

Weber's *Science as a Vocation*: A moment in the history of "is" and "ought"

Journal of Classical Sociology

1–18

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DOI: 10.1177/1468795X19851408

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Abstract

This essay situates Weber's 1917 lecture *Science as a Vocation* in relevant historical contexts. The first context is thought about the changing nature of the scientific role and its place in institutions of higher education, and attention is drawn to broadly similar sentiments expressed by Thorstein Veblen. The second context is that of scientific naturalism and materialism and related sentiments about the "conflicts" between natural science and religion. Finally, there is the context of Weber's lecture as a *performance* played out before a specific academic audience at the University of Munich, and the essay suggests the pertinence of that performance to an appreciation of the lecture's meaning.

Keywords

Max Weber, vocation, science, Thorstein Veblen, scientific naturalism, scientific materialism

Science as a vocation: Its past and present

Here are several things typical of modern academic performances, whether written or oral. First, there is a display of efforts to get the facts right, to show that matters are indeed as they are said to be. Second, there is an expectation that at least some of those facts are, to a degree, novel, non-obvious, worth pointing out, and that the interpretations or explanations in which the facts are enlisted are plausible, similarly novel, and meriting consideration. A third feature – though not now universally accepted – is that such performances have, and should have, a matter-of-fact character. They either aim at disinterested description and interpretation or, while the performances may occasionally serve prescriptive goals, any such purposes are formally disengaged from their factual content or interpretative legitimacy. That's what many people mean when they describe academic performances as "academic."

To say that sort of thing is to position this piece of writing, and very many similar academic performances, in a tradition tracking back to Max Weber's *Science as a Vocation*.

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What marks out the present essay is that it is both *about* Weber's lecture and that it reflectively *participates* in many of the sentiments expressed in that now-more-than-century-old lecture. It looks *forward* from Weber – what is it about the form and manner of *this* performance, and of so many that are formally like it, that links them to the views expressed in *Science as a Vocation?* – and, at the same time, it looks *back* to the contemporary and just-past institutional and cultural circumstances that bore upon Weber as he delivered his lecture. I say some things about *Science as a Vocation* that I hope will be assessed by my academic peers as factually correct, to a degree interpretatively sound and novel, and formally disengaged from moralizing or political prescription.¹ So, despite widespread disputes about the politicization of modern academic life, this article is a still-typical matter-of-fact performance; it describes and interprets rather than evaluates and recommends. This places it in the lineage of Weber's conception of what an academic performance should be, though, between Weber's essay and my own, readers will, of course, notice a difference in quality. The idiom in which I want to say these things is *historical*. I treat Weber's lecture as a moment in the history of *institutions* – especially the institutions of higher education – and also a moment in the history of *ideas* – especially ideas belonging to natural science and to the philosophies of knowledge and morality. I ask how sentiments in Weber's lecture pertain to changes in then-current thinking about the relations between *description* and *prescription*, between *is* and *ought*, and between what counts as natural and what as supernatural. Those relations are recursive, their recursivity evident in the content of the lecture – including some of its more famous pronouncements about *disenchantment* – and also in its status as a *performance* enacted before the Munich university students on 7 November 1917.

The relationship between description and prescription is, of course, a topic that Weber reflected on in much of his work, but it's also a *problem* in understanding the conceptual content of *Science as a Vocation* and in understanding the lecture as performance. First, some of the global descriptions – of institutional and cultural realities – offered in the lecture are only, and interestingly, partially or conditionally correct. That's to say, there are aspects of the institutional facts that Weber was describing which bear reflection, unpicking, and critical evaluation. And many of the *descriptions* in the lecture contain within them – I dislike the sneering term “smuggled in,” but something like that – *prescriptions* about how institutional matters *ought* to be arranged and about how one *ought* to confront the realities described. The *is/ought* question is a *topic* in the lecture, but here, and in other then-current cultural commentary, it was also a *resource* for giving an account of modern life and for counseling how to live within its conditions.

Americanization as a *problem* for the scientific life

I begin with some pertinent institutional realities. One feature of Weber's lecture is sometime neglected, and, indeed, there is at least one anthology in which the relevant passages in *Vocation* are even deleted, including the first significant anthology in the sociology of science.² Social and cultural theorists are not greatly interested in the *job structure* of universities – unless, of course, it's their own job and their own university. But that's just how the piece starts out; it's what frames all the rest, advertising what the talk is *about*. Weber's address to the Munich students was about the circumstances, demands, rewards, risks, and obligations of the academic occupation – *Beruf* in the sense

of a job.³ You could say that it's one of modern culture's most eloquent and resonant passages of practical career counseling, in effect a warning to the Munich student-audience against taking up an academic life, telling them hard truths about what it's *really* like as opposed to what they might ideally (and wrongly) have been led to think it's like. Weber's lecture was substantially about the concrete nature of German university employment. It's the kind of thing, which – just like that – might have been published in the German equivalent of the *Chronicle of Higher Education*, were there such a thing in 1917. And, indeed, with appropriate but superficial changes, it might be published today, as it's an engagement with tensions between education-as-cultural-formation and education-as-practical-skill-provision that animate early twenty-first-century institutional conflicts.

The question Weber then addressed is why ever any rational young man would take on such a career. Once its problems and difficulties have been properly appreciated, why, if you were in your right mind, would you *do* that? What drifts of mind should satisfy you that you were suited to the academic life, and if you did take your chances with that career, what might you expect your life to be? And one of the reasons why you might not choose the life of scientific inquiry is conveyed in a tone of bitter-sweet nostalgia, concerning what Weber called “the external conditions” of the occupation. German university life, and especially scientific life, “is being Americanized”; the scientist needs a lot of money to support research and, echoing Marx, the scientific worker is separated “from his means of production” (Weber, 1958 [1917]: 129, 131).

So we early twenty-first-century moderns should know where we are, for not only have *American* universities been “Americanized” – “the German universities in the broad fields of science develop in the direction of the American system,” Weber (1958 [1917]: 131) said – but so too what has been taken as the “American model” is now embraced by academic governance in many other countries – the United Kingdom (spectacularly so), Australia, Canada, the Netherlands, Germany, and others – where “Americanization” equates broadly to the imposition of an audit culture on the academy, the constitution of students as customers in a marketplace,⁴ the democratization of intellectual authority, encouragement for the professor to become a large-crowd-pleasing entertainer, opening-up of university education to a larger fraction of the age-group, rewards for university responsiveness to commercial needs, and the restriction of professional autonomy (Borghans and Cörs, 2007).

Weber was anxious about this Americanization, but American academics at the same time were too, notably including a writer to whom no reference was made in the essay. After all, American professors were at the sharp end of the institutional changes that were part of modernizing processes. A year after Weber's lecture, Thorstein Veblen published his “memorandum on the conduct of universities by businessmen” – *The Higher Learning in America* (Veblen, 1957 [1918]). This was, in part, Veblen's reflection on his unhappy time (from 1892 to 1906) at the John D. Rockefeller-founded University of Chicago and, after that, a briefer spell at the Palo Alto university established by the railroad magnate Leland Stanford.⁵ Veblen presented himself as specially knowledgeable about what happened to intellectual inquiry when institutions of higher education were directed by people for whom knowledge was primarily a means to external ends. After all, the conjunction between tycoons and institutions of higher learning had defined his career up to that point and he saw this connection as a general trend. What was going wrong with businessman-controlled American universities

was a stress on teaching, on football-as-character-formation, on practicality of research. History showed a “long-term idealistic drift” – opposing all of these tendencies – but the businessmen didn’t *feel* that drift, and their management of universities was a mistake. The businessmen didn’t *get it*, and they were capable in the short-run of causing immense damage to institutions of higher education, but eventually history would *get them* (Veblen, 1957 [1918]: 31). There are only a few clear references to Veblen’s work in Weber’s writing,⁶ even though some of Veblen’s sentiments about academic life and its intellectual texture were on the record before the appearance of Weber’s *Science as a Vocation*.⁷ It’s instructive to read these two texts together, and it would be useful to add in Veblen’s 1906 essay on “the place of science in modern civilization,” which I have found no evidence that Weber knew.

So there were a number of writers from the end of the nineteenth to the early twentieth century anxious about the *institutional* changes bearing on academic life, and some of their commentaries linked those changes to the secularization of culture brought on by both industrial society and the rise of new frameworks for scientific thought emerging from about the middle of the nineteenth century. Veblen was one of those commentators, but there were other notable contemporary polemicists – some of them American – expressing anxiety about attitudes to academic science, about the value it was reckoned to have, and about the effects of these considerations on the work-world and lived experience of academic science.

Purity and its imperatives

To Weber, it was something like a category mistake to think that scientific inquiry had any external purposes or justifications, notably to see its point and purpose as the delivery of material goods. Scientists, Weber insisted, do science for its own sake, their motives essentially different from those of distinctly different sorts of people who “by exploiting science, bring about commercial or technical success.”⁸ That much counted as a commonplace for Weber and he devoted no special effort to documenting an essential distinction between scientific inquiry and practical concerns. But American scientists were there before him. In the 1880s and 1890s, the physicist Henry Rowland – writing from Johns Hopkins, America’s most Germanized research university – complained that the public culture failed to distinguish the practical applications of science from the thing itself – which he called – in a still-not routine usage – “pure science.”

Applied science was wholly derivative of pure science, and the inquiries of the pure scientist were motivated by no practical considerations at all (Rowland, 1883, 1899). At about the same time, the philosopher (and mathematician) Charles Sanders Peirce – who had done practical astronomical and metrological work for the US Coast Survey – also matter-of-factly excluded practically concerned workers from the category of scientists:

if a man occupies himself with investigating the truth of some question for some ulterior purpose, such as to make money, or to amend his life, or to benefit his fellows, he may be ever so much better than a scientific man, if you will – but he is not a scientific man ... True science is distinctively the study of useless things.⁹

(Peirce, 1940: 43, 48)

And Veblen complained that the imposition of practical concerns on academic inquiry had a “corrupting effect”: “Within the university precincts any aim or interest other than those of irresponsible science and scholarship – pursuit of matter-of-fact knowledge – are to be rated as interlopers”; “Work that has a commercial value does not belong in the university.” The pragmatist in these matters, Veblen announced, was nothing but a “barbarian” (Veblen, 1957 [1918]: 23, 25, 110). Those sorts of sentiments circulated widely. In Germany, the immensely influential physiologist and physicist Hermann von Helmholtz wrote in 1862 that “Whoever, in the pursuit of science, seeks after immediate practical utility, may generally rest assured that he will seek in vain” (Helmholtz, 1995: 93; also Cahan, 2018: 383).

A choice available to academic defenders of disinterested inquiry was to display applied science as dependent and derivative of pure science, so that (if necessary) pure science might be encouraged for this reason. This held out the possibility that the scientific life, even in academia, could encompass a range of motives and moments. A second option was to insist that the two had nothing to do with each other: applied scientists or technologists should not think to call themselves scientists at all and anyone entertaining such interests was not *doing* science. Although his engagement with these things was only glancing, Weber seems to have preferred the latter way of thinking.

So one way, in Weberian terms, of setting this sentiment in *context* is to note that the consequential distinction in motives that set apart the scientific calling from practical activities was an institution in the late nineteenth and early twentieth centuries. But another way of setting the notion in context is to compare those institutionalized sentiments to institutional realities. In fact, at just the time that Weber was giving his lecture, one of the most famous scientists in Germany was the University of Karlsruhe chemist Fritz Haber, just about to win the Nobel Prize for the discovery of methods for making synthetic ammonia, a discovery that not only belonged to “applied science” but which enriched him and his BASF industrial colleague Carl Bosch and which radically reshaped the world’s commercial and military future (Charles, 2005). The Nobel Prize committee, and many other commentators, saw Haber as a scientist and the Haber-Bosch process as science, even as they failed to satisfy Weber’s criterion. The deployment of natural scientific expertise in commerce and in the technologies of statecraft and of war were nothing new in the nineteenth century: the enfolding of natural knowledge in these, and many other, practical concerns went back to Archimedes, and it was a lineage that included Galileo, the work of the early Royal Society of London, and the forms of natural history, cartography, geophysics, and astronomy mobilized in imperialism and global trade through the nineteenth century. What was significantly novel in Weber’s lifetime was the formalization of specialized academic training in the natural sciences and the mass employment of academically trained personnel in organized industrial research and development. Indeed, Haber’s work came toward the end of decades-long German developments in industrial science – especially in dyes-stuffs, pharmaceuticals, optics, and electricity-generation – and almost contemporary with the emergence in the United States and Britain of similar industrial firms which also sucked in huge numbers of academically trained scientists and which endowed vast new industrial research laboratories.¹⁰ In the United States, Henry Rowland – distancing physics-proper from mere Edisonian tinkering – personally held patents and for years was a paid consultant to industry. Earlier in the nineteenth century, Hermann von Helmholtz, the German

physiologist who had announced that practical outcomes were rarely, if ever, to be expected from genuine scientific research, achieved more fame for inventing the ophthalmoscope than for any of his many discoveries in basic science (Cahan, 2018: 99).

It would be tempting, therefore, to say that Weber's "for its own sake" criterion for legitimate science was "mistaken," but it's better to consider what concerns were served by contemporary polemics supporting "pure science" and separating practical concerns from an authentic scientific calling. Hegel (1896 [1820]: xxx) famously said that the "owl of Minerva" – philosophical reflection – "takes its flight only when the shades of night are gathering," that's when "the ideal appears as the counterpart to the real." And there is a permissible extension of that sentiment in which the defense of scientific purity, always and everywhere disengaged from external practical concerns, should also be seen as a shadow cast in cultural and institutional twilight. The idea of disinterested "science for its own sake," and especially of science disengaged from technology and commerce, becomes salient just when that idea is massively threatened. And if, indeed, science-enlisted-toward-practical-ends was seen as a threat, then the late nineteenth and early twentieth centuries are among the historical settings in which one would expect purity and disengagement to be insisted upon as a defense of an old order passing. Here, description merges with wistful nostalgia. The *is* of Weber's account of the institutional setting and the purpose of science is only partially correct – some scientific practitioners and other contemporary commentators thought about these things differently – and it makes sense only on the condition that one recognizes the element of prescription within it.

Scientific modernity and amorality

Practical utility was only one of the disqualifying aims that Weber's lecture dismissed from the scientific calling. He devoted rather more space and energy to rejecting the very idea that science might answer recognized existential questions or identify moral goods – What is life's meaning? What is the right thing to do? Of course, that engagement extends a position Weber had developed from at least his 1904 treatment of objectivity in the social sciences. All sciences, Weber understood, arose historically from practical concerns, but the progress of science has, at least among right-thinking people, resulted in recognition of a *logical* distinction between knowledge of *what is* and knowledge of *what should be* (Weber, 2012 [1904]: 101–102; also Proctor, 1991: ch. 10).

The *is/ought* distinction in philosophy can be traced back to David Hume's eighteenth-century *Treatise of Human Nature*, but there was a resurgence of attention to it in Weber's time, with philosophical commentators including the English logician Henry Sidgwick (1874) and, more famously, G. E. Moore (1903) in the *Principia Ethica* of 1903.¹¹ Hume noted that, even though the fallacy was institutionalized in forms of Christian natural theology, it was not logically possible to move from description to prescription (Hume, 2007 [1739]: 302). And Moore argued that identifying a moral item with its supposed natural properties was incoherent: if you claim that something is "good" amounts to saying that something is "pleasurable," then the proposition that "Pleasure is good" would be the same as saying that "Pleasure is pleasure" – accomplishing nothing (*Stanford Encyclopedia of Philosophy*, n.d.). In his now-little-known-but-important 1906 essay on the cultural place of science, Veblen endorsed

what was then, in knowing secular circles, an apparent article of common sense: “Pragmatism creates nothing but maxims of expedient conduct. Science creates nothing but theories. It knows nothing of policy or utility, of better or worse” (Veblen, 1906: 600).

In the 1917 lecture, it was Leo Tolstoy with whom Weber explicitly aligned himself: “Science is meaningless because it gives no answer to our question, the only question important for us: ‘What shall we do and how shall we live?’” For Weber, it was “indisputable” that science neither gave nor could it give answers to such questions (Weber, 1958 [1917]: 143; also Tolstoy, 1937: 179, 186). If you want to know how to behave morally, or politically, or even to know the *meanings* of things, it was not the scientist to whom you should turn. Nor, continuing the career counseling, should students choose a scientific career if they thought that such a career could deliver answers to such questions. As a description, the modern historian might insist that Weber’s account was, again, only partially accurate. And there was also a prescription accompanying the description: it advised candidates that if this is what they expected from a scientific career, then they should pick some other line of work, or, if they persisted in this expectation, that they should acknowledge that what they were doing was not an extension of science but its perversion.

Weber was here subscribing to a position in *natural science*, treating as self-evident the world-picture developed in the Scientific Naturalism emerging from about the middle of the nineteenth century in both European and American cultural settings. In Britain, naturalism was powered by the evolutionism of Herbert Spencer and Charles Darwin, and by the polemical writings of Darwin’s follower the biologist T. H. Huxley, the statistician Francis Galton, and the physicist John Tyndall – all of whom, but most prominently Spencer and Darwin, very well known in Germany. Anglophone scientific naturalism was specially marked by the undermining of natural theology, a sensibility prominent in both Protestant theology and in natural science which sought to establish the existence and attributes of God from the evidence of design in the natural world.¹² Design was evident in nature; matter was inanimate, “brute and stupid”; there must be an external intelligent designer; and because you could and should infer the divine from the natural, so you could and should hear “sermons in stones,” as Shakespeare put it. There was no logical fallacy in so doing – because nature was a divine creation and reading the “Book of Nature” was a path to meaning and morality on a par with reading the Bible. But in a world lacking that intelligent design, you could not read God’s intentions from the evidence of nature, so undermining the institutions that had discerned moral lessons contained in natural structures and processes.

Natural theology, and its keystone Argument from Design, was more prominent in Anglophone than in German settings, but one of Darwin’s more aggressive followers was the celebrated Jena biologist Ernst Haeckel, who vigorously denied the necessity of inferring a non-natural intelligent designer from the evidence of design in nature, and whose embrace of a Spinoza-like pantheism attracted the animosity of the churches (Hopwood, 2015; Richards, 2008: 343–390).¹³ What was encompassed by German versions of scientific naturalism, and what figured in Weber’s specifically German cultural legacy, included, first, strands of academic biology – attempts to explain organic phenomena in mechanical terms. These included attacks on the notion of *Lebenskraft* – a special vital force that operated in living things – and the reflective ejection from scientific accounts of explanatory items other than those in the then-standard inventory of

physical forces and chemical entities. From mid-century, the “organic physics” group – centered on such celebrated physiologists as Karl Ludwig, Ernst von Brücke, Emil du Bois-Reymond, and, especially Hermann von Helmholtz, established the electrical nature of the nervous impulse, precisely measured its speed, and offered accounts of human sensation and perception in terms of physico-mathematical laws and physiological substrates.¹⁴ Weber knew this academic work quite well – his admiration of Helmholtz’s studies of physiological acoustics and of musical tones and responses expressed in his own sociology of music (Weber, 1958 [1921]).¹⁵

Scientific materialism as a disenchanting mode

But nineteenth-century German scientific and philosophical culture was characteristically marked by more aggressive forms of *scientific materialism*, and these too formed part of the cultural legacy of German writers about the scientific life into the early twentieth century. The main streams of German academic scientific materialisms also included the elimination of *vitalism* – the view that living things and their operations could not be wholly accounted for by physico-chemical processes – and an embrace of many aspects of the work done by the organic physics group. But the German setting also threw up vigorous forms of materialism that actively targeted clerical concepts and authority (Cahan, 2018: 155–157; Gregory, 1977). From the 1840s, the lessons of scientific materialism were broadcast in German culture by such polemical writers as the zoologist Karl Vogt, the physiologist Ludwig Büchner, and the Dutch physiologist and nutritional writer Jacob Moleschott, and, while Ludwig Feuerbach’s earlier anti-Hegelian work did not significantly draw on the natural sciences, it was an inspiration to all three of these German scientific materialists who developed their materialism into a major resource for German anti-clericalism, atheism, and radical political thought (Brooke, 2014 [1991]: 408–413; Gregory, 1977: x, 5–6, 22–23).

Weber declared that “There are no incalculable mysterious forces” at work in nature, there are no “magical means to master or implore the spirits,” and, indeed, there are no spirits. There was nothing supernatural about nature and there was nothing about the life of science that offered the goods traditionally provided by conceptions of the supernatural – answers to questions about the meaning of the world, the foundations of morality, a notion of death as anything but the end of life. Everything can be mastered by “calculation,” “the world is disenchanting” – de-magicified (Weber, 1958 [1917]: 139). Weber discerned a “process of disenchantment” working through Western culture for “millennia,” but the view of natural phenomena that most powerfully marked the Western mind during Weber’s time were little older than Weber himself, born 5 years after the publication of Darwin’s *Origin of Species*. Evolution by natural selection was one – crucially significant – mode of disenchantment, but so too, in the nineteenth and early twentieth centuries, were thermodynamics, research on the mechanical equivalent of heat, the germ theory of disease, the artificial synthesis of organic compounds, the rediscovery of Mendelian genetics, advances in nutrition science, the theory and practice of calculating engines, psychophysics, and even Freudian psychoanalysis.¹⁶ And all of these scientific achievements offered powerful mechanical and material accounts of phenomena once thought necessarily to involve divine wisdom and non-natural powers. The naturalistic fallacy was represented by many as a *logical* mistake, with clear consequences for moral

reasoning. But that fallacy was, under another description, a historically specific achievement of *natural science*, and those, outside of the natural sciences, who endorsed it on logical or moral grounds were taking a natural scientific position at one remove.

Naturalism, materialism, and cultural conflict

By 1917, claims of this sort were all over the public culture, and the “specifically irreligious power” of science was being asserted in concrete situations where there were contests over cultural and political authority: When you were ill, did you call on the prayers of priests or the therapies of physicians? When the country was afflicted with cholera, was it better to institute days of fasting or to clean up the water supply? Were floods and droughts punishments from God for immoral behavior or were they the result of wholly natural forces whose precise causal workings might not yet be known but which it was rational to believe could be known through progress in physics? And, notably, should sacred or secular experts be in charge of the education of youth? (Turner, 1974a, 1974b, 1978).

The cultural and institutional conflicts were real enough, and there were a number of widely distributed Victorian texts asserting that such fault-lines were historically pervasive; that, given what were taken to be the opposing logics of science and religion, cultural conflicts were inevitable; that, if the blessings of modernity were to be secured, these conflicts must be won by science; that science *was* winning; and that materialistic and causal science was the direction in which history was unstopably going. The definition of cultural modernity was partly given by capitalism and partly by science. Late Victorian bestsellers included *A History of the Warfare of Science with Theology in Christendom* (various versions from 1869 to the end of Weber’s life, and beyond) by the historian and Cornell President Andrew Dickson White, and *A History of the Conflict between Religion and Science* (1874) by the English expatriate chemist John William Draper. Weber was broadly on their side – though they thought the *Kulturkampf* was still going on while Weber’s lecture announced that it was substantially over and that science had definitively won the war.¹⁷ Anglophone “conflict theories” like those of Draper and White were well known in Germany, both of their “conflict” books were translated into German (Draper, 1875; White, 1895), the German scientific materialists were keen for the fight, and there were treatments of the impossibility of reconciling Darwinian natural science and religion by German academic theologians, notably Otto Zöckler’s *History of the Relations between Theology and Natural Science with Particular Reference to the Story of Creation* (Gregory, 1992: ch. 4; Zöckler, 1877–1879). Sensibilities about the cultural relations of natural science and religion varied among nineteenth- and early twentieth-century national settings, but the general sense of tension, and attributing the sources of that tension to scientific naturalism, were significantly shared.

The problematic death of the divine

The meaninglessness of science was a historical accomplishment, it was a fairly recent one, and, in fact, its status as an accomplishment was, and remains, problematic. Weber briefly acknowledged this by sketching a historical story about the modernization of natural science. “During the period of the rise of the exact sciences” – what historians are

accustomed to call the Scientific Revolution – the Dutch naturalist Jan Swammerdam reckoned that he had delivered proof of God’s providence from the anatomy of a louse (Weber, 1958 [1917]: 142). Weber was quite right: proving God’s existence and powers from the evidence of nature – natural theology – had been (as noted) an institution in seventeenth- and eighteenth-century science, most especially in the Anglophone and Dutch settings to which Weber gestured when he gave the example of Swammerdam. Robert Boyle participated in this exercise, and so did Isaac Newton, and so too did the naturalist Robert Hooke, whose microscopic representations of lice (and flies and other insects) were more celebrated than Swammerdam’s. For Boyle, science was a form of worship, so the scientific study of God’s creation was equivalent to going to church: you could legitimately do it on Sunday. When he was at Cambridge in the very early nineteenth century, Charles Darwin encountered the Argument from Design – recognizable to seventeenth-century naturalists – and was struck by its power.

But, Weber said, that conception of the theological and moral goods that science could deliver – a conception that he well understood was powerful in the early modern period – had now disappeared, gone forever: today, no one, or practically no one – “aside from certain big children [außer einigen großen Kindern] who are indeed found in academic natural science departments – still believes that [scientific findings] could teach us anything about the meaning of the world.” Indeed, the drift of modern science was to eliminate belief that there is such a thing as the “‘meaning’ of the universe.” Once, so the history-of-science lesson continued, science was conceived as a handmaid of religion; now, science was self-evidently a “specifically irreligious power” (Weber, 1958 [1917]: 143). Modern science had shown that there are no mysterious spiritual forces, there’s nothing that responds to prayer, nothing that makes manifest the existence, power, and wisdom of a God. If you want to know what God wants for the world, even to know if God exists, then you can’t know these things, or have the resulting comforts, through scientific inquiry (e.g. Schluchter, 1989: 254–264).

I have noted that these sentiments were pervasive at the time of Weber’s lecture, and that they had been for decades before, but, again, it’s not right simply to take such sentiments as unproblematic descriptions of contemporary cultural attitudes. First, while natural theological sentiments, and the design argument licensing the move from natural description to divine meaning and intention, were systematically attacked by nineteenth-century scientific naturalists and materialists, their substantial displacement did not spell the end of influential inference from *is* to *ought*. Evolutionary frameworks were notably enlisted in prescribing what people ought to do by conceiving of human beings as wholly natural entities and arguing that they ought to behave in conformity with what were taken as evolutionary imperatives. There was, however, no global consensus about whether natural/moral conduct was the struggle of each person with all other people, whether it was natural/moral that a race capable of dominating others races should do so, or whether the “struggle for existence” was rightly conceived as the struggle of all of humankind with its physical and organic environment, so implying the rightness of cooperation and socialism.¹⁸ That is to say, nature remained a great moral prize, but inference from the natural to the moral yielded no coherent and stable result. Second, very many scientists, as well, of course, very many theologians, did not see science as a “specifically irreligious power,” just as many did not agree that scientific

advances had rendered the world disenchanting and meaningless, that moral significance had been drained from scientific inquiry and the scientific role, or that the scientist-quasi-scientist could not pronounce on what should be done. All these, like the Death of God, were announced prematurely (Asprem, 2014, 2018; Josephson-Storm, 2017; Sedgwick, 2004). There were moments at which Weber seems clearly to have understood this: in the 1904 “Objectivity” essay, what he called “ethical evolutionism” had become, in the later part of the nineteenth century, an important, even (he said) “the predominant attitude” in at least sectors of the human sciences. That tendency to move from an evolutionary *is* to a social and political *ought* was called “confused,” even mistaken, but it was a persistent and pervasive cultural fact (Weber, 2012 [1904]: 101). Weber, of course, understood that he was doing *Wissenschaft*, while he was at one with natural scientists like Helmholtz (1995: 76–95) in accepting that there were fundamental differences between the sciences of nature and the sciences of culture – as there were differences in the objectivity that could be expected from each. But the tendencies in the human sciences that he cautioned against have persisted and are now flourishing, witnessed by resurgent tendencies in evolutionary psychology and related areas of the modern social sciences (Koshul, 2005: 98–99; Shapin, 2015).

The essential morality of the scientific life continued to be asserted, even – perhaps surprisingly so – by some industrial scientists. Consider this statement by the founder of the great American technical consultancy company Arthur D. Little, who in 1924 celebrated the theological uses of science and the moral heroism of scientists:

Theirs is a true vocation, a calling and election. It brings intellectual satisfactions more precious than fine gold. They live in a world where common things assume a beauty and a meaning veiled from other eyes; a world where revelation follows skillful questioning ... The laboratory may be a temple as truly as the church.¹⁹

(Little, 1924: 304)

A survey conducted in 1916 found that 40 percent of randomly selected American scientists professed belief in a personal God. This was a surprise to the author of the report and he expressed confidence that the figure would surely drop as scientific education spread (Leuba, 1916). But it has not. In a survey published in *Nature* in 1997, it was found that an identical 40 percent of American scientists counted themselves as believers in God, with only 45 percent willing to say they did not believe (Larson and Witham, 1997; Radford, 2003; Shapin, 2007).

There is no doubt that the idea of a necessary conflict between morally mute science and morally vocal religion was, and remains, a recognized cultural trope. But there should also be no doubt that the “scientization” of Western culture advertised by Weber, Veblen, and others did not accurately describe late nineteenth- and early twentieth-century realities: the world may well have been disenchanting for a few but not for the many. Veblen wrote that “science gives its tone to modern culture,” that it was “the dominant note of human culture,” and that “on any large question which is to be disposed of for good and all the final appeal is by common consent taken to the scientist” (Veblen, 1906: 587, 608–609). Yet, it’s fair to say that Veblen, and many other intellectuals writing in a similar vein, didn’t – so to

speak – “get out enough.” A failure to get out enough is a common enough intellectual pathology, and those who pronounced on the authority of science and the disenchantment of the world were subject to an endemic academic failing. The moral meaningfulness of science was even rejected by significant numbers of scientists – in America, to be sure, but also in other Western countries – not to mention (which one really ought to do) many non-Western scientists. And that state of affairs continues. If, indeed, science in Weber’s time was a “specifically irreligious power,” then it had *not* won the battle described by the nineteenth-century Scientific Naturalists and it still hasn’t. Rather than cite surveys and statistics, reference can now be made to the newspapers, the Internet, and the bizarre statements about climate change emerging from the Trump White House.

Weber’s lecture as performance

There are, of course, many contexts that bear on understanding the 1917 lecture. There’s the context of institutional changes in higher education, both German and American, of changes in the development of philosophy and of natural science, of the Great War and its specific state in late 1917, of German politics, and, of course, of the development of Weber’s own work and of his personal involvement in both social science and politics. One could go on. But one pertinent context was right in front of Weber in November 1917, indeed it included Weber himself: it was Weber at the lectern and the Munich students to whom he was talking, to whom he was offering career guidance, and whom he was trying to inform what the scientific vocation could and could not offer, what they should and should not expect of scientific deliverances.

That’s to say, one way of interpreting *Science as a Vocation* is to see it as a *performance*, a piece of theater along the lines that a later sociologist, Erving Goffman (born just 2 years after Weber died), suggested for the interpretation of face-to-face social life (Goffman, 1959). The plot of Weber’s performance was a morality play, or, to be more precise, a play about the often-conflicted and ambivalent tensions between not so much the moral and the immoral, but the moral and the amoral.

Finally, some remarks about Weber’s lecture as a performance, and specifically as a performance of *is* and *ought*, of morality and amorality, of meaning and meaninglessness (Kemple, 2014: ch. 1). Weber was, of course, addressing students at the University of Munich, and, as we now know, his lecture was, in part, an intervention in contemporary debates about whether universities were for *Bildung* or for *Beruf*, for character formation or for specialized intellectual inquiry. And we also understand that student organizations were divided between the aristocratic and conservative *Burschenschaften*, espousing the former, and the newer *Freistudentischer Bunde*, who were receptive to the notion that the purpose of university education was to prepare students for a specialized occupation (Kemple, 2014: 211–215; Poggi, 2014: 146–147).²⁰ It was the latter group that invited Weber to give his lecture, and, if Weber wasn’t exactly preaching to the choir, it was some of their concerns – as well as those personal to him – that he was addressing. And, while warning of the career risks and lack of rewards associated with the scientific occupation, it was their conception of university education that he was endorsing.

So this was the stage. What sort of play was being performed, and how did the form and staging of the performance bear on its content? Here, as Marshall McLuhan sort-of said, there is an explicit message, but it's the medium – the performance itself – that bears on understanding what that message is. The message here seems to be clear enough: don't do science if you think it's a comfortable career choice, don't do it if you want to become rich, if you can't face the risks of having your merit neglected. Don't do it if you want to produce useful goods or processes. Don't do it if you want to find the way to God, the meaning of life, or a source of personal happiness. Don't do it thinking that your personality will get you anywhere in science. Don't do it if you think that your contributions will secure personal fame or even survive very long. Don't do it if you want to moralize from the podium and cast yourself as a charismatic Pied Piper for the young and impressionable (Weber, 1958 [1917]: 145).

Don't do it unless have a *calling* for it – so to say, a vocation for the vocation. You may have that calling already or you may come to acquire it, as Aristotle said that you could acquire virtue through practice. Discovery in science – having new ideas – is not wholly, or even mainly, a matter of method. Rational calculation is over-valued, Weber said, in rational capitalism and so too in rational science:

Nowadays in circles of youth there is a widespread notion that science has become a problem in calculation, fabricated in laboratories or statistical filing systems just as 'in a factory,' a calculation involving only the cool intellect and not one's 'heart and soul.' First of all one must say that such comments lack all clarity about what goes on in a factory or in a laboratory.

Calculation without inspiration is nothing: "Ideas come to us when they please, not when it pleases us," and to have a calling is to accept the limits of calculation and of self-control over scientific outcomes (Weber, 1958 [1917] 135–136).

Only a calling can justify this decision, for the life of science has few comforts and few rewards, many pains and many risks. And the hardest of these discomforts to face is the juxtaposition of total dedication (to the precise placement of a comma and the determination of the most minute matter of fact) with the reality of total meaninglessness, a meaninglessness of knowledge claims which, in any case, would soon enough be forgotten, superseded, or rejected.

That was evidently meant to count as a description of the scientific life, but it was also a prescription, an *ought* as well as an *is* – and, in fact, there was not just one but two superficially opposing prescriptions of what the Munich students ought and ought not do. Given the description that Weber offered, there were many reasons why his audience should not enter into academic science. And I've already reviewed these. So that description functioned as a warning but also, and possibly more powerfully, it was an invitation. There are two modes of invitation in the lecture. The first was more or less explicit. The more harsh the requirements for a scientific vocation, the more attractive it was to think that you were up to it, that you were – in *Protestant Ethic* terms – one of the Elect. Did you not have the capacity for "passionate devotion" and did you not want to experience the "strange intoxication" of complete dedication? (Weber, 1958 [1917]: 135) And, finally, he challenged the students, repelled by the idea of meaninglessness, to "bear the fate of the times like a man" (Weber, 1958 [1917]: 155). The chivalric appeal was well gauged to appeal to this audience.

Earlier in the lecture, Weber had warned against moralizing from the professorial podium. He condemned a common student “error” – thinking that they were coming to lectures

“in order to experience something more than mere analyses and statements of fact.” ... The error is that they seek in the professor something different from what stands before them. They crave a leader and not a teacher. But we are placed upon the platform solely as teachers.

(Weber, 1958 [1917]: 149)

But the man standing before them on the platform in Munich was *indeed* offering himself as a leader. In warning against undue expectations, he was presenting and performing a heroic vision of the life that he formally warned against. He was prescribing as well as describing, articulating the *ought* as well as the *is*. A hundred years on, his message can still be read in books, but in November 1917, it was dramatically contained in the performance itself – with all the sensitivity to ambivalence and even contradiction that marked so much of Weber’s best history and social science.

Notes

1. That disengagement sets aside the limited prescriptiveness contained in the implicit or explicit commendation that fellow scholars *should* believe what one says about the topic and that they *should* value the exercise’s procedures as proper ways for certain types of academics to go on. All academic performances – however descriptive – embody prescriptive aspects of *that* sort.
2. The editors of that anthology (Weber, 1962 [1917]) omitted everything in the lecture before the sentence starting “This much I deem necessary to say about the external conditions ...” They offer no explanation of the omission, nor is there a summary of the portions left out. All subsequent references here to the Weber lecture are to the complete translated version (Weber, 1958 [1917]).
3. In *The Protestant Ethic*, Weber (1958 [1904–1905]) had already reflected on the religious-occupational resonance of both the German *Beruf* and the English *calling*.
4. “The American’s conception of the teacher who faces him is: he sells me his knowledge and his methods for my father’s money, just as the greengrocer sells my mother cabbage. And that is all” (Weber, 1958 [1917]: 149).
5. During Weber’s American tour in 1904, he spent a week in Chicago, visiting Northwestern University and probably meeting University of Chicago professors. Along with visits to other US institutions (including Johns Hopkins, Harvard, and Columbia), he formed distinct direct impressions of American academic life, some of which were evidently the basis of remarks in *Science as a Vocation*. I find no reference to meeting Veblen – who was then at Chicago – but it’s not impossible that he did (Poggi, 2014: 149; Scaff, 2011: 48–52).
6. Weber (2012 [1904]) was familiar with Veblen’s *Theory of Business Enterprise*: see S. P. Turner (2016: 168–173); also Scaff (2011: 36). I have no evidence that Weber knew Veblen’s *Higher Learning*.
7. John Diggins’s (1999: 111) book on Veblen judges that “No two social theorists could be more intellectually and temperamentally opposed than Thorstein Veblen and Max Weber,” save that both were concerned, in different idioms, with describing and interpreting “contemporary industrial society.”
8. “He maintains that he engages in ‘science for science’s sake’ and not merely because others, by exploiting science, bring about commercial or technical success and can better feed,

- dress, illuminate, and govern” (Weber, 1958 [1917]: 138). A reader of an earlier version of this article suggested that Weber was not here offering an account of the institutional realities of the early twentieth-century German scientific life but was invoking one of his *ideal types*. Given the pervasiveness of concrete career counseling in the lecture, and given the practical concerns of many in his audience, I find this suggestion unpersuasive.
9. Peirce’s essay was probably written between 1896 and 1899.
 10. On the emergence of industrial research laboratories and the normalization of the role of the industrial scientist from about Weber’s time to the present, see Shapin (2008: chs. 4–6). There is a rich literature on of the relations between nineteenth-century German academic science and industry: see Beer (1959), Meyer-Thurrow (1982), Johnson (1990), Wetzel (1991), Meinel and Scholz (1992), König (1995), Reinhardt (1997), Reinhardt and Travis (2000), Shinn (2001, 2003), and Wengenroth (2003: 248–250).
 11. See also Schultz (2004: 153–154) and Bloor (1983: 614–615).
 12. A basic resource for the nineteenth-century career of Scientific Naturalism is Turner (1974a); see also Turner (1974b, 1978).
 13. For Darwinism, and the rejection of design, in Germany, see, for example, Gregory (1992: chs. 2–3) and Kelly (1981).
 14. For Helmholtz, see, for example, Cahan (2018); for Haeckel, see Richards (2008); for the organic physics group, see Cranefield (1957, 1966); for the general embrace of mechanism in German life science, see Lenoir (1989).
 15. For Helmholtz’s influential public approval of the Darwinian jettisoning of the design argument, see Helmholtz (1995: 217–220) and Cahan (2018: 406).
 16. Taking in political economy, one should also include Marxist materialism as “scientific” disenchantment of social and political forms.
 17. For a brief introduction to the now-rich historical literature treating the Victorian “warfare” genre, and usually finding it descriptively misleading, see Lindberg and Numbers (1987) and Brooke (2014 [1991]: ch. 1).
 18. This latter was the conclusion of turn-of-the-century writers like the English statistician Karl Pearson (1900: 363–366) and the Russian scientist and philosopher Pyotr Alexeyevich Kropotkin (1902), while I have already noted that others, including G. E. Moore (1903), reckoned that it was a logical mistake to reduce the moral to the natural.
 19. While these sentiments persisted in some scientific quarters, even into the post-World War II period, there were also vigorous and influential insistences on the moral ordinariness of scientific life (Shapin, 2008: ch. 3).
 20. See, especially, Kemple (2014: Appendix B; “The Free Students Federation and ‘Intellectual Work as a Vocation’ [1917–19]”).

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