Chapter 8
Objectivities in Print

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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).

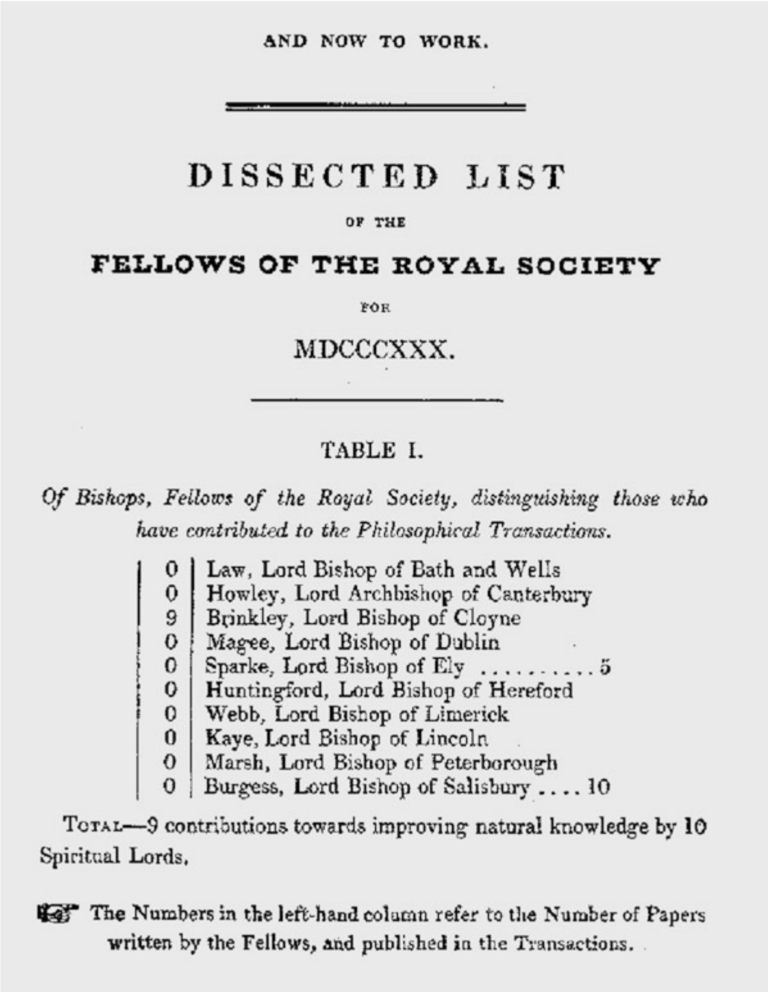
The establishment of the practices we associate with peer review as a general expectation in everyday scientific life did not occur until much later. In fact, despite the comparatively massive scale of German science by the late nineteenth century, such expectations were particularly slow to develop 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.

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