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The Scientific Life: A Moral History of a Late Modern Vocation. By Steven Shapin. (Chicago: University of Chicago Press, 2008. xvii + 468 p., ill., fig., bibl., notes, index. ISBN 978-0-226-75024-8 hc.)

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***The Scientific Life: A Moral History of a Late Modern Vocation.* By Steven Shapin.** (Chicago: University of Chicago Press, 2008. xvii + 468 p., ill., fig., bibl., notes, index. ISBN 978-0-226-75024-8 hc.).

The history of science began as philosophy, grew up as biography and lives on as sociology. Steven Shapin, best known for his highly-regarded work on early modern science, features an epigraph from Weber for each chapter of this book in which he investigates the epistemological basis of scientific authority mostly via a sort of informal prosopography of American industrial scientists. In particular he looks at what actual working scientists said they were doing and what they understood about what they did. He focuses a great deal of attention on the question of the scientist's "moral equivalence," the rise, fall and contending of the view of the scientist as no greater or lesser a moral being than anyone else. And, pace Weber, Shapin again and again comes back to insisting that the personal, the actual and perceived personal qualities of the scientists, remains important to authority in science. His conclusion, largely, is that individuals matter, not institutions, be they industrial or academic.

Shapin poses the rhetorical question of whether scientists have been people like any other or have they been or been seen to be something different, presumably because of the nature of the scientific endeavour. He begins his answer by showing the existence of all shades of opinion on the matter, as some people have said yes, some have said no, some have seen either answer as a positive or as a negative thing. While very fair it does not get us very far, although it is characteristic of the way the author makes his arguments. The book is called "nuanced" on a dust-jacket quotation from Barry Werth but is so hedged with qualifications and counterexamples that the reader longs for an unequivocal statement.

This work is deeply and meticulously researched and documented with 125 pages of notes and bibliography, including all the usual suspects from the secondary literature on the history of R & D. But even those who are very well informed about the history of research science in North America will be surprised by some of what Shapin unearths and will be able to mine the book for telling anecdotes and pithy quotes. It is however a very dense tome and not always easy reading. The author has a habit at first annoying and finally enraging of peppering his sentences with italics to lead us through a correct reading of his prose. We are also given such monstrosities as a sentence with eighteen words outside of brackets and forty inside in three separate parenthetical asides, two of which are separated solely by the sentence's principal verb. That would just get a red line drawn through it in an undergraduate essay.

Shapin argues that science, including pure science, had firmly found a home for itself outside of the academic world by the twentieth century. I don't see that needs much arguing; North American universities never were ivory towers. Most have nineteenth or twentieth century origins and were comfortably part of a liberal capitalist political economy generally and engaged with applied research particularly from those origins. He further asserts that the assumption that academic and industrial goals and values were in conflict was just that, an assumption. He focuses on the rhetoric around this, with claims of virtue and worries over purity. He is clearly unsympathetic to a "pure" university vs. "grasping" business dichotomy and has lots of good evidence to back this up. The notion of university science as somehow morally-culturally-epistemologically superior Shapin calls a creation of mid-twentieth century social scientists unsupported by empirical evidence. Shapin quotes scientists from both ends of the political spectrum insisting that they as a social group have no particular authority of any sort to pronounce on social questions even when those questions relate deeply to science and its applications. As an aside I would note the relevance of this in a country where the debate over climate change policy is shaped by a retired geneticist. But more fundamentally, is all this any different from, say, history? Are we uniquely able to make authoritative pronouncements about the present and future (or even the past) based on our professional understanding of the past?

Shapin observes that while industrial research has been extensively studied by historians and others, industrial researchers as a group have not. More particularly he notes that industry scientists and directors of research have had their voices little heard by academics who study science. He then proceeds to mine technical journals for data and to recover those voices to great effect. His Chapter 5 "Who is the Industrial Scientist?" is probably the best in the book. The picture we get of the world of industrial science from those who were doing it is entirely different from that described by academic discourse on the topic coming from those studying it from the outside. I do wonder though how surprising that is and again whether the same might not be true of any of a very large number of other social groups. Also, Shapin tends to conflate science with Big Science. Many of the things he says about science are true only of some parts of it—atomic physics and molecular biology are not geometry or variable star observation.

The author asks that this work not be confused as an apology for American business and it isn't, although a careless reader could make it out to be. Still, it must have been a very pleasant book to research as Shapin gained entree into the high-end cocktail party world of high-tech

venture capital. In the book's later chapters, which read more like very good journalism than history, we see how it has recently become possible for some scientists to get very rich indeed through commercialization of results. Given the tremendous uncertainties of leading edge research, especially as to its commercialization, venture capital investments often hinge on decisions about people and their virtues literal and figurative. That personal basis for dealing is for Shapin what connects the seventeenth century world he knows so well to this twenty-first century one. I would go further and argue that right from its earliest days, trying to convince a sceptical senior management to support research never rested on a dollars and cents argument but required a leap of faith. To the extent that Shapin is hammering that point home he is deeply convincing.

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***Einstein's Generation: The Origins of the Relativity Revolution.* By Richard Staley.** (Chicago: Chicago University Press, 2008. x + 494 p., ill., bibl., notes, index. ISBN 0-2267-7057-5 \$38).

The beginning of the twentieth century stands out in the disciplinary memory of today's physicists as the time of transition from classical to modern physics. But how did practitioners experience the changes in their discipline at the time? And how did that period become the scientific watershed that it is in our histories? In *Einstein's Generation*, Richard Staley revisits one of the most studied chapters of science history with the ambitious goal of inverting the traditional perspective. While most histories take for granted that Einstein's creation of the theory of relativity constituted the pivotal moment—indeed, a “revolution”—and measure the achievements of Einstein's contemporaries against it, Staley foregrounds the activities of the individuals or groups in relation to which Einstein sought to position his work, in the attempt to reach “a new understanding” of the origins of what we now call modern physics (p.3).

Einstein's Generation is a complex book that collects Staley's research into diverse facets of early twentieth-century physics, and is enriched by his command of an ample store of secondary literature. Rather than offering a single narrative or argument, it presents a spectrum of themes through a kaleidoscope of historiographic approaches. It is composed of four parts. The first part details the career of Albert Michelson, the first American Nobel laureate in physics. It follows Michelson from his engagement with the American astronomical community and the network of precision instrument makers for the measurement of the speed of light, through his creation of a new kind of instrument, the interferometer, for