ACTES D'HISTÒRIA DE LA CIÈNCIA I DE LA TÈCNICA
NOVA ÈPOCA / VOLUM 14 / 2021, p. 11-37
ISSN: 2013-9640 / DOI: 10.2436/20.2006.01.221
http://revistes.iec.cat/index.php/AHCT

TRUST ME, I'M A HISTORIAN OF SCIENCE

CHRISTOPHER HAMLIN
UNIVERSITY OF NOTRE DAME

Abstract: *This essay explores the role of the historian of science as a mediator in the current climate of distrust of science. I focus first on theories of trust before turning to the complicated evolution and ambiguous status of the history of science, first in postwar America, and then more broadly. Under the heading of the «chemists' way» I then sketch, as way to defuse distrust, an approach to narrating the history of science to highlight particular but varied sites of expertise rather than universal philosophical authority. Using examples from Catalan science, the fourth section explores similarities and differences in political cultures of the history of science. I conclude by contrasting the impediments that make it impossible to trust the history of science as a discipline with the possibilities that historians of science have for promoting trust.*

My neighbors in a rural area of the American Midwest seem dubious about science. The Covid-19 pandemic has made that clear, but conversations on it have led also to discussions of climate change, with new species and new diseases, and other issues. Discovery that I am an historian of science changes the conversation – I sense puzzlement about how I actually earn a living, but also interest in what I might know and think. There are many things I might say, but this is neighborly conversation, not professing.

So I probe too. What's at issue in these conversations? I assume I am being expected to represent some form of authority but have no reason to think that I or it will be trusted. My sense from those I talk to – all men so far – is of a savviness toward science, resembling what Henry Bauer has described among anomalistics believers (Bauer, 2001). But there is a weariness with public scientists, the chart-pointing talking-head savants who speak on its behalf – too many imperatives coming from too many messengers. And there is a seeing-through: the specialists get paid for grandstanding and magnifying, just as bureaucrats get paid for making rules. As for the pandemic, though no one uses the phrase «social construction» they might as well, at least insofar as they see it *as an event* requiring a response. Covid-19 is this year's worry; it was Ebola or Zika a few years back. Sure, the flu is worse some years than others; sure, some old people will die from it, but most get better. The flaws I find with their reasoning usually have to do with prudential responses or numbers – occasions for exposure, rates of infections, and thus numbers of deaths. As for climate, the sky is still there, and life goes on.

This weariness, what some call a crisis of faith in science, has roots. Some lie in class – evident in the demographic statistics of wealth and education. There are regional roots – «populism» once a political philosophy of rural resistance to urban and coastal elites still exists, but the term has degenerated into a dismissive, signifying ignorance and dangerous demagoguery (Goodwyn, 1976; Pollack, 1976). Beyond are peculiarities of American constitutional structure that affect role of science (Price, 1965), and of American culture, including the fantasy that America may interface with the rest of the world as it chooses and that no virus would dare cross its borders. Peter Sloterdijk's recognition of a *kynical* political response may be operating too. Publics, unable to challenge experts on their own turf, express contempt immediately and viscerally. They act out rather than engaging in unwinnable conversations. Polarization may then set in as experts become ever more condescending, which in turn encourages greater contempt from the sneering *kynics* (Sloterdijk, 1987). Scientists are not being singled out, authorities are. *Kynical* responses were common in the Reformation: then too, unintelligible elites were no longer authoritative.

Behind all this is a broader question: What shall a historian of science do in a world distrustful of science, or at least of scientists? And behind it is still another worry. Might there be roots of that distrust in the history of the history of science? Like it or not, we will be intermediaries between science and the world beyond it. As for me, I can't turn off being an historian of science and don't want to. I think the history of science has value in conversations about how to read the world and act in it. But my title, «trust me, I'm an historian of science» is ultimately reflexive: «trust me to do what, how, and on what basis?.»

I have no single or simple answer. I shall start by looking more closely at the concept of trust not as mindless submission, but as canny, integrative investment. It will be important too, to see how the history of science came to be. Once these are out of the way, I will turn to strains of scientific practice that took public trust seriously. I shall call these forms of *phronesis* – i.e., problem-solving practical reasoning – the «chemists' way» as distinct from a physicists' way -- a seeking of authority in abstractions and ultimately in metaphysics. While most of my critique is grounded in the American experience, I will turn briefly to uses of the history science in a Catalan setting, where the stakes were quite different. I close with thoughts on how historians of science might come to grips with their mediatory role – a concern since early in my career (Shepard and Hamlin, 1987; Hamlin, 1993).

I. Why «trust?»

«Trust me,» you say. What does *that* mean?

«Trust» has been overlooked in our field. Often it is taken for granted – if we define science in terms of trustworthiness, there's no need to go further. One of the few who has considered it, Steven Shapin, sees trust as the foundation of «truth» not its product. Early modern English science was built on virtues of truth-telling and sincerity. That «great civility» required knowing something of the others one was trusting (Shapin, 1994). In England groups of trusting comrades kept control of advanced studies for most of the nineteenth century, in Germany, united by the formative identity known as *Bildung*, they kept it going even as they were establishing the disciplines that would disintegrate knowledge into multiple sites of authority, each with its own jargon, methods, and means of accreditation (McClelland, 1980; Ringer, 2004). Modern science, however, is to be impersonal – trust is vested in a presumptive social system; «we are told things about the world from people we do not know, working in places we have not been» notes Shapin: Merton's famous norms did not reflect virtues; they imposed them (Shapin, 1994: 10-11, 13-14, 36, 411-13, 416).

Equally momentous was a prior change, the severing of natural philosophy from the traditional philosophical concern of supplying a trustworthy guide to private and public life. Shapin was concerned with the trust among knowledge producers, not the trust of outsiders, but others, like Stephen Toulmin, likewise intrigued by the linkage of «truth» to «trust» had begun to recognize the traditional conception of the scientist as detached theorist was inapplicable to areas of the environmental and human sciences where knowledge and public action were inseparable (Toulmin, 1982). In doing so, he and others (e.g., Jonas, 1973) were simply bypassing the so called «naturalistic fallacy.» If what «is» comes from what we choose to do, its insulation from «ought» is illusory.

Separately, the pioneering sociologist of science Bernard Barber had distinguished what one may call the «epistemic» trust of scientific rectitude from a moral or «fiducial» trust associated with relations between professional and client (or expert and public) (Barber, 1983). In well-organized, mature professions, codes of ethics dictated the kinds of trust one should expect, and a client might further reduce uncertainty by investigating credentials or track record. In most sciences, however, there were no codes of ethics because there were no identifiable clients. The «science says» pronouncements on issues like climate or Covid may appeal to ideals of professional responsibility to common good, yet they do so without any analogous framework of institutionalized ethics. It most fields, outsiders have no way of determining what marks good knowledge or practice, and thus to allocate their trust responsibly.

Moreover, epistemic norms valuable in regulating knowledge production in normal situations may be inappropriate in technical, environmental, and medical contexts that require immediate action – there we must trust the best guesses of captains with their ineffable expertise. The resulting dilemma of treating problems requiring practical action as if they are eternal philosophical questions that will remain open until the cognoscenti choose to close them (which may be never) has been recognized by many groups of science studies scholars concerned with trust – ethicists, feminist philosophers of science, social theorists, science policy analysts, and historians. Sometimes in such cases, science will have been weaponized to divert our attention, or real uncertainty will be artificially amplified to paralyze response (Collingridge and Reeve, 1986; Oreskes and Conway, 2010; Oreskes, 2019).

Yet a focus at the level of public policy, even policy made in democratic settings, still overlooks the relations of «experts» to individuals, the clients who must trust. Tellingly, such persons are sometimes labeled the «laity.» Again, we should keep in mind the Reformation precedent. If people do not trust authorities, it may already be too late to tell them that they should.

For laments that science is not trusted, and pleas that it should be, often bypass important points and perspectives. The first is power. People allocate trust on many and complex grounds; and what may be trustworthy may not be trusted. Here, for a generation now, the classic text has been Brian Wynne's account of the clash between Cumbrian sheep farmers and scientists of Britain's Ministry of Agriculture and Fisheries who were monitoring post-Chernobyl radiation to regulate the market for British lamb. While Wynne's case sharply contrasts differences between the epistemes of disciplinary science and «lay» expertise, many who have responded to it have presumed that expertise rightly conceived and presented overcomes the problem of power. The most general framework for such explorations, Collins' and Evans' articulation of a third-wave of science studies, has failed to gain traction chiefly because of that presumptiveness, I suspect (Wynne, 1992; 2001; 2002; Collins & Evans, 2002; 2003; Jasanoff, 2003; Wynne, 2003; Hamlin, 2008).

Second, connected with a focus on truth, and with it, belief, are unrealistic assumptions about the relation of propositions to social processes. Certainly, it is tempting to think that if only all would assent to a truth, all would agree on what is to be done. Not only does this not happen, grid-group analysis suggests that the assent of a group of free and equal actors to propositions often works against concerted action, owing to an inherent tendency to instability as persons seek ways to differentiate themselves (Douglas & Wildavsky 1982).

Third, as phenomenologically oriented trust theorists like Niklas Luhmann remind us, trust focuses on futures (Luhmann, 1979, 1989, 1993; Sztompka, 1999).[1] Inductions are great for regular phenomena; much less so for improbable historical events. That trust is usually invested in persons and locally reflects this situation. Trusting familiar persons over words, models, or disembodied professionals also means not having to defer to them absolutely – the English phrase «I trust him as far as I can throw him» expresses a calculated trust in which the trustor retains power. For associated reasons trust is integrative, notes

1. Though Shapin (1994, 38-9) connects with some of this literature, phenomenology has been largely absent from science studies (Embree and Barber, 2017).

Martin Marty – having a sense of how a person acts in many settings is good basis for predicting actions in a particular sphere. Among those settings must be sites of possible failure: thus, the banker retains trust for having not stolen my money so far (Marty, 2010; Shapin, 1994: 39).

Fourth, though I give it less attention here, trust persons is complemented by trust in the cosmos, no less opaque in «intentions and calculations» than are other humans, in Adam Seligman's wry phrase. This is the domain of theodicy. In exploring it, Seligman (like Max Weber) makes much of the legacy of English Calvinism and of the Reformation more broadly. The appeal to faith alone reflected a perceived failure of existing means of cosmic mediation, yet ironically, the Reformers' route to cosmic security came by highlighting cosmic insecurity. Their sectarian inversion of a universal authority for the authority of an elect turned out less to be a trust in Calvin's ineffable God than a trust in themselves, as a single, local, human authority. In that change Seligman finds the roots of reflexive modernity: faith in the transcendent potential of human relationships to overcome the cosmos (Seligman, 1997).

Last is an emergent quality – the reciprocity of trust. Because each needs to trust, if only for reasons of efficiency, the extending trust often begets a trust that becomes deeper and broader the longer it is exercised. Yet the same applies to distrust.

The following comparisons between trusting and scientific knowing are illuminating.

- Trust starts from an integrative perspective. Science presumes one, even as it produces disciplinary knowledge.
- Good scientific knowledge is usually presumed to be permanent: «contributions» accumulate. Trust involves action in novel circumstances.
- As accumulation, knowledge is transferrable, while trust, invested in persons, is not.
- Being possessable, knowledge is inherently elitist; being dispositional, trust is egalitarian. Everyone does it.
- We cannot dictate conditions of trust in some set of epistemologists' rules, nor can we restrict it to mere role fulfillment.
- «Trust» registers not only risks, but doubts and failures. Unlike «knowing» usually colored positive, the term often indicates failure: «I trusted you» declares the parent to the teenager. Often the statement «you can trust me» is heard as its negation.
- A trusting relationship may grow slowly yet become so indurated that trustors will recalibrate cognition rather than surrendering it and returning to uncertainty. We could call that «it» a «paradigm.»
- Both Shapinian knowing and trust are matters of division of labor. We might compare them in terms of means of allocating and enforcing responsibilities. Very likely, the trust-based mechanism of peer review operates more powerfully in baby-sitting than in journal publication.

II. Why trust a historian of science?

Whether or not my neighbors trust me, they seem to recognize that I am not a scientist, and yet that when I teach, which is presumably what professors do, I will be somehow representing science, perhaps as a translator or guide, making aspects of it accessible to lay audiences. To them, however, I am an unknown species: rare, but apparently innocuous -- I am not trying to sell anything, nor am I an active threat. Often, I feel like a meteorologist in a Jane Austen novel. The polite conversation may still be about the weather, but my comments will be a diversion from the usual banality. Yet these days, the weather (and health, the other conversational mainstay) are not innocuous. The storms are here, more are coming. So what should happen when the neighbors knock on the door, wanting a professional opinion, hoping to trust me to guide the investments in authority they must make?

Whether someone should trust a historian of science (and for what) depends on what the history of science is (and, in turn, what science is). In 1975, when I began my career, both were easy. Historians of science explained and justified the present. Like historians of art, music, or literature, we taught undergraduates to understand and appreciate science, but we differed from those fields too: *we* represented *the* history of accountability, of trustworthiness itself. Our job was to show how elite European males had somehow given birth to inevitability, that is, to the current state of disciplinary knowledge, even, sometimes, without knowing they were doing that. It seemed reasonable to assume that a colleague embodying that knowledge could fix a car – surprisingly, I fixed it. One needed only to choose whether to be an historian of astronomy, physics, chemistry, or biology (there was little else) and an era.

For that older history of science, science was a branching tree, notes Lynn Nyhart, a common trunk producing disciplinary branches. She uses another Darwinian metaphor for the contemporary view -- science is «a densely tangled bank of people and material things teeming with social, cultural, economic, and religious life, that covers the globe.» No longer are we apologists for inevitability, «the historian's task now is to tease out how certain forms of knowledge and practice within this mass of activity came to be understood as `science;' what has sustained science socially, culturally, and materially; and who has benefitted and who has suffered in its formation» (Nyhart, 2016: 7).

The representation is accurate; it also raises questions. Where while one could reckon with solidity of phloem and zylem of the tree, what pith is in these tangled vines? Is there anything beyond a label – «what is understood as `science'?.» The more closely we follow Darwin, the more ominous the picture becomes – his tangled bank is a site of endless struggle.

The chief challenge comes in her closing observation. «What happened in the past did not change: what we expect professional historians of science to know and care about has.» Key here is «professional.» Especially when coupled with «care» it brings an expectation of a trusting relationship. What are we to care about and on whose behalf?

Answering those questions requires exposing deeper roots of the history of science, and distinguish its multiple modes -- as field, subject to be taught, discipline, career, and profession. In the American academy there was significant interest in the field, sometimes as a subject to be taught before 1950 (Thackray, 1980; Laudan, 1993). Yet it was not seen as a career, much less a profession or a discipline. These latter were products of the atomic age and the ensuing cold war. Elsewhere I have described the "history of science movement," the campaign from 1945 to 1952 led by the Harvard president James Bryant Conant (Hamlin, 2016; 2007). He saw the history of science as the best hope for mediating a coming crisis. Initially democracy was at risk, a few years later survival would be. He is, I think, the most important articulator of a pedagogical rationale for our field – we still use his arguments to justify it.

Conant's agenda has been misrepresented by conflating it with those of Thomas Kuhn and of Vannevar Bush, his boss and colleague in the administration of war-time research and development efforts. Bush's «endless frontier» agenda would be vital to the establishment of a National Science Foundation through which elite scientists would allocate public funding to one another for the advancement of basic science. A subsidized science beyond public accountability was seen as the best hope for meeting unknown future needs, including novel weapons. Conant had first recruited the young physicist Kuhn in the late 1940s to prepare case materials for a year-long general education course on «The Growth of Experimental Science» that Kuhn would later co-teach. In June 1961, Conant would be a critical reader of a draft of Kuhn's *Structure of Scientific Revolutions*, taking issue, like so many in the next decades, with the coherence of the paradigm concept. Kuhn's biographer, Steve Fuller (Fuller, 2000b) finds a common theme: these are efforts to consolidate the power of science, insulate it from democratic accountability. In the cold war, "trust" would mean, «don't ask, just pay.»

That view understates changes between 1945 and 1962 and Conant's uniqueness. He had been a controversial choice when appointed Harvard president in 1933. Despite exceptional undergraduate and postgraduate records at Harvard, and a stellar and varied research career leading to chairmanship of its Chemistry department, he remained an outsider. He had not come from the elite that Harvard served and had little patience with it, rapidly transforming that university from regional gentility to world-class research. There, and in his later post co-managing America's wartime research, he had succeeded as a shrewd administrator-integrator, both in assessing persons and pursuing agendas of democracy, meritocracy, and pragmatism. Kuhn, by contrast, came to the field as a disenchanted graduate student, struck by the disjunct, one Max Weber (2004) had recognized, between fundamental intellectual inquiry and research practice in what was becoming «big science.»

The history of science movement Conant led was the most visible product of a faculty study he had commissioned in 1942 to rethink the rationales and means of postwar general education. Conant launched it in several books based on invited lectures. He promoted these energetically. He sent copies to colleagues, discussed the issues in radio interviews and public affairs periodicals, recruited sympathetic educators, and organized meetings to explore best practices in history-based science pedagogy.[2]

The case studies course Conant began teaching in 1947 would be one of five options for Harvard students not concentrating in science to meet a science requirement. The others too embodied approaches now included within science and technology studies, broadly conceived – the philosophy of the physical sciences, the place of science in western intellectual history, the history and effects of science-based technology, and social and ethical implications of contemporary life sciences. Conant's course, emphasizing process over product, was the most radical: it would give university graduates a sense of the inescapability of choice in research, a sensibility they would need, whether as leaders or as responsible citizens. He knew whereof he spoke. Amidst writing case studies and training assistants, he was continuing to advise the government on science and weapons policy. There he was struck by the collapse of accountability – a product of fear, ignorance, and of possibilities for exploitation.

Conant's course was a hard one. Fuller (2000b: 215) see its real message as one of surrender. Exposing students to the confusion of even relatively simple science would convince them to leave science to scientists, thus furthering Bush's endless frontier. Conant certainly recognized the inaccessibility of expertise, but he saw too that the public would always be the paymaster. What was needed was not deference but responsible delegation. Leaders were not surrendering authority in deploying specialists, but they did need to assess well – to shut down a program or allocate more funds. A good many scientists resented that reality and blamed Conant for it. Having tasted authority in the war years, they saw themselves as sole representatives of rationality.

In 1952 Conant would resign the Harvard presidency to become ambassador to West Germany, only to return to take on a campaign of wholesale reform of American secondary education later in the decade. He returned to science and society issues only occasionally thereafter -- the eight *Harvard Case Studies in Experimental Science* (1957) (four had been his), an important address on «History in the Education of Scientists» (1960), and *Scientific Principles and Moral Conduct,* the 1966 Eddington Lectures at Princeton in 1967.

It should be clear that Conant's case-study approach is only incidentally historical. Older cases were simply easier to anatomize, involving less of what Latour (Latour, 1987) would call «black boxing» reliance on all the prior science that researchers did not (and

2. The 1945 Sachs Lectures at Columbia, the basis of *Education in a Divided World* (1948), set out the broader rationale in terms of education-democracy issues. The 1946 Terry Lectures at Yale appeared as *On Understanding Science* (1947) and in expanded form as *Science and Common Sense* (1951). The 1950 Bampton Lectures at Columbia were published as *Science and Modern Man* (1952). For fuller discussion of these works and Conant's movement see (Hamlin, 2016).

probably could not) unpack. One asked a Harvard undergraduate to stand in Robert Boyle's shoes not as a means to teach the gas laws but simply to expose them to the continual choosing that confronted the scientist – and the science funder. Conant likewise anticipates Latour's *Science in Action* – there is the same moving back and forth between prospective and retrospective views, the tying of tactical choices to outcomes. The accountability one learns, is for, Conant, rationality. And, as far as possible it is to be Boyle, not Conant, who teaches this lesson. The instructor's task is less to explain than to display – the burden of trusting was on the student, not the teacher. Importantly, these the older cases were accessible to a phronesis-based epistemology of "common sense," rooted in William James and John Dewey. That approach pervades a now-forgotten book, *The Nature of the Natural Sciences*, published in 1963, a year after Kuhn's *Structure* by Kuhn's co-instructor in the case studies course, the chemist Leonard Nash (Nash, 1963; see also Nash, 1952).

Central elements in that appraisal are context and trajectory – track and track record. They are key elements of trusting; they are also what history offers. Independently of the instrumental use of historical cases in general science teaching, Conant valued history as the seminal integrative discipline. Consciousness of one's historical situation was a prerequisite for any critical and creative intervention in it. The case studies course included lectures on context, even Marxist interpretations, for always rational decision-making took place in a specific historical setting. Even before Robert Merton's famous thesis on science and puritanism, Conant himself had published on the effects of Cromwell's approaches to administering the English universities. He saw science and history in much the same terms -- both were accumulative rather than reflective or aesthetic endeavors. One brought knowledge of the human past, the other brought knowledge of the world. As a literature review of any research paper indicated, doing scientific research meant situating oneself within an historical trajectory. He had encountered chemistry in that way -- the history of physical chemistry course he had taken as an undergraduate from T.W. Richards, Nobelist and his future father in law, was in essence a run-up to the current state of that exciting field (Conant, 1960).

Conant's focus is mainly on scientists, not science, on imagination not production. It is here that the profundity of his differences with Kuhn (and with Bush) loom largest. From Conant's standpoint the historical view of science by a scientist, by an historian of science, and by a citizen, should not substantially differ. All will see trajectories within contexts. To Kuhn, however, the history of science presented a long succession of incommensurable paradigms, each founded in a truncated and false historical consciousness in which it alone represented the obvious and final rationality. Paradigm maintenance required damping that awareness, and, with it, damping creativity. The problem solvers could be kept at work only if they were kept from dreaming of radical differences. Anomalies in a paradigm might ultimately force change, but seeking change for its own sake was a waste of time.

Both Conant and Kuhn highlight aspects of science – Kuhn representing the inertial comforts of stasis, Conant praising flux. They differ on normative implications, an issue many of Kuhn's readers would raise. Was he describing science as it must be, should be, or simply happened to be? While views of his responses differ, my sense is that he was employing an historian's prerogative of refusing the gambit.

Because it is Kuhn's, not Conant's representations of science that have become so influential – indeed paradigmatic – in science studies, it is worth highlighting two implications of this stance. The first echoes Fuller's concern: to reject any explicit normativity was to become complicit in whatever institutional forms science came to have. The second is Kuhn's acceptance of what has been called the «two truths» view. Standing on a higher hill, the historian of science commands a view inaccessible to the proletarians stuck in their stultifying paradigms. The former can see possibility, the latter capitulates to inevitability.

For the publics which pay for that science, this situation would be a tragic one. Conant, while favoring modest public support for basic research, worried about the coming big science. The giant projects, possible only with government funding, left no obvious place for critical intellectual adventure. They converted experienced, reflective thinkers into administrators and grant-getters, filled the lower ranks with mediocre minds who would, in any case, never have the opportunity for truly original work. Paradigms, with their constricted horizons, would become a self-fulfilling prophecy.

While Kuhn and big science are among the roots of public unease both with science and with its historians, other changes were occurring independently at Conant's Harvard. One might expect a history of science movement to involve recognition not only of a field and of curricula, but to lead to a professional career path. While there had been sporadic efforts in America before 1950 to bring science into the new western civilization curricula, few who taught that subject would have conceived their professional identities as historians of science.

One who might do that, who had made Harvard his academic home since 1916, was George Sarton, the Belgian ex-chemist who was dedicating his life to nurturing the history of science. Conant's relations with Sarton were complicated. Though events at Harvard led to a career path in the history of science and to a History of Science Department, neither was pursuing those outcomes.

Insofar as the history of science was a means of science teaching, Conant had assumed scientists would teach the new courses just as Richards had taught him. Some did. Yet few felt prepared or interested. If indeed the sciences were becoming more paradigmatic, and thus de-emphasizing their own historicity, that was hardly surprising. While Conant saw a small market for librarian-editors to prepare critical editions of key texts, he did not see the history of science as a distinct career path requiring professional training. Sarton, while committed to the field, foresaw its development in terms of coordinated research institutes. Any responsible professing of the great human achievement known as science required mastery of the whole – all disciplines, times, places, and cultural contexts (and all languages). Sarton, embarrassed by his poor Arabic, could barely claim that mastery. The field would grow in gradual and orchestrated fashion. Delegating the sacred heritage to minimally trained science teachers or fragmenting it among so-called historians who could speak only for bits of it, were both unacceptable (Thackray and Merton, 1972).

Nonetheless, the cold war imperative for citizens to understand science, together with rapid growth of universities, created sufficient demand for professional disciplinary training. Even before it was clear what the profession would be, graduate programs sprang up. While Conant's campaign supplied the impetus, he was quickly forgotten, even (or perhaps especially), at Harvard. Sarton, and later Kuhn would be the iconic figures. In 1955, at an invitation-only meeting organized the National Science Foundation and the American Philosophical Society on the relation of the history, philosophy, and sociology of science to science itself, Sarton's protégé/successor I. Bernard Cohen declared his field's independence. Philosophers and sociologists of science accepted their work as important to the conduct of science, but Cohen insisted that the history of science lay outside it, accountable only to itself (Cohen, 1955).

Quite what had been granted independence was less clear. Each new program reflected local circumstances, relating to institutional missions and structures, and to the power bases of promoters. Many were hybrids, accommodating other fields: the histories of medicine and of technology, the philosophy of science, other elements from the humanities and social sciences, or later, reflecting environmental concern. Many were short-lived; others reinvented themselves, sometimes repeatedly. At first, most students came with some scientific background, and some would go on to work as Conantian general educators, but increasingly the sorts of preparation with which students entered broadened. So too did their interests, the skills they acquired, and the sorts of work they went on to do. Nyhart's «tangled bank» not only reflects the many meanings of «science»; it also reflects the many entities denominated «history of science.»

That variety and instability make it hard to say anything substantive about the relation of the relation of the history of science to science or about what kind of trust we might invest in an historian of science. At the same time, the very division of academic labor that sanctioned an independent history of science was also justifying the omission of history from training in the sciences. Courses like Richards' vanished as science came less to resemble Conant's conception -- in which historical consciousness is the fount of creativity – and more to resemble Kuhn's, in which the pressure to specialize as a paradigm problem-solver deflects the gaze from other possibilities, and thus from accountability. Only in a few detached fields – e.g. psychoanalysis, cell biology, theoretical physics, and forensic science – would present practice continue, at least sometimes, to involve critical engagement with the past.

Within the field, appeals for trust were changing too. So long as the field bore a Conantian stamp, the historian of science was something like a seller of rationality insurance. Just as there were good reasons to invest in insuring one's life, dwelling, and automobile, there were good reasons for investing not only in doctors and engineers, but even in astrophysicists, where the premiums were disguised and the payouts too. But once the history of science had gained its own wee plinth in the hall of disciplines, its axis of trust could become intra-disciplinary. «Contributions» would be judged by peers not the public, and, given the ill-defined character of the field, there might be many groups of these.

As historians of science were rushing off to odd corners of the past and as many scientists were becoming comfortable with paradigms – both the term and the reality it described – others were confronting the real political problems of policy-making in an era of big science. The optimism of the early 1950s, in which science epitomized the union of rationality and democracy, had given way to worries about unaccountability. Finding niches within bureaucracies, the cadres of science not only upset the delicate balance between executive and legislative divisions of government, they also transformed the relations of these with military and regulatory institutions, and with the corporate private sector. Postwar science had changed «the nature of political power,» wrote the most trenchant of the analysts, the political scientist Don K. Price, in *The Scientific Estate*. These events had exposed a «deeper trouble» – the «lack of a theory of the politics of science» (Price, 1965: 5). Price, who, like Conant, had experienced the inner world of defense policy-making, reconceived American governance in terms of four estates – the scientists, supported by the public but free to follow their interests, science-based professionals (e.g. physicians and engineers), who solved problems, administrators of science-based agencies, and the voting public and its representatives.[3] He was struck by the absurdity and wastefulness of this structure: the nation had made a huge investment in producing truth, but left its application to an uneducated electorate. But as an historian too, he recognized the cultural roots of this peculiarly American dilemma. Indeed, he foresaw how these roots might generate precisely the distrust of establishment scientific authority that has become evident in recent years. Ironically, the nation that for Jefferson and Franklin was to exemplify rationality could easily lapse into irrationality.

Similar concerns came in the same years from others: from scientists (Ralph Lapp), administrators of big science (Alvin Weinberg, director of the Oak Ridge National Laboratory), journalists (Daniel Greenberg), and even from the president. In his famous farewell address, Dwight Eisenhower had depicted a crisis in the «free university» as the «government contract becomes virtually a substitute for intellectual curiosity» and the possibility that public policy itself would become «captive to a scientific-technological elite» (Eisenhower, 1961). Though two key articulators of such concerns were Harvard deans – Price, who would become first dean of the Kennedy School of Government, and the physicist and

3. For an exemplary analysis of the conflicting rationalities see (Jasanoff, 1987).

ex-presidential science advisor Harvey Brooks who was dean of engineering -- neither made serious contact with Conant's legacy.

Almost a generation later, as the cold war receded, accountability issues reappeared in a different form in the reflexive modernism literature concerned with navigating the welter of risks that permeated modern life. Recognizing how illusory was the hope for a singular response from an ultimate authority, Ulrich Beck, Anthony Giddens, and others focused on Price's second estate, the scientist as professional, called in by persons and communities to deal with particular problems. Here, rationality appeared as disunity. To these experts, declared Giddens, «disagreement or critique» was the norm, «the *motor* of their enterprise» (Giddens, 1994: 85-89; Beck, 1994: 9, 27, 31, 33, 51-52; Marty, 2010: 43-44). Complementing such views was the emerging feminist standpoint epistemology. Highlighting the hubris of searching out the «God's-eye view,» a quest that had indeed characterized much science and thus much history of science, Donna Haraway urged us to recognize embodied and local perspectives – always, ways of seeing were ways of not seeing (Haraway, 1988). The apotheosis would be what Fuller called a «republican science» a colloquy of engagements. As everyone had some expertise, a democratic and egalitarian division of labor would solve problems (Fuller, 2000a: 112-14). But even if that client-centered approach worked in ideal communities, it was less suited to issues requiring trust in prompt, global action, like pandemics or anthropogenic climate change – a «dangerous adventure» that Giddens saw only as a possibility and Fuller doubted (Giddens, 1994: 59-60; Fuller, 2000a: 104).

For most of this period, historians of science were paying less attention to political than to epistemic accountability, however. Kuhn's postulation of incommensurable world views had raised the bogey of relativism and by the early 1980s many had found their way to the stronger brew of social constructivism. For Conant, to whom science was always situated, patently perspectival, and ever in flux, these had been non-issues. Scientists believed what they thought best to believe; to do otherwise was to risk being a victim in the cut and thrust of criticism.

That constructivism underlies Nyhart's allusion to benefits and harms done in the name of science, but it raises questions too – did benefits and harms indicate a zero sum game? Did they represent errors that a more historically savvy science planner might have avoided? Or were they simply perversities of contingency? No one answer fits all. Some happily wandered through underexplored contextual terrains of past science seeking their own hoard of nuts and berries, but even as they pointed to the interesting things they had found, they found it difficult to avoid engaging with questions of what science might otherwise have been. Steve Woolgar had been struck by the «irony» inherent in such explanation: that is, identifying contingent determinants of some event involves appeal to a counterfactual that would have occurred without these. Thus, to focus on harms justified in the name of science involved imagining a purer and better science that would have existed otherwise (Woolgar, 1983).

And while diversity was all very well, Giddens' alternative – spectating at gladiatorial contests between experts representing rival interests – was not, for most, a satisfactory alternative. It was particularly unsatisfactory with regard to «what to trust» questions. For many historians were involved in praxis; rather than seeking to escape a God's eye view, the historian-optician would simply offer us a better set of glasses.

Thus, what has been seen by some as a crisis: the appeal to epistemic relativism to do political work in fact fed the political relativism it was intended to resolve. Fear that science studies, broadly conceived, has contributed to distrust in science has understandably led to hand-wringing among some in the field, and to finding ways to reanimate roots of our common inquiry in ways that avoid that outcome (Lynch, 2020).

III. The Chemists' Way

Thus far, my sketches both of the problem of trust and of the history of the history of science have focused on American responses to American problems. Americans, noted Price, wanted scientists to "profess no interest in philosophy" but to provide practical goods. What struck him about post-war American scientists is that they were claiming to represent philosophical authority but failing to supply it. The new physicists could still build bombs, but they had given up promulgating any united knowledge in favor of paradoxes and pessimism. The enigmas they had to offer were no substitutes for public trust (Price, 1965: 30, 80, 102, 107).

In the next sections I consider more general approaches to trust by exploring a narrative of the history of science that unites Price's four estates – making truth, its application in professional settings, the coordination of policy, and responsiveness to lay publics. The approach resembles Susan Lindee's «frozen peas» epistemology of highlighting familiar sites of application though it goes beyond recent technologies to explore scientific authority as a means of open-ended problem-solving (Lindee, 2019). Mostly this authority avoids abstraction. Sometimes it is anti-hegemonic; sometimes it represents an integrative response to fragmented disciplinary authority. It exists in a wide variety sites and contexts, and, as Lindee suggests, should be recognized as making up a much larger portion of the vines on Nyhart's tangled bank than is usually the case.

I do so by explicating a cryptic comment made in a seminar by my doctoral mentor, the chemist and historian of chemistry Aaron Ihde, around 1978. Ihde had been a Conant teaching assistant in 1952; he would commit his career to the ideal of history- but also current events-based general science teaching (Rocke, 2000). Ihde's comment was that «all persons are either physicists or chemists.»

The contrast he was making was more Conantian than Price-ian, more about the practice of science than its political authority, but the division he saw was between pursuing metaphysical authority (physics) or solving practical problems (chemistry). He was not making a value judgment so much as recognizing that persons beginning careers in the physical sciences would confront that tension. The latter option was clearest in the pragmatic concept of the chemical element, formulated by Robert Boyle, formalized by Lavoisier. Ultimate composition was less important than having distinct entities that could be manipulated. To their successor Conant, the theories of chemistry, and of science more broadly, were heuristics to be assessed in terms of utility.

Ihde's own manifesto was a brief 1956 article on «The Pillars of Modern Chemistry» published in the *Journal of Chemical Education*, a prime venue for the shaping of Conantian case studies (Ihde, 1956). There he challenged two textbook views – one, that there was no chemistry before Lavoisier's belated scientific revolution; the other that chemistry was alchemy stripped of its nonsense. Instead, Ihde saw chemistry, a field largely excluded from university curricula until the mid-nineteenth century, as artisanal knowledge. Its three pillars were alchemy, medicine, and metallurgy. Notably, his analytic is Conant's, experiment. Notably too, this knowledge – what I will begin to call «expertise» -- is local, useable, and requires trust. Ihde was not explicitly making a point about authority, yet it is worth noting that he, who had grown up on a dairy farm and begun his career as a dairy chemist was (as it were) uncowed by a view of science grounded in the history of philosophy. His fellow chemist Conant, likewise a middle-class outsider from an artisanal background, had a similar impatience with supercilious intellectualism.

But both Conant and Ihde were challenging a unitary authority. Ironically, Descartes' resurrection of philosophy in the midst of the Thirty Years War had been a response to a crisis in religious authority (Funkenstein, 1986; Toulmin, 1990; Gaukroger, 2006). If most familiar as «Newtonianism» that claim to authority had flourished most fully in eighteenth-century Germany as the universal rationalization promulgated by Leibniz' disciple Christian Wolff. The title of the first of a series of vernacular bestsellers, *Rational Thoughts on God, the World and the Soul of Man, and on All Things Whatsoever* (1718), indicates the breadth of Wolff's ambition as a philosopher to the absolutist Prussian state. Wolff's metaphysical approach gained great popularity as *weltweisheit,* (world wisdom) (Schatzberg, 1973), and, at a general level, he and later Immanuel Kant, did consolidate knowledge of biogeochemical processes underlying much commerce, industry, and agriculture. Yet the scorn in Voltaire's *Candide* shows the great gap between universal authority grounded in metaphysics and local and personal accountability – Wolff offers nothing to trust; to call the cosmos wise is an insult to those it already injures. The «useful knowledge» Benjamin Franklin and his colleagues would develop in the next decades in Republican Philadelphia would largely avoid all that (Webster, 2010).

I focus here on six distinct modes of being both intellectual and expert, ways of intervening that are professional, integrative, and responsive. By focusing on a few elements in the careers of a few persons, I hope to suggest how widespread are such activities and roles.

As Price presciently recognized, a great deal of the American response to science has been grounded in religion, less as doctrine than as cosmology. Such cosmological concerns are not uniquely American. An interesting contrast with Wolff is his predecessor in the building of hope, *the chemist as pastor*, the Lutheran minister Johann Arndt (1555-1621). Arndt's *Four Books on True Christianity* (1605-1612), the most important German devotional of the seventeenth century, challenged the aloofness of Lutheran orthodoxy and would become the seminal text of Pietism. A part of that popularity rested on the worldly fourth book, a cosmology presented in terms of Paracelsian technologies that reflects Arndt's own work as physician-alchemist as well as pastor. Where Wolff would insist in the goodness of what was, Arndt focused on the goodness of what was to come. Dealing in purpose, providence, and grace – all in various ways exemplified in Paracelsus' ongoing legacy – he countered a prevailing gloom, grounded in experience (the coming of the little ice age on top of war and plague), theology (preoccupation with a fallen world and a damned species), and ecclesiology (disenchantment with the capacity of the orthodox Lutheran establishment to offer hope) (Trepp, 2009).

The chemist as politician-theologian is Robert Boyle (1627-1691). Boyle was Conant's model scientist, operating pragmatically as he moved back and forth between experiment and heuristic conceptualization. But Conant, as student of the history of the politics of learning in seventeenth-century England, was aware too of the political resonances of experiment as seen by Boyle, John Wilkins, and other founders of the Royal Society. Experiments would deflect the force of «intellectualist» interpretations of God's ways and means in running the world, of the Cambridge Platonists Henry More and Ralph Cudworth. In Boyle's view, such arguments, whether stemming from ancient philosophy or from moderns like Descartes, Paracelsus, or any number of others, enabled sectarian «enthusiasts,» claiming private lines to God, to confuse the citizenry and destabilize public order. In *A Free Enquiry into the Vulgarly received Notion of Nature,* Boyle outlined an individualism even more radical than that of the sectarians. Following the medieval nominalists, he urged the public to trust in things not words. In terms similar to his challenge to «element» he challenged «Nature»; it was a term without clear meaning, being used to claim authority. Rather than invoking it, one should accept the phenomena of the world disclosed by experiment, and understand them as God's free actions (Mandelbrote, 2007).

The chemist as public servant is A. L. Lavoisier (1743-94), not as the lone pioneer of chemistry martyred by a revolutionary mob, but as one of a stable of state experts. Along with Lavoiser one might highlight J. A. Chaptal (1756-1832), L. B. Guyton de Morveau (1737-1816), and A.F. Fourcroy (1755-1809), but there were many others too. In 1666, Louis XIV's finance minister-logistician J.B. Colbert had made expertise central in French public life by recruiting to the new Académie des Sciences illustrious savants who would not only be ornaments to the court, but rational policy analysts. A century later this had evolved into a practice of commissioning of small groups of savants to investigate and report to the government on particular problems and potential solutions. Rarely were the problems the exact specialties of any of commissions; the authorities were tapping a more general set of skills – the ability to think analytically and quantitatively; familiar with general principles; and access to suitable reference tools – both persons and texts that might be consulted. Lavoisier's *Oeuvres* are packed with reports of committees on which he served – on lighting, water, gunpowder, fires, food, noxious trades, and sanitation. Moreover, the solving of particular technical problems broadened into reform of institutions, educational and executive, for addressing these more generally, in industrial, infrastructural, and health-and-welfare contexts (McKie, 1962; Lavoisier, 1965; Hahn, 1971; Dhombres, 1989a, 1989b; Gillispie, 2004; Mukerji, 2009). It persisted too as a characteristic feature of French institutions after the revolutionary and Napoleonic eras. But though the experts were jealous for state honors, they rarely claimed to be embodying a metaphysical authority – they were calculators and positivists.

For *chemists as economists*, I pick Ellen Swallow Richards (1842-1911) and Alice Hamilton (1869-1970). Where recognition of the conservation of energy has attracted much attention as a metaphysical commitment, the conservation of matter is often overlooked. That everything must come from somewhere and go somewhere had long been a reality in many technical practices; with Lavoisier's revolution in chemistry, ideas of material budgets and chemical cycles would become prominent. They underlie the sanitarian consciousness and the unit operations approach in chemical engineering, but here I focus on the most local sites of budgeting, household and workplace. In America, these sites of expertise were intriguingly gendered around 1900: based at MIT, the chemist Richards expanded her horizon from water supply to all the materials of domestic life (she was a pioneer in the field that would come to be known as «home economics» and later «human ecology») (Richardson, 2002, Swallow, 2014). The physician-chemist Hamilton would expand from a foundation in the settlement house movement into becoming a pioneering authority on workplace toxicity, and the first female professor at Harvard -- of industrial hygiene (Sicherman, 1984, Ringenberg, 2019).

The chemist as historian is John Theodore Merz (1840-1922), author of the four volume *History of European Thought in the Nineteenth Century,* published between 1896 and 1914, a forgotten gem in the history of science. Though born in England, Merz was educated in Germany, and moved over his career from chemistry and mathematics to philosophy, then to chemical and electrical technology, and finally back to history and philosophy (Micheli, 2006). Readers who expect a book on *thought* to be a de-contextualized vindication of elite and abstract knowledge will be surprised. Merz writes not as an authority but as a (very well read) outsider, an appreciative but critical consumer of the intellectual wares on the marketplace. At a time when disciplines were becoming the authoritative units of science, Merz took a pluralistic ways-of knowing approach, recognizing disciplines and their composite, science itself, as collections of tools and choices of explanatory agendas. He saw too that these were cultural and contingent, describing how very different scientific authority was in France, Germany, and England, and apologizing for his inability to explore other traditions – he was clearly fascinated by Russia. He recognized too how certain concepts – or at least terms (e.g. «evolution» and «energy») -- had come to comprehend much territory, while recognizing too that they operated mainly as metaphors (Merz, 1965).

The chemist (or physicist) as skeptic, begins with Merz and ends with Conant. Merz, himself chemist and technologist, admired a group of contemporary chemists who maintained an austere positivism. Good scientists did not need to know. That experiments confirmed theories or models proved nothing about what could not be directly observed; more important was empirical knowledge of the relations of causes to effects. Conant's teacher Richards was a physical chemist. Others, like Gustavus Arrhenius (1859-1927), Wilhelm Ostwald (1853-1932), and J.H. van't Hoff (1852-1911), represented the forefront of rigor in his student days. They were hardly uninterested in authority; they aspired to a greater authority through greater abstraction. Merz, noting some of the early experiments what would explicate the structure and behavior of atoms in the next decades, thought they might be wrong about atoms, yet he welcomed their presence within a dialectic, and noted that particulate explanations, after all, still begged questions about the causes of forces.

Epitomizing such pragmatism was Conant's colleague, Percy Bridgman (1882-1961), awarded the 1946 Nobel prize for high pressure experimentation. It is Bridgman who takes us back to my original issues of the relation of scientific authority to public life, and of the role of the historian of science as a trusted intermediary. Most of what Conant and Price say Bridgman has said more bluntly – he pushes their arguments to logical extremes.

Bridgman had backed into philosophy. Disenchantment with the abstraction and unintelligibility of the physical theories of the quantum pioneers in the 1920s led him to articulate a boiled down philosophy of science that went beyond the skeptical physical chemists. Bridgman called it operationalism: the scientist only *knew* what the instruments did. This is no proto-paradigm concept; there is no surrender to a normative concept of normal science, no capture within a world view, for any concept of the world is superfluous and unwarranted.

Bridgman would extend his minimalism toward a generic denial of authority, manifesting variously as nominalism, existentialism, and libertarianism. We lived in a world of actions and consequences; no one was absolved of responsibility. Thus, he was contemptuous of the pretentious «new priesthood» of atomic physicists with their «social responsibility of science» movement. Robert Oppenheimer's famous statement about the physicists having known sin was so much whining. Physicists were neither martyrs nor sages, but only artisans who sold skills and made choices. As for Conant's history-based science teaching, he doubted that acquaintance with the remote scientific past would yield any useful practical skills for the scientific present. While Professor Bridgman was certainly in favor of teaching physics to those who wanted to learn, he could not commit to Conant's concept of a social good, for there was no «society» nor was democracy anything more than a default state. All choose all the time; public life was simply a composite (Bridgman, 1955; Walter, 1990).

To Conant, the chemist for whom history ruled, that ignoring of institutions and cultures was naïve. Totalitarianism of right and left threatened. Some futures were better than others; securing them was a progressive educator's job.

Notably, none of these interventions gains any strength from being called «science» In their various ways Boyle, Bridgman, and the physical chemists challenge such calls; the others highlight services. Chemistry, conceived in terms of Ihde's dichotomy, is already the site of many of our encounters with trusted expertise.

IV. Political cultures make a difference

Though the examples included under «the chemists' way» are not uniquely American, they address a political culture of longstanding tensions between authority and utility that have recently worsened.

But histories of science will always be parts of political cultures and we should not expect issues of trust that exist in one place to be identical to those in others (Jasanoff, 2005). My outsider's impression of issues in the history of Catalan science is in some ways of the American situation with reversed polarities. In connection with other projects, I have become interested in two Catalan scientists, Mateu Orfila (1787-1853) and Jaume Ferran y Clua (1851-1929). Both could be included among my chemists – equally in terms of their specialties and with regard to the local and practical contexts of their work. But a key part of the story for both is marginalization, inaccessibility to a more general authority. Thus, where Lindee's program and my «chemists way» are approaches to securing recognition for focused expertise in a setting suspicious of general authority, here, if I am right, the cultivation both of science and of its history reflect concern with an under-recognition of that general authority.

In Catalonia too, there was interest in history-based science teaching even before Conant (Roca-Rosell and Grapí-Vilumara, 2010), but the impetus to cultivate the history of science was more as an antidote to regional marginalization than a route to managerial meritocracy. Science would be a conspicuous element in the movement, beginning in the 1920s, to recast Barcelona and its region as a European metropolis rather than an Iberian province (Perdiguero, Pardo-Tomás and Martinez-Vidal, 2009). The success of that effort is evident in the Gallery of Catalan Scientists of the Institute for Catalan Studies (Roca i Rosell, 1988).

While we do not *need* to understand the work of Orfila and Ferran in terms of Catalan identity, doing so brings ways of understanding their careers, and the course of science more broadly, in ways that would not otherwise have been evident. Arriving in Paris in 1807, in the golden age of French medicine, Orfila had become by 1830 dean of the Paris Medical School, secretary of the Academy of Medicine, and founder of a new field, toxicology. Understandably, he would see his career as a success (Bertomeu Sánchez and Nieto-Galan, 2006). And yet we may ask what that judgment really meant. Had he done well for a young man from Minorca and Barcelona, or in more universal terms? At the time

French science and medicine were notorious not only for Franco-centrism and Paris-centrism, but for disciplinary hierarchies (Ackerknecht, 1948, 1967). Basic science took precedence over applied; clinical medicine and surgery over ancillary medical sciences like chemistry. Orfila's specialty, toxicology, was a branch of legal medicine, a field on the margins of *Hygiene publique,* itself marginal (Ackerknecht,1948). Then, as later, prestige and authority were tied to abstraction. At a time when pathological anatomy dominated research in the Paris hospitals, the Paris morgue was overlooked as a research site, as its director Alphonse Tardieu discovered, and there appears to have been a similar separation between Magendie's physiology and Orfila's toxicology: the methods and many of the questions were similar, the contexts were different (Lesch, 1984; Bertherat, 2019). Those divisions would affect medicine and public health, and the status of environmental toxicology in particular for some time to come. As Bettina Wahrig (2006) notes, a reunified pathology would come only in the 20th century.

Thus, to be first in a field that is seen in caste-like terms -- a domain doubtless necessary, but to be done by others – is an ambivalent achievement. A medical deanship, too, may represent willingness to do thankless administrative work more than intellectual leadership – as at contemporary Edinburgh. But simply to ask these questions reflects the importance of an historical agenda sensitive to regions and cultures.

Catalan status is more conspicuous with regard to the trial use of a live-culture cholera vaccine by the physician Jaume Ferran in 1884-5. Having been sent by the Barcelona authorities to learn bacteriology from students of Robert Koch, Ferran quickly developed a vaccine based on the precedents of Jenner and Pasteur, tested it on animals, on himself and other medical volunteers, and then, on his own authority, began to use it in the midst of a European epidemic, seeking as far as possible to collect the comparative data that would be needed to determine its effectiveness. When his approach seemed to work, he was besieged by investigatory commissions from many nations. Many investigators were critical, some were hostile. Within Spain, support came from Barcelona; opposition from Madrid, which temporarily banned the inoculations. It is hard not to see hypocrisy here. There was a crisis; others intervened on more dubious grounds, sometimes coercively. One can imagine a rational polity jumping in to support Ferran's research: that did not happen.

Here too, my understanding of Ferran's case, based mainly on the long narrative of the sympathetic American investigator E.O Shakespeare (Hamlin, 2009) evolved as I began to think about Ferran in terms of Barcelona, which was his career home both before the epidemic and subsequently – he served as a public health administrator and director of the city's bacteriology laboratory during the period in which science was coming to be seen as a central part of Catalan distinctivness.

Gradually, partly because making a good cholera vaccine remained difficult, Ferran has gained some respect in intra-medical contexts as a cholera vaccine pioneer. But almost always the recognition has been ambivalent – he had made a lucky guess but then acted audaciously and irresponsibly upon it. For good and trustworthy science, the critics had insisted, happened only in serious laboratories, like those in Paris or Berlin. In that history, the trials of Haffkine's vaccine on Indian tea plantation workers have exemplified both rigor and ethics, while Ferran, administering his vaccine to fellow citizens who not only consented but whose survival was at stake, exemplified neither (Bornside, 1982). In *Arrowsmith* (1925) the greatest of American medical novels, Sinclair Lewis would explore the dilemma of conducting controlled vaccine trials during a deadly epidemic; like Ferran, whose story was in fact widely reported in American newspapers, his protagonist would sacrifice producing knowledge for saving lives.

V. Trusting the History of Science or the Historian of Science

Both Ferran and Arrowsmith reject the discipline of a science to act as Giddensian experts. They exercise a professional's discretion in supplying an authority that is local and responsive. Our present situation – an effective, safe, and sanctioned Covid vaccine distrusted by some who might benefit from it -- is a sobering reminder that trustworthiness does not dictate trust.

I have suggested that, at least in America, trust in science is likelier to be rooted in trust in experts as problem solvers than in assertions of authority. Surely, historians of science have various problem-solving skills as individuals that they use in diverse settings, but do they have those skills as historians of science? Worries that the field's indulgence of postmodern relativism has led to distrust in science are predicated on some notion of what a more trustworthy version might have been. That in turn begs the question of the identity and purposes of the history of science, i.e., with what problems it can be trusted to solve. Thus, one might expect «professional» historians of science to resemble other professions in having a clearly defined domain of practice, a common mission, mastery of some core techniques, and a code of conduct. Many other fields – domains within philosophy, psychology, anthropology, sociology, theology, education – have much more of that than we do.

I have called attention to the extraordinary arrogance of I.B. Cohen's 1955 rejection of a public role for the history of science. Some in the new NSF had appreciated that those able to see the whole of science over the long term represented an expertise valuable in science policy. For Cohen, however, the enterprise was above utility. Some share Cohen's view, and I am not addressing them. Those who want to reopen the question, however, face the problem of finding some trustworthy expertise in an enterprise far less coherent than the one Cohen spoke for. It is quite true that we now recognize much more of the temporal, cultural, and topical complexity of science than Conant or Cohen did. But that incoherence goes beyond the multiplicity of specialist topics/contexts -- the fragmentation Shapin (Shapin, 2005) calls «hyperprofessionalism» – to include ambiguity about the nature of the enterprise, which in turn complex contingencies in its evolution, including accidents of boundary-drawing, and the problematic character of its most conspicuous exemplar.

The enterprise first: is the history of science field, discipline, profession, subject to be taught, or what? While the history of science has been a domain of science criticism, it has also been insular: we rarely think of readers who are not colleagues. Amid claims of disciplinary autonomy are claims of relevance, involving an implicit acknowledgment of professional responsibilities, usually to students, our main group of «clients». Explicitly or not, our subject will include «the nature of science» and our teaching will be loosely Conantian, an effort to mediate between sciences and publics. Conant distinguished questions of the teaching of a subject from the development of a discipline: history of science as a *means* did not imply history of science as an *end* – his view of Sarton's project.

For Conant, studying points of divergence – where science suddenly became not only different from what it had been, but different from other things that it might be – was valuable in exposing students to the eternal problems of rational choosing in settings that were both generic and unique, problems faced by administrators who must integrate or captains who must navigate. In these moral tales, a higher authority must undertake an antiauthoritarian critique of subordinate partial authorities. He was using history to represent to students the sort of real-time decision-making that had been required in managing wartime research. But he was telling moral tales about good and bad science and good and bad scientists. In that age of Lysenko, they certainly went together.

Many historians of science do tell moral tales – often the goal of their research will be to reveal the harms or benefits Nyhart alludes to. I have noted already the counterfactuals to which historians of science often appeal – Woolgar's paradox. Often these are framed in a familiar ideal, the Mertonian norms, which remain markers of what trustworthy science should be, even while they have been rejected as realities – the world they described never existed and cannot now. Thus, Conant appeals to a skepticism modeled on Hume's «mitigated skepticism.» The Catalan stories as I have told them, like other stories of cultural, gender, racial, or ethnic prejudices within science, concern the Mertonian norms of communism and universalism. Failure to follow these norms is not only a matter of fairness; it impedes scientific progress. Or, in the most direct treatment of «trust» in science, Oreskes (Oreskes, 2019) pours strong acid on pretensions of disinterestedness. If Merton is unrealistic and outdated (as he himself recognized), one may wonder why he is being appealed to. The answer is that descriptions/explanations of practices are not adequate responses to the problem of trust, which involves expectations of (and thus guides to) conduct (Mitroff, 1974; Ziman, 1996; Thorpe and Shapin, 2000; Shapin, 2008; Hicks and Stapleford, 2016).

Some of the reluctance to confront the differences between descriptive and normative is likely a holdover of Cohen's commandment. The history of science he envisioned replicated the ideals of the subject it studied – it would be pure inquiry into the purest of inquiry. Some is more general: views that normativity and with it trust-building, are inferior to truth telling as forms of academic existence, and even as *unprofessional* in the higher grades of academic life, have long histories and go far beyond our field. Yet much of the confusion in our field reflects changes in the enterprise in the seven decades since Cohen's declaration of independence. In the later 1950s, historians of science, confident that there would be a career path for them, erected fences. They confirmed the existing separation with historians of medicine – equally aloof – while ejecting the historians of technology, who would go on to develop their own disciplinary identity between 1957 and 1960 around a nexus of management theory, engineering education, and utopian social criticism. In the next decades, other approaches to what is now known as Science and Technology Studies would appear, each arising in a particular, often a local, institutional setting, and claiming an unoccupied niche of authority (Seely, 1995; Sinclair, 1995; Post, 2010; Fox, 2006; Edge 1995).

Students, however, often were concerned with normative issues and trust, in matters of medicine, technology, and the environment. Hence, the creating and maintaining of career paths would involve mergers or quiet readmissions to the history of science what had earlier been cast out. Yet pulling down the old fences did not automatically bring unity or even clarity about scholarly identities, roles, or conceptions of authority (Hamlin, 1992). Cohen had imagined a transcendent discipline in which he as Sarton's heir would do the disciplining, pruning any tangles to reveal the tree in the midst. But no one now does this job, nor is it clear how anyone could.

Last is the long infatuation with the legacy of Kuhn. That we can use a phrase like «post-Kuhnian» suggests how much his paradigm has dominated science studies for over a generation. Yet with regard to the key question, «Is science authoritative?» his paradigm concept was both confusing and stifling. The answer was «yes» and «no.»

So should someone trust me? Historians of science study trust in many settings. While being a professional historian of science brings no extra dose of trustworthiness, it should alert us to better and worse ways of responding where trust is at issue.

Like Shapin, I have been drawn to a common example of trust in science: the vast trust we are extending in boarding an aircraft. I agree with his historian's reflection – the difference between seventeenth-century gentlemen philosophers who knew the friends they were trusting and ourselves, who know nothing of the persons or the mysterious networks to which we are entrusting our lives (Shapin, 1994). Yet I would not cite Shapin's observations in seeking to calm a seat-mate anxious about takeoff. Instead, I would dredge up from history of science background, two other forms of rationality, familiar, but hardly unique to our field. One is the old saw that knowledge follows interests. Others, notably the aircrew, are investing in the same mysterious networks. They run the same risks as we do but more often. Their greater skills in achieving safe outcomes will benefit us too. The other is a reflection on the philosophy of air itself that is both Socratic and Conantian. Looking out at the curvature of the wing, I invite my seatmate (and myself) to consider the particulate quality of air, and then to try to disbelieve in the law of airfoils: can there not be lift?

Demanding trust will do no good, but comfort may come without drugs or authority. Of course, there is still the matter of thrust adequate to the payload.

References

ACKERKNECHT, E.H. (1967), Medicine at the Paris Hospital, 1794-1848, Baltimore, MD: Johns Hopkins University Press.

ACKERKNECHT, E. H. (1948), «Hygiene in France, 1815-1848», Bulletin of the History of Medicine, 22, (1), 117–155.

BARBER, B. (1983), The logic and limits of trust, New Brunswick, N.J.: Rutgers University Press.

BAUER, H. H. (2001), Knowledge Fights; Science or Pseudoscience: Magnetic Healing, Psychic Phenomena, and Other Heterodoxies, Chicago, IL: University of Illinois Press.

BECK, U. (1994), «The Reinvention of Politics». In: BECK, U.; GIDDENS, A.; LASH, S., Reflexive modernization : politics, tradition and aesthetics in the modern social order, Stanford, CA: Stanford University Press, 1-55.

BERTHERAT, B. (2019), «Cleaning Out the Mortuary and the Medicolegal Text: Ambriose Tardieu's Modernizing Enterprise». In: BURNEY, I. A; HAMLIN, C. (ed.). Locating Forensic Cultures: Making Fact and Justice in the Modern Era, Baltimore: Johns Hopkins University Press, 257–78.

BERTOMEU SÁNCHEZ, J. R. AND NIETO-GALAN, A. (2006), «Mateu Orfila and his Biographers». In: BERTOMEU SÁNCHEZ, J. R. AND NIETO-GALAN, A. (ed). Chemistry, medicine, and crime: Mateu J.B. Orfila (1787-1853) and his times, Sagamore Beach, MA: Science History Publications, 1–24.

BORNSIDE, G. H. (1982), «Waldemar Haffkine's Cholera Vaccines and the Ferran-Haffkine Priority Dispute», Journal of the History of Medicine, 37, (4), 399–422.

BRIDGMAN, P. W. (1955), Reflections of a physicist [2d ed.], New York: Philosophical Library.

COHEN, I. B. (1955), «Present Status and Needs of the History of Science», Proceedings of the American Philosophical Society, 99, (5), 343–347.

COLLINGRIDGE, D.; REEVE, C. (1986), Science speaks to Power: The Role of Experts in Policy-Making, New York: St. Martin's Press.

COLLINS, H. M.; EVANS, R. (2002), «The 3rd Wave of Science Studies», Social Studies of Science, 32, (2), 235–296.

COLLINS, H. M.; EVANS, R. (2003), «King Canute meets the Beach Boys: Responses to the Third Wave», Social Studies of Science, 33, (3), 435–452.

CONANT, J. B. (1960), «History in the Education of Scientists», Harvard Library Bulletin, 14, (3), 313–33.

DHOMBRES, N. (1989a), Les Savants en Révolution: 1789-1799, Paris: Cité des sciences et de l'industrie.

DHOMBRES, N. (1989b), Naissance d'un pouvoir: sciences et savants en France (1793-1824), Paris: Payot.

DOUGLAS, M; WILDAVSKY, A. (1982), Risk and Culture: an essay on the selection of technical and environmental dangers. Berkeley, Calif: University of California Press.

EDGE, D. (1995), «Reinventing the Wheel». In: Jasanoff, S. and Society for Social Studies of Science (ed.). Handbook of science and technology studies. Thousand Oaks, Calif.: Sage Publications, 3–23.

EISENHOWER, D. (1961), «Transcript of President Dwight D. Eisenhower's Farewell Address (1961)»: https://www.ourdocuments.gov/doc.php?flash=false&doc=90&page=transcript (Accessed: 11 June 2021).

EMBREE, L.; BARBER, M. D. (2017), The golden age of phenomenology at the New School for Social Research, 1954-1973, Athens, Ohio: Ohio University Press.

FOX, R. (2006), «Fashioning the Discipline: History of Science in the European Intellectual Tradition», Minerva, 44, (4), 410–432. doi: 10.1007/s11024-006-9015-x.

FULLER, S. (2000a), The governance of science: ideology and the future of the open society, Buckingham [England]; Philadelphia, PA: Open University Press.

FULLER, S. (2000b), Thomas Kuhn: A Philosophical History for Our Times, Chicago: University of Chicago Press.

FUNKENSTEIN, A. (1986), Theology and the Scientific Imagination from the Middle Ages to the Seventeenth Century, Princeton, N.J.: Princeton University Press.

GAUKROGER, STEPHEN. (2006), The emergence of a scientific culture: science and the shaping of modernity, 1210-1685, Oxford: Clarendon.

GIDDENS, A. (1994), «Living in a Post-Traditional Society». In: BECK, U; GIDDENS, A.; LASH, S., Reflexive modernization: politics, tradition and aesthetics in the modern social order, Stanford, Calif.: Stanford University Press, 56–109.

GILLISPIE, C.C. (2004), Science and polity in France: the revolutionary and Napoleonic years, Princeton: Princeton University Press.

GOODWYN, L. (1976), Democratic promise: the Populist moment in America, New York: Oxford University Press.

HAHN, R. (1971), The anatomy of a scientific institution: the Paris Academy of Sciences, 1666-1803, Berkeley, Calif: University of California Press.

HAMLIN, C. (1992), «Reflexivity in Technology Studies: Toward a Technology of Technology (and Science?)», Social Studies in Science, 22, (4), 511–44.

HAMLIN, C. (2007), «STS: Where the Marxist Critique of Capitalist Science Goes to Die?», Science as Culture, 16, (4), 467–474.

HAMLIN, C. (2008), «Third-wave Science Studies: toward a history and philosophy of expertise». In: Carrier, M.; Howard, D.; Kourany, J. A. (ed.). The Challenge of the Social and the Pressure of Practice: Science and Values revisited, Pittsburgh: University of Pittsburgh Press, 160–85.

HAMLIN, C. (2009), Cholera: the biography, Oxford: Oxford University Press.

HAMLIN, C. (2016), «The Pedagogical Roots of the History of Science: Revisiting the Vision of James Bryant Conant», Isis, 107, (2), 282–308.

HAMLIN, C; SHEPARD, P.T. (1993), Deep Disagreement in U.S. Agriculture: Making Sense of Policy Conflict, Boulder, CO: Westview.

HARAWAY, D. (1988), «Situated Knowledges: The Science Question in Feminism and the Privilege of Partial Perspective», Feminist Studies, 14, (3), 575–599. doi: 10.2307/3178066.

HICKS, D. J.; STAPLEFORD, T. A. (2016), «The Virtues of Scientific Practice: MacIntyre, Virtue Ethics, and the Historiography of Science», Isis, 107, (3), 449–472. doi: 10.1086/688346.

IHDE, A. J. (1956), «The Pillars of Modern Chemistry», Journal of Chemical Education, 33, (3), 107–110.

JASANOFF, S. (1987), «EPA's Regulation of Daminozide: Unscrambling the Messages of Risk», Science, Technology, & Human Values, 12, (3/4), 116–124.

JASANOFF, S. (2003), «Breaking Waves in Science Studies: Comment on H.M. Collins and Robert Evans `The Third Wave of Science Studies'», Social Studies of Science, 33, (3), 389–400.

JASANOFF, S. (2005), Designs on Nature: Science and Democracy in Europe and the United States, Princeton: Princeton University Press.

JONAS, H. (1973), «Technology and Responsibility: Reflections on the new tasks of ethics», Social Research, 40, (1), 31–54.

LATOUR, B. (1987), Science in Action, Cambridge, MA: Harvard University Press.

LAUDAN, R. (1993), «Histories of the Sciences and Their Uses: A Review to 1913», History of Science, 31, (1), 1–34. doi: 10.1177/007327539303100101.

LAVOISIER, A. L. (1965), Oeuvres de Lavoisier. New York: Johnson Reprint Corp, volume 3.

LESCH, J. E. (1984), Science and medicine in France: the emergence of experimental physiology, 1790-1855, Cambridge, Mass: Harvard University Press.

LINDEE, M. S. (2019), «The Epistemology of Frozen Peas: Innocence, Violence, and Everyday Trust in Twentieth-Century Science». In: Oreskes, N. et al., (ed.). Why Trust Science?, Princeton: Princeton University Press, 163–180.

LUHMANN, N. (1979), Trust and power. New York: John Wiley & Sons.

LUHMANN, N. (1989), Ecological communication. Translated by J. Bednarz jr. Chicago: University of Chicago Press.

LUHMANN, N. (1993), Risk: a sociological theory. Translated by R. Barrett. New York: de Gruyter.

LYNCH, W. (2020), Minority report dissent and diversity in science, New York: Rowman and Littlefield.

MANDELBROTE, S. (2007), «The Uses of Natural Theology in Seventeenth-century England», Science in Context, 20, (3), 451–80.

MARTY, M. E. (2010), Building cultures of trust, Grand Rapids, Mich.: WB Eerdmans.

MCCLELLAND, C. E. (1980), State, society, and university in Germany, 1700-1914, Cambridge UK: Cambridge University Press.

MCKIE, D. (1962), Antoine Lavoisier: scientist, economist, social reformer, New York: Collier Books.

MERTON, R. K. (1976), « The Ambivalence of Scientists». In: Merton, R.K, Sociological Ambivalence and Other Essays, New York: Free Press, 56–64.

MERZ, J. T. (1965), A history of European thought in the nineteenth century, New York: Dover Publications.

MICHELI, G. (2006), «John Theodore Merz». In: Grayling, A. C.; Pyle, A.; Goulder, N. (ed.). The Continuum encyclopedia of British philosophy. Bristol: Thoemmes Continuum.

MITROFF, I. I. (1974), «Norms and Counter-Norms in a Select Group of the Apollo Moon Scientists: A Case Study of the Ambivalence of Scientists», American Sociological Review, 39 (3), 579–595.

MUKERJI, C. (2009), Impossible engineering technology and territoriality on the Canal du Midi. Princeton: Princeton University Press.

NASH, L. (1952), «The Use of Historical Cases in Science Teaching». In: Cohen, I.B.; Watson, F.G., (ed.). General Education in Science, Cambridge MA: Harvard University Press, 97–118.

NASH, L. (1963), The nature of the natural sciences, Boston: Little, Brown.

NYHART, L. K. (2016), «Historiography of the History of Science». In: Lightman, B. (ed.). A Companion to the History of Science. New York: Wiley-Blackwell, 7–21.

ORESKES, N, ET AL. (2019), Why Trust Science?, Princeton: Princeton University Press.

ORESKES, N.; CONWAY, E. M. (2010), Merchants of doubt : how a handful of scientists obscured the truth on issues from tobacco smoke to global warming, New York: Bloomsbury.

PERDIGUERO, E. ET AL. (2009), «Physicians as a Public for the Popularization of Medicine in Interwar Catalonia: the Monografies Mèdiques Series». In: Papanelopoulou, F.; Nieto-Galan, A.; Perdiguero, E. (ed.). Popularizing science and technology in the European periphery, 1800-2000, Burlington, VT: Ashgate, 195–215.

POLLACK, N. (1976), The Populist Response to Industrial America, Cambridge MA: Harvard University Press.

POST, R. C. (2010), «Back at the Start: History and Technology and Culture», Technology and Culture, 51, (4), 961–994. doi: 10.1353/tech.2010.0078.

PRICE, D. K. (1965), The scientific estate, Cambridge, MA: Belknap Press of Harvard University Press.

RINGER, F. (2004), Max Weber: An Intellectual Biography. Chicago: University of Chicago Press.

ROCA I ROSELL, A. (1988), «The Catalan Scientific Heritage», Catalónia, no. 8, 20–22.

ROCA-ROSELL, A. AND GRAPÍ-VILUMARA, P. (2010), «Antoni Quintana-Marí (1907–1998): A Pioneer of the Use of History of Science in Science Education», Science & Education, 19, (9), 925–929. doi: 10.1007/s11191-010-9241-3.

ROCKE, A. J. (2000), «Eloge: Aaron J. Ihde, 1909-2000», Isis, 91, (4), 551–3.

SCHATZBERG, W. (1973), Scientific themes in the popular literature and the poetry of the German enlightenment, 1720-1760, Berne: Herbert Lang.

SEELY, B. E. (1995), «SHOT, the History of Technology, and Engineering Education», Technology and Culture, 36, (4), 739–772. doi: 10.2307/3106914.

SELIGMAN, A. (1997), The problem of trust, Princeton: Princeton University Press.

SHAPIN, S. (1994), A social history of truth: civility and science in seventeenth-century England, Chicago: University of Chicago Press.

SHAPIN, S. (2005), «Hyperprofessionalism and the Crisis of Readership in the History of Science», Isis, 96, (2), 238–243. doi: 10.1086/431535.

SHAPIN, S. (2008), The scientific life: a moral history of a late modern vocation. Chicago: University of Chicago Press.

SHEPARD, P. T.; HAMLIN, C. (1987), «How Not to Presume: Toward a Descriptive Theory of Ideology in Science and Technology Controversy», Science, Technology, & Human Values, 12, (2), 19–28. doi: 10.1177/016224398701200203.

SINCLAIR, B. (1995), «The Road to Madison and Back: Notes from a Traveler», Technology and Culture, 36, (2), S3–S16. doi: 10.2307/3106687.

SLOTERDIJK, P. (1987), *Critique of Cynical Reason*. Translated by M. Eldred. Minneapolis: University of Minnesota Press.

SZTOMPKA, P. (1999), Trust a sociological theory. Cambridge, UK: Cambridge University Press.

THACKRAY, A. (1980), «The pre-history of an academic discipline: The study of the history of science in the United States, 1891–1941», Minerva, 18, (3), 448–473. doi: 10.1007/BF01096952.

THACKRAY, A.; MERTON, R. K. (1972), «On Discipline Building: The Paradoxes of George Sarton», Isis, 63, (4), 472–495. doi: 10.1086/350998.

THORPE, C. AND SHAPIN, S. (2000), «Who Was J. Robert Oppenheimer?: Charisma and Complex Organization», Social Studies of Science, 30, (4), 545–590. doi: 10.1177/030631200030004003.

TOULMIN, S. (1982), The Return to Cosmology: Postmodern Science and the Theology of Nature. Berkeley, Calif: University of California Press

TOULMIN, S. (1990), Cosmopolis: the hidden agenda of modernity. New York: Free Press.

TREPP, A.-C. (2009), Von der Glückseligkeit alles zu wissen: die Erforschung der Natur als religiöse Praxis in der Frühen Neuzeit, Frankfurt am Main: Campus.

WALTER, M. L. (1990), Science and cultural crisis: an intellectual biography of Percy Williams Bridgman (1882-1961), Stanford, Calif.: Stanford University Press.

WEBSTER, E. E. (2010), American science and the pursuit of 'useful knowledge' in the polite eighteenth century, 1750-1806. Thesis PhD--University of Notre Dame. Available at: http://etd.nd.edu/ETD-db/theses/available/etd-04132010-110126/ (Accessed: 27 September 2020).

WOOLGAR, S. (1983), «Irony in the Social Study of Science». In: Knorr-Cetina, K.; Mulkay, M. J. (ed.). Science Observed: Perspectives on the Social Study of Science. London: Sage, 239–66.

WYNNE, B. (1992), «Misunderstood misunderstanding: social identities and public uptake of science», Public Understanding of Science, 1, (3), 281–304. doi: 10.1088/0963-6625/1/3/004.

WYNNE, B. (2001), «Creating Public Alienation: Expert Cultures of Risk and Ethics on GMOs», Science as Culture, 10, (4), 452–481.

WYNNE, B. (2002), «Risk and Environment as Legitimatory Discourses of Technology: Reflexivity Inside Out? », Current Sociology, 50, (3), 459–477.

WYNNE, B. (2003), «Seasick on the Third Wave? Subverting the Hegemony of Propositionalism: Response to Collins and Evans (2002)», Social Studies of Science, 33, (3), 401–417.

ZIMAN, J. (1996), «`Postacademic Science': Constructing Knowledge with Networks and Norms», Science Studies, 9, (1), 67–80.