge and consensus was to be in some significant way a collective product, and not simply what the most trusted individual *said* it was, then just where was it to be found? "One might say that a knowledge of science, like a knowledge of law, consists in knowing where to look for it. But even this kind of knowledge is not always easy to obtain" (Strutt 1894, 17).

These were genuine concerns of scientific practitioners in the late nineteenth century. But looking more closely at the context in which they were articulated in one iconic case – that of the French mathematician Henri Poincaré – suggests that conceptions of community and the epistemic virtues that they were supposed to undergird are best understood not simply as responses to internal problems of knowledge, but as part and parcel of the political cultures in which they were embedded.

Poincaré was among the first practitioners of a new kind of publishing enterprise that became immensely popular in late nineteenth century science. Poincaré's *Répertoire Bibliographique des Sciences Mathématiques* (Rollet and Nabonnand 2002) was a service that produced subject-classified index cards that informed readers of original published work on particular specialized topics. Poincaré began to plan this venture in 1885, as a young professor, and was the president of its central bureau for the rest of his life. Later he did much service for the French state as an expert on scientific bibliography and classification.

Poincaré ultimately came to visualize the process of making knowledge *through* the problem of managing the scientific literature. In 1900, addressing the first International Congress of Physics in Paris, Poincaré's lecture defended the role of mathematicians in contributing to knowledge of the physical world. He developed a vision in which nature was, not simply a *book*, but a vast expanse of print matter. Science, on the other hand, was just one particular library's collection, one that was woefully incomplete. "The librarian has but limited funds for his purchases, and he *must strive not to waste them*. Experimental physics has to make the purchases," he explained. The duty of mathematical physicists was "to draw up the catalogue. If the catalogue is well done the library will be none the richer for it, but the reader will be enabled to use those riches" (1900, 4).

Poincaré's bibliographic apologia for mathematical physics rested on and extended the epistemological views he was just then developing. Just a few days earlier, at the concurrent philosophy congress, he had extended his doctrine of conventions (first developed in the context of mathematics) to physics, arguing that Newton's Laws were neither a priori truths nor experimental facts but convenient choices (Poincaré 1901).[4] The doctrine that certain mathematical principles and physical laws were conventions made for a mutually constitutive relationship between experimental mechanics and "conventional mechanics." He now turned the epistemology into a schema for the division of labour in the sciences: mathematicians do organizational work, finding those generalizations that group together, as efficiently as possible, as many facts as possible.

Poincaré had hit on the idea for his index card enterprise not long after the formative episode of his early career. In the early 1880s Poincaré had taken a job teaching analysis in Caen, a city in Normandy near the English Channel. While in provincial exile he made the discoveries that established his international reputation. He elaborated the theory of what he called Fuchsian functions, gradually realizing that their study led to striking analogies with what were by-then famous – if still controversial – non-Euclidean geometries. But Poincaré's success was interrupted by a lengthy dispute with the distinguished German mathematician Felix Klein. In a long exchange of letters Klein repeatedly admonished Poincaré for his wilful ignorance of the mathematical literature. Poincaré had failed to cite several papers – many by Klein himself – that he deemed highly pertinent to Poincaré's ostensible discoveries. Klein chastised Poincaré for his faulty grasp of what was already known in his area of research, and his ignorance of the "whole bibliography."[5] Once he secured a professorship in Paris a few years later, Poincaré was quick to lay the groundwork for his new bibliographical enterprise. He warned European mathematicians in a circular that no one "can any longer avoid engaging in arduous bibliographical research – the day will soon come when it will become impossible

[4]For recent accounts of Poincaré's doctrine of conventions, see Walter (2009) and Ben-Menahem (2006).

[5]Klein to Poincaré, June 25, 1881 (Dugac 1986, 95). For other historical accounts of this episode see Rowe (1992) and Gray and Walter (1997).

to do anything without new tools in hand with which to work" (quoted in Eneström 1890, 39).

Poincaré's run-in with Klein combined with his well-known remarks about the importance of communicability as a guarantor of objectivity in science (Daston and Galison 2007, 273–89) seem to provide a credible explanation for his interest in providing mathematicians with a systematic tool for keeping up with the literature. But this requires qualification. Despite Poincaré's continued association with the *Répertoire* for decades, all evidence points to its having been singularly unsuccessful in assisting mathematicians in keeping up with the literature. Poincaré's cards never attracted many subscribers, they were produced extremely slowly, and several key mathematical journals – including Klein's own *Mathematische Annalen* – were never indexed in them at all. Why did Poincaré bother? The one feature of the operation that attracted anything resembling longstanding notice was its system of subject classification. Poincaré and his collaborators had worked at developing this system over several years, and they continued to revise it for decades. Although the cards themselves were mostly ignored, the classification system was taken up by several other publications in the exact sciences and received far wider attention.

Zeal for classifying the sciences was a feature of the bibliographical moment more generally. Earlier on, indexing projects in the sciences had not made much of subject classification, focusing instead on such things as alphabetical indexes. But in the late nineteenth century scores of scientists across Europe were engaged in building ambitious classification systems with which to archive scientific knowledge in print. The new enthusiasm for classificatory order fit well with the vision of the scientific enterprise as consisting of large, impersonal networks of scientists connected rationally through shared research interests rather than through ad hoc personal acquaintance. As one enthusiast put it in 1900, precise classifications of science had "for their aim and effect to simplify and facilitate the task of savants" in the same way that precise postal addresses had facilitated communication at a distance more generally (Durand de Gros 1899, 1–2).

This vision of efficient and impersonal communication was reflective more of a widely-held aspiration than it was of the reality of the scientific life of the late nineteenth century. As Lord Rayleigh recognized in 1884, when becoming "acquainted with what has been done in any subject, it is good policy to consult first the writers of highest general reputation" (Strutt 1885, 20). Late nineteenth-century savants promoted an ideal in which knowledge claims circulated efficiently and unfettered by personal acquaintance, but the reality was that it remained crucial to know who to ask, not only about what claims could be trusted, but even what claims had been made, and where they might be found (Csiszar 2010).

This cautionary note ought especially to be kept in view when such concerns are integrated with accounts of broader epistemic virtues such as objectivity, as they were by Poincaré. The celebrated account of scientific objectivity associated with Poincaré was put forward by him relatively late, in 1902, after he had already formulated most of his philosophical doctrine, and it followed the bulk of his service in the organization of science. His interest in objectivity arose in direct response to perceived threats to science, not from the spectre of internal disorder or of communications failure, but from the changing situation of the scientific enterprise in France that stemmed in part from the very changes that had followed the post-war reforms instituted by the French Third Republic. By century's end, it appeared that many of the reformist aspirations that French scientists had articulated in the post-war atmosphere of the 1870s were coming to pass. There were closer links between science and industry, funding and educational opportunities in the sciences had increased, and decentralization had begun to occur, with significant funding being put into applied science institutes in the provinces. However, the quick upturn in the public profile of French science, and the fact that the bulk of new funding was being generated as direct grants from industry, had raised a new set of concerns. Savants were increasingly valued, not because they produced knowledge or truth about nature, but due to their ability to provide instrumental and technical expertise. Poincaré (1897, 331) emerged as one of the most fervent critics of the new state of affairs, defending the autonomy of scientists against what he called "practical men demanding of us only new means of making money."

At the same time, Poincaré's views on the nature of knowledge were spreading to new, diverse audiences, and he was increasingly called on to explain what kept his views from reducing simply to skepticism. Part and parcel of this development was a wildly popular philosophical movement led by Henri Bergson that helped spread more radical interpretations of Poincaré's thought. Édouard Le Roy, a close disciple of Bergson, had appropriated Poincaré's doctrine of conventions in a way that the latter found deeply disturbing, deriving what Poincaré perceived as radical, skeptical conclusions which diminished the status of science. In response, Poincaré marshalled his conventionalist doctrine to put forward what I will call – following the intellectual historian David Hollinger – a *laissez-faire communitarian* defence of science.[6] Poincaré argued that conventions were exemplary of stable, objective knowledge precisely because they were based on collective beliefs. Moreover, according to Poincaré's mature epistemological view, the production of theoretical knowledge was indistinguishable from the *organization* of knowledge. The corollary was that only expert scientists could possibly make felicitous decisions about what kinds of research areas were worthwhile investments, but they were able to play this role precisely because the community kept itself strictly organized.

It was in the 1902 article "La valeur objective de la science" – written directly to counter misappropriations of his work – that Poincaré endeavoured to clarify how it was that conventions were not only a substantive form of knowledge, but in many ways represented the *most* stable and objective aspects of our knowledge of the world. With his attention turned to explaining why his doctrine was not skepticism, but was rather a better account of objectivity, he focused on the idea that it was the collective seat of those conventional choices that ultimately produced stability:

[6]Hollinger (1990) used the phrase *laissez-faire communitarianism* to describe a defence of scientific autonomy that became popular in the American 1960s. This view includes: (1) Support of science is key to national progress, (2) scientists must have autonomy to determine research directions, (3) This autonomy is collective rather than individual: it resides in a concrete, organized, social constituency.

"What guarantees the objectivity of the world in which we live is that this world is common to us with other thinking beings. Through the communications that we have with other men, we receive from them ready-made reasonings." Conventional choices became objective knowledge insofar as they were transmitted among several minds: "Pas de discours, pas d'objectivité" (1902, 288).

Since the objectivity of knowledge was to be based on its being a matter of collective belief and experience, Poincaré also ultimately required a view of what made these collectives cohere in the first place. He regularly drew on prominent doctrines of French social and political thought for this purpose. The intellectual and political movement known as *solidarisme* provided a ready-made vocabulary with which to explain the necessarily collective nature of all progressive human endeavour. Poincaré's particular vision of scientific solidarity emphasized hierarchy, discipline, and self-sacrifice. In a letter celebrating the centennial of the University of Berlin, dated September 30, 1910 (Archives Henri Poincaré), he observed that even the man of genius "would not be what he is if he did not have behind him a mass of more modest workers." It was precisely because of the increasing scale of the scientific enterprise that strict, disciplined organization had become so important: "These are the virtues that are now becoming increasingly important; the more that Science is able to conquer, the more Science is in need of a disciplined army. It is modest and obscure soldiers that support the glorious generals and render their task possible." Poincaré's scientific generals were theoreticians and mathematicians such as himself, and their task was the maintenance of the integrity of scientific knowledge itself.

This vision of science as a vast, functionally-differentiated, and disciplined network made knowledge as a collective exercise in classification the key to a new managerial epistemology. Poincaré had gotten his first idea of what such a system could look like when as a young mathematician he pioneered the first close-classified index card service for a scientific discipline.

8.4 Conclusion

Scientific publishing is today undergoing rapid change. At one end of the spectrum, authors in disciplines such as mathematics and much of physics have shifted the bulk of their publishing activities away from typical scientific journals to online repositories of preprints. As this occurs, conventions surrounding submission, pre-publication review, and priority adjudication are being reformulated in significant ways. At the same time, the last decades have seen a dramatic rise in the use of metrics for the evaluation of scientific work based on quantitative measures of the prestige of scientific journals. The first and most prominent of these, the ISI Impact Factor, was based on citation counting; others have begun to take account of downloads and clickstream statistics. The standing and career prospects of scientists in some fields – particularly, but by no means exclusively, in the life sciences – are potentially at the mercy not simply of what they have published, but how often and – most crucial of all – where they have done so.

Advocates of these schemes argue that the system works because there is generally a good correlation between the highest-ranked journals and those with the most rigorous submission standards. Detractors point to instances in which this is not the case and the various means by which the system may be gamed, thus distorting the very values the system is meant to upheld. But the idea remains common that the peer-reviewed journal literature, when it is working at its best, epitomizes the particular epistemic virtues that have given modern science its power to describe the world.

Similar considerations have loomed large in recent public debates about expert consensus in science, such as in public controversies regarding climate change. A rich spectrum of evidence suggests that expert opinion in climate science, by any reasonable criterion, strongly supports the reality of anthropogenic global warming. But the workings of the journal literature of climate science has played a particularly weighty role in public debates on this topic (Oreskes 2005, 2007; Pielke and Oreskes 2005).[7] Naomi Oreskes and Erik Conway argue that a principal strategy climate skeptics have used to produce an impression of disconsensus has been to muddy the genre boundaries that separate scientific journals from newspapers, each of which rely on distinct conceptions of objectivity. The canons of objectivity in a journalistic report, where representing all sides of an issue – no matter how trivial or idiosyncratic – may be deemed a virtue, are very different from those that attach to specialized scientific publications, where expert authors make claims that are normally vetted by anonymous peers, and where the subsequent reception of these claims is highly consequential for their professional reputation. To ignore the generic specificity that the modern scientific paper has achieved is to misrecognize what is crucial about scientific – as opposed to journalistic – objectivity (Oreskes and Conway 2010).[8]

But there are risks to ascribing too much importance to the apparatus of scientific journal publishing.[9] To see this, consider the idealized image of the scientific literature as the crucial foundation of scientific trust that has been embraced by many climate-change skeptics themselves. This was made clear by the leak of thousands of emails and documents from the Climate Research Unit at the University of East Anglia in November 2009. The emails seemed to reveal climate scientists engaging in secretive behaviour, fudging data, and playing politics with scientific publishing and peer review. For many, it was a revelation that science behind the authoritative printed page was not an exact reflection of its better-behaved public face (Tierney

[7] Andrew C. Revkin (2007) gives a further account of public confidence in scientific claims about climate science.

[8] Oreskes emphasized more explicitly this point about two different conceptions of objectivity at work in these two different genres during her keynote address at the conference on Objectivity held in Vancouver, BC, June 19, 2010.

[9] Oreskes herself has focused on a much wider spectrum of strategies for detecting social consensus in climate science than counts of the peer-reviewed literature. But the immense popularity of the 2004 *Science* note is compelling evidence of the elevated regard in which editorial peer review is held as a guarantor of scientific objectivity.

2009). Evidence that the apparatus of scientific journal publishing was subject to political manoeuvring and strategy was seized upon by commentators, such as Patrick J. Michaels, to demonstrate that the field of climate science as a whole was in a state of corruption. Michaels described the behaviour as tampering with "what goes in the Bible": "The bible I'm referring to, of course, is the refereed scientific literature. It's our canon, and it's all we have really had to go on in climate science" (2009b; see also 2009a).

However, both ignoring the generic distinctions that govern modern scientific communication as well as exalting the scientific literature as a transparent reflection of scientific objectivity – as if the integrity of the scientific enterprise were founded exclusively on the basis of impersonal, mechanical checks on accountability – rely on impoverished views of the complex dynamics of scientific consensus-formation, of the relations between science and its wider publics, and of the changing roles that scientific publishing has played in these.

The historical studies in this essay suggest that it is no accident that public controversies over the credibility of scientific research can become controversies about scientific publishing. The institution that is the scientific literature did not develop purely as a means of guaranteeing objectivity within expert communities. Rather it evolved through the relationship that these communities have cultivated with the wider polities within which they are active participants. Since the nineteenth century, the apparatus of specialized publishing has been an intersection point where expert cultures of credibility have overlapped, uneasily, with public criteria of accountability. Systems of refereeing evolved as men of science adapted themselves to new social and professional realities in reform-Era England. In the late nineteenth century, Henri Poincaré articulated a doctrine of objectivity, integrated with a concern for community organization, as he sought to defend the autonomy of the scientific enterprise in France, taking up the very discourse of solidarity then dominating French political culture. We ought to be wary of taking objectivity-talk at face value as addressed simply to epistemic conundrums. Contemporary attempts to fix peer review or rationalize the production and distribution of scientific credit cannot simply be reduced to economic problems of maximizing the efficiency and fairness of the *machine scientifique*. We need richer, more nuanced, ways of talking about collective belief that take into account the complexity of scientific interactions and how these forms evolve along with the regulatory frameworks used for evaluating scientific claims relevant to public policy. Objectivity is neither a straight-forward possession of select individuals nor is it a label that may be affixed to the cover of a periodical.

References

Airy, George. 1830–1831. On an inequality of long period in the motions of the Earth and Venus. *Proceedings of the Royal Society* 3: 108–113.

Apel, Karl-Otto. 1998. *Towards a transformation of philosophy*. Milwaukee: Marquette University Press.

Archives Henri Poincaré. Preußischer Kulturbesitz. [Copy consulted at Archives Henri Poincaré, Nancy, France.]

Ayrton, William. 1899. Presidential address – Section A. In *Report of the sixty-eighth meeting of the British Association for the advancement of science, held at Bristol in September, 1898*, 768–777. London: J. Murray.

Babbage, Charles. 1830. *Reflections on the decline of science in England and on some of its causes*. London: B. Fellowes.

Babbage, Charles. 1837. *The ninth Bridgewater treatise: A fragment*. London: J. Murray.

Barker, Hannah. 1999. *Newspapers, politics and English society 1695–1855*. New York: Longman.

Ben-Menahem, Yemima. 2006. *Conventionalism*. Cambridge: Cambridge University Press.

Berthelot, Marcellin. 1897/1901. La direction des sociétés humaines par la Science. In *Science et éducation*, ed. Marcellin Berthelot, 1–9. Paris: Société française d'imprimerie et de librairie.

Billings, John S. 1881. Our medical literature. In *Transactions of the seventh session of the international medical congress*, 54–71. London: J. W. Kolckmann.

Canales, Jimena. 2009. *A tenth of a second: A history*. Chicago: University of Chicago Press.

Clark, Jonathan Charles D. 2000. *English society, 1660–1832: Religion, ideology, and politics during the ancien regime*. Cambridge: Cambridge University Press.

Csiszar, Alex. 2010. Seriality and the search for order: Scientific print and its problems during the late nineteenth century. *History of Science* 4: 399–434.

Daston, Lorraine. 1999a. Moralized objectivities of science. In *Wahrheit Und Geschichte: Ein Kolloquium zu Ehren des 60. Geburtstages von Lorenz Krüger*, ed. Wolfgang Carl, 78–100. Göttingen: Vandenhoeck & Ruprecht.

Daston, Lorraine. 1999b. Objectivity versus truth. In *Wissenschaft als Kulturelle Praxis, 1750–1900*, ed. Hans Erich Bödeker and Peter Hanns Reill, 17–32. Göttingen: Vandenhoeck & Ruprecht.

Daston, Lorraine. 2001. Scientific objectivity with and without words. In *Little tools of knowledge: Historical essays on academic and bureaucratic practices*, ed. Peter Becker and William Clark, 259–284. Ann Arbor: University of Michigan Press.

Daston, Lorraine, and Peter Galison. 1992. The image of objectivity. *Representations* 40: 81–128.

Daston, Lorraine, and Peter Galison. 2007. *Objectivity*. New York: Zone Books.

Domestic Manuscripts. DM/1/30. Archives of the Royal Society of London.

Drayton, Richard. 2000. *Nature's government: Science, imperial Britain, and the 'Improvement' of the world*. New Haven: Yale University Press.

Dugac, Pierre, ed. 1986. La correspondance d'Henri Poincaré avec des mathématiciens de A à H. *Cahiers du Séminaire d'histoire des mathématiques* 7: 89–140.

Duke of Sussex. 1832–1833. Anniversary address. *Proceedings of the Royal Society* 3: 140–155.

Durand de Gros, Joseph-Pierre. 1899. *Aperçus de taxinomie générale*. Paris: Felix Alcan.

Eamon, William. 1985. From the secrets of nature to public knowledge: The origins of the concept of openness in science. *Minerva* 23: 321–347.

Eneström, Gustaf. 1890. Sur les bibliographies des sciences mathématiques. *Bibliotheca Mathematica* 4: 37–42.

Faye, Hervé. 1874. Allocution du président. *Comptes Rendus Hebdomadaires des Séances de l'Académie des Sciences* 79: 1525–1531.

Foote, George A. 1951. The place of science in the British reform movement 1830–50. *Isis* 42: 192–208.

Foster, Michael. 1900. Presidential address. In *Report of the sixty-ninth meeting of the British Association for the advancement of science, held at Dover in September 1899*, 3–23. London: J. Murray.

Fraser, Kevin J. 1994. John Hill and the Royal Society in the eighteenth century. *Notes and Records of the Royal Society of London* 48: 43–67.

Galison, Peter, and Lorraine Daston. 2008. Scientific coordination as ethos and epistemology. In *Instruments in art and science: On the architectonics of cultural boundaries in the 17th century*, ed. Helmar Schramm, Ludger Schwarte, and Jan Lazardig, 296–333. Berlin: De Gruyter.

Gascoigne, John. 1998. *Science in the service of empire: Joseph Banks, the British state and the uses of science in the age of revolution*. Cambridge: Cambridge University Press.

[Granville, Augustus Bozzi]. 1830. *Science without a head; or the Royal Society dissected.* London: Ridgway.

Granville, Augustus Bozzi. 1836. *The Royal Society in the XIXth century*. London: G. Hayden.

Granville, Augustus Bozzi. 1874. *Autobiography of A. B. Granville, M.D., F.R.S.* London: H. S. King.

Gray, Jeremy, and Scott Walter. 1997. Introduction. In *Henri Poincaré, Trois Suppléments sur la Découverte des Fonctions Fuchsiennes. Three Supplementary Essays on the Discovery of Fuchsian Functions. Une édition critique des manuscrits avec une introduction*, ed. Jeremy Gray and Scott Walter, 1–25. Berlin: Akademie Verlag.

Hollinger, David A. 1990. Free enterprise and free inquiry: The emergence of Laissez-Faire Communitarianism in the ideology of science in the United States. *New Literary History* 221: 897–919.

Hull, David L. 1988. *Science as a process: An evolutionary account of the social and conceptual development of science*. Chicago: University of Chicago Press.

Huxley, Leonard. 1900. *Life and letters of Thomas Henry Huxley*, vol. 2. London: Macmillan.

Irving, A. 1892. An obstacle to scientific progress. *Chemical News* 46: 61.

Johns, Adrian. 1998. *The nature of the book: Print and knowledge in the making*. Chicago: University of Chicago Press.

Johns, Adrian. 2000. Miscellaneous methods: Authors, societies and journals in early modern England. *The British Journal for the History of Science* 33: 159–186.

Johns, Adrian. 2003. The ambivalence of authorship in early modern natural philosophy. In *Scientific authorship: Credit and intellectual property in science*, ed. Mario Biagioli and Peter Galison, 67–90. New York: Routledge.

Jupp, Peter. 1998. *British politics on the eve of reform: The Duke of Wellington's administration, 1828–30*. Basingstoke: Macmillan Press.

Kennefick, Daniel. 1999. Controversies in the history of the radiation reaction problem in general relativity. In *The expanding worlds of general relativity*, ed. Hubert Goenner, Jürgen Renn, Jim Ritter, and Tilman Sauer, 207–234. Boston: Birkauser.

Kitcher, Philip. 1993. *The advancement of science: Science without legend, objectivity without illusions*. New York: Oxford University Press.

Laisant, Charles-Ange. 1904. Le rôle social de la science. *Enseignement mathématique* 6: 337–362.

Langlois, Charles-Victor. 1900. La question bibliographique. *La grande revue* 4: 22–53.

Liebig, Justus. 1834. Bemerkungen zu der vorstehenden Abhandlung des Herrn Dr. Reichenbach. *Annalen der Pharmacie* 10: 315–323.

Lockyer, Norman. 1893. Order or chaos? *Nature* 48: 241–242.

Longino, Helen E. 1990. *Science as social knowledge: Values and objectivity in scientific inquiry*. Princeton: Princeton University Press.

MacLeod, Roy M. 1983. Whigs and Savants: Reflections on the reform movement in the Royal Society, 1830–48. In *Metropolis and province: Science in British culture, 1780–1850*, ed. Ian Inkster and Jack Morrell, 55–90. Philadelphia: University of Pennsylvania Press.

Merton, Robert K. 1942/1968. Science and democratic social structure. In *Social theory and social structure*, 604–615. New York: Free Press.

Michaels, Patrick J. 2009a. Climate scientists subverted peer review. *Washington Examiner*, December 2.

Michaels, Patrick J. 2009b. How to manufacture consensus a climate consensus. *Wall Street Journal*, December 17.

Miller, David P. 1981. The Royal Society of London, 1800–1835: A study in the cultural politics of scientific organization. Ph.D. Dissertation, University of Pennsylvania.

Morrell, Jack, and Arnold Thackray. 1981. *Gentlemen of science: Early years of the British Association for the advancement of science*. Oxford: Clarendon.

Morrell, Jack, and Arnold Thackray (eds.). 1984. *Gentlemen of science: Early correspondence of the British Association for the advancement of science*. London: University College London.
Oreskes, Naomi. 2007. The scientific consensus on climate change: How do we know we're not wrong? In DiMento et al. 2007, 65–99.
Oreskes, Naomi, and Erik M. Conway. 2010. *Merchants of doubt: How a handful of scientists obscured the truth on issues from tobacco smoke to global warming*. New York: Bloomsbury Press.
Parry, Jonathan. 1993. *The rise and fall of liberal government in Victorian Britain*. New Haven: Yale University Press.
Peirce, Charles Sanders. 1905. Adirondack summer school lectures. MS 1334, Houghton Library, Harvard University.
Peirce, Charles Sanders. 1871. Review of the works of George Berkeley, D. D., formerly Bishop of Cloyne. With prefaces, annotations, his life and letters, and an account of his philosophy, by Alexander Campbell Fraser. *The North American Review* 113: 449–472.
Pielke, Robert A., and Naomi Oreskes. 2005. Consensus about climate change? *Science* 308: 952–953.
Poincaré, Henri. 1897. Sur les rapports de l'analyse pure et de la physique mathématique. *Acta Mathematica* 21: 331–341.
Poincaré, Henri. 1900. Les relations entre la physique expérimentale et la physique mathématique. In *Rapports du Congrès international de physique*, 1–29. Paris: Gauthier-Villars.
Poincaré, Henri. 1901. Sur les principes de la mécanique. In *Bibliothèque du Congrès international de philosophie*, 457–494. Paris: Colin.
Poincaré, Henri. 1902. Sur la valeur objective de la science. *Revue de Métaphysique et de Morale* 10: 263–293.
Popper, Karl. 1945/1963. *The open society and its enemies*. 2 vols. Princeton: Princeton University Press.
Revkin, Andrew C. 2007. Climate change as news: Challenges in communicating environmental science. In DiMento et al. 2007, 139–159. Cambridge: MIT Press.
Rollet, Laurent, and Philippe Nabonnand. 2002. Une bibliographie mathématique idéale? Le Répertoire Bibliographique des Sciences Mathématiques. *Gazette des mathématiciens* 92: 11–26.
Rowe, David E. 1992. Klein, Mittag-Leffler, and the Klein-Poincaré correspondence of 1881–1882. In *Amphora: Festschrift für Hans Wussing zu seinem 65. Geburtstag*, ed. Sergei S. Demidov et al., 597–618. Basel: Birkhauser.
Schweber, Silvan S. 2008. *Einstein and Oppenheimer: The meaning of genius*. Cambridge, MA: Harvard University Press.
Shapin, Steven. 2008. *The scientific life: A moral history of a late modern vocation*. Chicago: University of Chicago Press.
Snyder, Laura J. 2011. The great battle. In *The philosophical breakfast club: Four remarkable friends who transformed science and changed the world*, 128–157. New York: Broadway Books.
Solomon, Miriam. 2001. *Social empiricism*. Cambridge: MIT Press.
Strutt, John [Lord Rayleigh]. 1885. Presidential address. In *Report of the fifty-fourth meeting of the British association for the advancement of science, held at Montreal in August and September 1884*, 3–23. London: J. Murray.
Strutt, John [Lord Rayleigh]. 1894. The scientific work of Tyndall. *Chemical News* 70: 17–20.
Tierney, John. 2009. Fracas over hacked climate e-mail shows the perils of spinning science. *New York Times*, December 1.
Vittu, Jean-Pierre. 2001. Qu'est-ce qu'un article au Journal des savants de 1665 à 1714. *Revue française d'histoire du livre* 112–113: 129–148.
Volhard, Jakob. 1909. *Justus von Liebig*. Leipzig: J.A. Barth.
Walter, Scott. 2009. Hypothesis and convention in Poincaré's defense of Galilei Spacetime. In *The significance of the hypothetical in natural science*, ed. Michael Heidelberger and Gregor Schiemann, 193–220. Berlin: Walter de Gruyter.

Ziman, John. 1968. *Public knowledge: An essay concerning the social dimension of science*. London: Cambridge University Press.
Ziman, John. 1969. Information, communication, knowledge. *Nature* 224: 318–324.
Ziman, John. 1995. *Of one mind: The collectivization of science*. Woodbury: American Institute of Physics.
Zuckerman, Harriet, and Robert K. Merton. 1971/1979. Patterns of evaluation in science: Institutionalisation, structure and functions of the referee system. In *The Scientific Journal*, ed. Arthur J. Meadows, 112–146. Dorset: Henry Ling.

Part III
Securing Objectivity in Scientific Communities

Chapter 9
Objectivity, Intellectual Virtue, and Community

Moira Howes

9.1 Introduction

In this paper I argue that the objectivity of persons is best understood in terms of intellectual virtue, the *telos* of which is an enduring commitment to salient and accurate information about reality. On this view, an objective reasoner is one we can trust to manage her perspectives, beliefs, emotions, biases, and responses to evidence in an intellectually virtuous manner. We can be confident that she will exercise intellectual carefulness, openmindedness, fairmindedness, curiosity, perseverance, and other intellectual virtues in her reasoning.

I argue further that the cultivation and exercise of such virtues is a social phenomenon, and challenge highly individualistic notions of intellectual character and epistemic autonomy. Community intellectual virtue is necessary for the development of personal objectivity. An advantage of conceptualizing objectivity in terms of community intellectual virtue is that it better equips us to address failures of objectivity in scientific research, science policy, and public debates about science. Normally, the blame for such failures is placed on the politicization of science, communication problems, scientific illiteracy, or industry propaganda. But responses within these frameworks often amount "to telling citizens why they should love science" (Brown 2009, 17). The difficulty is that no matter how much we love science or how well we communicate, confusion about what it is to be an objective, epistemically trustworthy person and what it is like to reason in objective, epistemically trustworthy communities will continue to undermine reasoned debate. Sorting out the virtue epistemological issues associated with objectivity therefore stands to help us better cultivate it in research, policy and public debate.

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In the next section, I provide a working definition of objectivity in terms of intellectual virtue and then consider two problems for conceptualizing objectivity in virtue epistemic terms. The first is that accounts of objectivity tend to travel implicitly between the objectivity of persons and the objectivity of methods. The second is that accounts of biased reasoning in science and science policy often assume, but fail to explicate, virtue epistemological perspectives of objectivity. Addressing these problems will help emphasize important connections between objectivity and virtue. In the third section, I argue that to consider adequately intellectual virtue with regard to the objectivity of persons, we need to address its social epistemic dimensions. To support this view, I show that the objectivity of persons is deeply tied to epistemic trustworthiness, a social intellectual virtue. Finally in the fourth section, I raise and respond to three objections to my argument.

9.2 Objectivity as Intellectual Virtue

Virtues are usually defined in philosophy as excellences of character. Linda Zagzebski, for example, defines a virtue as "a deep and enduring acquired excellence of a person, involving a characteristic motivation to produce a desired end and reliable success in bringing about that end" (1996, 137). The excellences virtue epistemologists consider fall into two general categories. Reliabilist virtue epistemologists stress perceptual and functional virtues such as good eyesight and good memory, whereas responsibilist virtue epistemologists focus on aspects of intellectual virtue analogous to moral virtue. Responsibilists are interested in virtues that we are more likely to hold others accountable for, such as openmindedness, intellectual courage, and intellectual honesty. While I focus on responsibilist virtues, reliabilist virtues clearly also contribute to our motivation for desirable epistemic ends and reliable success in achieving those ends.

Adapting Zagzebski's definition of a virtue to the case of objectivity, we can define an objective person as one whose objectivity is deep, enduring, and acquired. They will have a characteristic motivation to produce a desired end and reliable success in bringing about that end. In this case, there are both ultimate and proximate desired ends. The ultimate end of objectivity is the epistemic value of salient and accurate information about reality. This end helps explain why objectivity is often conflated with impersonal knowledge, truth, or even rationality itself. The proximate ends of objectivity concern more subsidiary epistemic values. For example, Jason Baehr argues that objectivity promotes the proximate end of "consistency in evaluation" (2011, 21). Tara Smith claims that "fidelity to reality" is the "heart of objectivity," and this suggests that *faithfulness* to reality is another proximate end of objectivity (2004, 147). I consider the proximate end of objectivity along similar lines: it is to maintain an *enduring commitment* to salient and accurate information about reality. The objective person is one who has, among other things, an enduring commitment to salient and accurate information about reality.

But the idea that objectivity involves an enduring commitment says little about what the objectivity of persons actually is. Is objectivity a specific intellectual virtue, or is it related to intellectual virtue in some other way? Baehr treats objectivity as a specific intellectual virtue. Another possibility is that "objectivity" is a vague term that refers to the integrated exercise of various intellectual virtues. I am partial to the latter view; however, because little conceptual and normative work has been done for individual intellectual virtues (Riggs 2010), it would be premature to decide presently which of these two possibilities is best. Fortunately, either option is consistent with my argument that we should pay greater conceptual and normative attention to the objectivity of persons using the resources of virtue and social epistemologies. Moreover, my intention is not to create an ahistorical definition of the objectivity of persons in terms of intellectual virtue. As we come to learn more about our psychology, sociality, and rationality, our understanding of the relations between objectivity and virtue will likely improve.

One challenge for understanding objectivity in virtue epistemic terms, however, is that accounts of objectivity tend to shift between the objectivity of persons and the objectivity of claims, theories, and methodologies (Daston 1992; Daston and Galison 2007; Douglas 2009). Smith, for example, says that

> objectivity is a deliberate commitment to keeping one's beliefs grounded in reality by thinking logically. Objectivity is a method for human beings—who are fallible—to employ to discipline our thinking to help us gain an accurate understanding of the world (2004, 153).

In the first instance, objectivity is a personal virtue; in the second, a method. Similarly, while Heather Douglas focuses "on the objectivity of knowledge claims, and the processes that produce these claims, rather than the objectivity of persons" (2009, 116), some of her categories of objectivity clearly involve intellectual character. Detached objectivity "keeps one from wanting a particular outcome of inquiry too much, or from fearing another outcome to such an extent that one cannot see it" (122). And interactive objectivity requires that "[i]nstead of immediately assenting to an observation account, the participants are required to argue with each other, to ferret out the sources of their disagreements" (127). This requirement is reminiscent of the Stoic intellectual virtue of "non-precipitancy," which is "a disposition not to assent in advance of cognition" (Sherman and White 2003, 48). It also characterizes objectivity in terms of persons who are capable of critical engagement and intellectual perseverance.

Daston and Galison are very mindful of the issue concerning conceptual shifts between personal objectivity and the objectivity of claims and methods. They say:

> Understanding the history of scientific objectivity as part and parcel of the history of the scientific self has an unexpected payoff: what had originally struck us as an oddly moralizing tone in the scientific atlas makers' accounts of how they had met the challenge of producing the most faithful images now made sense. If knowledge were independent of the knower, then it would indeed be puzzling to encounter admonitions, reproaches, and confessions pertaining to the character of the investigator strewn among descriptions of the character of an investigation. Why does an epistemology need an ethics? But if objectivity

> and other epistemic virtues were intertwined with the historically conditioned person of the inquirer, shaped by scientific practices that blurred into techniques of the self, moralized epistemology was just what one would expect (2007, 39).

Prior to the emergence of the concept of objectivity in the mid-nineteenth century, Daston and Galison argue that Enlightenment scientists involved in image creation held an epistemic virtue of "truth-to-nature." To achieve truth-to-nature, careful observation, patience, good memory and a "talent to extract the typical from the storehouse of natural particulars" were required on the part of the investigator (58). Truth-to-nature presumed that variability existed in the objects of inquiry and it was up to the investigator to make sense of it. By the mid-nineteenth century, however, variation in natural objects "shifted inward, to the multiple subjective viewpoints that shattered a single object into a kaleidoscope of images" and scientific image-making was now thought to require "a set of procedures that would, as it were, move nature to the page through a strict protocol, if not automatically" (121). Mechanical objectivity came to the fore and was intended to exclude the self from inquiry.

As mechanical objectivity was discovered to be insufficient for the creation of images, *trained judgment* assumed a greater role. Student training consisted of "internalized and calibrated standards for seeing, judging, evaluating, and arguing" and "the scientist of the twentieth century entered as an expert, with a trained eye that could perceive patterns where the novice saw confusion" (328). Trained judgment emphasizes honed perception, skill in recognition, and interpretation in image creation in order to exceed the objectivity made possible by mechanical procedures. Unlike the ethically virtuous "sage" who served as the ideal in nineteenth century concepts of truth-to-nature, the scientist is now a trained expert in whom emotional, ethical, and social dimensions of intellectual virtue are suppressed in favour of a quantitative and perceptual-interpretive understanding of judgment. The self returns to objectivity, but is suppressed. We are left somewhere between the objectivity of persons and the objectivity of method.

A second challenge for understanding objectivity in virtue epistemic terms is that virtue epistemic concerns are generally left implicit in contemporary philosophical and historical discussions of science and science policy. For example, concerns about intellectual virtue underlie Don Howard's (2009) analysis of weaknesses in public discourse about science and the failure of philosophers of science to participate in that discourse. One reason Howard thinks philosophy of science has been irrelevant to public discourse about science is that it erroneously treats the relationship between the mind of an individual scientist and the world as the most important epistemic relationship. This view, he argues, ignores social dimensions of scientific inquiry and emphasizes narrow technical problems. Instead of this, he calls for a philosophy of science that takes seriously motives, values, and sociality in science. He argues that the consideration of motives invites "healthy scepticism;" that reasonableness "is not modeled by inductive logic alone;" that philosophers of science need to engage questions of morality and justice within their epistemologies; and that philosophers of science should help us to "distinguish the crackpot from the scientist who is "thinking outside the box"" (205–207). Implicit in each of these claims are matters of intellectual virtue. Intellectual virtue is needed for good motives, healthy sceptical attitudes, and crackpot identification.

Questions of intellectual virtue and objectivity are also implicit in Naomi Oreskes and Eric Conway's (2010) *Merchants of Doubt*, a fascinating history of a select group of American scientists, industries and think tanks who worked together to mislead the public about the harms of tobacco, secondhand smoke, acid rain, ozone depletion, global warming, and pesticides. One can read Oreskes and Conway's history as a thoroughly documented account of intellectual vice. To mislead the public, the scientists and organizations involved made use of many fallacies of reasoning, including red herrings, straw arguments, poisoning the well, persuasive definitions, phony facts, and false histories. Particularly interesting is that those involved high-jacked the language of objectivity and intellectual integrity in order to undermine objectivity and intellectual integrity. Significant issues of intellectual virtue are therefore implicit in Oreskes and Conway's account of biased science and policymaking about science.

The implicit nature of intellectual virtue in such accounts indicates that it will take work to excavate and understand its roles. But the fact that virtue epistemological concerns about objectivity are present, and that there is conceptual movement between the objectivity of persons and methods, suggests that the objectivity of persons is a significant issue. Why, then, is there little explicit conceptual, normative, and empirical work on the relations between virtue and the objectivity of persons? There are several possible reasons. The first concerns the relation of subjectivity and detachment to objectivity. Subjectivity is typically regarded as the objectivity's opposite, and thus one way to increase objectivity is to "detach" oneself from the process and object of inquiry. As Daston and Galison point out, the view that objectivity involves "the suppression of some aspect of self," is commonly held (2007, 36). Because intellectual virtues are frequently assumed to be personal, that is, as part of the minds of individual scientists, they may be implicitly associated with subjectivity. And given this association, intellectual virtues might not be considered sufficiently robust to act as objectivity-enhancing standards of scientific reasoning.

Subjectivity, however, is not the vicious opposite of virtuous objectivity. And "suppression" of some aspect of the self poorly describes the subjective interrelationships of belief, desire, emotion, reasoning, and character involved in the production of objective research. On a virtue epistemological account, objective persons have the *right kinds* of subjective experience: they are self-aware, sufficiently self-critical, intellectually honest, able to manage their emotions well, and so on. The difficulty with tying subjective suppression or detachment to objectivity is that objective people are good at regulating inquiry and maintaining their commitment to salient and accurate information about reality in complex value-laden circumstances. Subjective detachment might actually signal weak value-management and self-regulation skills; the self detached from emotional, social and normative concerns will not be able to reason virtuously.

This interpretation of virtuous subjectivity is supported by Douglas's view that social and ethical values are needed to set burdens of proof in scientific evaluation of evidence. She points to Richard Rudner's argument that

> [i]n accepting a hypothesis the scientist must make the decision that the evidence is sufficiently strong or that the probability is sufficiently high to warrant the acceptance of the hypothesis. Obviously, our decision regarding the evidence and respecting how strong is "strong enough," is going to be a function of the importance, in the typically ethical sense, of making a mistake in accepting or rejecting the hypothesis (Rudner 1953, 2).

Rudner argues that the concept of scientific objectivity thus needs revision. He says that "[o]bjectivity for science lies at least in becoming precise about what value judgments are being and might have been made in a given inquiry" (6). Intellectually virtuous reasoners understand how to adjudicate between relevant and irrelevant values and when and where to use those values. Carefully regulating the role of values in science and being precise about our value judgments requires fairmindedness, intellectual humility, intellectual courage (especially for transformative criticism), open-mindedness, and resiliency. Intellectual virtue is essential to the process of assessing value judgments in relation to evidence. Thus, we need to move from thinking of epistemic subjectivity in terms of bias to thinking of it in terms of intellectual virtue. Doing so suggests a solution to the "double bind" identified by Oreskes and Conway wherein objectivity requires scientists to "keep aloof from contested issues" but this aloofness prevents them from helping others learn "what an objective view of the matter looks like" (2010, 264).

A second reason why there is little explicit work on intellectual virtues and objectivity concerns vagueness about the distinction between justified true belief and understanding. One criticism virtue epistemologists make about traditional epistemology is that it focuses on individual beliefs and tends to ignore contextual factors, such as virtues and values, that are required for understanding. Zagzebski says:

> making the single belief state of a single person the locus of evaluation is too narrow. For one thing, it has led to the neglect of two epistemic values that have been very important in the history of philosophy: understanding and wisdom (1996, 2–3).

Without understanding, there is no way to distinguish between trivial and salient true beliefs. Salience is determined contextually through complex evaluative processes. Virtue epistemology, which is organized around the concept of understanding rather than knowledge as justified true belief, thus enables us to make distinctions between trivial and salient beliefs. And the ability to determine salience accurately is important for objectivity (Howes 2012).

The emphasis on understanding is part of the reason why virtue epistemologists shift the focus of epistemic analysis to agents. As Heather Battaly explains, virtue theories

> take the epistemic virtues and vices—types of agent-evaluation—to be more fundamental than any type of belief-evaluation. Accordingly, virtue theories in epistemology define belief-evaluations—justification and knowledge—in terms of the epistemic virtues, rather than the other way around (2010, 2).

From this standpoint, the scientists who sought truth-to-nature and trained judgment are quite alike. They each conceptualize *understanding* in their emphasis on grasping order, relevance, and patterns that exceed the mere collection of true beliefs. In truth to nature, the natural philosopher must extract the typical from the various; in trained judgment the scientist sees patterns emerging from the confusion. Each aligns with the virtue epistemological emphasis on understanding as opposed to traditional epistemology's emphasis on justified true belief. Accounts of objectivity that focus on the objectivity of individual claims may thus be inherently inadequate. Instead of such "static evaluations," Christopher Hookway argues that epistemology is better served by focusing on how character, habits, and reflection give us confidence in our inquiries and deliberations, than on puzzles about justified true belief or scepticism. He suggests we therefore address the evaluation and regulation of "activities of inquiry and deliberation" (2003, 193). Expanding the focus of epistemology may thus make issues of objectivity and intellectual virtue more visible.

A third reason that matters of intellectual character remain implicit in analyses of objectivity is simply that we lack tools for thinking about it. We lack these tools for a variety of reasons. The first is that we generally develop and exercise intellectual virtues tacitly. Nancy Daukas points out that

> our epistemic activities and attitudes are 'framed' by a usually unarticulated, continually evolving 'sense' of the status of our (also continually evolving) epistemic character . . . and inflected by our continual, often tacit, 'sense' of the epistemic status of others . . . (2006, 112).

We are not always "self-consciously assessing whether we are accurately representing our epistemic competencies, or explicitly deliberating about whether or not to extend the epistemic principle of charity to others" (112). Moreover, we talk about intellectual virtue with an "impoverished and confused vocabulary" and we do not have "as firm or precise a prephilosophical grip . . . on the specifically intellectual virtues as we do on the ethical virtues" (Riggs 2010, 173, 174). As Roger Crisp observes,

> there is no analogue to common-sense morality in epistemology. We do not, it might be suggested, set out explicitly to teach our children epistemological principles in the way that we teach them moral principles (2010, 33).

We might also avoid making detailed epistemic evaluations of others because we are unclear about the social and moral rules involved in doing so. As Nancy Sherman and Heath White explain:

> we openly talk about people's raw smartness or cleverness, their diligence or laziness, their conscientiousness or sloppiness. But do we freely talk about their zeal or cautiousness, their impetuousness or diffidence, their passion or lack of engagement? We think less so, and it probably has to do with the fact that we think that we are overstepping etiquette boundaries when we engage in these kinds of assessments (2003, 44).

Virtue-based evaluations might also make us feel socially or morally uncomfortable because of their potentially "self-centred" nature. As David Solomon points out, one of the principal objections to moral virtue is that "[i]nstead of my needing to be good in order to benefit others, I am required to be the sort of person who benefits others in order to be fulfilled myself. Virtue seems to be itself compromised by a kind of vanity or prissiness" (2003, 74). Some may worry that intellectual virtue is similarly compromised by vanity or prissiness. Intellectual virtues may also uncomfortably call to mind an elitism linked to the early modern idea that only "gentlemen of science" were epistemically trustworthy (Shapin and Schaffer 1989). We might also feel uncomfortable with the apparent self-centredness of intellectual virtue because it seems antithetical to objectivity in light of its historical conceptualization as suppression of self.

Despite such concerns, however, there are good reasons to develop the social, moral and epistemic tools needed to examine openly the relations between intellectual virtue and objectivity. Our ability to cultivate and promote objectivity could increase substantially and there may be other epistemological benefits as well. For example, to help people succeed intellectually it appears to be better to draw attention to the effort they put in when they get something right (Moser et al. 2010) than to praise them for being "smart." It turns out that identifying people as "smart" might actually make them less smart (Mueller and Dweck 1998). This suggests that evaluating the virtues and strategies people use to reach an answer is more important to epistemic success than focusing on whether the people involved are "smart" or not.

I have now argued that matters of intellectual virtue are close at hand in accounts of scientific objectivity and public controversies about science. Such accounts shift between the objectivity of persons, claims, and procedures, and often implicitly rely upon virtue epistemological concepts. I have also outlined several reasons why matters of intellectual virtue are generally submerged. While these problems are challenging, they also show that intellectual virtues hold promise for expanding our understanding of objectivity. With this in mind, I turn to another aspect of this virtue epistemological account of objectivity: its inherently social nature.

9.3 Community Intellectual Virtue: Objectivity and Epistemic Trustworthiness

When intellectual character is considered explicitly, it is usually in the context of *individual* achievement. We praise or blame individuals for intellectual honesty or dishonesty. We assume that the "acquisition of knowledge, while by no means a strictly solitary enterprise, is generally *more* solitary (or capable of being so) than the acquisition of moral goods" (Baehr 2010, 211). And intellectual virtues can themselves be thought of in individualistic terms: they are "constitutively good-making, counting as such towards our being good reasoners in this or that capacity" (Garcia 2003, 107). Virtue and social epistemologists argue, however, that we often overlook the social dimensions of epistemic well-being, that is, those aspects of well-being related to the knowledge that we have, our ability to discover and learn truths, and the reliability of our minds. In this section, then, I challenge the view that good intellectual character principally reflects individual achievement and argue for the sociality of intellectual virtue. Bringing these arguments together with research about the virtue of epistemic trustworthiness and the development of epistemic trust in children, I claim that our current educational, research and public institutions could do more to provide the ingredients needed for the development and promotion of objectivity.

There are two general reasons why virtue epistemologists think that "a social context is intrinsic to the nature of a virtue as traditionally understood" (Zagzebski 1996, 44). First, virtuous activity is determined contextually and the social roles of agents are an essential part of this context. Sarah Wright says:

> virtue is a mean between extremes, and because the location of the mean depends on the social roles of the person who must act, there is no way to be a courageous person *simpliciter*; there is no mean in a vacuum. Similarly, there is no way to exhibit the epistemic virtues except within a social role. One is only courageous *in the role of a bystander*, or epistemically careful *in the role of the doctor* (2010, 106).

Social roles are also important because intellectual virtues are acquired in part by following the examples set by role models. Sherman and White argue that

> [i]ntellectual virtue will itself involve the example following and habituation of moral virtue: inspiration by role models will be important as will be learning through critical practice the habits of careful reasoning, methodological argument, and assessment of data. We study modes of reasoning and research, but we also practise them and model them (2003, 39).

A second and related reason why virtue epistemologists emphasize sociality is that communities shape our cognitive character through epistemically virtuous or vicious relations. Christine McKinnon (2003) argues that we need to attend carefully to the fact that we form our cognitive selves in response to the ways others engage with our explanations, justifications, and arguments. The development of cognitive character also requires that we successfully manage our emotions and desires and this too depends on characteristics of our epistemic communities. Amy Coplan argues that to achieve good cognitive character we must train

> our emotions so that they can be conditioned, through practice and experience, to track appropriate things. Our education, including the stories we are told and the music we listen to, our environment, and the company we keep all matter greatly ... since they can prevent or encourage virtue ... We must take more seriously the facts of our embodiedness and sociality, and come to terms with the ways in which our sensory experiences and social interactions influence our emotions, and thus our behavior, our thoughts, and our values (2010, 148).

The sociality of intellectual virtue should not, however, be taken to mean that intellectual virtues are relative to one's local epistemic community. As McKinnon points out,

> the most commendable kinds of epistemic acts [are] those performed by agents who exercise in a cognitively responsible manner those belief-acquiring dispositions or faculties deemed to be reliable *and* who are motivated to do so by a desire to know how things are in the world (2003, 245).

A virtuous epistemic agent exercises skills deemed reliable by others in their epistemic community and is motivated by that community to commit to salient and accurate information about reality. Moreover, reliability is also evaluated in relation to all epistemic communities, not just those closest to the inquirer.

The social nature of intellectual virtue is thus quite important for understanding how objectivity is developed and exercised in our epistemic communities. The virtue of epistemic trustworthiness provides a particularly illustrative example. Daukas argues that epistemic trustworthiness is "a *social* epistemic virtue ... insofar as it depends on appropriate attitudes towards others, as well as toward oneself, as epistemic agents" (2006, 113). She also points out that "the character traits and skills required for epistemic trustworthiness are developed over time, through interaction with others in the context of normative social practices" (2006, 113–14). This underscores McKinnon's view that we "learn whom and what to trust. And, we do so, in part, by learning about our own and other's cognitive selves" (2003, 249). Epistemic and moral trust are here intertwined—and each are clearly social in nature.

Epistemic trustworthiness is also tied to personal objectivity, and this gives us reason to think of objectivity in terms of social epistemic virtue. Scheman says that "[c]entral to what we do when we call an argument, conclusion, or decision "objective" is to recommend it to others, and, importantly, to suggest that they ought to accept it, that they would be doxastically irresponsible to reject it without giving reasons that made similar claims to universal acceptability" (2001, 24). These recommendations and epistemic responsibilities related to objectivity depend on epistemic trustworthiness. As Douglas says, all types of objectivity involve a "sense of strong trust and persuasive endorsement, this claim of "I trust this, and you should too"" (2009, 116). Epistemic trust in turn seems to depend on our confidence that others value objectivity.

This contrasts how sociality and epistemic trust in others is generally regarded in traditional epistemology. Naomi Scheman argues that in such accounts, if epistemic dependency

> is acknowledged at all, it is to mark it as something that has, intellectually, to be superseded: The ground that we in fact traversed in our parents' arms has to be retraversed under our own power, in order to prove that the place where we have ended up is one we could and would have reached had we done the entire journey under the direction of our own adult intelligence (2001, 41–2).

Dependency is denied, and with it epistemic trust of others. But this is a mistake. In the first place, we are never truly epistemically autonomous. And even if we were, highly individualistic understandings of epistemic autonomy would not guarantee epistemic trustworthiness any more than sociality. As Zagzebski argues, we do not actually have evidence that we ourselves are generally "more trustworthy than other people" (2007, 253–54). Moreover, we have evidence that we are sometimes *less* epistemically trustworthy than others. Given this, Zagzebski does not think we should prefer individualistic idealizations of epistemic autonomy over idealizations of epistemic dependence on trustworthy others. That we *do* prefer idealizations of autonomy suggests that we have an underlying problem with social epistemic trust. This problem is serious for, as Oreskes and Conway note, trust underlies all relationships: we "trust other people to do things for us that we can't or don't want to do ourselves" (2010, 272).

A further link between epistemic trust, social intellectual virtue, and objectivity concerns evidence that very young children evaluate character holistically when forming epistemic trust (Koenig and Harris 2008). They do not form epistemic trust solely by evaluating the reliability of claims people make. Epistemic development thus appears to align more closely with the agent-evaluation model of virtue epistemology than the belief-evaluation model of traditional epistemology. And if it does, detaching objectivity from intellectual character and social context may interfere profoundly with the maintenance of objective, epistemically trustworthy individuals and communities.

The failure to relate explicitly intellectual character with objectivity also helps to explain why epistemic dependence on trustworthy others is unrecognized as an ideal in traditional epistemology. Because in current epistemic communities we either lack explicit information about intellectual character or are poorly skilled in its evaluation, one of the main ingredients required for epistemic trust is missing. As a result, we become less trusting of others and the less we trust others, the more attractive individualistic ideals of epistemic autonomy become. In an effort to save objectivity, we place responsibility for objectivity squarely onto the autonomous inquirer and try to bolster it by suppressing self-characteristics and encouraging detachment. This does not work, so we then locate objectivity entirely in methodologies we think we can trust because they are not people. This does not work very well either (though it often helps).

Given the social nature of intellectual virtues and the requirements for epistemic trust, it is likely wrong to think that practices that forbid scientists to include anything of themselves in their published research promote objectivity. Such practices send matters of intellectual virtue underground and hide information that we might otherwise use to evaluate objectivity and epistemic trustworthiness. Even if someone accepts that a certain scientific procedure is objective, they may reject results based upon it because they do not have what they need to trust epistemically those using the procedure. And in mistrustful situations, people are more vulnerable to intellectual vices that might lead to the premature rejection of scientific research employing objective methodologies. This phenomenon may be especially damaging to epistemic trust in the public domain wherein most individuals, in addition to their exclusion from scientific epistemic communities, are unfamiliar with academic standards of inquiry and lack the specialization needed to understand publicly available academic research.

Thus, Oreskes and Conway's sensible observation that if scientists do not get involved in public controversies about science no one will know what objectivity looks like provides support for conceptualizing objectivity in terms of community intellectual virtue. Though the exclusion of the public from scientific communities is a difficult problem to address, this need not stop us from better integrating academic work, public inquiry, and policymaking with social support for the formation of objective intellectual character. Explicit instruction in intellectual virtue and an explicitly positive valuation of intellectual virtues in the public realm would provide such support. But such considerations suggest that we need to study more thoroughly community intellectual virtue and its relation to the objectivity of persons. We need to find out what kinds of individual and community-level approaches best support the objectivity of persons. We need to understand better how intellectually trustworthy, objective people develop.

9.4 Objections

The first objection I consider concerns implicit bias. Contemporary psychological research shows that implicit biases against disadvantaged groups can be activated by virtuous efforts to counteract them (Fine 2006). It seems that when people explicitly claim that they are unprejudiced, or are asked to assess their own objectivity, their objectivity *decreases* (Lee and Schunn 2011). These counterintuitive results suggest that the social promotion of intellectual virtue might encourage intellectual vice instead.

To counteract the effects of implicit biases, Carole Lee and Christian Schunn (2011) recommend that we introduce a specific criterion for diversity in epistemic communities wherein individuals would "hold different implicit assumptions about the cognitive authority of individuals associated with different stereotypes" (364). They suggest further that

> individuals with different background beliefs might be in a better position to render visible and to critique the negativity of others' evaluative styles. This would require that procedurally objective communities make efforts to foster forms of diversity bearing not on the *content* of theories but on the cultural beliefs and norms of the community itself (365).

This is a reasonable remedy and one to which the structured promotion of community intellectual virtue could contribute.

We have, for example, a number of intellectual virtues in our epistemic toolbox, such as intellectual honesty, fairmindedness and intellectual courage, that require and enable us to challenge implicit biases. If implicit biases fool even the egalitarian-minded, then intellectual virtue demands that the egalitarian-minded find ways to outsmart them. Cordelia Fine points out that "implementation intentions" such as telling yourself not to stereotype help to reduce stereotyping. She says that "people who form egalitarian implementation intentions of this sort are happily impervious to the usual unconscious effects of stereotype priming" (2006, 200). Seeing the potential of implementation intentions, epistemic communities valuing fairmindedness could adopt them strategically.

A second objection to my view is that intellectual virtue is too weak to promote objectivity given certain truths about human irrationality. As Fine observes,

> psychology texts like to make a few half-hearted suggestions as to how we can combat the mulish tendencies of our minds. "Entertain alternative hypotheses," we are urged. "Consider the counterevidence." The problem, of course, is that we are convinced that we are already doing this; it's simply that the other guy's view is absurd ... It is a sad fact that the research fully bears out the observation by the newspaper columnist Richard Cohen that, "The ability to kill or capture a man is a relatively simple task compared with changing his mind." (127)

In addition, scholars like Oreskes and Conway show just how easy it is for the epistemically vicious to manipulate intellectual virtue. If the *appearance* of intellectual integrity can be recruited to undermine intellectual integrity, perhaps virtues are simply too flimsy to counter such attacks.

But we could also argue that the reason people are so easily tripped up by the *appearance* of integrity is that they poorly understand intellectual integrity in the first place. It is easy to imagine people in communities that explicitly care about intellectual character resisting vicious epistemic manipulation. Moreover, the objection that virtues are too weak to defend us against intellectual manipulation relies on a rather individualistic understanding of virtue. The vices of politically powerful scientists might not be visible when they are considered a contextually, but the vice-laden goals of the epistemic community in which they reside might be easy to spot. Robert Roberts and Jay Wood point out that "intellectual carelessness tends to spread through a community" (2007, 232) and when it does it becomes more visible. We can take advantage of the visibility of community-level vice to create explicit standards of virtuous inquiry in industry and politics. The community virtue perspective could also be used to quell unjustified attacks on the credibility of scientists: it is harder to poison the well against individual scientists when the virtues of the epistemic community in which they work are explicitly upheld.

The third and final objection I consider concerns the fact that social practices of science can secure the production of accurate and salient information even if the scientists participating in those practices are epistemic jerks. Perhaps, then, intellectually virtuous people are epistemically unnecessary. But consider that at some point in the creation of these social practices at least someone virtuously cared about the epistemic good of securing accurate and salient information. Even jerks might care about this epistemic good and thus attain some minimal level of virtue. It helps to realize that intellectual virtue admits of degree. None of us is perfect and few, if any, of us are complete jerks.

We also need to distinguish between moral and intellectual character when considering knowledge production in vice-laden communities. While there is overlap, these two conceptions of character are not identical. You might be unflinchingly honest in your research but deceptive in your romantic life, or arrogant with your colleagues but impressively courageous about ideas. So while some vice-laden communities successfully create accurate and salient information, their vices may be generally moral, and their virtues generally intellectual. While moral and intellectual virtues are more frequently found together, communities of intellectually upstanding jerks are obviously possible.

It is also worth considering that certain intellectual vices may be useful in dysfunctional epistemic environments. Roberts and Wood (2007), for example, consider the possibility that intellectual vanity and arrogance might lead in some cases to more epistemic goods than intellectual humility. Such situations could be explained

> by reference to some other fault in the individual or some corruption in the epistemic environment. Perhaps individuals need vanity as a motivation, because their upbringing does not instill in them an enthusiasm for knowledge as such. Or we might locate the pathology socially—say, in the fact that the whole intellectual community is warped by vanity and arrogance, hyper-autonomy and unhealthy competitiveness, so that in that fallen community some vices actually become more functional than their counterpart virtues. (252)

Because we learn to reason in very imperfect epistemic communities, intellectual vices may well lead to accurate and salient information and perhaps knowledge. This is analogous to cases wherein moral vices help people to function and survive in uncaring or abusive environments.

Finally, while intellectually virtuous and vicious communities might both produce accurate and salient knowledge of the world, intellectually virtuous communities will generally produce more epistemic goods and fewer epistemic evils. Intellectually virtuous communities are likely to have more time, energy, and support in the pursuit of understanding. Flawed reasoning and counterproductive social relationships will slow intellectually vicious communities. Roberts and Woods say that "in the long run, just about everybody will be epistemically better off for having, and having associates who have, epistemic humility" (2007, 251). This is surely true for other intellectual virtues as well.

9.5 Conclusion

In controversies about scientific research and policy, scientists and other academics are increasingly accused of biased "subjective" reasoning in cases where objective methods and strong empirical support undoubtedly exist. In such situations, various intellectual vices and inadequate ideas about objectivity are usually at play. To defend against unwarranted accusations and persevere intellectually against those who intentionally distort reality, it is therefore important that we explicate clearly the relations between objectivity, intellectual virtue, and community. Fortunately, because philosophers of science, social epistemologists, and virtue epistemologists are already concerned with the *process* of inquiry, they are in an excellent position to develop accounts of objectivity that are openly informed by intellectual virtue and social epistemic relationships. Their combined resources stand to contribute significantly to our understanding of objectivity and its promotion in diverse epistemic communities.

References

Baehr, Jason. 2010. Epistemic malevolence. In *Virtue and vice, moral and epistemic*, ed. Heather Battaly, 189–213. Malden: Wiley-Blackwell.

Baehr, Jason. 2011. *The inquiring mind: On intellectual virtues and virtue epistemology*. Oxford: Oxford University Press.

Battaly, Heather. 2010. Introduction: Virtue and vice. In *Virtue and vice, moral and epistemic*, ed. Heather Battaly, 1–20. Malden: Wiley-Blackwell.

Brown, Mark. 2009. *Science in democracy: Expertise, institutions, and representation*. Cambridge, MA: MIT Press.

Coplan, Amy. 2010. Feeling without thinking: Lessons from the ancients on emotion and virtue acquisition. In *Virtue and vice, moral and epistemic*, ed. Heather Battaly, 133–151. Malden: Wiley-Blackwell.

Crisp, Roger. 2010. Virtue ethics and virtue epistemology. In *Virtue and vice, moral and epistemic*, ed. Heather Battaly, 21–38. Malden: Wiley-Blackwell.

Daston, Lorraine. 1992. Objectivity and the escape from perspective. *Social Studies of Science* 22: 597–618.

Daston, Lorraine, and Peter Galison. 2007. *Objectivity*. New York: Zone Books.

Daukas, Nancy. 2006. Epistemic trust and social location. *Episteme: A Journal of Social Epistemology* 3: 109–124.

Douglas, Heather. 2009. *Science, policy, and the value-free ideal*. Pittsburgh: University of Pittsburgh Press.

Fine, Cordelia. 2006. *A mind of its own: How the brain distorts and deceives*. New York/London: W. W. Norton and Company.

Garcia, Jorge L.A. 2003. Practical reason and its virtues. In *Intellectual virtue: Perspectives from ethics and epistemology*, ed. Michael DePaul and Linda Zagzebski, 81–107. Oxford: Clarendon.

Hookway, Christopher. 2003. How to be a virtue epistemologist. In *Intellectual virtue: Perspectives from ethics and epistemology*, ed. Michael DePaul and Linda Zagzebski, 183–202. Oxford: Clarendon.

Howard, Don. 2009. Better red than dead—Putting an end to the social irrelevance of postwar philosophy of science. *Science and Education* 18: 199–220.

Howes, Moira. 2012. Managing salience: The importance of intellectual virtue in analyses of biased scientific reasoning. *Hypatia* 27: 736–754.

Koenig, Melissa, and Paul Harris. 2008. The basis of epistemic trust: reliable testimony or reliable sources? *Episteme: A Journal of Social Epistemology* 4: 264–284.

Lee, Carole, and Christian Schunn. 2011. Social biases and solutions for procedural objectivity. *Hypatia* 26: 352–373.

McKinnon, Christine. 2003. Knowing cognitive selves. In *Intellectual virtue: Perspectives from ethics and epistemology*, eds. Michael DePaul and Linda Zagzebski, 227–253. Oxford: Clarendon.

Moser, Jason S., Steven B. Most, and Robert F. Simons. 2010. Increasing negative emotions by reappraisal enhances subsequent cognitive control: A combined behavioral and electrophysiological study. *Cognitive, Affective, and Behavioral Neuroscience* 10: 195–207.

Mueller, Claudia, and Carol Dweck. 1998. Praise for intelligence can undermine children's motivation and performance. *Journal of Personality and Social Psychology* 75: 33–52.

Oreskes, Naomi, and Erik M. Conway. 2010. *Merchants of doubt: How a handful of scientists obscured the truth on issues from tobacco smoke to global warming*. New York: Bloomsbury Press.

Riggs, Wayne. 2010. Open-mindedness. In *Virtue and vice, moral and epistemic*, ed. Heather Battaly, 173–188. Malden: Wiley-Blackwell.

Roberts, Robert C., and W. Jay Wood. 2007. *Intellectual virtue: An essay in regulative epistemology*. Oxford: Oxford University Press.

Rudner, Richard. 1953. The scientist qua scientist makes value judgments. *Philosophy of Science* 20(1): 1–6.
Scheman, Naomi. 2001. Epistemology resuscitated: Objectivity and trustworthiness. In *Nancy Tuana and Sandra Morgen,* eds. *Engendering rationalities*, 41–42. Albany: SUNY Press.
Shapin, Steven, and Simon Schaffer. 1989. *Leviathan and the air pump. Hobbes, boyle and the experimental life*. Princeton: Princeton University Press.
Sherman, Nancy, and Heath White. 2003. Intellectual virtue: Emotions, luck, and the ancients. In *Intellectual virtue: Perspectives from ethics and epistemology*, ed. Michael DePaul and Linda Zagzebski, 34–53. Oxford: Clarendon.
Smith, Tara. 2004. Social objectivity and the objectivity of values. In *Science, values, and objectivity*, ed. Peter Machamer and Gereon Wolters, 143–171. Pittsburgh: University of Pittsburgh Press.
Solomon, David. 2003. Virtue ethics: Radical or routine? In *Intellectual virtue: Perspectives from ethics and epistemology*, ed. Michael DePaul and Linda Zagzebski, 57–80. Oxford: Clarendon.
Wright, Sarah. 2010. Virtues, social roles, and contextualism. In *Virtue and vice, moral and epistemic*, ed. Heather Battaly, 95–113. Malden: Wiley-Blackwell.
Zagzebski, Linda. 1996. *Virtues of the mind: An inquiry into the nature virtue and the ethical foundations of knowledge*. Cambridge: Cambridge University Press.
Zagzebski, Linda. 2007. Ethical and epistemic egoism and the ideal of autonomy. *Episteme: A Journal of Social Epistemology* 4: 253–254.

Chapter 10
A Plurality of Pluralisms: Collaborative Practice in Archaeology

Alison Wylie

Innovative modes of collaborative practice are transforming archaeology, in the process generating examples of methodological and conceptual pluralism that are proving to be powerful catalysts for creative insight. What I have in mind are not the interdisciplinary collaborations that have long been a staple of archaeological inquiry but, rather, intellectual as well as pragmatic partnerships with descendant communities, especially Aboriginal and Indigenous communities. While the impetus for these collaborations is often, in the first instance, moral and political – they arise from demands for respect, reciprocity, consultation – increasingly they are also robustly epistemic. Descendant communities and archaeologists jointly define the research agenda and pursue programs of historical, archaeological inquiry together, sometimes bringing strikingly different conceptual schemes and methodologies to bear on questions of common concern.

This growing tradition of collaborative practice has provoked sharply critical Science Wars style rebuttals from skeptics who decry the compromises they believe it entails for properly scientific archaeology. They insist that the kinds of pluralism endorsed by the advocates of these projects cannot but undermine objectivity conceived in terms of the traditional ideal that science, proper, should be value free[1]; they open the door to parochial, sometimes highly politicized interests and to non- or anti-rational values that are anathema to scientific inquiry. High profile reactions

[1]The history and contemporary formulations of this "value-free ideal" are usefully explicated by Douglas (2009, 44–66) and by Lacey (2005, 23–27, 59–80), and assessed by contributors to Kincaid et al. (2007), and Machamer and Wolters (2004).

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of this kind within archaeology include denunciations of repatriation as an assault on reason that threatens to roll back all the accomplishments of the Enlightenment (Clark 1996, 1998),[2] and a challenge to the very idea that an Indigenous culture could sustain an epistemically distinctive standpoint, unless cultural difference is reified and indigeneity is understood in perniciously essentialist terms (McGhee 2008).

Within philosophy, Paul Boghossian begins *Fear of Knowledge* (2006) with discussion of a *New York Times* article on debate about the Native American Grave protections and Repatriation Act (NAGPRA) that characterizes the struggle over access to and ownership of human remains as a conflict between traditional Native American beliefs about tribal origins and the conclusions drawn from scientific investigation of the archaeological record (Johnson 1996).[3] Boghossian is chiefly concerned by the stance taken by two archaeologists cited in this article – Roger Anyon and Larry Zimmerman – who resist this stark opposition and insist that Native Americans have interests and insights that archaeologists should take seriously. The quotes that Boghossian extracts from this article exemplify, on his reading, a self-defeating turn to postmodern relativism that he finds pervasive in the contemporary social sciences and humanities (2006, 1–3): Anyon is quoted as saying that "science is one of many ways of knowing the world . . . [the Zuni world view] is just as valid as the archaeological viewpoint," and Zimmerman as conceding that he does "reject science as a privileged way of seeing the world" (Boghossian 2006, 1–3; Johnson 1996). On Boghossian's account they are in the grip of a "doctrine of equal validity" from which it follows that there is no basis for choosing among alternative world views (2006, 2); we are left with Rorty's frank ethnocentrism (1991) as the only grounds on which we can endorse our own "way of seeing the world."

Boghossian does not consider the archaeological debate in any more detail; it stands as a negative object lesson that motivates *Fear of Knowledge* as a whole, an example of capitulation to an ill-considered relativism that has "achieved the status of orthodoxy" in many fields (2006, 2; see also Koskinen 2011, 105). Given his subsequent analysis, however, Anyon's and Zimmerman's statements would seem to represent the second of three kinds of constructivism that Boghossian means

[2]In these articles, the first entitled "NAGPRA and the Demon Haunted World," Clark was particularly concerned with impact of the Native American Grave Protection and Repatriation Act (NAGPRA), signed into law in the U.S. in 1990. For a more detailed account of this analysis of Clark's critique, see Wylie (2005, 63–65).

[3]Koskinen (2011) addresses this aspect of Boghossian's critique of relativism with attention to ethnographic as well as archaeological examples. She argues that the kinds of statements Boghossian cites as evidence of widespread endorsement of a self-defeating epistemic relativism in the social sciences and humanities are, in fact, more plausibly construed as a much less threatening methodological relativism: a stance that involves withholding epistemic judgment of unfamiliar, apparently irrational beliefs, rather than embracing a "doctrine of equal validity" (Koskinen 2011, 105). I am similarly skeptical of Boghossian's reading of these claims but make a case here for delineating a spectrum of different degrees and types of pluralism that are taking shape in archaeological practice; I do not believe they are all examples of methodological relativism, but do not pursue this line of argument here.

to reject: constructivism about justification.[4] This he characterizes in terms of a challenge he calls "Encounter" (ENC): would our system of epistemic norms be called into question if we were to encounter a genuine alternative to it (96–102)? Boghossian sets some constraints on what could count as an alternative. It must conform to "norms of coherence . . . [which] flow directly from the very nature of an epistemic system" (98); it must have a "proven track record" of epistemic success (101); and it must generate claims that contradict those ratified by our system of epistemic norms (91). He allows that we might legitimately come to doubt our own "classical picture of knowledge"[5] (19) if a society we recognized as having "much more advanced science and technological abilities" proved to follow different epistemic norms (101). But he finds none of the cases presented by would-be relativists compelling. He considers just two: Rorty's treatment of the confrontation between Cardinal Bellarmine and Galileo, and the well worked-example of the apparent failure of the Azande to respect modus ponens. In the end, he argues, Cardinal Bellarmine was really operating on the same principles as Galileo in adjudicating the relevance of biblical evidence for astronomy (104), and the Azande case most likely reflects errors in the translation of logical operators; they must mean something different by conditional, "if . . . then" statements than we do (108). From the failure of these cases to instantiate ENC, Boghossian infers that no serious (coherent, action-guiding) contender will diverge significantly from our own epistemic system. Consequently, "we have no option but to think there are absolute, practice-independent facts about what beliefs it would be most reasonable to have under fixed evidential conditions" (110). Moreover, he holds that the norms embodied in science and captured by the "classic picture of knowledge" are a good approximation to these facts.

My interest here is not so much the specifics of the arguments by which Boghossian first conjures and then meets the imagined threat of corrosive relativism, but in what they obscure. They are a particularly stark example of an "anxious nightmare" that haunts contemporary philosophy by which, as Alan Richardson describes it, any weakening of commitment to epistemic foundationalism is presumed to carry the threat of mutual incomprehensibility and entail an inescapable slide into epistemic nihilism (2006, 9). I am in substantial agreement with Richardson and the Minnesota pluralists in regarding this conundrum – embodied in especially uncompromising terms in Boghossian's framing of ENC – as an artifact of the

[4]The other targets of his critique are constructivism about facts and about the prospects for rationally explaining the beliefs we hold.

[5]Boghossian characterizes this "classic picture of knowledge" as a "broad consensus among philosophers, from Aristotle to the present day, on the nature of the relationship between knowledge and the contingent social circumstances in which it is produced": that what we "take ourselves to know" is not, in fact, dependent upon the conditions of its production (2006, 19). This "independence of knowledge from contingent social circumstances" turns on three claims: that "many facts about the world are independent of us" (20); that facts can have standing as evidence that justifies belief in the truth of a claim independent "of our social makeup" (21); and that evidence alone can sometimes justify belief – social conditions do not necessarily figure in explanations for true beliefs (21).

terms in which philosophers have theorized knowledge (Kellert et al. 2006). It may be clever philosophy, but it radically misrecognizes the complexity of actual research practice and ignores the contingency of our evolving epistemic norms. In the process, it rules out of consideration the kinds of critical challenge that arise from more prosaic forms of pluralism that are often a key source of creative insight and the impetus for ongoing refinement of this practice.

By contrast to the fear-mongering critiques of relativism that make for headline news, the archaeologists who take seriously the claims of Native Americans routinely argue that collaborative practice enriches their research practice in any number of ways, not only adding useful detail but generating new questions and forms of knowledge.[6] This more optimistic appraisal is evident in several statements by Zimmerman that are quoted in the *New York Times* article but not discussed by Boghossian. His observation about science not being privileged is prefaced by the assessment that there is need for “a different kind of science, between the boundaries of Western ways of knowing and Indian ways of knowing.” He continues: “That’s not to say [science] isn’t an important way [of seeing the world] that has brought benefit. But I understand that, as a scientist, I need to constantly learn.” (cited in Johnson 1996)

I contend that some of the most creative archaeological learning now taking place is in the context of collaborations that draw on the resources of a rich pluralism, and exemplify the best of what Helen Longino has described as transformative criticism (1990, 73–74); they can and do significantly improve archaeological practice empirically, conceptually, and methodologically. I will argue that when these projects succeed they powerfully illustrate the virtues of extending the cognitive-social norms of Longino’s proceduralist account of objectivity – specifically, her “tempered equality of intellectual authority” – beyond the confines of the scientific community (2002, 128–135). In the process, my aim is to bring into focus the diversity of pluralisms that are a source of creative insight in these projects. By no means do they all, or even often, exemplify the stark oppositions between science and nay-science that are the stuff of anxious philosophical and archaeological nightmares.

10.1 Demands for Accountability: Consent, Consultation, Reciprocity

Consider first the kinds of challenges to which archaeologists now respond. By the early 1970s a primary target of Native American activism (and, indeed, of Indigenous and Aboriginal activism around the world) was the desecration of sacred

[6]For one of the most recent and comprehensive discussions of these initiatives and the epistemic contributions of collaborative practice to archaeology, see Atalay (2012). And for a more general account of the ways in which pluralism can benefit science see Chang’s discussion of “Pluralism in Science” (2012, 268–284).

sites and burials by archaeologists: American Indian Movement (AIM) activists, tribal leaders, and traditionalists demanded rights of repatriation and reburial for individuals whose remains had languished in museums often for a century or more.[7] They make these claims on a number of bases: by appeal to civil rights and freedom of religion where treatment of the dead are concerned and, more generally, on grounds that the material that makes up archaeological record is part of a living cultural tradition – their cultural tradition. In this, Indigenous and Aboriginal peoples rejected the presumption that had animated archaeological (and anthropological) practice for well over a century: that indigenous peoples were disappearing, or had disappeared, and that the cultural history and traditions to be salvaged were significant not to a living community but as an element of world history or, often enough, natural history. Angry reactions to repatriation legislation of the 1990s make these underlying assumptions explicit: "ancient skeletons belong to everyone"; they are "the remnants of unduplicable evolutionary events [about] which all living and future peoples have the right to know" (Ubelaker and Grant 1989, 260; cited in Thomas 2000, 209). By extension, no "living culture, religion, interest group, or biological population" has the right to restrict the research mandate of scientific experts who have the necessary skills and knowledge to make the best use of surviving "remnants" as evidence (Ubelaker and Grant 1989, 260; cited in Thomas 2000, 209–210).

In the context of centuries-long political and legal struggle these claims could hardly be more provocative. Consider Laurie Anne Whitt's classic assessment of strategies of appropriation by which Indigenous peoples had been dispossessed of their land, and then their material culture, music, intellectual property, and now their "genetic wealth and pharmaceutical knowledge" (1998a, 149).[8] Beginning with territorial rights, Whitt argues that appropriation turns on two reinforcing legal claims. The first is a declaration that the land Europeans encountered in the Americas (and elsewhere) was unoccupied – that it was *terra nullius* and therefore in the public domain – usually by fiat of European definitions of what counts as occupation, or otherwise by the forcible displacement of Aboriginal peoples. This opens the way for the conversion of definitionally public property into alienable, privately held property. Whitt observes that "the politics of property has never been confined to land" and shows how the same logic operates in a range of other domains. The declaration by archaeologists and physical anthropologists that Indigenous skeletal remains and cultural material are a "human heritage," and that the interests of science should determine their disposition, is immediately recognizable as yet another instance of this two-step move to seize and privatize Indigenous property, tangible and intangible, the second step being justified in this case by appeal to the specialized expertise and objectivity of the scientific community.

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[7]For an accessible history of this controversy see Thomas (2000), and for a trenchant assessment of where the debate stands, Watkins (2000). See also contributions on the ethics of repatriation to Young and Brunk (2009) by Youngblood Henderson and by Scarre, and to Scarre and Coningham (2013) by Thompson and by Zimmerman. This summary is based on Wylie (1999, 2005).

[8]See also Whitt (1998b, 254–255), and discussion in Nicholas and Wylie (2013, 201).

Legal counter-arguments to the effect that traditional territories, material culture, music, genetic information and bio-medical samples belong to identifiable descendant communities – that they cannot be treated as public domain, as without claimant – are, of course, the stuff of ongoing land claims and use rights struggles in a great many former settler and colonial contexts. In the case of archaeology, they have been the impetus, in many jurisdictions, for legislation that mandates repatriation, like NAGPRA in the United States. This regulatory framework and the political activism that brought NAGPRA into being is one of the most visible and highly formalized but by no means the only challenge to longstanding attitudes and ways of doing business that archaeologists now face. In the last 25 years demands for accountability have fundamentally transformed the conditions under which archaeology is now practiced.

This sea change has generated responses that fall along a continuum, ranging from hostile resistance at one extreme, through grudging compliance with requirements of consent and consultation, to a range of creative, collaborative forms of practice in which control over archaeological goals and products, conduct and authority is redistributed among partners.[9] The latter responses all involve some reconfiguration of disciplinary authority structures but they have quite different epistemic and methodological implications.

At a minimum, what is at stake in demands for accountability is that archaeologists respect the interests and sensibilities of descendant communities, even if they don't credit them with epistemically compelling norms of justification. This includes requirements, ethical and sometimes legal, that archaeologists actively consult with Indigenous communities whose heritage they study and, increasingly, negotiate terms of access and consent for any research they undertake.[10] Many tribes now run their own review process, vetting research proposals and requiring researchers to sign Memoranda of Agreement or Understanding that may include provisions for control not only over tangible and intangible cultural heritage, but also over the use and distribution of the results of archaeological research (see Atalay 2012, 130–134). More generally, archaeologists are expected to practice archaeology in culturally sensitive ways. Guidelines for such practice may include proscriptions against destructive testing or the excavation of sacred sites and burials; they may call for blessing or cleansing ceremonies; and they typically require that archaeologists respect Indigenous cultural norms of access to and publicity about special objects, sites, and traditional knowledge. Increasingly they also include requirements of reciprocity and participation. Archaeologists are expected to give something back to the communities whose heritage they study: at a minimum, plain language reports that make research results accessible to the community; by extension, education and outreach programs; more ambitiously, capacity-building

[9] For a more detailed account of these responses, see Nicholas and Wylie (2009, 2013).

[10] The second of eight "Principles of Archaeological Ethics" adopted by the Society for American Archaeology in 2009 requires "an acknowledgement of public accountability and a commitment to make every reasonable effort, in good faith, to consult actively with affected group(s), with the goal of establishing a working relationship that can be beneficial to all parties involved" ("Accountability," SAA 1996).

training and employment for community members. They also find themselves drawn into community-initiated projects that may have little connection to their research interests; they are enlisted to help develop not just community museums and interpretive centers, but eco-tourism and fair trade networks.

In principle, and often in practice, even though requirements of consent, consultation, and reciprocity may constrain what archaeologists can study and publish, they leave the archaeology itself unchanged. Archaeologists pursue questions of their own internal definition, in accord with disciplinary conventions that define what counts as empirical adequacy, interpretive or explanatory credibility, but they do this subject to requirements that they explain what they're doing and why, that they get permission to proceed and follow protocols of respectful, culturally appropriate practice. Sometimes these protocols are predicated on Indigenous traditions that include belief systems and oral histories of the kinds that those committed to "scientific" history and archaeology have long disdained. A classic statement that set the tone for much that followed was Lowie's pronouncement, in 1915: "I cannot attach to oral traditions any historical value whatsoever" (598).[11] But adhering to protocols of consent and respectful practice carries no presumption that archaeologists must embrace the substance of these beliefs beyond respecting the fact that they matter to those with whom they work and whose cultural traditions they study.[12] They set constraints on access and behavior that function much like an injunction that visitors be quiet when they enter a cathedral.

10.2 Beyond Syncretic Pluralism

The pluralism represented by this baseline of respectful practice is, I suggest, tolerant but non-interactive, a form of syncretism by which archaeological and indigenous modes of understanding and methodologies co-exist, not always easily but no longer with priority granted automatically to the scientific when it comes

[11]Lowie argues that the only basis for establishing historical truths, and for disentangling them from mythological fiction, is evidence from archaeology and historical linguistics, in which case the purported evidence from oral traditions adds nothing (1915, 598). See Thomas' discussion of context in which Lowie published this critique. It was a response to the use that two influential archaeologists, Roland B. Dixon and John Lee Swanton, had made of oral tradition as the basis for reconstructing the affiliations between archaeologically identified cultures and contemporary descendants, including the Hidatsa who figure in the second of the two cases I discuss in what follows (2000, 99–101).

[12]This is a point Koskinen makes when she observes that, although researchers engaged in community archaeology need to "understand that the Native Americans believe their stories," it does not follow from this that they "have to believe what the Native Americans believe" (2011, 104). She emphasizes the ways in which Zimmerman and others mark the differences between their own epistemic goals and practices and those of Indigenous communities. I am interested here in examples of collaborative engagement in which these boundaries are productively transgressed.

to questions of access and use.[13] There is nothing here to alarm Boghossian and, in fact, the statements he quotes from the *New York Times* are not unusual in contemporary contexts of archaeological practice; often enough they signal respect for epistemic difference in the absence of epistemic engagement.

Frequently, however, the process of consultation, and especially that of finding meaningful forms of reciprocity, gives rise to more robust and epistemically consequential forms of collaborative practice. This is research undertaken in various forms of partnership with indigenous communities in which, for better or worse depending on your perspective, there is the kind of interaction that does affect the substance of the science – the questions addressed, its norms of practice, presuppositions, and its results– sometimes in transformative ways. Consider an example that mobilizes interactive forms of expertise in ways that begin to push epistemic boundaries: the 1999 discovery of the frozen remains of a young man on the edge of a high elevation glacier in northern British Columbia near the Yukon border. The site of this find is within the traditional territory of the Champagne and Aishihik First Nations (CAFN), who decided that a program of research should be undertaken to learn who this person was, where he had come from, and who had been his kin. They refer to him as Kwäday Dän Ts'ìnchí (Long-Ago Person Found), and negotiated an agreement with provincial authorities designed to ensure that "cultural concerns are respected while recognizing the significant scientific considerations inherent in a discovery of this nature" (British Columbia Ministry 2011; see also Beattie et al. 2000, 135; Dickson and Mudie 2008, 27–28). The description in the *Yukon News* echoes the sentiment expressed by Zimmerman: "The project became a blend of traditional values and modern science. Rather than claiming ownership of the find, the First Nations shouldered the responsibility for the stewardship of this remarkable discovery" (Gates 2009).

In this spirit the CAFN have worked closely with provincial authorities and a diverse team of researchers as full partners, reviewing and approving all research related to the human remains and associated artifacts. The province administered the agreed-upon research and the CAFN took the lead in ensuring that local Indigenous values were respected and played a central role in interpreting the results of scientific analysis.

The research protocols approved by the CAFN include destructive testing: the radiocarbon and collagen dating which establish that Kwäday Dän Ts'ìnchí likely lived sometime between A.D. 1670 and 1850 (Richards et al. 2007, 720–723), as well as a full autopsy which provided the data necessary for a pathology workup and food residue analysis that became the basis for a detailed reconstruction of what the glacier traveler had ingested in his last 3 days (Dickson and Mudie 2008, 42–45). The CA also approved isotope and trace element analysis of hair, bone, and muscle

[13]The distinction I draw between "syncretic" and "dynamic" pluralism parallels Chang's distinction between "tolerant" and "interactive" pluralism (2012, 254, 270–284). I use "interactive" as an adjective here, and specify it below in terms suggested by Collins and Evans's account of "interactional expertise" (2007).

tissue which made possible the reconstruction of a lifetime dietary profile (Richards et al. 2007, 723–728; Dickson and Mudie 2008, 32; Dickson et al. 2004, 482–484). The analysis of associated artifacts, especially the traveller's spruce root rain hat and squirrel skin robe, tool kit and cache of food, provides intriguing evidence of cultural affiliation, and the various pollens, microbes, parasites, and insects lodged in his hair and clothes were environmental clues to the route he had taken to the glacier where he died (Dickson and Mudie 2008, 31–35). Crucially, the CAFN initiated a community DNA analysis for which 248 community members volunteered DNA samples (Brown 2008); this was motivated by a concern to determine which family and clan groups should handle the disposition of Kwäday Dän Ts'ìnchí's remains and his memorial.

The results thus far published indicate that Kwäday Dän Ts'ìnchí' was 18–20 years old, had traveled roughly 100 km in the 3 days before his death, likely in the summer and originating on the coast given evidence that, early in his journey, he had eaten salmon, shellfish, mosses and flowering beach asparagus, and had been exposed to chenopodium pollen. Mineral residue from water consumed as he travelled reinforce these conclusions; 2–3 days before he died he drank brackish water that occurs only in marine environments, and in his final hours, glacier melt water (Dickson and Mudie 2008, 42–44). Most interesting, his lifetime dietary profile indicates that he had lived predominantly on the coast, eating a marine diet, but hair composition analysis suggests that, in the last year of his life, he had shifted to terrestrial, inland foods (Richards et al. 2007, 730; Corr et al. 2008). His clothing and tool kit likewise incorporate both coastal and interior elements; his robe was made of the skins of arctic ground squirrel which live only in the interior, but his hat was woven of Sitka spruce which grows only on the coast (Dickson and Mudie 2008, 31, 44). Finally, the community DNA study is reported to have identified some seventeen living matrilineal relatives; they are affiliated primarily with the Wolf Clan, and live both in the interior and on the coast (Brown 2008; Gates 2009). There is some controversy about the DNA results, but their broad significance lies in the fact that they call into question the reified ethnic categories that underpin much conventional ethnography and archaeology; they challenge the assumption that tribal identity is geographically localized, rather than a spatially extended network of family and clan affiliations that, in this case, link coastal and interior communities. As reported in the local news: "the DNA research has been a scientific confirmation of something that the people have long known, that the traditional ties between the coastal Tlingit and the people of the Southwest Yukon transcend artificial political boundaries" (Gates 2009).[14]

This is, then, a case in which a formal infrastructure for collaboration made significant scientific work possible including destructive testing and a DNA study,

[14]Kwäday Dän Ts'ìnchí's remains were cremated in 2001 and his ashes returned to the area where he lost his life. Analysis of the recovered samples continues, extensive oral history is under way, and the Royal British Columbia Museum is in process of publishing a book that assembles the research results and provides an account of the collaborative research process.

both types of research that are, for good reason, often unacceptable to Indigenous descendant communities.[15] Crucially, this research program addressed questions about clan and family affiliation that were of interest to the First Nations but were not a priority for the archaeologists, medical anthropologists, paleo-ethnobotanists and other scientists who made up the research team, even though the kinds of analysis required to answer them does fall squarely within the ambit of conventional archaeological science. Moreover, addressing these questions destabilizes key framework assumptions with potentially transformative implications for the archaeology and ethnography of the region.

Kwäday Dän Ts'ìnchí' is also a case that brings into sharp focus the asymmetries that structure these collaborations. Indigenous partners have long had to cultivate an understanding of, and to navigate, the norms of Euro-American knowledge production and aligned legal conventions. They are, of necessity, skilled cultural translators who have considerable "meta-expertise" and often "interactional expertise" with respect to archaeology and related fields, to use concepts put in circulation by Collins and Evans (2007).[16] This puts indigenous community partners in a position, if they have a voice in the process, to assess what various scientific specialisms might contribute to a project, and what they can ask of their collaborators at the same time as they bring to bear their own contributory expertise as cultural advisors, and museum and cultural heritage professionals.

10.3 Dynamic Pluralism

Most challenging and rewarding epistemically are collaborations in which archaeologists develop enough reciprocal (interactional) expertise to appreciate and actively engage the specialist knowledge of their community partners. Collaborative practice grades into a dynamic pluralism the goal of which is not only to make archaeology accountable – to "redress real and perceived inequalities in the practice of archaeology" – but also, and crucially, to "inform and broaden the understanding and interpretation of the archaeological record through the incorporation of Aboriginal worldviews, histories, and science" (Nicholas 2010, 11). It is, of course, the very idea that archaeological inquiry might be in any way influenced by, or held accountable to, Indigenous communities' understanding of their own history and cultural traditions that really raises hackles. From the perspective of those who

[15]For discussion of issues raised by archaeological DNA studies, see Pullman and Nicholas (2011).

[16]Collins and Evans describe "contributory expertise" as the cognitive and embodied skill, and socialization into a community of expert practitioners, that puts members in a position to contribute to the production and ratification of specialist knowledge (2007, 24–27). Interactional experts have communicative competence; they have "expertise in the language of a specialism, [without] expertise in its practice" (28). And "meta-experts" have a level of understanding that puts them in a position to adjudicate expertise in fields in which they are not themselves contributory or interactional experts.

defend reason, objectivity, and disciplinary autonomy, to accede to these demands is to capitulate to forces of unreason that threaten to undermine the scientific research enterprise as a whole. And yet a growing number of practitioners make the case that, far from compromising the integrity of their archaeology, active collaborative engagement with Indigenous communities has greatly enhanced their research, in scientific terms.

Consider an argument for this kind of engagement in which Roger Echo-Hawk, a Native American historian and archaeologist, addresses directly the question of what should count as evidence for credible claims about the cultural past. It is striking that, as Echo-Hawk notes (1997, 92–93) and as Boghossian's opening examples illustrate (2006, 1–4), some of the starkest confrontations between science and indigenous knowledge have been over the epistemic standing of oral history and oral tradition. Echo-Hawk identifies a number of reasons for this. The tribal elders who are most knowledgeable about oral tradition are often religious leaders for whom oral traditions are spiritual traditions, to be treated as "holistic truths"; they are likely to reject any analysis designed to extract historical content as fundamentally misguided, another example of colonial imposition and appropriation. On the other side there is the long-held, field-defining convention, reflected most starkly in Lowie's denunciation of "native traditions" (1915, 597), that oral history is completely lacking in substantive content and objectivity; it is, at best, too unstable, too "malleable" (Echo-Hawk 1997, 92), to be considered to carry any evidential weight and, at worst, simply a projection of faith-based religious commitment that, as genre, could not be expected to bear any historical information about the cultural past. Echo-Hawk rejects this "confrontational polarization" (1997, 93; see also 2008), arguing that Indigenous oral traditions are complex and multi-dimensional. They do certainly incorporate spiritual, metaphorical references to supernatural spirits and mythic creatures, but they also carry rich historical information about community migrations and lifeways, geological and climatic as well as cultural events, sometimes of remarkable time depth.[17]

Taking this appraisal as his point of departure, Echo-Hawk develops an analysis of various strands of Caddoan oral tradition, identifying recurrent and convergent narratives about the movements and cultural practices of Pawnee ancestors in the Central Plains of the United States (1997, 93). These include, for example, accounts of immigration and the diffusion of cultural traditions from the eastern Central

[17]In calling into question this long-held disciplinary norm defining what counts as evidence, Thomas considers a number of examples of Native American oral traditions that converge upon, correct, and extend historical reconstructions based on archaeological and geological evidence going back as far as the late Pleistocene (2000, 244–253). What these examples show is that oral traditions cannot be rejected out of hand as inherently untrustworthy and entirely without evidential value. This is not, however, to endorse the equally strong counter-claim that they are a privileged source of evidence; as Echo-Hawk argues, they require discerning assessment and interpretation, as does any source of evidence. He makes this point explicitly in a sharply critical review of Mason (2006) where he rejects the presumption that oral tradition must be endorsed or rejected in categorical terms (2008, 124).

Plains to western groups associated with the emergence of animal ceremonialism and the advent of square and then circular earthlodge architecture (1997, 98–100). Echo-Hawk makes the case that what had been understood as references to animal spirits should be interpreted as descriptions of animal ceremonialism; it is not literally eastern animal spirits who instructed the Skidi Pawnee in earthlodge construction, but "humans engaged in a specific form of religious life" (1997, 99) who transmitted these architectural traditions. The Pawnee oral tradition thus details historical events and affiliations that converge on and significantly extend what can be learned from archaeological evidence of the appearance and distribution of design traditions and earthlodge architecture. The archaeology establishes a chronology for the transitions described by oral tradition which, in turn, suggests that diffusion of earthlodges may be explained by the advent and influence of animal ceremonialism. It further suggests an affiliation between Central plains and neighboring groups of considerable time depth, linking the contact period Pawnee with Central Plains cultural traditions that archaeologists have dated to AD 950–1400. The significance of this finding is that, on conventional archaeological and historical wisdom, the Pawnee could not have entered the Central Plains until the Spanish drove them out of the Southwest sometime around AD 1600. The evidence from oral tradition, in conjunction with archaeological sources, suggests a continuous Caddoan presence in the Central plans for the past 1000 years. This represents a significant challenge to the framework within which Plains archaeology has developed, calling into question the conventions of description and analysis that dissociate contact period from pre-contact cultural traditions in the region. In short, there is much to be gained, Echo-Hawk argues, from "a more complete review of Pawnee and Arikara oral traditions and a fuller consideration of the archaeological record . . . integrat[ing] data from two different sources" (1997, 99).

Echo-Hawk's argument for taking oral history seriously – for reconsidering entrenched norms of justification in archaeology and history that privilege the written and the material record over oral history – turns on an appreciation of the discipline involved in learning and transmitting oral traditions (2000, 2008).[18] Angela Cavender Wilson describes the early training and lifelong practice in telling and retelling key life and community histories typical among the Dakota. It is assumed, she observes, that "the ability to remember is an acquired skill, one that may be acutely developed or neglected" (Wilson 1998, 29). If learning and maintaining oral traditions is a form of expertise, a rigorous community practice, it should not be surprising that they could be a rich repository of evidence about past events and conditions of life. What Echo-Hawk calls for is not, then, uncritical acceptance but a combination of this contributory expertise with the standard historical practices of reading sources against the grain, cross-examining them,

[18]For comparison with European oral traditions, see Carruthers' account of medieval "memory culture": the now neglected "arts of memory"; the modes and uses of trained memory, and the "recollection devices" that can give oral traditions considerable stability (2008). I thank Conor Mayo-Wilson for this reference.

considering the contexts and purposes of their production: a mode of historical inquiry described by Collingwood as the hallmark of "scientific history" (1946, 222–227, 274–282).

Echo-Hawk's purpose is to reframe the polarized debate in which oral sources are written off by archaeologists frustrated with yet another "collision between science and religion" (98), and Native Americans who reject insights from the archaeological record as irrelevant to understanding Pawnee oral tradition on the other. He is intent on drawing all parties to an appreciation that each source has the capacity to refine and enrich other. To return to my purpose, Echo-Hawk's practice illustrates what is to be gained by engaging the "new kind of science" advocated by Zimmerman (Johnson 1996); it constitutes a form of dynamic pluralism in which diverse and hybrid forms of contributory expertise are brought to bear on conventional archaeological problems, reframing focal questions and orienting assumptions.

No doubt a critic like Boghossian would assimilate Echo-Hawk's analysis to his treatment of the Bellarmine and Azande cases: one part translation error and one part vindication of Western knowledge systems in which scientific norms of justification ultimately prevail. From a stance of confident adherence to the epistemic foundationalism and objectivism of "our classical picture of knowledge" (19), if Echo-Hawk contributes anything to our understanding of Central Plains prehistory, it must be because he is positioned to recognize and appropriate scientifically credible insights that have somehow been stumbled upon and incorporated into a non-scientific cultural tradition. His success is just a recent example of a longstanding practice by which the agents of colonial and imperial power have selectively assimilated to their own systems of knowledge what they find useful in the traditional knowledge of subdominant "others." But this, I submit, is to miss the point, indeed, it is to miss several key points.

For one thing, the translation process is much more complex than a simple appraisal that Pawnee oral tradition (somehow) "got it right" historically. It requires attention to contexts of use, transmission practices and, above all, a capacity to distinguish the diverse registers in which the claims constitutive of historical narratives are made.[19] More to the point, it requires that archaeological practitioners develop robust interactive expertise with respect to Indigenous oral traditions. And as both cases illustrate, this process of translation and assimilation has the capacity to destabilize settled assumptions, raising questions about the subject domain and, crucially, about norms of justification that practitioners had never considered.

[19]This last requires a sensitivity to distinctions of the kind Sperber has drawn between different propositional attitudes and strengths of commitment, with respect to different types of factual and representational belief (1982, 166–177). Koskinen draws on Sperber to make the case that, in fact, Zimmerman's brief for a "different kind of science" is best understood, not as "a mixing of different epistemic practices," but as a juxtaposition of propositions that convey quite different kinds of ethical and epistemic commitment (2011, 102–103). I concur that these distinct purposes should be recognized, but find them much more deeply and productively intertwined than Koskinen allows. See Chang on the complexities of pluralist "co-optation" (2008, 281–282).

More fundamentally, to frame the debate in terms of epistemic absolutes is to systematically obscure the contingent nature of the goals and norms of evidential reasoning that animate our own research practice; in dynamic, productive research programs these are the subject of continuous negotiation at the level of practice. Boghossian's claim that "we have no option" but to endorse "[our] classical picture of knowledge" casts our epistemic norms – the norms currently underwritten by the epistemic objectivism of this "classic picture" – as static, settled, necessarily foundational and, in this, unresponsive to critical challenge.[20] By taking our norms and the forms of knowledge they ratify as the baseline for assessing the track record of alternative systems, the only condition under which we can be compelled to critically examine current practice is when we encounter (in the sense of ENC) a mode of inquiry that relies on radically divergent norms of justification but shares our goals and meets our standards of success – one that delivers "advanced science and technological success." In this the terms of ENC foreclose the possibilities illustrated by the examples discussed here, where active engagement with alternative epistemic systems brings into focus cognitive goals we have not thus far considered and, in the process, throws into relief the limitations of practices we have evolved in response to these goals. This openness to learning from perspectives that diverge from our own embodies an epistemic principle that is at least as central to the traditions of inquiry we consider scientific as those captured by the tenets of Boghossian's "classical picture of knowledge": the commitment to hold open to critical scrutiny even our most deeply held convictions, including foundational epistemic and methodological norms. Boghossian's sustained argument for deflecting any challenge to settled conventions of inquiry is an abrogation of this core principle.

10.4 Implications for Archaeology

To draw together the threads of this argument, consider three final questions:

1. What do collaborative projects contribute to archaeology?
2. What kinds of pluralism do they represent, and what challenge(s) do these pose to ideals of objectivity?
3. What is the epistemic rationale for these kinds of pluralism? How do we account for the fact that, despite anxious nightmares about the threat they pose, they can and often do significantly improve archaeological science?

[20]Indeed, Boghossian's rebuttal to Rorty's treatment of the Bellarmine case makes this explicit. He rejects the suggestion that the scientific world view in terms of which we now understand the confrontation between Bellarmine and Galileo was in process of formation; there must be "system-independent fact[s]" of justification to which our evidential standards (those that now settle the question for us) approximate (2006, 69).

Where the first question is concerned, the Kwäday Dän Ts'ìnchí case illustrates how even consultative engagement – on the face of it a non-interactive, epistemic syncretism – can result in a productive recasting of focal questions, enlarging the research agenda and bringing a critical perspective to bear on assumptions about Indigenous culture that have long informed archaeological, historical, and ethnographic inquiry. Echo-Hawk's brief for a "New Ancient History for Native America" shows how a more dynamic engagement across traditions can destablize entrenched norms of justification, opening space to recognize new lines of evidence and interpretive resources that have the potential to significantly reconfigure received wisdom, empirically and conceptually. Crucially, both examples illustrate how fruitful it can be to bring a critical (outsider) perspective to bear on disciplinary conventions; sustained interaction with descendant communities that goes beyond a respectful appreciation of difference can put archaeologists in a position to recognize just how purpose-specific, contingent, and tradition-bound are the epistemic goals and the methodological and epistemic norms that define what it is to do archaeological science. It provokes a consideration of alternatives that might never have arisen through internal deliberation. In short, archaeologists are finding that, if they take as a point of departure something other than an intellectual version of *terra nullius*, they have potential partners who can significantly enrich their store of facts, expand their repertoire of working hypotheses and keep them epistemically honest, ensuring that their own epistemic norms continue to evolve.

On the second question: I have identified a spectrum of pluralisms ranging from respectful co-existence to much more dynamic and generative forms of collaborative practice.[21] Reflecting on these it is immediately obvious that even respectful co-existence, with no significant cross-fertilization of substantive beliefs and epistemic conventions, depends on cultivating some degree of mutual understanding. It requires that archaeologists and non-archaeologists alike develop a modicum of meta-expertise with respect to each other's knowledge traditions. This sustains what I have referred to as a form of syncretic pluralism: a recognition of difference without significant epistemic engagement.

Collaborative practice in its various grades requires, in addition, that archaeologists recognize that their community partners – in the examples considered here, Indigenous peoples – do in fact bring various forms of contributory expertise to their joint projects; they are experts in their own epistemic traditions. As in the case of more familiar forms of inter-disciplinarity, these extra-disciplinary engagements will only succeed if the partners move beyond respectful toleration of epistemic difference and develop significant interactional expertise with respect

[21] Or, as Chang might describe it, these examples lie along a spectrum that runs from a minimalist "tolerant" pluralism to various forms of robustly "interactive" pluralism that involve cross-fertilization of various kinds between traditions (2012, 254).

to the "specialism" in which their partners have contributory expertise. This gives rise to dynamic pluralism of at least two kinds[22]:

– Limited cross-fertilization: a practice of assimilating to an existing archaeological framework elements of factual or interpretive knowledge that originate in an autonomous epistemic tradition, but are relevant to established lines of inquiry and can be seen to conform to extant norms of justification (a matter of appropriating external resources that pose no challenge to existing disciplinary norms);
– Epistemic engagement: an exchange in which archaeological partners learn to see their own research traditions from the standpoint of other ways of understanding the world; a comparative, reflexive stance that throws into relief the limitations and the strengths, the problem and convention-specificity of their own contributory expertise.

While syncretic pluralism poses little threat to Boghossian's epistemic objectivism, this last grade of pluralist engagement does open up the possibility that interaction with external, alternative knowledge systems will destabilize entrenched epistemic and methodological norms. It may not pose the kind of global, all-or-nothing challenge called for by Bhogossian's "Encounter" (ENC), but it can put significant pressure on goals of inquiry and norms of justification insiders to an established research tradition take to be self-evident. Crucially, when these more prosaic pluralist encounters draw attention to the contingent, evolving nature of our disciplinary goals and norms they call into question Boghossian's confident conclusion that our current best practices approximate to "absolute, practice-independent facts" about what counts as justification (2006, 110). The conviction that we have "no option but to think there are [such facts]" can only be sustained as an act of epistemic faith.

Finally, consider the third question: what is the epistemic rationale for these stronger grades of collaborative engagement? The central principle here is articulated by Zimmerman when he observes that, as a scientist, he must be prepared "to constantly learn" (Johnson 1996). Given his longstanding commitment to critical, reflective forms of epistemic engagement,[23] I understand Zimmerman to be taking a stance of openness, not just to a cross-fertilization of useful facts, but to new ways of learning; he sees the need to take distance from the science he practices, to consider its established research agenda and norms of justification in light of other epistemic traditions.

The philosophical rationale for such a stance is captured by the liberal democratic conviction that more ideas, diverse voices and angles of vision is inherently a

[22]On Chang's scheme, the first of these two "interactive" types of pluralism is an instance of what he describes as "co-optation" (2012, 281), and the second is similar to the more pro-pluralism-friendly forms of "integration" he discusses (2012, 279–280).

[23]Zimmerman has been an outspoken internal critic of archaeologists who have refused engagement with Native Americans, and was an early advocate within archaeology of Indigenous archaeology (e.g., 1989).

good thing epistemically. The wider the range of perspectives an individual or a community can bring to bear on a question, or in assessment of prospective knowledge claims, the more likely it is that error and bias will be exposed, that the full complexity of the subject and all relevant implications will be appreciated. Helen Longino articulates this principle in terms of a proceduralist account of objectivity, arguing that the beliefs we should count as knowledge are those that arise from the right kind of process of critical scrutiny, and the right processes are those which ensure that contending beliefs are subject to "criticism from multiple points of view" (2002, 129). These she characterizes in terms of a set of four jointly social and cognitive norms that govern processes of deliberation within well functioning scientific communities, and that bear not only on specific knowledge claims but also on norms of justification (2002: 128–131).[24] Most relevant here is the fourth of these norms, "tempered equality of epistemic authority" (202, 131–133), which requires that mechanisms be in place to counteract exclusionary practices. Longino argues, in this connection, that "not only must potentially dissenting voices not be discounted, they must be cultivated"; to fail to do this is "not only a social injustice but a cognitive failing" (2002, 132).

Longino is clear on the point that this norm of "tempered equality of epistemic authority" raises complex questions about community membership; it "makes us ask who constitutes the 'we' for any given group" (2002, 134). As formulated, however, it delineates "duties of inclusion and attention" (132) that apply to the members of a community of scientists; it includes no provision for seeking out external communities that might be the locus of relevant expertise or critical perspective and extending this norm of epistemic authority to them.[25] This carries the risk that communities of epistemic peers who share cognitive goals and conventions of practice will also share cognitive lacunae, about their subject domain and about the

[24]These four social-cognitive norms require the following: that there be public venues for criticism which ensure that dissent can be voiced; that criticism gets uptake; that the standards by which theories, hypotheses, evidential claims are evaluated are publicly recognized and are themselves open to critical assessment; and that research "communities . . . be characterized by equality of intellectual authority" (2002, 131). The rational for this suite of practices is that an epistemic community must maintain conditions of critical adjudication that secure the possibility of "transformative criticism" (1990, 73–74).

[25]In *The Fate of Knowledge* Longino does emphasize that "a diversity of perspectives is necessary for vigorous and epistemically effective critical discourse" (2002, 131), so the research community has an obligation to ensure that alternative views are "developed enough to be a source of criticism and new perspectives" (132). More recently, she has argued that, to counter the risk that idiosyncratic assumptions may dominate a research community, it may be important to "require openness to criticism both from within and from outside the community" (2004, 134). She notes, however, that communities with the resources to "demonstrate the non-self-evidence of shared assumptions or to provide new critical perspectives may be too distant, spatially or temporally, for contact" (134). I argue here that it should be a priority, in some contexts at least, to seek out interlocutors who can bring external, critical perspectives to bear on the knowledge claims and norms of justification that define a research community's practice.

epistemic, methodological standards "by appeal to which criticism is made relevant to the goals of the inquiring community" (130).

To motivate a discerning extension of Longino's norm of "tempered equality of intellectual authority" to external communities, I argue that we need the resources of a sophisticated standpoint theory.[26] As a form of social empiricism, the central tenet of standpoint theory is a "situated knowledge thesis": the recognition that what we experience and what we know (well) is conditioned by our social experience. Standpoint theorists formulate this thesis in structural terms. They emphasize the ways in which epistemic situatedness is not just idiosyncratic, a consequence of our individual talents, dispositions, and unique personal histories, but must be understood to arise from contingent yet powerful lines of social differentiation that make a systematic difference to the material conditions of our lives, to the relations of production and reproduction that shape our identities and opportunities, and therefore to our capacities as knowers. These conditions are understood to shape not only our standing as knowers – whether we will be recognized as epistemically credible – but also our cognitive and epistemic resources.[27]

A distinctive feature of standpoint theories of particular relevance here is an appreciation that those who are socially marginal may, in fact, have considerable epistemic advantage that typically goes unrecognized. Most prosaically, they may be privy to evidence and may develop the interpretive heuristics necessary to understand and to effectively navigate dimensions of the social and natural world that the comparatively privileged rarely engage, or are invested in avoiding. More controversially, this "inversion thesis" draws attention to distinctive forms of knowledge that arise from non-mainstream social locations, embodied in tacit knowledge, sensibilities, and conceptual resources that have taken shape independently of, or in opposition to, the traditions that constitute the dominant culture. Finally, the experience of exclusion or marginalization may itself be as source of insight. The stance of an insider-outsider may give rise to the kind of "double consciousness" made famous by W.E.B. Du Bois; it may require robust interactional expertise with respect to the norms of the dominant culture, affording comparative perspective and throwing into relief assumptions and conventions of practice that those in positions of relative privilege take for granted. In the process this may catalyze counter-narratives and counter-norms that have the conceptual resources to capture forms of experience, dimensions of the world (social and natural) and ways of navigating it that are absent or excluded from the dominant culture. Sometimes this dissident experience gives rise to a critical "standpoint on" knowledge production (Weeks 1996).

Taken together these considerations constitute grounds for a principle of contingent epistemic advantage on the margins: those who are socially marginal may

[26] This account of standpoint theory summarizes an argument for reconceptualizing its central tenets that I originally proposed in Wylie (2003) and have since developed in Wylie (2012).

[27] I draw here on Fricker's distinction, in *Epistemic Injustice,* between forms of testimonial injustice which involve the misrecognition of epistemic credibility, and hermeneutical injustice in which a lack of conceptual resources in the dominant culture may preclude uptake of critical or dissident perspectives (2007).

be epistemically advantaged in ways that are relevant to specific epistemic projects and that conventional working indicators of epistemic credibility do not track.[28] In particular, they may be uniquely situated to recognize and to counteract the kinds of group think and aligned failures of collective imagination by which our current best practices come to be canonized as embodying "absolute" facts about what counts as justification that we have "no option" but to embrace.

To make sense of the kinds of epistemic advantage associated with pluralist, collaborative practice in archaeology, I propose the following standpoint theory-derived principle for extending Longino's norm of tempered equality of intellectual authority:

> In order to counteract the risks of insularity and the effects of dysfunctional group dynamics that can insulate foundational assumptions and norms of justification from critical scrutiny, well functioning epistemic communities should actively cultivate collaborations with external communities whose epistemic goals, practices, and beliefs differ from their own in ways that have the potential to mobilize transformative criticism.

On this principle the impetus for dynamic, interactive pluralism is not just that it may fill lacunae and correct errors in the substantive beliefs of a research community, but that it can bring community members to a critical standpoint on their established knowledge making and ratifying practices. It is one way to cultivate an awareness of the contingency of our current epistemic goals and standards, to open them to critical scrutiny and, in this, to ensure that we "constantly learn" from our evolving practice. Where archaeology is concerned, the rationale for duties of "attention and response" to collaborative partners thus arises both from moral obligations to descendant and affected communities, and from an epistemic obligation rooted in norms of critical engagement that are constitutive of scientific inquiry.

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[28] I emphasize that what I posit here is not automatic epistemic privilege but contingent epistemic advantage that (may) accrue to a structurally location and standpoint on knowledge production (Wylie 2012, 62).

the Institute for Advanced Study at Durham University, King's College London and the University of Leicester; and in 2013, Denison University (Titus Hepp Lecture), the European Philosophy of Science Association (Springer Lecture), the InterAmerican Philosophy Society in Salvador (Brazil), and the Minnesota Center for Philosophy of Science. I particularly thank my hosts at the Australian National University for making two separate visits to Australia possible in Spring 2013 and 2014; I got wonderful feedback from colleagues in the ANU Schools of Philosophy and of Archaeology, and at the University of Queensland, and at Sydney University.

A much compressed discussion of the Kwäday Dän Ts'ìnchí case and of its philosophical implications appears in *Philosophy of Social Science: A New Introduction*, edited by Nancy Cartwright and Eleonora Montuschi (2014).

References

Atalay, Sonya. 2012. *Community-based archaeology: Research with, by, and for indigenous and local communities*. Berkeley: University of California Press.

Beattie, Owen, B. Apland, E.W. Blake, J.A. Cosgrove, S. Gaunt, S. Greer, A.P. Mackie, K.E. Mackie, D. Straathof, V. Thorp, and P.M. Troffe. 2000. The Kwäday Dän Ts'inchi discovery from a glacier in British Columbia. *Canadian Journal of Archaeology* 24(1): 129–148.

Boghossian, Paul. 2006. *Fear of knowledge: Against relativism and constructivism*. Oxford: Oxford University Press.

British Columbia Ministry of Forests, Lands and Natural Resource Operations, Archaeology. 2011. *Kwäday Dän Ts'ìnchí chronology*. British Columbia Provincial Government Cited 20 Dec 2011. Available from http://www.for.gov.bc.ca/archaeology/kwaday_dan_tsinchi/chronology.htm

Brown, Chris. 2008. Scientists find 17 living relatives of 'Iceman' discovery in B.C. glacier. *CBC News*, British Columbia, April 25.

Carruthers, Mary. 2008. *The book of memory*. Cambridge: Cambridge University Press.

Chang, Hasok. 2012. *Is water H_2O? Evidence, realism and pluralism*. London: Springer.

Clark, Geoffrey A. 1996. NAGPRA and the Demon-Haunted world. *Society for American Archaeology Bulletin* 14(5): 3. 15 (2): 4.

Clark, Geoffrey A. 1998. NAGPRA: The conflict between science and religion, and political consequences. *Society for American Archaeology Bulletin* 16(5): 24–25.

Collingwood, Robin G. 1946. *The idea of history*. Oxford: Oxford University Press.

Collins, Harry, and Robert Evans. 2007. *Rethinking expertise*. Chicago: University of Chicago Press.

Corr, Lorna T., Michael P. Richards, Susan Jim, Stanley H. Ambrose, Alexander Mackie, Owen Beattie, and Richard P. Evershed. 2008. Probing dietary change of the Kwäday Dän Ts'ìnchí individual, an ancient glacier body from British Columbia. *Journal of Archaeological Science* 35(8): 2102–2110.

Dickson, James H., and Petra J. Mudie. 2008. The life and death of Kwäday Dän Ts'ìnchí, an ancient frozen body from British Columbia: Clues from remains of plans and animals. *The Northern Review* 28(Winter): 27–50.

Dickson, James H., Michael P. Richards, Richard J. Hebda, Petra J. Mudie, Owen Beattie, Susan Ramsay, Nancy J. Turner, Bruce J. Leighton, John M. Webster, Niki R. Hobischak, Gail S. Anderson, Peter M. Troffe, and Rebecca J. Wigen. 2004. Kwäday Dän Ts'ìnchí, the first ancient body of a man from a North American Glacier: Reconstructing his last days by intestinal and biomolecular analyses. *The Holocene* 14(4): 481–486.

Douglas, Heather. 2009. *Science, policy, and the value-free ideal*. Pittsburgh: University of Pittsburgh Press.

Echo-Hawk, Roger. 1997. Forging a new ancient history for native America. In *Native Americans and archaeologists: Stepping stones to common ground*, ed. Nina Swidler, Kurt E. Dongoske, Roger Anyon, and Alan S. Downer, 88–102. Walnut Creek: AltaMira Press.

Echo-Hawk, Roger. 2000. Exploring ancient worlds. In *Working together: Native Americans and archaeologists*, ed. Kurt E. Dongoske, Mark Aldenderfer, and Karen Doehner, 3–7. Washington, DC: Society for American Archaeology.

Echo-Hawk, Roger. 2008. Review: *Inconstant companions* by Ronald J. Mason. *Great Plains Research* 18(1): 124–125.

Fricker, Miranda. 2007. *Epistemic injustice: Power and the ethics of knowing*. Oxford: Oxford University Press.

Gates, Michael. 2009. Kwäday Dän Ts'ìnchí teaches us how to work together. *Yukon News*, March 20.

Johnson, George. 1996. Indian tribes' creationists thwart archaeologists. *The New York Times* October 22, 1996.

Kellert, Stephen H., Helen Longino, and C. Kenneth Waters (eds.). 2006. *Scientific pluralism*. Minneapolis: University of Minnesota Press.

Kincaid, Harold, John Dupré, and Alison Wylie (eds.). 2007. *Value-free science? Ideals and illusions*. Oxford: Oxford University Press.

Koskinen, Inkeri. 2011. Seemingly similar beliefs: A case study on relativistic research practices. *Philosophy of the Social Sciences* 41(1): 84–110.

Lacey, Hugh. 2005. *Values and objectivity in science: The current controversy about transgenic crops*. New York: Roman & Littlefield.

Longino, Helen E. 1990. *Science as social knowledge: Values and objectivity in scientific inquiry*. Princeton: Princeton University Press.

Longino, Helen E. 2002. *The fate of knowledge*. Princeton: Princeton University Press.

Longino, Helen E. 2004. How values can be good for science. In *Science, values, and objectivity*, ed. Peter K Machamer and Gereon Wolters, 127–142. Pittsburgh: Pittsburgh University Press.

Lowie, Robert H. 1915. Oral tradition and history. *American Anthropologist* 17(3): 597–599.

Machamer, Peter K., and Gereon Wolters (eds.). 2004. *Science, values, and objectivity*. Pittsburgh: Pittsburgh University Press.

Mason, Ronald J. 2006. *Inconstant companions: Archaeology and North American Indian oral traditions*. Tuscaloosa: University of Alabama Press.

McGhee, Robert. 2008. Aboriginalism and the problems of indigenous archaeology. *American Antiquity* 73(4): 579–598.

Nicholas, George P. 2010. Introduction. In *Being and becoming indigenous archaeologists*, ed. George Nicholas, 9–18. Walnut Cree: Left Coast Press.

Nicholas, George P., and Alison Wylie. 2009. Archaeological finds: Legacies of appropriation, modes of response. In *The ethics of cultural appropriation*, ed. James O. Young and Conrad G. Brunk, 11–55. Oxford: Wiley-Blackwell.

Nicholas, George P., and Alison Wylie. 2013. 'Do Not Do Unto Others . . . ': Cultural misrecognition and the harms of appropriation in an open-source world. In *Appropriating the past: Philosophical perspectives on the practice of archaeology*, ed. Geoffrey Scarre and Robin Coningham, 195–221. Cambridge: Cambridge University Press.

Pullman, Daryl, and George P. Nicholas. 2011. Intellectual property and the ethical/legal status of human DNA: The (Ir)relevance of context. *Érudit* 35(1–2): 143–164.

Richards, Michael P., Sheila Greer, Lorna T. Corr, Owen Beattie, Alexander Mackie, Richard P. Evershed, Al. von Finster, and John Southon. 2007. Radiocarbon dating and dietary stable isotope analysis of Kwäday Dän Ts'ìnchí. *American Antiquity* 72(4): 719–733.

Richardson, Alan W. 2006. The many unities of science. Politics, semantics, and ontology. In *Scientific pluralism*, ed. Stephen H. Kellert, Helen Longino, and C. Kenneth Waters, 1–25. Minneapolis: University of Minnesota Press.

Rorty, Richard. 1991. *Objectivity, relativism, and truth*. Cambridge: Cambridge University Press.

Scarre, Geoffrey, and Robin Coningham (eds.). 2013. *Appropriating the past: Philosophical perspectives on the practice of archaeology*. Cambridge: Cambridge University Press.

Society for American Archaeology. 1996. Principles of archaeological ethics. *American Antiquity* 61: 451–452.

Sperber, Dan. 1982. Apparently irrational beliefs. In *Rationality and relativism*, ed. Martin Hollis and Steven Lukes, 149–180. Cambridge, MA: MIT Press.

Thomas, David Hurst. 2000. *Skull wars: Kennewick Man, archaeology, and the battle for native American identity*. New York: Basic Books.

Ubelaker, Douglas H., and Lauryn Guttenplan Grant. 1989. Human skeletal remains: Preservation or reburial? *Yearbook of Physical Anthropology* 32: 249–287.

Watkins, Joe. 2000. *Indigenous archaeologies*. Walnut Creek: AltaMira Press.

Weeks, Kathi. 1996. Subject for a feminist standpoint. In *Marxism beyond Marxism*, ed. Saree M. Makdisi, Cesare Cesarino, and Rebecca Karl, 89–118. New York: Routledge.

Whitt, Laurie Anne. 1998a. Cultural imperialism and the marketing of Native America. In *Natives and academics: Research and writing about American Indians*, ed. Devon A. Mihesuah, 139–171. Lincoln: University of Nebraska Press.

Whitt, Laurie Anne. 1998b. Indigenous peoples, intellectual property and the new imperial science. *Oklahoma City University Law Review* 23(1–2): 211–259.

Wilson, Angela Cavender. 1998. Grandmother to granddaughter: Generations of oral history in a Dakota family. In *Natives and academics: Research and writing about American Indians*, ed. Devon A. Mihesuah, 27–36. Lincoln: University of Nebraska Press.

Wylie, Alison. 1999. Science, conservation, and stewardship: Evolving codes of conduct in archaeology. *Science and Engineering Ethics* 5(3): 319–336.

Wylie, Alison. 2003. Why standpoint theory matters: Feminist standpoint theory. In *Philosophical explorations of science, technology, and diversity*, ed. Robert M. Figueroa and Sandra Harding, 26–48. New York: Routledge.

Wylie, Alison. 2005. The promise and perils of an ethic of Stewardship. In *Embedding ethics*, ed. Lynn Meskell and Peter Pells, 47–68. London: Berg Press.

Wylie, Alison. 2012. Feminist philosophy of science: Standpoint matters. Pacific APA Presidential Address. In *Proceedings and Addresses of the American Philosophical Association* 86(2): 47–76.

Wylie, Alison. 2014. Community-based collaborative archaeology. In *Philosophy of social science: A new introduction*, ed. N. Cartwright and E. Montuschi, 68–82. Oxford: Oxford University Press.

Young, James O., and Conrad G. Brunk (eds.). 2009. *The ethics of cultural appropriation*. Oxford: Wiley-Blackwell.

Zimmerman, Larry J. 1989. Made radical by my own: An archaeologist learns to understand reburial. In *Conflict in the archaeology of living traditions*, ed. Robert Layton, 61–68. London: Unwin Hyman.

Chapter 11
The View from Here and There: Objectivity and the Rhetoric of Breast Cancer

Judy Z. Segal

After a tumor, the world looks much more huggable.

Nicholas Kristof[1]

This paper is, in part, a metapaper—about two research papers and two researchers. One of the original papers ("Breast Cancer Narratives . . . ") was published in the journal, *Linguistics and the Human Sciences*, in 2007, and the other ("Cancer Experience and Its Narration . . . .") appeared in 2012 in *Literature and Medicine*. Both of the authors/researchers are me. The object of study in each case is the breast-cancer narrative, especially its public function. In 2007, I wrote about publically-rehearsed and widely-circulating cancer narratives of battle, triumph, and survival as instances of an overdetermined genre with a questionable public function. The second paper is more empirical than the first (which was more theoretical); it takes up the written responses of people with cancer, and people close to them, to that same widely-circulating story. Between the writing of the first paper and the writing of the second—in December 2009—I was diagnosed with breast cancer, and my position for any future papers on breast cancer necessarily changed.

[1]In a personal op-ed in the Sunday *New York Times*, columnist Nicholas Kristof (2010) explained that "the world looks more huggable after you have a tumor"—or, actually, in his case, that it looks more huggable after you think you have cancer (but actually do not have it). Some *New York Times* readers might have objected to Kristof attaching himself to a narrative of survivorship when what he had was a cancer *scare*, and not cancer. That is, perhaps having cancer does change one's outlook on the world—that change is, in part, what my chapter is about—but does significant change come also from spending a month thinking you might have cancer? More likely, the appreciation following that experience is like the appreciation you have of running water after you've been camping for a week: it's real appreciation, but it seldom lasts past your first couple of showers. Kristof's huggability claim exemplifies the tone of public discourse about cancer, and the difficulty, in part because of that tone, of subjecting the personal cancer narrative to any unblinking critique. Kristof: "A brush with mortality turns out to be the best way to appreciate how blue the sky is, how sensuous grass feels underfoot, how melodious kids' voices are" (n.p.).

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What I will explore in this essay is the shift in my authorial position as I went from being one sort of researcher—if not exactly objective, then at least unencumbered by a recognizably contaminating identity—to another. This second position, I wish to argue, is not best called "subjective." In fact, it aspires to a "stronger" objectivity: a nearer view, with standpoint. The title of my essay is a play on Thomas Nagel's (1986) "view from nowhere"; my guide for thinking about objectivity in my own case is standpoint theory, and Sandra Harding's (2002) claim that, "in a certain range of cases, maximizing neutrality is an obstacle to maximizing objectivity" (341).

Before my diagnosis, I observed cancer from a respectable distance. After my diagnosis, my position close to, but outside of, the cancer establishment (a world co-constituted by research scientists, physicians, other health professionals, fundraisers, and the most vocal—and sometimes distressingly univocal—cancer narrators), makes me an epistemic outsider, possibly with (quoting Harding again) "a critical edge for generating theoretically and empirically more accurate and comprehensive accounts" (348): more accurate and more comprehensive than accounts rendered under the objectivity more typically associated with distance and disinterestedness. This is not to suggest a person has to have breast cancer in order to write about it. I do not believe this is the case, although, as I will note, a lot of people who write about cancer have/had it, and cancer cred is not nothing in the literature of cancer scholarship. It is only to suggest that my shift from not having cancer to having it is not, at the same time, a shift from objectivity to subjectivity.[2]

Notwithstanding that my second paper is more data-based and observational than my first—the second one documents the unmediated accounts of people who have dealt with or are dealing with cancer—it is irretrievably rooted in my own experience as a cancer patient, while my first paper was, in a word, academic. My purpose, in contrasting my two ventures as an author about cancer, is not only to indicate some of the problematics of an objective/subjective divide, but also to argue for a Science-and-Technology-Studies (including a Critical-Medicine-Studies [see Chambers 2009; Paul 2009]) approach to patient narratives. An STS approach, for example, would be in contrast to much of the current treatment of patient narratives in bioethics, medical humanities, and, increasingly, medicine. Over the years since cancer experience became a topic of public attention (with the publication of Betty

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[2]In *AIDS and its Metaphors* (1990), Susan Sontag wrote the following about the writing of *Illness as Metaphor* (1978):

> I didn't think it would be useful and I wanted to be useful—to tell yet one more story in the first person of how someone learned that she or he had cancer, wept, struggled, was comforted, suffered, took courage ... though mine was also that story. A narrative, it seemed to me, would be less useful than an idea. (101)

My own essay strives to be more idea than narrative, although it will sound like a narrative at times. It is not meant to be about me, except as I now occupy the space vacated by another researcher: the previous me.

Rollin's *First You Cry* in 1976, following the public presentation in 1974 of Betty Ford as a person who had undergone a mastectomy—and following Edith Bunker's breast-cancer scare in a 1973 episode of *All in the Family*), patient narratives have become precious objects, on the idea that they are, each of them, subjective ("personal") accounts, worthy of, as narrative-medicine specialist Rita Charon (2006) argues, being "honoured." I will suggest that illness narratives themselves are not simply subjective, just as accounts of apparently disinterested cancer researchers and clinicians are not simply objective. Illness narratives, in fact, are authored in part by scientific and institutional forces that are rendered invisible in narration; personal accounts may include medical ventriloquism. The sort of attention we ought to pay to illness narratives is, in part, for that reason, *critical* attention. Yet stories tend to end conversations rather than continue them: "This happened to me" often signals the beginning of the final turn in a conversation. That personal narratives occupy this privileged conversational space means a lost opportunity for getting things right. A disciplinary corrective may be found in STS.

My first breast-cancer essay began as a paper for a genre-theory conference. When the call for proposals came for that conference, I had been reading Londa Schiebinger and Robert Proctor (2005) on "agnotology": the production and maintenance of ignorance. Schiebinger and Proctor had said that we deploy the resources of research and scholarship to investigate how we know what we know—epistemology—but we do not marshal the same resources to investigate how, and why, we don't know what we don't know: agnotology.[3] The argument of my paper was that genre itself, when we understand it as recurring textual form for routine social action,[4] can be a technology of ignorance; moreover, in the case of the standard breast-cancer narrative, that is what it is. My revised paper appeared in a special issue on genre of *Linguistics and the Human Sciences*.[5] Despite the title of the journal, I make no claims about the paper's scientific nature. It was a humanities essay with a theoretical framework, and more procedural competence (in rhetorical analysis) than structured methodology.[6] However, I had assembled the literature, cited the authorities, made new claims and offered evidence for them, and drew conclusions that followed logically along. The paper was a credible report on research. Where objectivity is a scholarly stance, implying some remove from the object of study, neutral and disinterested except for a stated theoretical perspective, the paper was objective enough.

In fact, a critique levelled at my paper—and not by peer reviewers, in readers' reports, but by two acquaintances with breast cancer, in conversation—was that it was too removed and disinterested; that is, I had written a paper about narrating the

[3]I have since learned from Ian Hacking (in conversation) that the more common term for the study of ignorance is *agniology*, so named in the nineteenth century. My thanks to Professor Hacking.

[4]The most oft-cited source on this understanding of genre is Carolyn Miller (1984).

[5]The special issue was guest-edited by Débora Figueiredo, Charles Bazerman, and Adair Bonini.

[6]On rhetorical analysis as a methodology, see the Introduction to my *Health and the Rhetoric of Medicine* (Segal 2005).

breast-cancer experience without having had the breast-cancer experience.[7] Indeed, a search of the social-science and humanities scholarship on cancer will reveal that a great deal of it is produced by authors who have had cancer themselves (see, for example, Batt [1994]; Bryson [2010]; Ehrenreich [2001]; Herndl [2006]; Jain [2007]; Orenstein [2013]; Sedgwick [1994]; Sontag [1978]; Stacey [1997]). I was not one of those scholars, and, according to my breast-cancer-experienced critics, that was a liability for me as an author on cancer. In fact, in breast-cancer scholarship, more than in other areas of scholarship, questions of authorial objectivity are subordinated to questions of authorial knowledge, and knowledge is understood as lived knowledge.[8] Objectivity and its correlative distance will offer, the argument seems to go, less than you need to know. I would argue that personal experience does not *de facto* destroy objectivity.

I will say something briefly about the object itself of my inquiry: the breast-cancer narrative. A typical breast-cancer story begins at the moment of the discovery of a lump in the breast or an irregularity on a mammogram; it does not, although it might, begin, for example, at the moment of moving next door to a chemical plant or taking a job at a drycleaner. It proceeds through the anxiety of diagnosis, the challenges of treatment, and the triumph of survival.[9] The narrative, we know, is typically, as Sontag (1978) importantly explained, on the metaphor of battle.[10] The narrator of the story is ennobled by what is often called, though now the metaphors

[7]While, as I have said, I do not believe that authors must experience what they write about, I did wonder then, faced with this critique, whether, if I were ever diagnosed with breast cancer, I would go to bed as myself and wake up as someone with a sudden taste for pink t-shirts and group runs. That didn't happen. My critics were right that breast cancer would reveal itself to me differently once I became a character in a breast-cancer story. They were wrong, however, about the sorts of things that would change with diagnosis. Both my critics, for example, had said that, as a breast-cancer patient, one is so grateful for breast-cancer fundraising that one is not appalled by pink merchandising. That turned out not to be the case for me.

[8]Moreover, books about cancer are frequently reviewed by reviewers who have/had cancer, and who foreground their own experience in the review. See, for example, Sarah Harvey's (2006) review of Marisa Acocella Marchetto's *Cancer Vixen*—or Adam Baer's (2011) hybrid personal narrative/book review of Siddhartha Mukherjee's *Emperor of all Maladies*. Even cancer television is reviewed by reviewers who have/had cancer. See, for example, reviews of Showtime's *The Big C* by Jenni Murray (2010) and Deborah Orr (2010), each review referencing the author's personal cancer experience. (Murray's byline includes, "Journalist and broadcaster who was diagnosed with breast cancer in 2006.")

[9]"Survival" itself is a contested term—not only ideologically (what does the term connote?) but also empirically (when does survivorship begin?). While, in some accounts, survivorship begins when a person, having been diagnosed, is 5-years cancer-free, in others, it begins with completion of treatment; in still others, survivorship begins from the moment of diagnosis. For discussions of survival, see Mullen (1985) and Rowland (2008).

[10]Despite critiques of the battle metaphor for cancer—and for medicine more generally (see, e.g. Fuks [2011])—the metaphor persists, and seems even to have become more aggressive. In a recent advertisement in the *New York Times Magazine*, North Shore-LIJ [Long Island Jewish] Hospital Cancer Institute promises to marshal "a relentless army of doctors": "Isolate. Attack. Overwhelm. Together, it's what we do to cancer" (North Shore-LIJ 2013).

are a bit mixed, her "journey" (see Silcoff [2011]). She has, in the end, a deeper understanding of all things, especially herself—and she has, reportedly, a better life than she had before. The standard story includes the sentiment and, more often than one might expect, the actual sentence, "Cancer is best thing that ever happened to me."[11] In my genre essay, I argued that features of the standard breast-cancer narrative are so conventionalized, the stories so pre-scribed, that the genre writes the story. Then, every person diagnosed with breast cancer has to contend with this narrated set of values in the performance of her own illness, one way or another. The agnotology thesis is that the generic story suppresses not only other cancer stories (like environmental or population-health stories) but also other genres (like genres of protest) in which cancer might be told.

My later breast-cancer-narratives paper (2012) began as a talk for an interdisciplinary social-science and humanities workshop on "critically interrogating cancer survivorship."[12] The paper is, for the most part, a report on how people with cancer, and people who care(d) for and about people with cancer, respond to the conventionalized story of battle, triumph, and survivorship I had written about before. The paper is an "accidental" study, as I will explain. It is, more to the point, inescapably written by a person who has/had cancer. For the same reasons that some readers may find my second paper less persuasive than my first (any distance I had in relation to breast cancer is gone), some may find it more persuasive (I have knowledge that was not available to me before).

Here is how the second paper came to be. Early in 2010, shortly after my lumpectomy and before the start of my radiation treatments, I set out to meet a commitment I had made, some months before, to write a topical op-ed for the *Vancouver Sun*, one of the two mainstream daily newspapers in the city where I live. I did not plan to write about cancer; in fact, my proposal and initial drafts

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[11] For example, a set of personal narratives appears in the breast-cancer-survivor magazine, *Beyond*, under the heading, "Cancer is one of the best things that ever happened to me" (2007). This excerpt is from a story called, "Curly Hair and Other Gifts Cancer Gave Me":

> Just before my diagnosis, my husband and I decided to try for a third child. Instead of getting pregnant, I got cancer. A cruel trade, I thought at the time, but now ... I think perhaps I was not meant to conceive a child at that moment .... Another baby might have sent me over the edge. Cancer was a blessing in disguise. ... Also, I have always wanted curly hair. ... Cancer inspires me. I've been given a wake-up call that many people will never receive [and] I am happy to simply be alive. (Donaldson 2007, 26)

For a recent illustration of cancer-discourse tone, see Silcoff (2011). The subject of her "Every Cancer Has a Silver Lining" is "wellness warrior" and "cancer entrepreneur," Kris Carr, maker of the film, *Crazy Sexy Cancer*, and author of its associated books.

[12] The workshop, "Critically Interrogating Cancer Survivorship: Social Science and Humanities Perspectives," was held in Vancouver, British Columbia, July 21–22, 2011. The workshop was organized by Kirstin Bell and Svetlana Ristovski-Slijepcevic, with funding by the Canadian Institutes of Health Research, the Social Sciences and Humanities Research Council, and the University of British Columbia. Bell has recently (2014) published an illuminating essay on the "breast-cancerization of cancer survivorship."

were for a short piece on swine flu, much in the news at the time, and health inequities. In the end, however, I did write about cancer, almost as if I couldn't stop myself. I submitted a 700-word essay on the tyrannical nature of the triumphal cancer narrative I had already been thinking a lot about and was now contending with myself as a cancer patient. I called my essay, "Cancer isn't the best thing that ever happened to me" (2010). The *Sun* liked my cancer essay (arguably, for all the wrong reasons; more on that in a moment) and published it on April 1, 2010, to mark the beginning of Cancer Awareness Month. April 1, 2010 happened also to be the first day of my radiation treatments, and so the reader email the op-ed generated arrived at an interesting time for me. My workshop paper, later published in *Literature and Medicine*, took those emails as its data.

Reader email in response to my op-ed was surprising, both in its quantity—I received about 50 direct replies, separate from Letters to the Editor, despite the fact that the article did not include my email address or solicit response—and in its substance: every email, bar none, expressed gratitude and support. Not a single response was the disapproving pink-inflected mail I had convinced myself I should expect.[13] The emails were versions of (I paraphrase), "Thank you for that. I have cancer; I feel nauseated, sad, and terrified, and I'm incredibly sick of pretending this has all been a fabulous, if difficult, experience in personal growth." Following is the text of one email. I include it to illustrate the point I am about to make: that my correspondents were not writing to me as a researcher; they were writing to me as a cancer patient. This email is from Kathleen Beaumont, who has given me permission to use her name:

> I read your article in the Vancouver Sun which resonated with me to the point that I was motivated to write and thank you.
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> Many of the thoughts that you identified have crossed my mind and I have dismissed them because of the disconnect that I have experienced between my personal attitudes towards having had breast cancer and the conventional attitudes that are projected in the media. . . . I sometimes feel guilty that I'm not out there with the other "survivors" running, walking and singing the praises of the sisterhood. I'm in great physical shape and I could do it, but its not my style and it never will be, yet I'm still left with the feeling that I let someone down or I didn't pay back my debt to society. . . . I see myself as a person who got sick, very sick, then fought my way through it, then got on with my life. . . . . Sure there were changes and like you I felt I had been tossed around vigorously. . . . I can recall the strength that I received from individuals who supported me and I am happy to offer the same back, but the pink club, that bothers me. . . . Your article gave me affirmation that it was OK to think about my experience in any way I like. Not conforming to the pink code is OK too. Thank you for so eloquently putting some of my deepest thoughts into words. (Personal correspondence, 2010)

[13]While not comparing myself to Ehrenreich, I could not stop thinking about her. Her well-known *Harper's* essay, "Welcome to Cancerland" (2001), documents the reception of her negative postings about her cancer experience to the Susan G. Komen message board: her postings were met, she said, with "a chorus of rebukes" (50). Letters to the editor of *Harper's* in response to "Cancerland" were no more sympathetic. Even cancer researcher Barron H. Lerner (2002) chastised Ehrenreich: "Although pink ribbons and teddy bears may be infantilizing," he wrote, "many survivors appreciate these touches or at least tolerate them as furthering a worthwhile cause" (4).

I answered each email as it arrived; in some cases, my answers elicited further responses and more narratives that were, in relation to the dominant narrative I had described, counternarratives. (One of my correspondents, a physician with breast cancer, was undergoing radiation treatments exactly when I was, and I wondered if we passed each other anonymously in the radiation waiting rooms of the Cancer Agency before we each went home and wrote to each other.) When, nearly a year later, I was invited to contribute a paper to the "survivorship" workshop, I thought back to the op-ed and the reader response, and I wrote again to my correspondents and requested permission to reproduce their messages for presentation and possible publication. Everyone who received the request consented enthusiastically, and I began to draft the second breast-cancer-narratives paper.

The first thing that my shift in authorial position gave me, then, was access to speakers who might not, in other conditions, have spoken at all. If I had the perceived objectivity of an academic researcher, I would not have this data for the second essay. I am not saying that I could not have written an op-ed about cancer narratives without having cancer; of course, I could have, and readers may have responded to it. However, my self-presentation in the op-ed as a person with cancer was, at least in part, what elicited the particular reader response that I got. My correspondents did not write about their cancer experience simply in reply to my commentary on cancer narratives; they wrote about their experience in reply to my account of my own experience *as a cancer patient*. I did not write the op-ed to collect data—I did not recognize the responses *as* data until long after I had collected them—but I know, from their messages, that the person my correspondents were writing to was, in the first instance, a cancer patient.

Insider credibility is only one, obvious, researcher advantage of surrendering the (putative?) objectivity of the uninvolved. Another advantage of experiential knowledge is the ability to pose new research questions: before I had cancer, I more clearly didn't know what I didn't know. Most importantly, however, as a researcher with cancer, I could discover the *mechanisms* of certain social processes whose existence I had only previously noted or deduced.[14]

For example, there has been, for decades, a field of research, populated mostly by nurses, counsellors, and social workers, aimed at understanding and improving what is called, "the cancer experience." Nevertheless, this is a fact about cancer in public institutions: despite this expert focus, there remains a relatively poor understanding of the experience of cancer patients. In Spring 2010, the cover story of the official magazine of the B(ritish)C(olumbia) Cancer Foundation, *Vim and Vigour*,[15] was,

[14]For some of the same reasons that we sometimes pluralize "knowledge," we take a special interest in health professionals who have become patients (see, for example, Glouberman [2011] and Klitzman [2008]), and patients who, on particular topics, have become medically expert (see, for example, Montgomery [2006], on breast cancer). In certain situations, it is possible to think about empathy and epistemology together.

[15]The BC Cancer Foundation "raises funds to support research and enhancements to patient care at the BC Cancer Agency." The mandate of the BC Cancer Agency "covers the spectrum of cancer care and research, from prevention and screening to diagnosis, treatment, supportive care,

"High Notes: Singer Diana Krall stays healthy by looking on the bright side." Inside the magazine, the story began, "Even as she faced a terminal illness, Adella Krall [Diana's mother] always saw the good in life. She liked to say that, if her barn ever burned down, it would make it easier to see the beauty of the moon at night" (McCafferty 2010, 28).[16]

I have nothing but respect for Diana Krall's late mother, and others who find strength where she did. I am, however, concerned about others who are ill, who experience a wider range of affective response. When I left the Cancer Agency after my tenth radiation treatment and leafed through the free copy of *Vim and Vigour* I had picked up in the lobby, I was most offended not by the (s)mug shot of Diana Krall (30), but by the advice across the page on how to turn my negative thoughts into positive ones: how to replace, "I never have enough time" with "I can prioritize my commitments"; how to trade in "I look old" for "I look and feel good for my age" (31)—and, implicitly, how to replace, "I feel like total crap and I cry all the time" with "cancer is a wonderful learning experience." I had just lain supine and motionless on a table while a man in his twenties—who thought to tell me, after he'd asked what sort of work I did ("English Professor"), that he had always hated English and done poorly in it—directed a radiation machine at my fully exposed breast. The official Cancer Foundation magazine lacks the understanding that saying "improve your attitude" to cancer patients is not necessarily helpful.[17]

One reason that cancer outreach fails so many cancer patients is that institutional research on the cancer experience is not itself objective—although, claiming a scientific character, it appears at first to be. It is hardly neutral or disinterested. Research and clinical practice at the BC Cancer Agency are informed by the ideology of, for example, the American and Canadian Cancer Societies and the massively-successful fundraiser Susan G. Komen for the Cure. (That foundation claims its position as pro-woman, but not pro-feminist[18]). The ideology of the breast-cancer establishment, as we know from Ehrenreich (2009), Samantha King (2006), and other authors, as well as from the documentary film, *Pink Ribbons Inc.*

rehabilitation and palliative care" (*Vim and Vigour* masthead). In other words, *Vim and Vigour* is the institutional voice of cancer in British Columbia.

[16]I could cite countless such breezy articles from *Vim and Vigour*. More recent cover stories are headlined, "Breath of Fresh Air: The always perky—and quirky—DIANE KEATON doesn't let asthma slow her down" (Paterik 2013) and "New Hope for Brain Cancer Patients" (Anonymous 2013). (Really? The "new hope" trope—here? The phrase "new hope for" today gets over 80 million hits on Google [July 14, 2014], most pertaining to illness or disease.)

[17]The lack of fit between institutional messages and the needs of cancer patients is well documented (see, for example, Lorde [1980]; Batt [1994]; Ehrenreich [2001; 2009]; Sinding and Gray [2005]). It is also well described by the respondents to my op-ed (Segal [2012]).

[18]In 2012, Nancy Brinker, CEO of Susan G. Komen for the Cure proved this by moving to defund Planned Parenthood, despite the work Planned Parenthood does in making screening mammography available to women who could not otherwise afford it. (The benefits of screening mammography are another topic, and controversial; the point here is that Komen is not pro-feminist and, it seems, not completely pro-woman.) Brinker reversed her position under public pressure.

(Pool and Din 2011), is characterized by positive thinking, free-market fund-raising, and individualism. Breast cancer is constructed as occupying one hapless body at a time, producing one "survivor" at a time. The focus on the individual body prevails even though the story of diagnosis is told about 270,000 times every year in North America. The dominant breast-cancer ideology is also an anti-disabilities ideology: the ideal breast cancer patient does not seek to find common ground with other people who may be ill or weak; rather she seeks to claim the space of a new normal (see also Herndl [2006]). In the standard cancer story, illness and disability are implicitly shameful, while *normal* has been expanded to include women who may be amputated, vomiting, and burned, but are still keen to walk and run and climb for the cure, and, until they can do that, to shop for it. As King (2006) has noted, in the public presentation of cancer, activity replaces activism.

Institutional cancer research is aimed at producing a particular kind of patient: docile, civil, unthreatening, and as easy as possible to live with. As I have been swept up in the social effort to produce that patient, I have learned more about its routines. Moreover, I can see the discursive interplay between the cancer-care establishment and the oft-published stories that so persistently attracted my attention before.

If narrative is a technology of ignorance, it works on the order of a drama. The personal breast-cancer story can be described using the terms of rhetorical theorist Kenneth Burke's ([1945]1969) *dramatism*, where dramatism suggests a symbolic order and a procedure of analysis in contrast, Burke says, to *scientism*. In dramatism, the resources of ambiguity, resulting from shifts of focus, are exploited, contra objectivity. The key terms of Burke's dramatism are *act*, *agent*, *agency*, *scene*, and *purpose*. On a dramatistic model, we might say the telling of the breast-cancer story is an act; the story-teller is an agent, but not a free agent. The story itself is an agency or an instrument—of a larger, unarticulated, policy for regulating illness behavior. The story has many purposes: its personal purpose is a proxy for its cultural purpose. Its social purpose is to smooth over difficult things. The act of story-telling takes place on the scene of medicine, the scene of competitive fundraising, and the scene of the self. Burke would have us go on, with new ways to fill the slots created by his pentad.

My cancer places me in the drama and I come to see the means by which the standard story asserts itself. I do not do this on purpose. I learn about the requirements of breast-cancer story-telling the way a child learns language in social interaction: mostly inadvertently. I'm at dinner with a friend, across a table at a restaurant. It's a month after my surgery and she asks how I am. I begin to complain, but not about the cancer; I complain about all the things I don't like about being a cancer patient. I say, anyway, I feel neither positive nor embattled; I say it in a way that's not funny. I see my friend sit back in her chair, just a little. She folds her arms; I change the subject. Next week, different restaurant, different friend; I know what I have to do. In any case, I am a participant in my own research.

In the months between surgery and radiation, my research continues. I am getting cards and emails from old friends, former students, distant family members. I am touched to receive their good wishes (although, frankly, some people say some really scary things to me). In fact, I'm impressed that anyone is speaking to me at all;

I know it's hard to know what to say. Here is something I start to notice: many of the messages I'm getting are congratulating me for being strong and positive and brave. "I know you are a fighter," they say. "You can beat this thing." I begin to find these messages disconcerting—because I am certain I've provided no evidence at all of possessing the virtues for which I am being praised.

I begin to think of Aristotle's classification of rhetorical occasions (as one does). I think especially of epideictic rhetoric. Epideictic rhetoric is, in its prime example, the rhetoric of the funeral oration, or, perhaps, the rhetoric of the Academy Awards. Epideictic speeches are not aimed at a particular course of action; their *raison d'être* is not exhortation and dissuasion but rather praise and blame.

In eulogies, people are praised for embodying community values: they are praised for being generous, for example, and when they are so praised, the value of generosity in the community is not only invoked but also reinscribed. People are seldom *blamed* in eulogies, but blame is established implicitly in respect to values opposite to the ones admired: if it is good to be generous, then it is bad to be miserly. At the Oscars, people are praised for their humility, their pleasure in their work, their ability to be good friends. Speakers typically exhibit the very qualities for which they praise others, making the speech reflexively epideictic.

I stare at my cancer messages. People are not telling me I *should be* strong and positive and brave; they are (ingeniously, really) instead praising me for already being that way. At the same time, they are, implicitly, advising me that it would be disappointing if I were otherwise. If what I was about to say in reply to these messages was that I was exhausted and afraid, I think again; I really should stifle that. I write back and say, "I'm fine. … The pathology report was great. I'm not teaching, but I'm still advising graduate students. I'm looking forward to the radiation starting so that it can finish." I write many of these messages. I'm not lying, but I'm not telling the truth either. The possibilities for the exhibition of me were narrowed with every message that praised me already for being brave and positive. These are the mechanisms by which the standard story comes to dominance: a conversational partner folds her arms; a well-wisher confers approval pre-emptively.

In my first breast-cancer-narratives essay, I had written that the generic cancer narrative, with its gospel of positivity, was coercive, that it made it harder for people with cancer to report honestly on their experience. My research had been both theoretical and observational and my essay included a discussion of the public reception of unconventional narrators.

Both Ehrenreich (2001, 2009) and King (2008) had noted that breast-cancer discussion groups and internet message boards exert a conservative influence on breast-cancer discourse, with many web sites discouraging contributors from raising questions about environmental carcinogens and pharmaceutical-company profits. That conservative force, I discovered, was evident elsewhere as well. In 2006, Canadian broadcast journalist Wendy Mesley went public with a cancer story that was jarring to an audience primed for a pinker story. Mesley's television documentary, *Chasing the Cancer Answer*, was rooted in her own breast-cancer experience, but it was not a survivor's inspirational tale; it was an account of what we know and do not know about the causes of cancer in populations. The day after the documentary ran, *Globe and Mail* columnist Margaret Wente (2006) accused Mesley of "drive-by" journalism, calling the documentary, "stunningly simplistic," "full of misleading information and fear mongering" (A17). Physicians for a Smoke-Free Canada (Collishaw 2006) was quick to publish an open letter to Mesley, accusing her of telling only part of the cancer story.[19]

My essay documented a further example of narrative regulation in the public realm, this one from personal experience: One afternoon in 2007, I was listening to and recording a radio phone-in show (CFUN Vancouver) on the topic of breast cancer. I heard a caller, who self-identified as a "survivor," say this to a fawning host: "I just kept the image of my kids in front of me and refused to die." The host praised her lavishly for her personal triumph over cancer, and I found myself moved to contribute to the conversation. I did not phone the program, but I sent an email to the host, still on air, to say that I was concerned about the implication that women who had died of breast cancer had just not tried hard enough, had not loved their children enough to save their own lives. The host emailed me back in a commercial break. "I wish you had phoned the show," she wrote. "Then we might have helped you have a better attitude."

In my first essay, I displayed examples of the public reception of the renegade cancer story and leavened these with various things I had learned about narrative regulation. More of a sense of how things worked was obscured, however, until I was diagnosed, and those cards and emails came, and I saw that I was being formed in the image of one of those "survivors" who believes we can put the picture of our children in front of us, and refuse to die; I was being formed in the image of someone I hated.

In March of 2010, when I wrote to the *Vancouver Sun* Arts editor to describe the cancer op-ed I wanted to write (in lieu of the swine-flu one), she wrote back immediately to say that the newspaper would publish it. In in-house correspondence forwarded to me later, she had said to another editor, "Judy's story is beautiful and amazing." Of course, my story was exactly neither of those things: it was a complaint against the very idea that cancer stories should *be* beautiful and amazing—but the liaison in people's minds between breast cancer and a particular narrative aesthetic is strong. The connection is nearly impossible to loosen, and difficult, really, even to see: A 26-year-old woman (Elizabeth Sarah Barry) dies of lymphoma; her father (Barry [2010]) writes a "Lives Lived" column (an extended obituary/eulogy) in Canada's national newspaper, the *Globe and Mail*. He says his daughter wanted her cancer journals to be published, and that publication had begun at *blogspot.com*: "Elizabeth's thoughts," he wrote, "were that we as a society read

[19]The response might seem a curious one to what was really just investigative journalism. I believe Mesley inspired such ire for two reasons: first, she did, in the documentary, directly challenge the Canadian Cancer Society, an agency that is seen by many to be sacrosanct; second, she jumped genres. Here was a national celebrity (Mesley) who was known to have had breast cancer. Members of the viewing audience expecting to be treated to a personal narrative were jolted by a different sort of report—not about Mesley's own cancer experience but about carcinogens and the public policies that keep them in our midst.

and hear of so many 'feel good' stories about cancer survivors, but we need to realize there are many more stories that do not have positive endings. She believed that we can lose sensitivity to the fact that cancer is so personal, and so very devastating." But Elizabeth's father is clearly unaware of the contradiction of that sentiment with this one in his own eulogy: "When [Elizabeth] was diagnosed with lymphoma in January, 2009, she remained positive. She would always respond to the question, "How are you doing?" by saying, "I'm doing great." This when she had spent days vomiting, or was struggling to breathe." (L6) Why in the world did Elizabeth respond that way? Why, *given the terms of her own journals*, was it praiseworthy that she did?

My cancer experience was not immune to this cancer aesthetic either; it included a set of virtues I had internalized against my will. I had written critical papers; I had pilloried the standard story; I had called it "the standard story"! Yet, one night in the course of my treatment, I said to my partner, "I think I would be prouder of myself if I didn't still go to sleep crying sometimes—if I muscled through, if I never missed a deadline because I was tired." That is, in spite of everything I knew and had said and had written, I had expectations of myself—preferences, certainly: in the face of illness, it is better to be strong than weak; coherent than chaotic; hopeful than despairing; angry than sad. It is better to resist than to rest. When the graphic novel *Cancer Vixen* appeared in 2006—an attractive and successful book about an attractive and successful woman with breast cancer—the Breast Cancer Research Foundation reviewed it, saying, "We salute women like [Marisa Marchetto, the author], who not only have the courage to battle breast cancer, but are able to do it with ... unflagging optimism, creativity, and humour." The moral and aesthetic values of the review (let alone of the book) are almost impossible not to absorb.

How does all this add up for Objectivity?

As Miriam Solomon (2008) has argued, medical knowledge does not divide neatly or hierarchically into science/evidence-based medicine on the one hand and narrative/experiential knowledge on the other. The problem of epistemic authority is not solved, in any case, if we say that there are complementary knowledges: doctors know about disease, and patients know about illness; doctors are expert in diagnosis and treatment and patients are experts in their own experience. (That is the principle on which much of narrative medicine is based: patients have a special sort of knowledge and good doctors know how to listen to what patients have to say.) Solomon queries and complicates the science/art binary in medicine, saying, among other things, that proponents of narrative medicine ought not claim for narrative a special epistemic status and moral authority over science. I have Solomon in mind when I say that I did not, with my cancer diagnosis, slide from having one sort of knowledge to having another, along an objective/subjective axis. Such an axis is too complicated to exist anyway.

In much of current bioethics, medical humanities, and medicine (see, for example, Nelson 2001; Montgomery 2006, and Charon 2006, respectively), discussion about patients' knowledge of their own experience, and what that knowledge may bring to bear on patient care, takes for granted certain oppositions: for example, expert/layperson; scientific knowledge/personal knowledge; evidence-based medicine/narrative medicine; fact/value. All these may be understood in terms of the opposition, objective/subjective. A claim frequently made of late in doctor-patient literature is that all the bottom terms (layperson, personal, narrative, value, subjective) have special worth and deserve a place in medical decision-making.[20] That liberalism, however, often does not go far enough, as it does not capture the presumptions and ideologies that characterize scientific (and social-scientific) investigations, and it does not recognize the medical values that are *already* in place in patients' stories.

When I wrote my first breast-cancer-narratives essay, I believed that generic narratives were produced by irresistibly-constituted generic narrators, and that these narrators, so fully instructed in the values of a bright-sided culture, might, for that reason, narrate a somewhat false experience. I now believe things are more complicated than that. Patient narratives are composed with the material of medical culture. They are not simply subjective, and science and medicine are, likewise, not simply objective (along Mertonian [1973] lines: neutral, disinterested, and so on).[21] Cancer stories are not simply produced, governed, and policed by cultural habit and genre; rather they are engineered—not necessarily on purpose—by the American and Canadian Cancer Societies, Susan G. Komen for the Cure, and other institutional actors (pharmaceutical manufacturers, diagnostic-technology marketers, and so on) with an interest in cancer. Among the people who produce the stories that patients tell, are scientists and physicians who deposit their values and expectations in these stories and then disappear from view as authors. Genres may write stories, but institutional medicine writes them too.

This shift in my view returns me to my original question about the shift in my own authorial position: like the patient narrators and stealth institutional authors I have been describing, I exist in an ambiguous space as regards objectivity. In the shadow of prior questions in this volume about what we mean when we talk about objectivity—and, indeed, whether we should talk about Objectivity at all (see Chap. 2 by Hacking, this volume)—it is difficult to assert with confidence claims about more objectivity and less of it. Still, I would say that my ability to see the breast-cancer narrative, the object of my study, was enhanced, rather than vitiated, by my diagnosis; what I took on with my cancer was not subjectivity but standpoint. In Harding's (1993) strong objectivity, quality of observation does not depend on distance from the thing observed or denial of perspective, but rather on a view from outside the usual positions. For Harding, those positions are male, white, European (among others). For me, they are positions, in the first instance, medical-institutional. I have also, as much as possible, stood outside the patient

[20] But also see Ho (2009) on "epistemic humility."

[21] In a landmark essay in rhetoric of science, Paul Newell Campbell (1975) takes up questions of objectivity through the trope of *persona*, the implied character of the speaker in any work. *Persona* itself, he says, which is unavoidable, is also at odds with claims of objectivity, because it necessarily calls for ethical judgment: there is no character without values. In this view, objectivity itself is a stance (and therefore not objective [in the sense of aperspectival]): to view something dispassionately *is* to stand in relation to it—to attribute a value to it, only not a very high one.

positions that, notwithstanding their place in the category of the personal, the medical-institutional has already wrought.

An exchange economy, in which the objective has elements of the subjective and vice-versa, raises questions for any scientific/narrative divide that persists as a concept in medical epistemology. If a patient's story is not simply an innocent account of personal experience, then the weight of research on patient narratives should move away from disciplines in which these stories are treated as sacred objects: personal, precious, and protected. Research on patient narrative should shift to STS and Critical Medicine Studies. Charon (2006) has said, as I have noted, that patients' stories are an important source of medical knowledge, and should be honored. Patients' stories are also a source of cultural knowledge, including knowledge of institutions of health and medicine, and should be studied. It might even be good to *argue with* them, from time to time, to plumb their values. (As things stand, that constitutes a pragmatic violation: an argument is not a response to a story.) I am not saying that we should not honor patients' stories, that medicine should return to a time when patients' stories were silenced or interrupted, ignored or appropriated, corrected or reconstituted. To argue with—at least, to answer—stories, however, would be a form of respectful engagement, epistemic in itself.

References

Anonymous. 2013. Technology brings hope to brain cancer patients. *Vim and Vigour* (Spring): 8.

Baer, Adam. 2011. Speak, Malady: An autobiography of cancer [including review of *Emperor of all Maladies*, by Siddhartha Mukherjee]. *Harper's* 322 (May): 71–77.

Barry, Lynn. 2010. Elizabeth Sarah Barry. *The Globe and Mail* (Toronto, ON, Canada), May 3.

Batt, Sharon. 1994. *Patient no more: The politics of breast cancer*. Charlottetown: Gynergy.

Beaumont, Kathleen. 2010. Personal email, April 4.

Bell, Kirsten. 2014. The breast-cancerization of cancer survivorship: Implications for experiences of the disease. *Social Science and Medicine* 110: 56–63.

Bryson, Mary K. 2010. *Cancer knowledge in the plural: The queer biopolitics of 'DIY' health*. Presentation at the University of British Columbia, Vancouver, BC, Canada, November 30.

Burke, Kenneth. 1945/1969. *A grammar of motives*. Berkeley: University of California Press.

Campbell, Paul Newall. 1975. The *personae* of scientific discourse. *Quarterly Journal of Speech* 61: 391–405.

Chambers, Tod. 2009. A manifesto for medicine studies. *Atrium* 7: 4–5.

Charon, Rita. 2006. *Narrative medicine: Honoring the stories of illness*. New York: Oxford University Press.

Collishaw, Neil. 2006. An open letter to Wendy Mesley. *Physicians for a smoke free Canada*, March 10. http://www.smoke-free.ca

Donaldson, Jackie. 2007. Curly hair and other gifts cancer gave me. *Beyond* (Spring/Summer): 25–26.

Ehrenreich, Barbara. 2001. Welcome to Cancerland. *Harper's* 303 (November): 43–53.

Ehrenreich, Barbara. 2009. *Bright-sided: How the relentless promotion of positive thinking has undermined America*. New York: Metropolitan Books.

Fuks, Abraham. 2011. Healing, wounding, and the language of medicine. In *Whole person care: A new paradigm for the 21st century*, ed. Tom A. Hutchinson, 83–96. New York: Springer.

Glouberman, Sholom. 2011. *My operation: A health insider becomes a patient*. Toronto: Health and Everything.

Harding, Sandra. 1993. Rethinking standpoint epistemology: 'What is strong objectivity?'. In *Feminist epistemologies*, ed. Linda Alcott and Elizabeth Porter, 49–82. New York: Routledge.

Harding, Sandra. 2002. 'Strong objectivity': A response to the new objectivity question. In *The gender of science*, ed. Janet A. Kourany, 340–352. Upper Saddle River: Pearson Education.

Harvey, Sarah. 2006. A cartoon but no joke: A review of *Cancer Vixen: A true story. The Globe and Mail* (Toronto, ON, Canada), October 7.

Herndl, Diane Price. 2006. Our breasts, our selves: Identity, community, and ethics in cancer autobiographies. *Signs: Journal of Women in Culture and Society* 32(1, December): 221–245.

Ho, Anita. 2009. 'They just don't get it!' when family disagrees with expert opinion. *Journal of Medical Ethics* 35(8): 497–501.

Jain, S. Lochlann. 2007. Cancer butch. *Cultural Anthropology* 22(4, November): 501–538.

King, Samantha. 2006. *Pink Ribbons Inc.: Breast cancer and the politics of philanthropy*. Minneapolis: University of Minnesota Press.

King, Samantha. 2008. *The great pinkwashing: Breast cancer, cause marketing, and the politics of women's health*. Presentation at Simon Fraser University, Vancouver, BC, Canada, February 18.

Klitzman, Robert. 2008. *When doctors become patients*. New York: Oxford University Press.

Kristof, Nicholas. 2010. A scare, a scar, and a silver lining. *New York Times,* June 5. http://www.nytimes.com/2010/06/06/opinion/06kristof.html?n=Top%2fOpinion%2fEditorials%20and%20Op%2dEd%2fOp%2dEd%2fColumnists%2fNicholas%20D%20Kristof

Lerner, Barron H. 2002. Letter to the editor. *Harper's* 304 (February): 4.

Lorde, Audre. 1980. *The cancer journals*. San Francisco: Aunt Lute Books.

McCafferty, Dennis. 2010. High notes. *Vim and Vigour* (Spring): 28–32.

Merton, Robert K. 1973. In *The sociology of science. Theoretical and empirical investigations*, ed. Norman W. Storer. Chicago: University of Chicago Press.

Mesley, Wendy. 2006. Chasing the cancer answer. *CBC marketplace*. March 5. Toronto: CBC Television. Television broadcast.

Miller, Carolyn R. 1984. Genre as social action. *Quarterly Journal of Speech* 70(2): 151–167.

Montgomery, Kathryn. 2006. *How doctors think: Clinical judgment and the practice of medicine*. New York: Oxford University Press.

Mullen, Fitzhugh. 1985. Seasons of survival: Reflections on a physician with cancer. *New England Journal of Medicine* 313(4): 270–273.

Murray, Jenni. 2010. Who's afraid of the Big C? *The Guardian,* September 19. http://www.guardian.co.uk/tv-and-radio/2010/sep/19/the-big-c-cancer-tv

Nagel, Thomas. 1986. *The view from nowhere*. New York: Oxford University Press.

Nelson, Hilde Lindemann. 2001. *Damaged identities, narrative repair*. Ithaca: Cornell University Press.

North Shore-LIJ Cancer Institute. 2013. What if cancer faced overwhelming odds? *New York Times Magazine* (June 2): 10.

Orenstein, Peggy. 2013. The problem with pink. *New York Times Magazine* (April 28): 36–39, 42–43, 68–71.

Orr, Deborah. 2010. *The Big C* is not the cancer comedy for me. *The Guardian,* February 3. http://www.guardian.co.uk/commentisfree/2011/feb/03/cancer-comedy-the-big-c

Paternik, Stephanie. 2013. A breath of fresh air: How Diane Keaton controls her asthma and lives life to the fullest. *Vim and Vigour* (Spring): 28–33.

Paul, Norbert W. 2009. Medicine studies: Exploring the interplays of medicine, science and societies beyond disciplinary boundaries. *Medicine Studies* 1(1): 3–10.

Pool, Léa, and Ravida Din. 2011. *Pink Ribbons Inc*. Montreal: National Film Board of Canada.

Rowland, Julia H. 2008. What are cancer survivors telling us? *The Cancer Journal* 14(6): 361–368.

Schiebinger, Londa, and Robert Proctor. 2005. Agnotology*: The cultural production of ignorance (conference web site)*. Stanford University. http://www.stanford.edu/dept/HPS/AgnotologyConference.html

Sedgwick, Eve Kosofsky. 1994. White glasses. In *Tendencies*, ed. Eve Kosofsky Sedgwick, 247–260. New York: Routledge.

Segal, Judy Z. 2005. *Health and the rhetoric of medicine*. Carbondale: Southern Illinois University Press.

Segal, Judy Z. 2007. Breast cancer narratives as public rhetoric: Genre itself and the maintenance of ignorance. *Linguistics and the Human Sciences* 3(1): 3–23.

Segal, Judy Z. 2010. Cancer isn't the best thing that ever happened to me. *Vancouver Sun* (Vancouver, BC), April 1: A15.

Segal, Judy Z. 2012. Cancer experience and its narration. *Literature and Medicine* 30 (2): 292–318.

Silcoff, Miereille. 2011. Every cancer has a silver lining. *New York Times Magazine* (August 14): 18–21, 46, 49.

Sinding, Christina, and Ross Gray. 2005. Active aging—Spunky survivorship? Discourses and experiences of life and of the years beyond breast cancer. *Journal of Aging Studies* 19(2, May): 147–161.

Solomon, Miriam. 2008. Epistemic reflections on the art of medicine and narrative medicine. *Perspectives in Biology and Medicine* 51(3): 406–418.

Sontag, Susan. 1978/1989. *Illness as metaphor; and AIDS and its metaphors*. New York: Doubleday.

Stacey, Jackie. 1997. *Teratologies: A cultural study of cancer*. New York: Routledge.

Wente, Margaret. 2006. The cancer answer is no answer. *The Globe and Mail* (Toronto, ON, Canada), April 29: A17.