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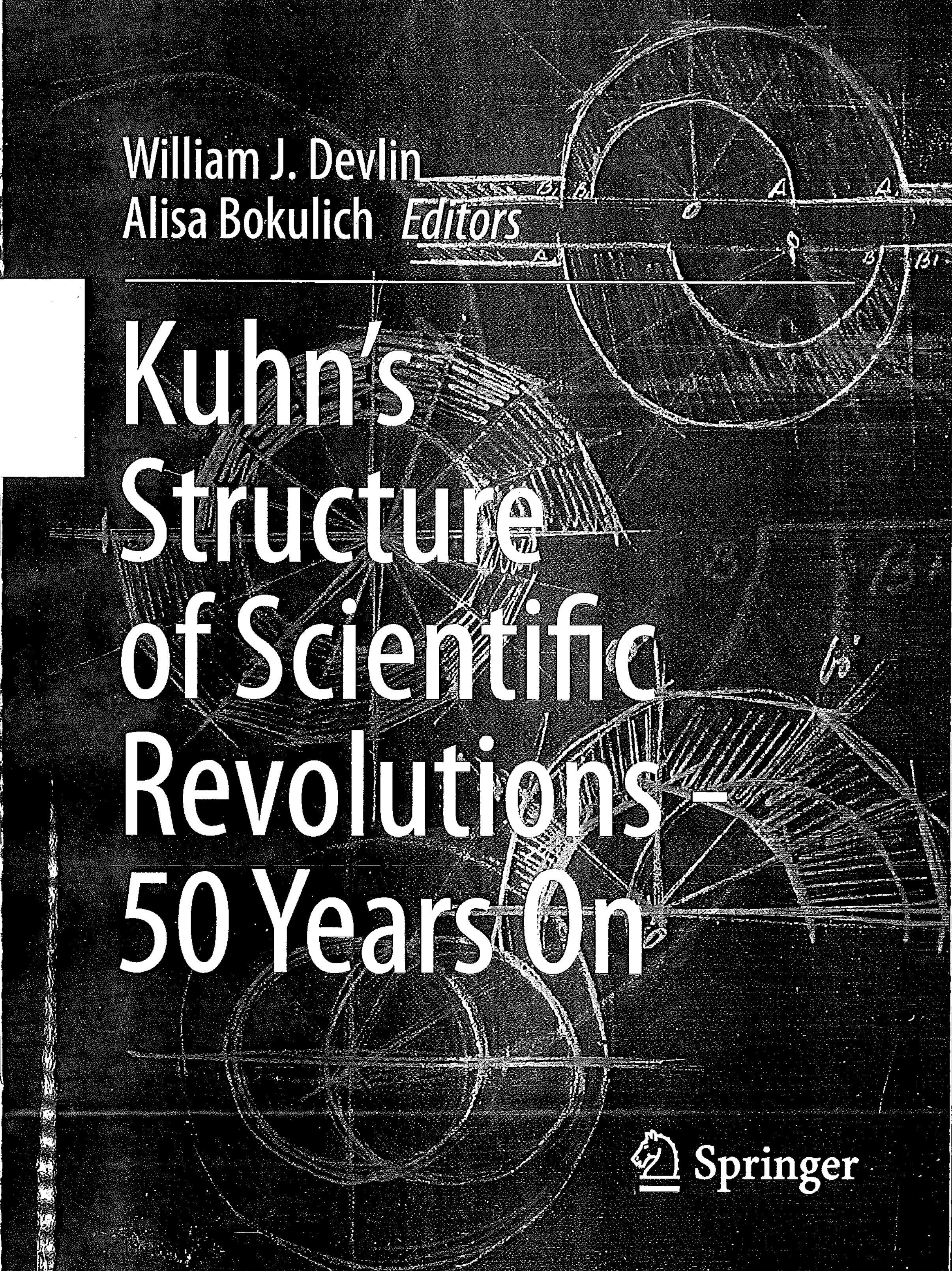
Kuhn's Structure of Scientific Revolutions - 50 Years On



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Chapter 2
Kuhn's *Structure*: A Moment in Modern Naturalism

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The Structure of Scientific Revolutions (henceforth, *Structure*) is history. That's a matter of course; the book offered a theory of historical change in science; it started out by promising a far-reaching change in how we write the history of science; and the cases that made up much of the empirical content of the book were canonical in the academic history of science. *Structure* is, for all that, an odd exercise in the history of science: it's a historically-informed and historically-framed *theory* of science, and, while philosophers routinely produce that sort of thing, historians do so only rarely. The point was made by the Princeton historian of science, Charles Gillispie (1962, p. 1251), reviewing *Structure* for *Science* magazine in 1962: Thomas Kuhn "is not writing history of science proper. His essay is an argument about the nature of science." And this perhaps explains the fact that, when it appeared a half century ago, the historians didn't really know what to make of it, while the philosophers instantly, if perhaps wrongly, thought they knew exactly what kind of thing it was. It was a theory of science which most philosophers attacked whenever they encountered it, and which, if they didn't encounter it, they might conjure up as an ideal-type enemy. *Structure* was a bête-noir of the philosophy of science— it was seen to deny the role, or even the sufficiency in science, of truth, reason, method, reality, and progress. It dismissed method in favor of social consensus or of inarticulable informal criteria; it challenged the notion that science was a peculiarly open-minded practice; it elevated practice over formal theory, the hand over the head and the community over the free and rational individual knower. It commended the philosophical importance of describing science realistically in its making, rather than as its finished products were enshrined in the textbooks.

The philosophical critics were right. Kuhn was a fine rhetorician and he offered his opponents a series of stick-in-the-mind sound-bites, the take-aways, the things you remember about *Structure* when you can remember almost nothing else. On truth:

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"We may... have to relinquish the notion" that scientific change brings scientists "closer and closer to the truth" (Kuhn 1962, p. 169).On scientific education and the mental habits it fosters: "it is a narrow and rigid education, probably more so than any other except perhaps in orthodox theology" (165). On Scientific Method: what Kuhn famously called paradigms "may be prior to, more binding, and more complete than any set of rules for research that could be unequivocally abstracted from them" (46). On the unity of science: science is "a rather ramshackle structure with little coherence among its various parts" (49). On a distinctive scientific rationality: "As in political revolutions, so in paradigm choice—there is no standard higher than the assent of the relevant community" (93). On the insufficiency of logic in science: we must take seriously "the techniques of persuasive argumentation effective within the quite special groups that constitute the community of science" (161). On progress: accepting *Structure*'s picture of science may make "the phrases 'scientific progress' and even 'scientific objectivity'... come to seem in part redundant" (161).

Those sentiments are remarkable, the more so as they were written not, as some critics supposed, by someone meaning to denigrate or attack science, but by someone who, so far as one can tell, thought that, of course, science was a powerful and reliable cultural practice, perhaps the most powerful and reliable way of knowing the world. How is that possible? The answer points to a second sense in which Kuhn's *Structure* is history. It *belongs* to history; it is a historical object, produced in a historically specific set of circumstances. For all that the ideas in *Structure* continue to influence, inform, and, for many, to irritate and enrage, it emerged from a particular historical conjuncture and one way of understanding it is to take a look at some features of that conjuncture—as Kuhn liked to say, *grosso modo*.

The call for understanding *Structure* as coming from, and making sense in, its specific historical circumstances isn't exactly unique. Indeed, during the celebration of fifty years of *Structure*, historicizing the book has probably been the standard gesture in framing commemorative exercises, especially by identifying 'influences' on the type of project represented by *Structure* or on its central ideas—for example, the influence of Conant's pedagogical project on Kuhn's use of case-studies in *Structure*; the influence of what Joel Isaac has recently termed Harvard's "interstitial academy" on Kuhn's interdisciplinarity; the influence of Kuhn's own strikingly loose educational background on what Isaac called his notable "independence of mind" (Isaac 2012, pp. 31–62, 213);[1] the influence of Michael Polanyi on his deployment of the idea of tacit knowledge; the influence of Bruner on his use of Gestalt psychology; of Wittgenstein on rules and rule-following; of Stanley Cavell on all sorts of things, including the awareness of Wittgenstein and of the under-appreciated role of philosophical aesthetics.

[1] Robert Merton similarly pointed to Harvard's "microenvironments," allowing Kuhn, or indeed anyone so placed in the institution, serendipitously to stumble on resources and to acquire perspectives which they might not otherwise encounter (Merton 1977, pp. 76–109; Merton and Barber 2004, pp. 263–266).

Still, there's a kind of historical story about *Structure* that isn't so easily folded into notions of 'influence': this is an account of the *conditions of possibility* of some of the basic sentiments in *Structure*, sentiments that mark this book out from almost everything else previously said about the nature of science and its modes of historical change. Those basic sentiments are the ones represented in the sound-bites about truth, reason, method, reality, and progress, and the social virtues of science. They are, so to speak, the water in which the fish of *Structure* move and have their beings, the environment for the rest of *Structure*'s more specific claims, for example, about incommensurability, anomalies, and crisis. When you read *Structure*, it's the nose in front of your face, the things you tend to forget about when your view is set on finer discriminations. It is the historicity of these sentiments that I want to describe, the dispositional framework of *Structure*, not its fine structure, its historical or philosophical scope, or the validity of its propositions about science.

I call these basic sentiments about science *naturalistic*—where naturalism is opposed to normativity, where the naturalist intention is to describe, interpret, and explain and not to justify, celebrate, or, more rarely, to accuse.[2] My historical claim about *Structure* is very simple: its naturalistic sentiments represent some of the things that are intelligibly sayable about science when the normative and celebratory loads of commentary are lightened or removed. It's not hostility to science that makes these sentiments seem like criticism; it's just the absence of celebration. And that's one reason Kuhn was so mystified by scientists who thought that he had described "normal science" as some form of hack-work ideally to be dispensed with, so puzzled by 1960s student radicals who took it as an exposé of scientific authority, and so upset by philosophers like Imre Lakatos who saw a causative link between those "contemporary religious maniacs ('student revolutionaries')"—and what he called Kuhn's view of scientific consensus as "mob psychology" and "mob rule" (Kuhn 1970, p. 259, 2000, p. 308; Marcum 2005, pp. 74–75; Lakatos 1970, pp. 93, 178). Kuhn did not conceive of naturalism *about* science as criticism *of* science; for him, it had no prescriptive or advisory function. There's no sign that in 1962 he saw the avalanche of criticism coming: *Structure* does not have a defensive tone. And Steve Fuller's (2000) dyspeptic assault on Kuhn is surely right on the point that Kuhn intended nothing remotely like criticism of the status quo, though Fuller set aside as insignificant that Kuhn never intended celebration either.

What was it was about the particular cultural and political environment from which *Structure* emerged that offered the conditions of possibility for its naturalism? Almost needless to say, this environment is not a sufficient condition for sentiments such as Kuhn's—after all, Kuhn's many critics inhabited much the same macroenvironment—but, if they are not sufficient conditions, and if one must also consider smaller-scale environments offered by Kuhn's institutional settings and

[2] "Naturalism" in these matters is, of course, a notoriously disputed notion. Here I use it in a deflationary sense routinely deployed by such sociologists of scientific knowledge as Barry Barnes and David Bloor (Barnes et al. 1996, pp. 3, 106, 173, 182, 185, 202, 208; Bloor 1991, pp. 77–81, 84–106, 177–179), where a naturalistic account of science as it actually proceeds is juxtaposed to its celebration, defense, rational reconstruction, or essentialization.

disciplinary identity (or lack of identity), nevertheless I suggest that it was the new cultural and political place of science in the post-War decades that made the naturalism of *Structure* possible.

With the notable exception of Ludwik Fleck's (1935/1979) neglected work—neglected, that is, by practically everyone but Kuhn before the 1960s—there was in academic writing little unambiguously naturalistic sentiment about the nature of science or its modes of change during the first part of the twentieth century. Science was too precious, and especially too fragile, a flower to be dealt with in an ordinary, matter-of-fact sort of way. What it urgently needed was defense, celebration, and justification—demarcation from intellectual pretenders and lesser breeds. Defense and justification were not just ideologically commended; they presented themselves as intellectually compelling. As David Hollinger (1983) and others have shown, Merton's sociological project was crafted partly to display the liberal, critical, and open condition of science as a social institution and so to hold up the scientific community as a virtuous mirror to totalitarian societies thinking they could interfere with its liberal processes and align science with either Fascist or Communist social agendas. Michael Polanyi's anti-rationalist picture of science (1940, 1946, 1958) was an explicit counter to Marxist rationalist projects which reckoned that science could be enrolled in socially valued planned projects in the same way as technology. Polanyi showed that rationalist accounts were contingently, not logically, attached to the defense of science, and it was that defense, the celebration of science as a unique and powerful form of tacit knowledge, that Polanyi had in view. In philosophy, the epistemological project described by Vienna Circle philosophers like Hans Reichenbach (1938) admitted what was called the "sociological task" of *describing* scientific conduct as it is and as it was, but identified the peculiar epistemological tasks as the normative work of criticism and advising, and, among some members, displaying the Unity of Science that was deemed essential to its cultural authority (Creath 1996; Galison 1998). Karl Popper (1963) took on the urgent job of addressing and identifying the methodological distinctions between authentic science and its illegitimate pretenders. In the history of science, George Sarton (1936) famously insisted that science was culturally unique, that the historian of science was not doing anything like the same sort of thing as the historian of religion, war, politics, or art, and that the history of science should show humankind at its most noble and uplifting.[3] Historians of what was once known as an "internalist" disposition took the writings of Marxist historians as denigration and threat, but the Marxists were celebrating science too, though taking a different view on what science was, what its cultural value consisted in, and the conditions of its historical change (Shapin 1992; Kuhn 1968, 1977). For the Marxists, scientific agendas responded to all sorts of economic and social forces, but the location of science between "base" and "super-structure"

[3] Alexandre Koyré's work (1939), aimed at displaying the intellectual coherence and intelligibility of past science, drifted into the consciousness of Anglophone historians during and after the War, and Kuhn's excitement at that project is evident in *Structure* and elsewhere. One can see Koyré's historical sensibilities as naturalistic, but he did not offer a *theory* of science and some of his historian-followers would have been appalled at the very idea.

was contested within Marxist thought. Marxism was itself seen as a science, and that tells you much of what you need to know about the extent to which writers like J. D. Bernal thought of science as an ordinary cultural practice.

The conditions of possibility of naturalism about science in the second part of the twentieth century were framed by changes in its political and economic circumstances. Naturalism in the intellectual view of science followed normalization in its institutional environment. The story of the changing place of science in the political economy of post-War America has now been well told by, among others, Daniel Kevles, Paul Forman, Peter Galison, and David Kaiser, and I have nothing here to add to their accounts. State funding for science exploded: in the mid-1960s, it was reckoned that the U.S. government was then spending more on research and development than the entire Federal budget before Pearl Harbor (Price 1962, p. 1099, 1965, p. 3). Physics blazed the trail to Fort Knox but the range of American sciences that benefited from huge increases in Federal financial support was very large. Vannevar Bush's dream in *Science, the Endless Frontier* (1945/1995) was substantially realized in the National Science Foundation, while the National Institutes of Health expanded its already huge existing support of the biomedical sciences. First the GI Bill and then the National Defense Education Act transformed the scale of graduate training in the sciences and, as Kaiser has shown, altered the substance of physics teaching and research (Kaiser 2002, 2004, 2005). A vocabulary was developed to talk about the value of science and it was a vocabulary that testified to the simultaneous normalization of science and to its immense civic worth. The Steelman Report to the President of 1947 referred to scientists as "an indispensable resource" for all sorts of national "progress" (Steelman 1947, Vol. IV, p. 1). With the outbreak of the Korean War, the rhetoric of "resource" was sharpened: scientists now appeared specifically as "tools of war," "a war commodity" and "a major war asset" that could be "stockpiled" just like "any other essential resource" (Smyth 1951). The argument that fundamental research should be valued and supported because of its contribution to civic, commercial, and military goals was institutionalized in American political economy. And, while the material value attributed to scientific research was, and continues to be, subjected to periodic skepticism and even ridicule, it provided a solid and endurable basis for the institutional security of science.

From the point of view of leaders of the scientific community, enough has never been enough, and lamentations over public "ignorance" of science, over rampant pseudoscience and antiscience, and over dangerous declines in funding never ceased (Gordin 2012). Yet, as Daniel Greenberg and others have noted since the early 1960s, these complaints don't very well describe either the continuing largesse of the State or the durable public esteem in which science has been held in this country through the Cold War and beyond (Greenberg 1967/1999, 2001; Shapin 2007). An occasional blip in funding or admiration is no apocalypse and no amount of hand-wringing could persuade disinterested observers that science was not more securely established than it had ever been.

The point here is not whether science has been well, or even very well, treated since the War; it's that it has been increasingly enfolded into normal political, civic, and commercial institutions. Though many people continue intelligibly to talk of relations between "government and science," "the military and science," and "business and science," in fact it has become difficult to understand the nature of government, of war, or of business without understanding the extent to which they all build science into their quotidian conduct. And the talk of science as a separate and distinct institution—as when we routinely refer to the relations between "science" and "society"—increasingly picks out the decreasing quantum of science that is conducted supposedly "for its own sake" and in institutions that Max Weber assumed were uniquely dedicated to the stewardship of such inquiry.

A way into those structures is through three texts produced a year either side of *Structure*. Two appeared in 1961: the first was President Eisenhower's Farewell Address delivered on January 17, 1961 and the second was a paper titled "Impact of Large-Scale Science on the United States," given as a talk in May 1961, and appearing in *Science* several months later, by the Director of the huge Oak Ridge National Laboratory, Alvin Weinberg. Neither of these texts dealt in any substantial way with scientific practice, scientific method, or with cultural change in science—that is, with the central concerns of *Structure*—but each expressed sentiments that relieve science of the cultural armor which historically protected it from the naturalism central to *Structure*.

Two phrases are about all that's commonly remembered from the two 1961 pieces—from the Farewell Address, the coining of the tag "military industrial complex" and, from Weinberg's text, "Big Science," a phrase which was not in fact wholly original. The pieces emerged, with Eisenhower, from the Heart of Political and Military Power and, with Weinberg, from the Heart of Science. And the remarkable thing is that they were *critical* of aspects of science—Big Science, Weinberg suggested, was "ruining science"; scientists were "spending money instead of thought" (Weinberg 1961, pp. 161–162)—and, more to the point, they were *fearful* of it. Science, they said, had grown great, powerful, politically secure, and politically influential. The post-War institutional successes of Big Science had immeasurably enhanced the resources for doing science while they had endangered its integrity and lured science into political arenas in which it historically had no legitimate place. The seventeenth-century Royal Society had committed itself not to "meddle" with "affairs of Church and State," while Eisenhower warned that its current "meddling" threatened the very nature of the democratic order that so recently Merton and others saw as the internal guarantee of its intellectual authenticity and the external guarantee of its institutional existence.[4]

[4] Eisenhower noted (1961/1972, p. 207) that the organization of science had experienced a "revolution": the traditional individualistic picture of a "solitary inventor, tinkering in his shop" had quite recently been replaced by "task forces" of scientists, lavishly funded by government contracts and orientated not to the search for truth but to securing even more money to pay for even more expensive equipment. The American scientific community was shocked both at this depiction of their institutional circumstances and at the idea that they should be thought so powerful, and Eisenhower's scientific advisor George Kistiakowsky (1961; see also Price 1965, p. 11) had to reassure them that Eisenhower really meant only to criticize military-orientated research.

The shift from science perceived as delicate to science perceived—at least by some influential commentators—as powerful, even too powerful, was rapid (Agar 2008). In the same year that *Structure* was published, the political scientist Don Price at Harvard wrote (1962, p. 1099) of "the plain fact . . . that science has become the major Establishment in the American political system," and a survey of scientists' involvement in nuclear weapons policy by the Princeton political economist Robert Gilpin noted that "The American scientist has become a man of power to perhaps a greater degree than scientists themselves appreciate." In no other nation, "nor in any other historical period, have scientists had an influence in political life comparable to that exercised by American scientists," (Gilpin 1962, p. 299). The reviewer of Gilpin's book in *Science* magazine agreed that, until Hiroshima, "nobody would have dreamed of writing a book on [scientists'] political influence," for they had none (Rabinowitch 1962, p. 974). The points at issue here are not whether these perceptions were either accurate or novel. Criticisms of scientific expertise were *not* unprecedented or global; Eisenhower had quite specifically in mind the activities of such scientist-politicians as Edward Teller and Wernher von Braun (York 1995, p. 147); and what Weinberg meant by Big Science did not describe the institutional environment in which all, or even most, American scientists did their work. Yet these criticisms were targeted at the commanding heights—the most visible sectors—of post-War science and they were articulated from within the corridors of power. Indeed, the most pertinent thing about these views is that they were credible, that they were sayable at all.

The third text, appearing the year after *Structure*, is the now neglected *Little Science, Big Science* by the sociologist of science Derek de Solla Price (1963/1986). Price, like Kuhn, offered not just a theory of science but a wide-focus view of its mode of historical change. As in *Structure*, this was a theory of science wholly disengaged from celebration or justification. Differences between Price's and Kuhn's enterprise are obvious: science for Price was a unity, while for Kuhn it was an unruly collection of practices each regulated by its own paradigm; Price treated science as a black-box, sucking in quantifiable inputs (scientific practitioners, financial resources, instruments) and generating quantifiable outputs (publications, discoveries, more scientists), while for Kuhn science was, again, an assemblage of conceptual and instrumental projects. Science for Price was no special thing, standing outside of history: Price aimed at, and thought he had achieved, a science of science, establishing that scientific growth could be understood as a natural phenomenon, displaying a "common natural law of growth." All elements of science grew exponentially, but there were others things in society that grew in similar ways. If the doubling period for scientific outputs was fifteen or twenty years, about the same period obtained for such non-scientific things as the Gross National Product and the increase in college entrants per thousand of population. In that sense, science *was* progressive but not uniquely so. Even the sense of remarkable acceleration in scientific growth since the War was normalized in Price's account: in fact, science had *always* grown at the rate seen in the past generation; it was *always* modern, *always* seeming to stand outside of history. The only thing that one might identify as historically novel about present circumstances was that this long-standing rate of growth was about to reach "saturation": you could not have more scientists than there were people, more funds for science than the GNP, and that inflection point in the logistic curve was now visible just over the horizon. Yet, in this academic idiom so different from Kuhn's, Price's enterprise also naturalized and normalized science, and in that respect it was also a sign of its times.

The institutional, economic, and political circumstances of Big Science in the Cold War decades formed the conditions of possibility for *Structure*'s naturalism, but this is not the same thing as saying that naturalism about science was normal in that setting or that justificatory and celebratory sensibilities did not continue to flourish. Academic disciplines do respond to their contexts, but they usually do so in mediated ways, shaped by long-established evaluative traditions, and maybe Kuhn reflected Cold War conditions of complacency about science so well just because he was, in the best sense of the word, a great amateur, not formally trained in, and not securely belonging to, *any* of the academic disciplines concerned with talking about the nature of science.

Structure's naturalism, in the event, was precarious and unstable, and one mark of that precariousness appeared in subsequent work by Kuhn himself. After *Structure*, and especially after the hostile 1965 London conference whose proceedings were published as *Criticism and the Growth of Knowledge*, Kuhn (1970) was cautious about repeating the naturalistic sentiments quoted at the beginning of this piece. He defended *Structure*, of course, but he devoted much energy to specifying just how those naturalistic sentiments should *not* be understood. "'But I didn't say that! But I didn't say that! But I didn't say that!'" Kuhn found himself repeatedly insisting, especially in response to irritating misreadings by student radicals who saw the paradigm concept as evidence of "oppression," but more subtly with respect to academics made anxious by the naturalistic sentiments of *Structure* (Kuhn 2000, p. 308). The last chapter of *Structure*, the 1969 "Postscript" to the second edition, and subsequent essays, all testify to Kuhn's anxieties. There must, he thought, be ways of talking legitimately about scientific progress, about scientific truth, about the moral and procedural specialness of scientific communities, and, of course, there must be a way to produce a historically robust theory of science while avoiding odious relativism. He knew that *Structure* had exploded the usual supports for ideas of scientific progress, rationality, and realism, so new ones should be found.

Late in his life, Kuhn observed that "I haven't produced any children." He greatly admired his students John Heilbron and Paul Forman, but said that both had "turned entirely away from" the sort of history of science that he did, and that only Jed Buchwald, an under-graduate, not a graduate, student of Kuhn, did the close analysis of scientific ideas with which Kuhn identified his own historical work (Kuhn 2000, pp. 304, 319). But Kuhn *did* have intellectual offspring; and his reaction to those children is further evidence of his reflective ambivalence towards the naturalism of *Structure*.

The scholars who not only found Kuhn's naturalism congenial but who enthusiastically incorporated aspects of it into substantive sociological and historical work were, of course, my former colleagues at the Edinburgh Science Studies Unit— Barry Barnes and David Bloor— and associated sociologists in England, including Michael Mulkay, Harry Collins, and Trevor Pinch. Bloor (1976/1991) understood the "Strong Programme" in the sociology of knowledge as a form of Kuhnian naturalism and Barnes's book *T. S. Kuhn and Social Science* applauded *Structure* as "one of the few fundamental contributions to the sociology of knowledge" (Barnes 1982, p. x). To my knowledge, Kuhn never commented on the substance of any of this work, but his overall assessment is well known: addressing Harvard's Department of the History of Science in 1991, he announced that all of it was "deconstruction gone mad," a judgment which soon went viral among the anti-relativist warriors in the science wars of the 1990s (Kuhn 2000, p. 110). The point is not whether Kuhn disowned his intellectual progeny for good reasons—in my view, his account of this work was unfortunately quite wrong—rather, it's one index among many of how fragile naturalism about science was and continues to be.

That's because the institutional and cultural normalization of science that was the condition of possibility for *Structure*'s naturalism was never complete, not in the culture as a whole and only partially in the academic disciplines concerned with the nature of science and its history. The science wars were one sign of this patchy normalization; the fetishization of Scientific Method in the contemporary human sciences is another. Here again, the history of science is much more than a topic of inquiry for the academic discipline of the same name. For instance, the scientific naturalists of the Victorian era thought that the march of progress would inevitably deliver a secularized culture, science triumphant over religion. They were wrong about the religion bit, but they could not visualize the institutional and civic security of science a hundred years on.

What about the stories historians of science tell themselves about their own field? In recent times, we have become very good at debunking teleologically progressivist narratives about science, and, in that debunking, Kuhn has been a hero. (After all, that's how *Structure* begins, with a promise to deliver history from the myth-tellers.) But historians have not been keen to see themselves and their work as historical objects. Rejecting simple-minded stories about *scientific* progress, we tend to take for granted that the *historical* stories we now tell about science are so obviously better than they used to be, and we lack curiosity about the circumstances that have made those stories possible. Kuhn's *Structure* was a moment in modern naturalism, not a rung on the ladder of inevitable historical progress. Its conditions of possibility include the institutional state of science in the post-War decades; its conditions of fragility include the only partly normalized institutional and cultural state of science today.

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