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**It’s like getting married**

Barbara Herrnstein Smith

*The Scientific Life: A Moral History of a Late Modern Vocation*

by Steven Shapin

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The practices of science, it appears, are increasingly industrial in location, corporate in organisation, and product and profit-minded in motivation. In the eyes of various commentators, these trends represent an obviously undesirable state of affairs: inhospitable to the genuine scientific vocation, dispiriting for the scientists involved and seriously at odds with the sustaining norms of science itself. But how well do such commentators, or any of us outside that world, know what industrial-corporate-commercial science is really like? Can we be sure that research conducted under such sponsorship is so different from academic research – and necessarily worse? Is an orientation towards marketable products and monetary profit inherently incompatible with the characteristic motives, satisfactions and personalities of scientists? And what basis is there, anyway, for our notions of either the character of scientists or the nature of ‘science itself’? In posing these questions and seeking to answer them, Steven Shapin has produced a work of exceptional originality, power and significance. He has also given readers much to chew over in regard to contemporary developments and perennial issues.

The Scientific Life departs along several lines from the more strictly historical enterprises of Shapin’s earlier studies, Leviathan and the Air-Pump: Hobbes, Boyle and the Experimental Life (1985), co-authored with Simon Schaffer, and A Social History of Truth: Civility and Science in 17th-Century England (1994). Those detailed, in what were historiographically innovative and sometimes epistemologically disruptive ways, the social and political conditions of the rise of experimental science in 17th-century England. Here Shapin offers what he describes as a sociologically inflected ‘cultural history’. It is actually two such histories. One is a genealogical account of Western conceptions of the scientist from the early modern period to our own. The other is an often on-site report of the changing practices of science in the United States since the early 20th century. Shapin frames these accounts with an introductory chapter and an epilogue, each titled ‘The Way We Live Now’. For Trollope, the phrase served as an ironic comment on the world of financial chicanery and personal corruption that he saw in England and depicted in his novel. For Shapin, it still serves as an ironic comment, but its objects are more ambiguous. ‘The way we live now’ refers most obviously to how science operates in the high-tech, high-finance world to which it has increasingly moved, at least in the US. But the irony seems to be at least partly at the expense of those who think we do or should or might live some other way.

At its most heroic, science has been seen as a quasi-divine calling pursued by men of monastic virtue. At its least heroic, increasingly since the end of World War Two, it has been seen as a job like any other pursued by men (and some women) no better than they should be. Shapin emphasises that the story of that transformation is not a linear one, a simple matter of decline and fall. On the contrary, the changes, though immense, were always differentiated by particular cultural sectors, occasions and circumstances. They were also assessed in very different ways from different perspectives.

Economic and political developments from the 17th century to the 19th strongly affected both the structure of scientific careers and public perceptions of science. In the early modern period there were very few state-supported scientists. Most were amateurs, many were clerics. Men of science were seen as uniquely virtuous, given to stoic fortitude and self-denial in the service of truth, not material gain, public fame or political power. By the 18th century, the centralised nation-state gave systematic institutional forms to the mobilisation of technical expertise. In the 18th and 19th centuries, there was a vast increase in the number of scientifically trained people employed as experts in commercial, military and governmental settings. Gentlemen amateurs survived (Darwin was one) but as the pursuit of science was increasingly integrated into the structures of power and profit, the public’s appreciation of science was itself transformed – for better and for worse.

The notion that scientists are ordinary people – what Shapin terms ‘the moral equivalence of the scientist’ – was asserted more and more frequently over the course of the 20th century and, after World War Two, became a commonplace. With the show of power represented by the atom bomb, scientific knowledge received greater public respect, but a different sort from that won by such 19th and early 20th-century icons as Pasteur or Einstein. In the United States, while scientists were increasingly represented in popular culture as frightening, unfeeling experts, the government was inclined to view them as too sophisticated, too independent-minded or too ethically sensitive (and therefore presumptively ‘pinko’) to be trusted. Under these conditions, the idea that ‘scientists are human, too’ was invoked as a reassuring, counter-stigmatising self-defence.

Shapin tells this story exceedingly well, framing its episodes richly and developing them through vivid depictions of representative figures, texts, incidents and anecdotes. Also, in line with explicitly formulated epistemological convictions – in brief, a radical particularist nominalism – he pays careful attention, here and later, to the significantly differing institutional contexts in which views of science and the scientist have emerged, and to significantly differing perspectives on these developments. These are respectable convictions and generally admirable procedures. They are attended, however, by what can be seen as a gratuitous and inevitably strained commitment to strictly descriptive neutrality. Shapin authorises this commitment by invoking Max Weber’s counsel to scholars not to moralise. But Weber was addressing scholars principally in their role as teachers talking to captive audiences of students. That is not Shapin’s role here. As an author and cultural historian, he is writing at a particular moment, in and from a particular place. One is not, as a particularist or even as a historical relativist, obliged to renounce commentary on one’s own world. Accordingly, his interpretation of Weber’s ‘don’t moralise’ as ‘never cast a critical eye on anything except the views of fellow academics’ is more likely to frustrate his readers than oppress them.

Turning to the scientific life in the 20th century, Shapin presents two sets of views – one ‘external’, the other ‘internal’ – of industrial science in the US. The first, which he calls ‘the view from the tower’, is represented by the accounts and analyses of various academic commentators, with selections from William H. Whyte’s The Organisation Man, first published in 1957, as a central example. The second, which Shapin calls ‘the view from the managers’ (it was ‘from the shop-floor’ in an earlier published version of this material), is assembled from descriptions of the scene in speeches, writings and interviews given by various industrial scientists, especially managers of major laboratories. Key examples are drawn from a repeatedly revised and updated book, The Organisation of Industrial Scientific Research by C.E. Kenneth Mees, manager and director of the laboratories at Eastman Kodak from 1912 to 1955. As might be expected, the two sets of views differ considerably. What is less expected is the extent to which Shapin represents the academic view as trumped by the managers’ view, with ‘abstract’ theoretical ‘presumptions’ and ‘Ivory Tower’ ‘laments’ confronted and presumably corrected by ‘practical’, ‘quotidian’, ‘concrete realities’.

A number of key distinctions and contrasts drawn by external commentators between industrial and academic science and between applied and basic research are, Shapin observes, dubious. Social theorists imagine conflicts between the strong individualism of academic science and the obligations of corporate teamwork, or between the intellectual criteria of scientific disciplines and the project-goals of commercial industrial laboratories, but few industrial scientists declare themselves unhappy on such counts. Important basic research has been done in industrial laboratories. Both industrial research and university-based science are marked by uncertainty of process and outcome. What is different is that the degree of toleration of uncertainty is far greater in the academy than in industry, where corporate headquarters must commit large resources to projects with outcomes that can’t be known in advance. ‘That is the fundamental fact about industrial research,’ Shapin writes. That fact has significant consequences for the life of the industrial scientist. Laboratory managers know that research is both a gamble and that it has to contribute to the company’s bottom line. CEOs know that R&D doesn’t operate like the manufacturing, packaging or marketing sectors, but also know, or learn, that too strict an accounting, too much standardisation or too much obvious control will stifle the research on which they are gambling. Here is where Mees figures centrally. His light-hand-on-the-tiller policy and dictum to the effect that ‘the best way to plan science is not to’ became models for success. Accordingly, industrial research is often less intellectually stifling than supposed by academic commentators.

Conversely, academia is not a natural home for science and certainly not always an ideal one. The best place to do research varies with different times, conditions and types of industry and research. The life of an academic scientist in the well-equipped laboratory of an elite university during an era of lavish, no-strings-attached government funding is one thing. But it is quite another in institutions with limited resources, heavy teaching obligations, continuous grant application imperatives and intractable publish-or-perish deadlines. The life of the industrial scientist also varies with conditions. Working on the shores of the Pacific in a small start-up computer company formed by a group of smart, ambitious schoolfriends is different, of course, from working in the R&D department of a mega-corporate pesticide manufacturer in central New Jersey, though Shapin, bending over backwards here as elsewhere, evokes the former quite vividly and the latter virtually not at all.

Big industrial projects, Shapin points out, require teams from different disciplines; sometimes, researchers have different collaborators from year to year. It is understandable that managers continually stress the importance of ‘teamwork’ and personal adjustability. After the war, Eisenhower worried about the effects of ‘the military-industrial complex’ on democratic values and, along with other scientists (many of them émigrés), Alvin Weinberg, director of the Oak Ridge National Laboratory, worried about the effects of corporate science on science (he was later fired for voicing worries about the safety of nuclear reactors). Shapin assigns these anxieties to lingering romantic conceptions of the scientist as individual genius and of scientific discovery as inherently irrational and not subject to planning or organisation, but it may be thought that more than romanticism or nostalgia was involved. Clearly the images and consequences of goose-stepping soldiers and chanting crowds were still vivid for the postwar generation, and what managers of corporate science were urging as the necessity of ‘teamwork’ and adjustability many émigré scientists and social critics associated with Fascist collectivism and mind-numbing conformism.

Shapin offers two presumably corrective ‘realities’ here. One is evidenced by a mountain of highly instructive reports and statistics that document the increasingly industrial sponsorship and corporate organisation of science in the US. These properly challenge uninformed, anachronistic characterisations of science that take individual, university-based research as a normative model. The second is suggested by the often shrewd and colourfully expressed views of industrial science voiced by those who sponsor, manage and earn their living in it. The latter certainly represent a ‘reality’ in the sense that such views really are maintained; and they should certainly figure in any general account of how the scientific life has been experienced and assessed in the 20th century. However, the picture of industrial science that emerges from such views has no more authority than that which emerges from the writings of sociologists, social theorists and other academic commentators, even though the latter are commonly expressed, as Shapin repeatedly observes, in general, abstract, academic terms. Indeed, with regard to the dominance of corporate commercialised science in the US, some views from ‘the tower’ might be accorded a certain degree of respect precisely insofar as they are expressed in such terms – which is to say, historically informed, intellectually responsible and, in comparison with views closer to the ground, alert to broader implications and concerned with longer-range consequences.

This is not to say that official residents of ‘the tower’ always operate very high above the ground themselves. Shapin’s account of the postwar years is especially enlightening and, one may think, cautionary in tracing the ways academic attitudes towards for-profit science have changed in line with the changing economic circumstances of universities. Through the 1970s and 1980s, as business leaders worried about competition from Japan, congressional leaders worried about maintaining the economic and military dominance of the United States. At the same time, previously principled university administrators, feeling the cutbacks of government funding during the Vietnam War era, began to look more favourably on the private sector to fill the gap. Just as helping the military had been framed and accepted as a patriotic duty for scientists during the war, now ‘helping American industry compete’ could be seen and endorsed as a public service. Shapin quotes the chancellor of the intensely entrepreneurial University of California at San Diego (formerly his own institution) declaring to its faculty in 2001: ‘As scholars, we should not seek knowledge for its own sake.’

The culmination of these 20th-century trends is the emergence of the unabashedly profit-seeking scientist and his (rarely her) commercial partner, the venture capitalist, a pairing memorably represented in The Scientific Life by a photo of Craig Venter, super-mapper and super-marketer of the human genome, posed in half lab coat, half business suit, staring out impassively with evident self-satisfaction. It is here, too, at the conclusion of all his stories, that Shapin’s own efforts become most ambitious and most problematic.

Seeking, as throughout, to challenge familiar academic assumptions and attitudes, Shapin attempts to demonstrate that, far from being cold, impersonal or amoral, the worlds at the ‘cutting edge of late modernity’ are positively, necessarily and even exceptionally virtuous, marked by ‘trust’, ‘patterns of familiarity’, and a host of other ‘personal’ and ‘moral’ ‘virtues’. Readers will be sceptical of such claims, and rightly so. For the terms in which Shapin frames them are used equivocally or euphemistically to describe qualities and practices that are altogether to be expected in those worlds and are as cool and calculating – or, if one prefers, ‘amoral’ – as anyone might suppose.

Under conditions of ‘radical uncertainty’, Shapin writes in his first chapter and repeatedly thereafter, ‘people matter.’ What this refers to, it turns out, is the high degree of material-financial interdependence of enterprising scientists and the investors who bankroll their projects and, accordingly, the continuous need for them to size each other up correctly. The ‘personal virtues’ Shapin emphasises had always included, in industrial scientists, due loyalty to the company. Now they include due respect for money, the willingness to ‘cash out’ of a project at the right time, and what his informants call ‘performance’: that is, either a scientist’s ability to turn out a product with the sort of market share he predicted during a project-pitch, or a venture capitalist’s (VC’s) ability to turn up with the cash he seemed to command. ‘Trust’ is largely a matter of betting on such virtues on the basis of such pitches and flashes. (Shapin quotes an investor: ‘Oh, you do have to trust the people … It’s like getting married.’ Well, yes and no.) Most significantly, perhaps, ‘patterns of familiarity’ turn out to be nothing other than corporate-style social networking: games of squash, business breakfasts and buffet lunches among groups of entrepreneurial researchers, techies, potential investors and sometimes university administrators.

Shapin is centrally concerned with the ‘interpersonal relations’ among and between enterprising scientists and potential investors, which are often built up in the course of their working and playing together. As he notes, deals are done on golf courses, fishing trips, sailing boats – sites of male bonding. ‘That’s one way of thinking about why it’s such a masculine world,’ he comments. The exclusionary effects of such work-and-play networks are well known to sociologists. Shapin acknowledges them obliquely: ‘Patterns of familiarity powerfully regulate access to VCs’ attention’; ‘Without the advantages of familiarity, the satisfaction of formal criteria means almost nothing.’ But he brushes aside the significance of those effects for his claims about what he calls ‘the moral texture of networking’ in the world he is describing:

Gender and ethnicity are, indeed, elements in the patterns of familiarity that structure relations in the world inhabited by VCs and entrepreneurs, but they do not map unproblematically onto psychological states; they are not the only elements in such patterns; and they are cross-cut by considerations that work in ways whose effect on members of specific groups is impossible to determine.

The points here are elusive. What does it mean, for example, that gender does not ‘map unproblematically onto psychological states’? That VCs and enterprising techies do not actually experience themselves as having contempt for women? What does it mean that gender and ethnicity ‘are not the only elements’ in those patterns of familiarity, and what exactly are the ‘considerations’ that ‘cross-cut’ the ways they do figure there? Family connections? Yes, understood: the Asian guy might be someone’s brother-in-law. Old school connections? True, a woman might be considered OK because she was a classmate at Harvard Business School. A shared love of fly-fishing? Unlikely, but not out of the question. The possibility that this odd fellow, wherever he’s from, really has the money or a knockout gadget? Yes, of course: the meritocracy of the market. That, apparently, is what makes it ‘impossible to determine’ – and makes it easier for Shapin to avoid taking into account – the specific inequitable effects of the entrepreneurial-VC networks that he describes as ‘morally textured’.

Anticipating misunderstandings, Shapin declares that he is personally attached to ‘the regulative ideal of disinterested inquiry’. Nevertheless, he goes on to write, he differs from many other academics ‘in rejecting the notion that there is some essential and necessary difference between academia and industry with respect to the possibilities of inquiry’ and believes that ‘the university should welcome, not resist, comparison with the managerial ethos of industry in the management of the lives of creative people.’ Perhaps. Granted, many or most industrial scientists are happy with their lot while academic scientists often have a hard time finding the resources they need to pursue their research. Granted, much important science is done in industrial laboratories and the products and technologies developed in Silicon Valley contribute greatly and even crucially to the safety, convenience and good health of mankind – or, to be accurate, that portion of mankind which can afford to buy them. Still, there is much to be said, I think, for the idea that there should be spaces, even if they aren’t specifically university-based (Shapin does not discuss research institutes, such as the National Institutes of Health or the Institute for Advanced Study at Princeton), in which the focus and direction of research are not constrained by considerations of the profits to be gained and where the efforts of administrators are aimed at securing conditions that maximise the realisation of the ideal to which Shapin is attached. Perhaps there is even something to be said for the notion that undergraduates should learn about science from practising scientists and that graduate students should be encouraged, inspired and trained by them. Shapin observes that the scientific life in academia and in industry are becoming increasingly convergent. That certainly seems to be the case and it’s important to know about it. But, he might agree, some aspects of that development can also properly be lamented.

In Weber’s essay, ‘Science as a Vocation’, (1919) which serves as a thread, foil and reference point throughout The Scientific Life, the vocation (Beruf) portrayed is Wissenschaft, which is not laboratory science but something better translated as ‘the disciplined pursuit of knowledge’. As Weber makes clear, it consists of the conjoined practices of disinterested, open-ended scholarly or scientific inquiry and the teaching of the products, methods and value of such inquiry to new generations of scholars and scientists. For that vocation to be honoured in Weber’s sense, the scientific life would have to be, as it was for him, the life of the dedicated pedagogic researcher. It would be, in short, wherever it was pursued, the academic life.