**The History and Psychology of Scientific Publication**

**15 November 2013**

 I’ve been asked to speak briefly today about the history and psychology of scientific publication, and I am acutely aware of two things. First, as one of the earliest speakers on the program, I have the power (which I am determined not to exercise) to derail the entire day’s schedule. Second, as a historian of science, I don’t have much of an idea about the psychology of scientific publication, except as it applies to a sort of social psychology that influences a sense of occupational identity, and so guides a scientist’s sense of what to do and how to do it, as well as a sense of what distinguishes scientists from non-scientists. Thus, immediately effecting a time-saving maneuver based in ignorance, I can subsume psychology under the general rubric of the history of scientific publication, and go on.

 Dealing with the history of scientific publishing then, I want to assert – without pausing to make much of an argument – three things that may be of some use to you all at the beginning of your meeting today. First, it is no accident that emergence of the idea that knowledge of nature could be much more reliable that other types of knowledge – that it could be *scientia*, or certain knowledge – that is, what we call the Scientific Revolution, occurred at the same time that the printing press was being adopted within the intellectual economy of Europe. But this was not a friction-free process. Those who determined to put knowledge into print faced questions of how to claim ideas as property, how to associated value with those products, and how to determine who had the authority to put new ideas before the literate segment of the public. It’s no accident that we date the Scientific Revolution from the appearance of books like Copernicus’s *On the Revolutions of the Heavenly Sphere*s or Vesalius’s *On the Fabric of the Human Body*, both of which appeared in 1543.

 Second, in the nineteenth century, when natural philosophers and natural historians became first “scientists,” and then, physicists, botanists, geologists, and so forth – and when science became a specialized, paying profession – the disciplines came to be defined around the publishing programs of specialized societies. As the number of disciplines, subdisciplines, and quasi-disciplines has increased, so too has the number of titles in publication, and publishing in a specialty journal became part of the process of accrediting scientists as professionals.

 Third, this process between the sixteenth and the twentieth centuries came to serve in part as a way to establish and patrol the boundaries between scientific experts and the lay public. It may be – and this is not history, but contemporary affairs – that the current state of technology has cast some of these older arrangements into doubt. What is the point of publishing in an age of ubiquitous electronic communication? Is there a reason for peer review anymore, or do we now operate a sort of free market of ideas, with all consumers able to participate in the evaluation of anyone’s contributions? And if the economic barriers to entry – that is, the wherewithal to set up a web site as an online journal – are quite low, how do we rigorously distinguish between professional work – which is to be trusted – and the work of others? How do we establish deference to experts on matters of specialized knowledge?

 So, let’s begin by first considering publication in the early modern period. The printing press with movable type, of course, developed in the German-speaking lands and was first used for the business of mass production of the Holy Scriptures. But the invention was adopted very rapidly in Italy as well and turned to a secular market place. Engineers, in particular, began to use printed, copiously illustrated books of their “diverse and ingenious machines” as resumes, directed at potential employers and patrons.

 Now more than the history of a particular case (Copernicus, say, or Vesalius or Galileo) is the general problem that arises with the printing press, what one scholar has called “the nature of the book”. It is in claiming knowledge of nature as property – that is, one’s own ideas have to be identified, first of all, as one’s own, and second of all as a commodity, the owner of which is free to trade for other objects of value. After all, experience of nature is common to everyone – what makes it a sort of property?[[1]](#footnote-1)

 This is where the state stepped in. Various states were in the business of rewarding useful subjects with letters patent – ennobling them in some way, for military or political service; or providing a financial reward for making public a useful invention rather than keeping it a secret. Copyright involves the award of a privilege to an author, and gives them a foundation to defend ideas as their own and to reap the benefits of them. This is the ideal, anyway – in practice it can be difficult to defend. One botanist in 16th century London, for example, found that his laboriously compiled and illustrated survey of flowering plants was pirated, published under someone else’s name, with many of the plants misidentified – and there was nothing he could do about it. Early scientific texts often involved a certain amount of appropriation along these lines.[[2]](#footnote-2)

 Publishing also involved dealing with the gatekeepers. Printers, publishers, and book-sellers – often one in the same businessman – had to decide to bring a book to market. And in many countries, secular or religious authorities had to grant a license to publish – not just an award of a patent, but permission that might be predicated on the assessment of the truth of the book’s content. Thus we have the King James Version of the Bible – the authorized, printed version of the good news – and we have John Milton’s energetic protest that Protestants should not be subject to the kind of censorship and restrictions on publication taken for granted in Catholic states.

 In the seventeenth century, scientific societies became the agents of the state in certifying essays for publication. Given royal commissions, bodies like the Royal Society of London and the French Royal Academy did the work of assigning priority and giving the author credit for his work, of certifying the work as true and useful, and of bringing the work to market, managing the finances of publication. Of course, publishing in the *Philosophical Transactions of the Royal Society* did not bring a financial return to the author. This was a good thing, because if the scholar needed money, his word could not be trusted and he was an unreliable scientific informant.[[3]](#footnote-3)

 The early scientific societies, then, by establishing rules for what could be published in their proceedings, defined what natural science was and established the idea that certain kinds of knowledge about nature was reliable in ways that other sorts of knowledge was not. They set the rules for how to deal with observations, experiment, mathematical analysis; and for what sorts of people would have access to the society and its publishing venues. They certified to distant readers that the things represented in texts had actually taken place, via reports buttressed with illustrations and with the attestations of aristocratic and gentlemen witnesses whose collective word, according to legal standards, was good. Outsiders, like Thomas Hobbes and Jonathan Swift in the English case, could criticize or make fun, but their reactions would not find a place in the natural science literature produced by virtue of royal privilege accorded to the kingdom’s scientific society.[[4]](#footnote-4)

 The nineteenth-century scientist was in a different position than that of the early modern natural philosopher. In English, the word “scientist” itself was of nineteenth-century coinage; by the end of the century, go-ahead young scientists might be willing to be referred to as “physicists,” along with chemists, botanists, geologists. The proliferation of “ists” reflected the fact that in many countries, the practice of science began to have the status of a learned profession, like medicine, law, and ministry. In Britain, especially, this reflected the fact that a growing number of young students of nature needed a paying job, wanted to work in the intellectual capital, London, and were not particularly interested in what was traditionally the greatest gift that Cambridge University could bestow on its best graduates – a living, that is, a well-endowed parish that would make relatively little demands on the time of a Church of England minister with scholarly inclinations.

 Now, one problem with professionalized science is that it did raise questions about the credibility of scientists who worked for money. This made their disciplinary credentials vital; membership in a specialized society, publication in a disciplinary journal, access to which was guarded by editors and manuscript reviewers, served to certify as reliable practitioners who, because they worked for money, were not gentlemen in the strictest sense of the word.

 In various ways, in the nineteenth century technological change raised the issue of a too-democratic access to publication. Steam-powered presses and cheap paper made an ever-growing number of popular scientific sources available to readers of all sorts. The anonymously-published *Vestiges of the Natural History of Creation*, a mid-century mixture of cosmic and biological evolution, raised questions not just about who wrote it, but about who should read it. Women, workers, crypto-socialists – between the bevy of problematic readers and the bare possibility that the text might have been written by a member of the aristocracy, reviewers were in a tough position.[[5]](#footnote-5)

 In Britain, scientists could respond in a relatively new literature of Reviews, which mediated among science, political and religious orthodoxy (for which scientists were prominent spokesmen) and progressive democracy. Specialty journals provided a base for experts, and semi-popular publications drew on that expert leverage to instruct and correct a broader public. In the United States, the problem of democracy in the mid-nineteenth century was even more acute. The American Association for the Advancement of Science in its early years around 1850 suffered from an expectation that any American ought to be able to contribute their observations and insights at a scientific meeting, something that appalled the leaders of American science. On the other hand, the journal *Science*, that came to be associated with the Association, needed to reach out to a broad readership for economic reasons, and so featured a steady diet of reports on inventions and patents along with laboratory science and geological, botanical, and ecological studies. This was even more the case in the *Scientific American*, which addressed a similarly broad audience before many specialty journals appeared in the US.[[6]](#footnote-6)

 While specialty journals began to proliferate in the United States in the 1870s, a certain breadth of vision came handy at the beginning of the twentieth century when it proved very difficult to create an American physics discipline. Many trained as physicists refused to join the American Physical Society if membership meant having to subscribe to the *Physical Review*, a journal which, in the 1920s, was identified with other-worldly, introspective hocus-pocus such as relativity and, especially, quantum theories. The American Institute of Physics was created to provide parallel tracks, via multiple journals, within the physics community, leading to multiple professional identities. A quarter-century later, though, the physics of atoms, nuclei, and microwaves had demonstrated their concrete utility in the course of World War II, and the leadership of American science made a strong case for ongoing Federal support for peacetime academic research. Drawing on one of the most consequential inventions of the war (that is, the research contract), after World War II American scientists began to negotiate the allowable costs for their endeavors, which came to include page charges for scientific publications, a feature in the economic landscape of scientific communication that indirectly makes the resources of science funders, including the federal government, available for the production of journals.[[7]](#footnote-7)

 By the late twentieth century, members of the general public, even if they could not understand the contents of *Reviews of Modern Physics* or the *Astrophysical Journal,* were assured of their significance, not just intellectually but also politically. In the courses on science for nonscientists that began to proliferate on college campuses in the postwar decades, science was held up as a model for the deliberations of a democratic people: it represented ideas and information openly presented, democratically debated, and rigorously tested. Because the truth about nature did not defer to wealth or power, but was the same for all, the processes for arriving at the truth had to be similarly enlightened. Harvard’s president, the chemist James Bryant Conant, and his acolyte, physicist Thomas Kuhn, began to develop case studies in the history of science to get such ideas across.[[8]](#footnote-8)

 How disconcerting, then, to learn, the same day that I was invited to speak here today, that *Popular Science* was removing the comments section on the magazine’s web site. The explanation given by the web site’s editors: “comments can be bad for science.”

 Now *Popular Science* is not a cutting-edge, disciplinary journal, but I suspect we could find similar sentiments elsewhere in the world of scientific publication, even though they fly in the face, it seems, of an ideal of science as a collective and democratic enterprise in which ideas are tested by exposure to the criticism of qualified commentators. As it evolved, though, this picture assumed a certain measure of deference to experts, on the part of non-experts.

 Part of what humanistic students of the sciences now refer to as the “problem of expertise” seems to be exactly this problem of authority: that technical experts seem to be less and less able to dictate the right answers, or the best answers, to technical problems. It’s certainly easy to overstate the power that scientists ever did have in this regard, but in the current day I think it is useful to think about how changes in publication practices and in the production of scientific texts have contributed to this state of affairs.

 First, publication in print now represents something like what the social theorist Thorstein Veblen called “conspicuous consumption” – a symbolic act that in part confers prestige because of its inefficiencies. We suspect that most crucial communication among working scientists is done electronically, via emails, video conferencing and digital preprints, along with refereed digital publication. But the number of specialized journals in which research reports can appear physically in print continues to be very large, even as they report things that the people who care have known for some time. (Contrast this with the early twentieth century, for example, when physicists eagerly awaited the latest European journals to learn what was new in theoretical physics.) What explains this, in part, is the economic value of publishing for professional scientists, reflecting the patterns of ideas as property and professionalization from the age of print.

 Second, while frequency of publication seems large, in many fields the production of truly significant work seems to have slowed. This is especially true in fields that require large, costly research apparatus, access to which has been delayed by the availability of financial resources to build and maintain them. Again, this can produce a large number of preliminary research reports while awaiting the wherewithal to actually undertake a research program.[[9]](#footnote-9)

 And, third, those reports may have – almost certainly do have – a very large number of authors, confusing the question of authorship that goes back, as we have seen, to the scientific revolution. What authorship means when an individual is among tens of co-authors is now a sort of classic question in contemporary science studies.

 Fourth, the sheer number of journals in the postwar period can mean that a question resolved in print in one venue may not be regarded as resolved elsewhere, in another subdiscipline or in a parallel intellectual world such as Intelligent Design or anthropic cosmology. Again, if what defines a science is the existence of a disciplinary foundation, signified by a specialized journal with a board of editors and perhaps an institutional affiliation of some sort, inexpensive publishing, including web publishing, makes it relatively easy to construct alternative disciplines with a claim on the attention of scholars. Again, this sort of thing was reflected in the organization of publishing programs for physicists in the 1920s; now extend this along an every-greater variety of conceptual, economic, and political axes.

 Fifth, and finally, it seems ever more clear that in the past three decades or so, cultural authority in general has passed from academics to entrepreneurs. We now live very much in a marketplace of ideas, one in which the truth is what sells. I think it’s possible that science has not fully made the adjustment from leveraging the resources of the state in the pursuit of truth, to operating in a quite different political economy. I would expect that the state of scientific communication at present reflects that unsettled situation. But here we’ve clearly passed from history into current affairs – so this question I will leave for you all to pursue over the rest of the day.

1. Adrian Johns, *The Nature of the Book*; William Eamon, “From the Secrets of Nature to Public Knowledge,” in David C. Lindberg and Robert S. Westman, *Reappraisals of the Scientific Revolution*, pp. 333-365. [↑](#footnote-ref-1)
2. Deborah Harkness, *The Jewel House: Elizabethan London and the Scientific Revolution*, ch. 1; see ch. 4 on the award of royal privilege to useful inventors. [↑](#footnote-ref-2)
3. Steven Shapin, *A Social History of Truth* [↑](#footnote-ref-3)
4. Steven Shapin and Simon Schaffer, *Leviathan and the Air Pump: Hobbes, Boyle and the Experimental Life* [↑](#footnote-ref-4)
5. James Secord, *Victorian Sensation: The Extraordinary Publication, Reception, and Secret Authorship of* Vestiges of the Natural History of Creation [↑](#footnote-ref-5)
6. Sally Gregory Kohlstedt, *The Formation of the American Community: The American Association for the Advancement of Science, 1848-1860*; Kohlstedt, Michael M. Sokal, Bruce V. Lewenstein, *The Establishment of Science in America: 150 Years of the American Association for the Advancement of Science.* [↑](#footnote-ref-6)
7. Karl T. Compton, “The Founding of the American Institute of Physics,” *Physics Today,* February 1952. [↑](#footnote-ref-7)
8. James Bryant Conant, *On Understanding Science*; Thomas Kuhn, *The Copernican Revolution* [↑](#footnote-ref-8)
9. National Academy of Sciences, *Setting Priorities for Large Research Facilities Projects Supported by the National Science Foundation* [↑](#footnote-ref-9)