IDEAS

on the Nature of Science

Edited by
DAVID CAYLEY

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Goose Lane Editions
Suite 330, 500 Beaverbrook Court
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CANADA E3B 5X4
www.gooselane.com

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Introduction

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Ideas on the Nature of Science began life as a series of radio broadcasts called "How to Think about Science" that I presented on the CBC Radio program *Ideas* between November of 2007 and June of 2008. The series was inspired by an intuition that something had changed within that vast complex of ideas, institutions, practices, and products that we summarize and make manageable under the name of science, and changed quite dramatically. This was a change, I thought, not just in the practice of science, but in its public reception, in the way in which it is understood. "How to Think about Science" was my attempt to bring before my listeners some of the writers and thinkers who have had a hand in creating and promoting this new understanding of science.

Science has long been taken as the very definition of modernity. Historian of science Alexandre Koyré called the rise of modern science and the dissolution of the ancient cosmos "the most profound revolution achieved or suffered by the human mind." Herbert Butterfield claimed that the Scientific Revolution of the seventeenth century "outshines everything since the rise of Christianity and reduces the Renaissance and the Reformation to mere episodes. [It is] the real origin both of the modern world and of the modern mentality." American philosopher John Dewey saw science as synonymous with democracy. "The experimental method," he says, "is the only one compatible with the democratic way

of life." For him, science was "the organ of general social progress." Others have connected science with civility, skepticism, tolerance, and respect for evidence — all the virtues on which modern citizenship is thought to rest. Even George Grant, an anti-modern philosopher who considers "the modern paradigm of knowledge" to be a source of tragic alienation, still puts science "at the core of the fate of Western civilization."[1] In short, science has been thought of as the practice and the mentality that have made modern societies what they are and made them utterly unlike any previous form of society.

At the heart of these various pronouncements is a certain confidence that in speaking of "science" one knows what one is talking about — that the word refers to a more or less unified, more or less consistent body of ideas and practices, perhaps even to one overarching "scientific method." This confidence has been unsettled in recent years, as science has been exposed to a new kind of scrutiny. A characteristic note is sounded in the memorable opening sentence of historian of science Steven Shapin's book *The Scientific Revolution*: "There was no such thing as the Scientific Revolution, and this is a book about it."[2] It's a witticism that nicely captures a new attitude to the history of science. The phrase "the scientific revolution" — neat, pat, familiar — suggests, first of all, that this is a subject that we already know all about, and, second, that it is something that can be modelled as a compact and consistent event: a revolution. These are assumptions that Shapin wants to resist. The conventional label deprives the seventeenth century of too much of its strangeness and unfamiliarity. It suggests that we already know how the story is going to come out.

Historians of science, Lorraine Daston says in this book, have tended to be "people who . . . read the last page first." They told their story, in other words, as the unfolding of an end already implicit in its beginning. She and Steven Shapin belong to a generation of historians who have tried to rewrite the history of science with the controversy and contingency put back in. Anthropologists, sociologists, and philosophers of science have also tried to estrange themselves from familiar narratives. They have tried, anthropologist Allan Young says here, "to make explicit what is taken for granted." "Our impression was that we hadn't engaged closely enough with the work scientists . . . actually do," Simon Schaffer

told me. "We relied almost entirely on what they said. We hadn't been to look." This "going to look" involved close observation in laboratories, hospitals, and field stations to find out how scientific knowledge is actually produced, warranted, and institutionalized. And it involved the study of controversies, present and past, where the facts were in dispute, the truth had not yet stabilized, and one could see scientific knowledge still, as it were, in its molten state. Finding out "what it is like to put the ship into the bottle" is the telling image used by Harry Collins, one of the pioneers of this type of study.[3]

These new studies have produced a new picture of scientific knowledge. There has been much more emphasis on the productiveness of science, on the ways in which science actively makes and remakes the world it lives in. Philosopher Ian Hacking, for example, points out that science doesn't just represent the world, it also changes it, creating phenomena that have never existed before. And much more emphasis has been placed on the diversity and heterogeneity of scientific practices. Philosophers in the first half of the twentieth century argued strenuously for the unity of science, which they saw as humanity's only effective bulwark against the obscurantism and irrationality of fascism. Today, the disunity of science is a more common topic. Thomas Kuhn set the tone with his idea that science is done within all-determining "paradigms" that lack common terms with the ways of thinking that come before and after them. Subsequent scholarship has tended to soften this austere view of knowledge as a sequence of locked rooms. Physicist/historian Peter Galison, for example, has argued that though the sciences do often speak mutually unintelligible languages, they communicate via "pidgins" that are elaborated in the "trading zones" that form at the boundaries between sciences or between theorists, experimentalists, and engineers within a science.[4] Even so, the picture which emerges is one in which it makes much more sense to speak of "the sciences" than simply of Science.

Along with this recognition of diversity has come a new appreciation of the limits to scientific knowledge. In these pages, for instance, you will find Dean Bavington's account of the collapse of Canada's cod fishery. Science once seemed capable of confidently modelling the ecology of the oceans, its synoptic gaze reducing codfish to a predictable

population for which a maximum sustained yield could be reliably calculated. Today, a more humble science recognizes that the web of interactions within the oceans is too complex to allow this kind of prediction. Wendell Berry makes a similar point with regard to the way in which scientific agriculture overlooked the peculiar requirements of local ecosystems in its rush to expand production. And medical anthropologist Margaret Lock rejects the assumption of bio-medical science that there is a "universal body," and consequently a universal procedure for treating it, in favour of what she calls "local biologies."

Other thinkers I interviewed have studied the shadow that science throws on daily life when terms that make sense only within a network of precise scientific definitions leak out of science and into the vernacular. Geneticists know what they mean when they speak to one another about genes, but when genes enter popular parlance, they become what German scholars Barbara Duden and Silya Samerski call "pop genes." Pop genes have broken free of all the stipulations that make the scientific gene an intelligible object and become, in a sense, pure ideology, a bridge over the abyss that separates laboratory knowledge from everyday life. Believing ourselves to be the products of genes, Duden and Samerski say, transports us from the sensible world into a disembodied realm of risks. Ruth Hubbard underlines their point by pointing out that the genetic screening technologies that are now routinely used in pregnancy are "infinitely cruel to women" in the way in which they demand choice on the basis of impersonal and imponderable probabilities. Sajay Samuel, in a similar way, thinks that the prevalence of scientifically defined objects in politics disables political judgment. When politics revolves almost entirely around expert opinions about what is feasible, he says, there is little scope for citizens to express their judgments as to what is good or what is sufficient. His proposal for a return to common sense is echoed in a somewhat different key by David Abram. Abram's concern is with the habitual reduction of the sensible world to its supposedly more basic constituents. To this way of thinking, science gives us the real, our senses the merely phenomenal. He thinks this is entirely the wrong way round. The full dimensional world of our experience has the ultimate significance. Science, as an abstraction from this primary world, is useful and illuminating but ultimately secondary.

Restricting science to its proper sphere has also been the objective of those who have tried to demystify the political authority of science. British sociologist Brian Wynne, for example, has been interested in the ways in which political commitments structure ostensibly scientific judgments. Along with several other thinkers in this collection, he argues that science cannot currently answer the question of its own purposes. Science can create nuclear reactors or a race of transgenic marmosets whose skin glows in the dark; it cannot tell us whether this is a fitting thing to do. The answer lies outside of science. Yet these properly political questions, Wynne says, are still often disguised as scientific ones. He studied a public inquiry into the building of a new nuclear reactor in Britain and came to the conclusion that scientific rationality is very often used as "a ritual form of authority" in which the appearance of carefully investigating risks and assessing technical feasibilities is used to suppress questions of justice and propriety.

The reason scientific and political questions are still kept separate in this way, according to French thinker Bruno Latour, is due to the residual effect of what he calls "the modern constitution." At the beginning of the modern era, he says, after the devastation of the Wars of Religion, an attempt was made to establish a domain of reliable facts and to distinguish it from the realm of mere opinion. Science pertained to nature, the region of ascertainable fact, and politics to society, where opinion prevailed. Humans had agency and voice, nature was passive — science spoke for it. This was a fruitful fiction, Latour maintains, but it is long out of date. What was true all along has now become blindingly obvious — society and nature are inextricably mixed, and nature displays undeniable agency. Think, for example, of the international climate conference in Kyoto in 1997. Was this a political or a scientific gathering? Think of the response bacteria have made to antibiotics by evolving into what we call superbugs. Is this not a fateful intervention in the political world? "The 'Body Politik,'" Latour remarks in a recent essay, "is not only made of people," and the way scientists "make things public" is consequently no less political than the representation of people.[5]

Latour's argument suggests that it is not just the image of science, our way of talking and thinking about it, that has changed. The world itself has changed, and, in many ways, it is science that has changed it. In our

interview, he makes the point, with characteristic wit, that if you had told René Descartes or Immanuel Kant that humans can influence the climate, they would have taken you for a believer in outmoded myths. But humanity *has* altered the composition of the atmosphere, and that has transformed not just the way we think about the relationship of science to society, but the relationship itself. German sociologist Ulrich Beck speaks of the emergence of a "risk society," by which he means, among other things, a society in which science can no longer predict or control its effects. The atomic bomb was not tested on people before it was dropped on them. There is no atmosphere other than our own in which we can measure the result of rapidly increasing the concentration of greenhouse gases. No one knows how genetic interference with plants, animals, and people will play out in the long term. Society has become, in effect, a scientific laboratory. For Beck, this radical novelty urges what he calls a second modernity, a reflexive or self-conscious modernity with institutions that enable us to begin to take responsibility for the consequences of this so-far uncontrolled experiment. Latour uses a very different terminology, arguing for a recognition that we have never, in fact, been modern. For him, modernity is a myth whose time is over. Its critical separations and analytical distinctions must give way to an ethic of care and composition which recognizes that all beings are now in the same soup and that "we can get rid of nothing and no one." The differences of language and approach are significant, but both thinkers point to the fact that science must be brought, as Latour says, "into democracy."

During the 1990s, the new anthropology, sociology, history, and philosophy of the sciences that I've been briefly summarizing engendered a reaction that became known as the science wars. The gist of the critique that was put forward in books like *The Higher Superstition* and *Fashionable Nonsense* was that "the academic left," as these books styled it, had betrayed the old left's devotion to science and Enlightenment and waded off into the swamps of relativism. The main bugbears for these writers were feminism, with its investigation of the ways in which masculine bias has coloured the practice of science, and the many varieties of the view that scientific knowledge is "socially constructed,"

that is, produced in social settings under social assumptions and not just innocently discovered. A lot of the public controversy seemed to turn on caricatures and taunts — "if you're so sure the laws of gravity are socially constructed, maybe you'd like to jump out of my window" — but, when I look back on it, the whole affair looks to me less like a debate and more like a last-ditch effort to save the credit of an obsolete image of science.[6] This is not to deny that science had lost some of its innocence and some of its aura — its high-modern mystique — but it was hardly because a handful of radicals in science studies had pulled back the curtain and revealed the great Oz as a mere mortal. What was at stake was nothing less than what Bruno Latour calls the "modern constitution," with its strict distinction between nature and society and its elevated view of science as an authority exempt from any touch or taint of politics. No wonder deep anxieties were stirred.

Today, at the end of the first decade of the new millennium, the situation feels very different. It seems easier to discuss science quizzically without being immediately asked to declare whether you are a friend or an enemy or whether you "believe" in Boyle's Law or Maxwell's equations. Yes, there are still people who think the earth is six thousand years old, as there are still scientific zealots who accord science the place of true religion, but, in general, I would say that a lot of what was threatening in the new studies of science of the 1980s and 1990s is now much less so. The time seems right, then, to look at the new accounts of science that emerged during the last half of the twentieth century and at the marked differences amongst them. *Ideas on the Nature of Science* is the record of one such attempt. It reflects the vicissitudes of preparing a radio series — not all the people I would have liked to interview were available — and it reflects the partialities of my reading and my acquaintance; suggestions from listeners as to all the people I had left out would have easily supplied a second series of comparable length and comparable interest. The interviews were recorded between the fall of 2006 and the spring of 2007. Some episodes of the full broadcast series have been omitted to conserve space.[7] But, even so, I think the collection provides an interesting and representative sample of a field whose riches no one reader could ever exhaust. The points of view

represented are diverse and sometimes contradictory. Themes recur, but no attempt has been made to reduce them to a common denominator. My hope is simply that readers will find here resources with which to think about science. Much depends on it.

NOTES

1 Alexandre Koyré, *Metaphysics and Measurement* (London: Taylor and Francis, 1992), p. 20; Herbert Butterfield, *The Origins of Modern Science 1300-1800* (London: G. Bell and Sons, 1949), p. viii; John Dewey, *Underlying Philosophy of Education*, in *Later Works*, Vol. 8, *Collected Works of John Dewey*, ed. Jo Ann Boydson (Carbondale IL: Southern Illinois University Press, 1969-91), p. 102, and *Democracy and Education*, in *Middle Works*, Vol. 9, *Collected Works of John Dewey*, ed. Jo Ann Boydson (Carbondale IL: Southern Illinois University Press, 1969-91), p. 239; and George Grant, *Technology and Justice* (Toronto: House of Anansi, 1986), p. 9.

2 Steven Shapin, *The Scientific Revolution* (Chicago: University of Chicago Press, 1996), p. 1.

3 Harry M. Collins, *Changing Order: Replication and Induction in Scientific Production* (Chicago: University of Chicago Press, 1992), p. 145.

4 Peter Galison, *Image and Logic: A Material Culture of Microphysics* (Chicago: University of Chicago Press, 1997).

5 "From Realpolitik to Dingpolitik," in *Making Things Public: Atmospheres of Democracy*, ed. Bruno Latour and Peter Weibel (Cambridge MA: MIT Press, 2005). See also *We Have Never Been Modern* (Cambridge MA: Harvard University Press, 1993) and *The Politics of Nature: How to Bring the Sciences into Democracy* (Cambridge MA: Harvard University Press, 2004).

6 See Paul R. Gross and Norman Leavitt, *The Higher Superstition: The Academic Left and Its Quarrels with Science* (Baltimore: Johns Hopkins University Press, 1994 and 1998) and Alan Sokal and Jean Bricmont, *Fashionable Nonsense: Post-Modern Intellectuals Abuse of Science* (New York: Picador, 1998). The invitation to test the social construction of the laws of gravity comes from Alan Sokal's unpublished letter to the *New York Times*, which he posted on line; see *http://www.jwalsh.net/projects/sokal/articles/skl2fish.html*. You can get a bit of the flavour of the rest of the debate from a published exchange between Bruno Latour and Ashraf Noor; see *Common Knowledge*, 8, no. 1 (2002), pp. 71-79 or *http://muse.jhu.edu/journals/common_knowledge/v008/8.1latour.html*.

7 A full transcript of "How to Think about Science" is available by writing to *ideas@cbc.ca* or calling the CBC Shop at 1 800 955-7711.

Knowledge Is an Institution

SIMON SCHAFFER

I think there are two standard images of what the sciences are. One image is that scientists are absolutely special people, that they're much more moral and much more virtuous and much, much cleverer, and that they do things that are nothing like what anybody else does. And on the other hand, there's an equally powerful public image of science, which is that science is organized common sense, that it's just cookery raised to a fairly sophisticated art. Those are the two dominant images of public science in our culture, and neither of them is right.

— Simon Schaffer

In 1985, a book appeared that seemed to sum up a new approach to the history of science — one that had been gradually taking shape since the 1960s. There had been precursors, of course, but up to that time the history of science, broadly speaking, had meant biographies of scientists and studies of the social contexts in which scientific discoveries had been made. Scientific ideas were discussed, but the procedures and axioms of science were not put directly into question. *Leviathan and the Air Pump* involved a more searching interrogation of the history of science. Subtitled *Hobbes, Boyle and the Experimental Life*, the book's avowed

purpose was "to break down the aura of self-evidence surrounding the experimental way of producing knowledge." This was a work, in other words, that wanted to treat something obvious and taken for granted — that matters of fact are ascertained by experiment — as if it were not at all obvious, that wanted to ask, how is it actually done, and how do people come to agree that it has truly been done?

The authors of this path-breaking book were two young historians, Steven Shapin and Simon Schaffer, and both have gone on to distinguished careers in the field they helped to define, science studies. I spoke with Simon Schaffer at his office at the Whipple Museum of the History of Science at Cambridge, where he teaches. We talked first about why science came under a new kind of scrutiny during the period when he was just beginning his studies, the 1970s. This was a time, he said, when many modern certainties were shaken, and there was no greater modern certainty than the authority of science. In this atmosphere, a new generation of scholars began to ask new questions. No longer content just to take scientists at their word, they wanted to see for themselves how science is made.

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Our impression was that we hadn't engaged closely enough with the really lived work scientists and technologists actually do. We'd relied almost entirely on what they said. We hadn't been to look. So significant groups of social scientists, mainly in Britain, interestingly, began to work alongside scientists in labs, in field stations, in research clinics, in zoos and botanic gardens — to follow the scientists around to try and look at what they did. We were using field methods, in other words, borrowed entirely from the field sciences, except this time, instead of looking at lemurs, we were looking at physicists. Instead of looking at Trobriand Islanders, we were looking at Californians.

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These observations yielded a picture of science that varied dramatically from the image that philosophers of science had put forward. Science, according to these philosophers, was essentially reason in action. Schaffer and his colleagues came to a very different conclusion.