



Pure science and the problem of progress



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ABSTRACT

How should we understand scientific progress? Kuhn famously discussed science as its own internally driven venture, structured by paradigms. He also famously had a problem describing progress in science, as problem-solving ability failed to provide a clear rubric across paradigm change—paradigm changes tossed out problems as well as solving them. I argue here that much of Kuhn's inability to articulate a clear view of scientific progress stems from his focus on pure science and a neglect of applied science. I trace the history of the distinction between pure and applied science, showing how the distinction came about, the rhetorical uses to which the distinction has been put, and how pure science came to be both more valued by scientists and philosophers. I argue that the distinction between pure and applied science does not stand up to philosophical scrutiny, and that once we relinquish it, we can provide Kuhn with a clear sense of scientific progress. It is not one, though, that will ultimately prove acceptable. For that, societal evaluations of scientific work are needed.

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1. Kuhn and the problem of progress

Kuhn's vision of science, as articulated in *The Structure of Scientific Revolutions*, has been an influential one. When his book came out over 50 years ago, as an installment in the Encyclopedia of Unified Science (an Encyclopedia now largely moribund except for Kuhn's work), it gained a wide audience with its eloquent account of how science works. Its account was, ironically, anything but a picture of unified science, at least at the global level. From a historical perspective, Kuhn painted a picture of radical revolutions between dominant paradigms, with the overthrow of a paradigm instigating complete disarray in the ordered world of the practicing scientist. Methods thought to be reliable suddenly needed to be questioned. Fixed categories fractured. Problems dissolved or emerged with vivid force. What a scientist was to do at these times of crisis was suddenly unclear. Kuhn quoted Pauli, describing the state of things just before the development of quantum mechanics, to make the sense of conceptual chaos palpable:

"At the moment physics is again terribly confused. In any case, it is too difficult for me, and I wish I had been a movie comedian

or something of the sort and had never heard of physics."
(quoted in Kuhn, 1962, p. 84)

When a revolution occurs, when one paradigm displaces another, "the scientist works in a different world." (ibid., p. 121) Unified science this was not.

But Kuhn's work was not just about radical change in the broad sweep of history. He was also concerned to articulate what science was "normally" like. Here, at the local level, paradigms ruled science, and these paradigms gave a field of science its coherence, its purpose. By defining the ontologies, methodologies, problems, and goals within science, Kuhn's paradigms provided a clear rubric with which to measure progress, at least within a particular paradigm. Yet Kuhn was well aware that our general sense of progress in science is not progress confined within a particular paradigm. Indeed, echoing historian George Sarton, he wrote "that we tend to see as science any field in which progress is marked." (Kuhn, 1962, p. 162). In other words, our expectations of science making progress is so entrenched, we cannot but see a field as being scientific if it progresses. Our expectation of progress is so deep that science is the epitome of intellectual endeavors in which we

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feel progress has been made, and not just across paradigms, but across centuries. How, given the radical discontinuity across paradigms that Kuhn invokes, can we have such a sense of progress in science?

Kuhn hints at a sense of progress throughout the book. When describing how normal science solves well defined problems, Kuhn notes that “at least part of that achievement always proves to be permanent.” (Kuhn, 1962, p. 25). When looking at discoveries that initiated crises in science, he writes that “after the discovery had been assimilated, scientists were able to account for a wider range of natural phenomena or to account with greater precision for some of those previously known.” (ibid., p. 66) Or perhaps most definitively, Kuhn wrote that “the nature of [scientific] communities provides a virtual guarantee that both the list of problems solved by science and the precision of individual problem-solutions will grow and grow.” (ibid., p. 170).

But none of these hints can provide a robust account of progress that is not undermined by the radical nature of paradigm change, of scientific revolutions. Kuhn described paradigm shifts as revolutionary, as presenting scientists a “choice between incompatible modes of community life.” (Kuhn, 1962, p. 94). Paradigm change involves, for Kuhn, changes in the world view of the scientist such that nothing across paradigms is guaranteed to be stable. After a paradigm change, “the scientists works in a different world.” (ibid., p. 121) Even something as basic as “data are not unequivocally stable.” (ibid.) What part proves permanent, if both ontologies and methods shift? If the world is a different world with new problems and puzzles, what can we say has grown across a paradigm change? Kuhn explicitly acknowledges that sometimes, problems are merely set aside as irrelevant or ill-formed in the course of a revolution. For example, the problem of why metals have their defining characteristics of malleability and shine is simply set aside as an important problem following Lavoisier’s chemical revolution in the 18th century. So although he gestures at problem-solving ability in his chapter on progress as the key to progress across paradigms, he cannot convincingly endorse this view, as some problems get tossed aside in a paradigm change. (ibid., pp. 169–170) Kuhn has no clear answer to this conceptual problem. Precisely because of the radical nature of change across paradigms, because scientists have to give up on some aspects of the old paradigm in order to embrace the new, any clear rubric for measuring change across paradigms is elusive for Kuhn.

Philosophers of science have been generally horrified at this aspect of Kuhn’s work ever since. The cumulative model, of an increasing collection of “facts,” which Kuhn so devastated, has not been returned to. In its place, philosophers, particularly of the realist stripe, have wanted to talk about science “getting closer to the truth” or as increasingly “approximately true,” a view of progress Kuhn explicitly rejected. (ibid., p. 170) Since 1970, a cadre of logicians have attempted to explicate precisely what we might mean by approximate truth, or verisimilitude, and how we could say we are moving towards truth, without knowing where truth is.¹ While these projects have been technically deft, they require presumption of a particular language within which claims can be compared and beliefs revised piecemeal as new evidence arises. Science is thought to progress as it gains more verisimilitude. Assessments of verisimilitude depend on the evidence that supports a theory. Thus evidence is used to both support the theory and to support claims that it is close to the truth. Such a view is not without its critics.² More relevant for my purposes, this account of scientific theory change devolves back to precisely the picture Kuhn was challenging in his book—that evidence accumulation leading to belief revision

gets one closer to the truth. How technical accounts of verisimilitude, which depend on a fixed language context, could grapple with genuine conceptual, linguistic, and paradigmatic change is unclear. Verisimilitude will not provide an account of progress applicable to Kuhnian revolutions.

Rather than pursue these technical arguments, I want to try a different tack, to try to get at the underlying sense of progress that seems undeniable for science. This sense, that across the broad sweep of history, that across deep ontological and conceptual change, science has been getting better at something, undergirds much public appraisal of science, even as the public might contest particular scientific claims. I think that this problem of characterizing the progress of science arises for Kuhn, and indeed for philosophers of science generally, primarily because Kuhn (and the current philosophical community) is focused on pure science, quite divorced from applied science. It is an interest in theory, in the theoretical development of science, and theory alone, that generates the puzzle of progress. As such, it is somewhat an artificial problem. If we relinquish the idea that science is only or primarily about theory, the problem of progress disappears. If instead we see science as both a theoretical and a practical activity, progress becomes easier to track and assess.

First, we need to understand how philosophical accounts of science came to see science as primarily about theory.³ I will give a historical account reaching back into the 19th century that shows how this picture of science, that the main focus of science is the development of theory, that real science is pure science, became such a dominant view by the 1950s. It was so dominant that when Kuhn was developing his own ideas about science, he could not help but adopt it. I will argue both that it would have been difficult for Kuhn not to have this understanding of science and that this picture is the reason why Kuhn cannot quite solve the problem of progress satisfactorily.

2. The emergence of science, pure and applied

Rich histories have been written on the emergence of science and scientists from the folds of natural philosophy in the 19th century (e.g., Snyder, 2011; Phillips, 2012). A confluence of factors, including the rise of the German university system and changes in the curriculum of universities across the world, the development of new scientific societies and journals, and new government support and recognition of the importance of science (although never enough for the scientists), led to increasing professional status for scientists. In the education system, as science needed its own system of required courses, laboratory space, and training in specialized mathematics (especially the calculus), the generally learned man (and we are unfortunately talking nearly universally about men) became an increasingly elusive goal. Scientists were specialists, and embraced the distinct nature of their training.

But pure science did not emerge immediately with science. When William Whewell coined the term “scientist” in 1833, it was in response to a challenge from Samuel Taylor Coleridge that men who worked in these areas no longer deserved the name “philosopher.” (Would that the term philosopher was still held in such high esteem.) But for Whewell and his like-minded colleagues, described in Laura Snyder’s *The Breakfast Club*, science was not an enterprise solely for the discovery of new knowledge for the edification of one’s soul. Science was an eminently practical enterprise as well. Whewell pursued a better understanding of the tides not just to advance projects in physics, but to save lives

¹ For a recent overview of this approach, see Niiniluoto (2011).

² E.g., Bird (2007), Piscopo & Birattari (2010).

³ Indeed, in verisimilitude accounts, it is beliefs regarding theory that get revised in the face of new evidence. Theory is the primary focus of our assessment of change.

lost from mistimed boat landings. John Herschel's efforts to unpack the mysteries of terrestrial magnetism similarly was aimed both a theoretical development and a practical application for improved navigation. Charles Babbage wanted to build his calculating machines to make better tables—tables which were crucial for real decisions. Errors in them could cost human life (See Snyder, 2011). There were real practical problems to be solved, and this was both why the men pursued these projects and the basis for them seeking public funds to support their work. There was no distinction for them between pure and applied science. But, by the middle of the 19th century, as the sciences grew and became more specialized, as the gentleman scientist gave way to the professional scientist, the issue of pure vs. applied science took shape.

2.1. Origins in the 19th century

The term “applied science” first appeared in the English language, according to historian Robert Bud, in 1817, in Coleridge's *Discourse on Method*. It did not mean what we commonly mean by applied science today. Rather, it drew its meaning from a Kantian context, where pure science was *a priori* knowledge and applied science was *a posteriori* (Bud, 2012, pp. 538–539). Thus any knowledge with empirically-based content would be applied science—i.e. everything we would call science today.

It was not until the Great Exhibition of 1851 that applied science came to mean something like what we mean today, that of knowledge geared towards practical uses and outcomes (Bud, 2012, p. 543). But it was still not conceptualized to be dependent on pure science. Indeed, it was conceived as an independent enterprise from more theoretical pursuits, and as such, could serve as “a potent rallying cry” in the 1870s and 1880s for the founding of educational institutions in Britain dedicated to technical education (Bud, 2012, p. 545). Applied science was seen as having a life of its own, sometimes described as a “union of science and art” (Thurston, as quoted in Kline, 1995, p. 202; Lucier, 2012, p. 534) that was autonomous from “science for its own sake.” It was this conceptualization of autonomous *applied* science that “pleas for pure science,” on both sides of the Atlantic, would react against.

The pleas for pure science began by the 1870s, but reached their full fruition in the 1880s. They were first and foremost pleas for more state patronage of academic science or more external financial support for the pursuit of pure science. Applied science could depend on the fruits of its own labor (or the support of industry), but this left the increasingly professionalized professoriate class worried. Part of the plea for pure science was a plea to be able to teach science (with the appropriate needed laboratory facilities) for its own sake, for neither the sake of its eventual application nor for the supposed moral edification it provided, separate from a curriculum in either the classics or theology, but also not beholden to short term utility (Gooday, 2012; see also Daniels, 1967).

First efforts were made by chemist Alexander Williamson in Britain, whose 1870 “Plea for Pure Science” argued that “pure science [is] an essential element of national greatness and progress”, and thus that State support of pure science was also essential (quoted in Gooday, 2012, p. 548). The charge was taken up by both T. H. Huxley (in the UK) and Henry Rowland (in the US) in the 1880s. Huxley's 1880 essay, “Science and Culture,” argued for a new college curriculum, with science taught “as a coherent institutionalized body of knowledge, uncompromised by a concern with utility.” (ibid., p. 550) Pure science was unconcerned with practical applications, but applied science, Huxley argued, could not exist without pure science. As Huxley famously wrote:

“I often wish that this phrase, ‘applied science,’ had never been invented. For it suggests that there is a sort of scientific knowledge of direct practical use, which can be studied apart from another sort of scientific knowledge, which is of no practical utility, and which is termed “pure science.” But there is no more complete fallacy than this. What people call applied science is nothing but the application of pure science to particular classes of problems.” (Huxley, 1880, as quoted in Kline, 1995, p. 194)

Here we have a clear articulation, indeed perhaps the invention, of the so-called linear model. Pure science comes first, then the application of that knowledge is applied science, and it is that which produces utility. There is no applied science without pure science prior. But, at the same time, one cannot expect immediate utility from pure science. It is not pure science's job (nor pure scientists' job) to produce things of utility. That comes later, through application, which is often done by someone else, usually someone of lesser talent.

Henry Rowland's 1883 “A Plea for Pure Science” was to sound similar notes, although with a distinctly American flavor.⁴ In his address (at the American Association for the Advancement of Science), Rowland rails against the public misperception of the true nature of science, and the accompanying costs of such misperception. Because of the palpable success of “telegraphs, electric lights, and such conveniences” Rowland notes that “it is not an uncommon thing, particularly in American newspapers, to have *applications* of science confounded with pure science.” (Rowland, 1883, p. 242). Rowland bares his contempt for this conflation as he disparages the “applied scientist,” describing him as

“some obscure American who steals the ideas of some great mind of the past, and enriches himself by the application of the same to domestic uses, is often lauded above the great originator of the idea, who might have worked out hundreds of such applications, had his mind possessed the necessary element of vulgarity.” (Rowland, 1883, p. 242)

For Rowland, this is a national embarrassment, as he sees in American science no great contributions to pure physics as of yet, but instead only mere applications of the work of others. The linear model is on full display here, as he asserts: “to have the applications of science, the science itself must exist.” (ibid.) And Americans had not been contributing much to the science itself.

For Rowland, the challenge posed by the failure of American science was twofold: (1) young aspiring scientists needed to be steered away from the siren call of monetary gain to be had in the applied sciences, which meant working against the predominant materialistic culture, and (2) young scientists needed to be inculcated in the proper understanding of what doing science is, namely “the highest occupation of mankind.” (Rowland, 1883, p. 243). For Rowland there is literally no more laudatory calling than the study of nature for her own sake, and he lambastes the “professors degrading their chair by the pursuit of applied science instead of pure science.” (ibid.) Rowland spends much of the essay proposing reforms to the American education system to generate the change he seeks, which, ironically, consist mostly in more money—larger endowments for universities and colleges to provide appropriate facilities and reduce teaching loads for professors, better financial incentives for doing pure research, more money to hire more research assistants needed as experiments became more complex. Yet the ultimate justifications for this investment are not found *solely* in the unalloyed glory of a pure understanding of nature. Instead, Rowland describes pure science, and its benefits, like this:

⁴ David Hounshell's account of the motivations for Rowland's speech include a rather interesting friendship, and the falling out, between Thomas Edison and George Barker, a dispute that subsequently embroiled Rowland. Hounshell argues that these personal ties heightened the rhetoric around the pure science plea (Hounshell, 1980).

"For pure science is the pioneer who must not hover about cities and civilized countries, but must strike into unknown forests and climb the hitherto inaccessible mountains which lead to and command a view of the promised land—the land which science promises us in the future, which shall not only flow with milk and honey, but shall give us a better and more glorious idea of the wonderful universe." (ibid., p. 248)

Pure science promises *both* the glory of understanding *and* the benefits of improved lives, and it is in light of this promise that Rowland asserts "that we [the pure scientists] constitute the most important element in human progress." (ibid., p. 248) This intertwining of pure science with human (or societal) progress was to be increasingly eroded in the 20th century. More on this below.

The rhetorical invention of pure science, and the claim of the essential ontological dependence of applied science on pure science, was reinforced by sweeping historical claims. In order to cement the dependence relation, and its accompanying hierarchy of valuation, with pure science inherently more valuable than applied, it became commonplace to claim that every technological advance in the history of science had really depended on pure scientific work completed prior. This was a view that had developed in the decades leading up to Rowland's 1883 plea. For example, astronomer Benjamin Apthorp Gould wrote in 1868:

"It would be throwing words away were I to undertake to prove, what you all know already, that scarcely one of all the great advances in the material welfare of humanity would have been made but for the scientist in his closet, whose experiments, researches, and generalizations, incited by the love of nature and the aspiration to fathom her laws, have afforded the knowledge which the inventor's fertility of device has made subservient to human welfare." (quoted in Lucier, 2012, p. 532)

As historian Ann Johnson has noted, the understanding of the history of science laid bare in this remark, that pure knowledge always comes first, has distorted the enterprise of the history of science since its inception as a distinct discipline in the first half of the 20th century (Johnson, 2008). It has also distorted the language around science policy and indeed the philosophy of science, as we shall see.

Despite the eventual hegemony of the pure science ideal and the linear model, not everyone fell into line immediately. Challenges to the linear model, to the fundamental dependence of applied science on pure science, continued through the end of the 19th century. Engineers, as they themselves professionalized, initially resisted being seen as those who merely apply what pure scientists had discovered. Engineers such as Robert Thurston and Charles Steinmetz argued that the relationship between pure and applied science was clearly more complex, with applied science being more than a simple application of pure science, but often its own autonomous investigation of empirical regularities, one that used scientific methods more than established scientific theories (Kline, 1995, pp. 201–203). Such arguments helped to bolster engineers' claims to professional status distinct from craftsmen—engineering was more than rules of thumb.

Other critics wondered whether the pure vs. applied distinction made any sense. While Rowland never wavered in his commitment to the distinction and hierarchy of the linear model, William Anthony pointed out that in Rowland's own practices, he failed to live up to his ideal, taking out patents and working as an industrial consultant in addition to holding a professorial chair at Johns Hopkins (Kline, 1995, p. 200). Indeed, Anthony noted that this was common practice and that other eminent scientists, such as Sir William Thomson, took out patents on parts of their work. Anthony questioned why the motive for doing scientific work should be relevant to evaluating the quality of the contribution (Lucier, 2012, p. 533).

In a similar vein, Alexander Graham Bell protested that the best work is done when the researcher and the applier are the same person:

"When the investigator becomes himself the utilizer; when the same mind that made the discovery contrives also the machine by which it is applied to useful purposes—the combined achievement must be ranked as superior to either of its separate results." (quoted in ibid.)

Others protested that there was nothing laudable in the pure science ideal, that the seeking of knowledge for knowledge's sake was self-indulgent and missed the point about what knowledge was ultimately for. Thus, physician John Shaw Billings wrote in 1886 that "it is one thing to seek one's own pleasure and quite another to pride one's self upon doing so," and thus that the pure scientists' claim that they sought knowledge purely for its own sake was nothing to crow about (quoted in Lucier, 2012, p. 534). Billings was not alone in his sentiment that the pleas for pure science were self-serving, amounting to license for some to do what they want, using other people's money, without being held accountable.

But by the early 20th century, such complexity began to be lost, and opposition to Huxley's and Rowland's linear model dissipated. As scientists rose in status culturally, engineers increasingly were willing to ride on their coattails rhetorically. Thus, the president of the American Institute of Electrical Engineers, Gano Dunn, wrote in 1912:

"engineering is not science, for in science there is no place for the conception of utility. Engineering is Science's handmaid following after her in honor and affection, but doing the practical chores of life." (quoted in Kline, 1995, pp. 203–204)

With few engineers engaged in research, and scientists so ascendant, fully embracing the linear model had become both reasonable and strategic for engineers. Without a clear opposition, the ideal of pure science became a standard cultural presupposition, reflected in many dominant discourses in the 20th century. Nevertheless, it would be challenged again, both from Dewey's pragmatism and from Marxism.

2.2. Into the 20th century

The predominance of the linear model and the distinction between pure and applied science can be seen in contexts throughout the 20th century. For example, in World War I, when concerns of utility became paramount, the pure science ideal was still defended. In the U.S., as the Navy brought on Thomas Edison and a team of engineers to sort through inventions submitted by the public and to tackle submarine detection problems, George Ellery Hale was alarmed at the lack of engagement between academic scientific research, or pure science as he saw it, and war efforts. The rise of the industrial scientist, who sometimes did pure research, but outside of an academic setting, further complicated the picture. Hale created the National Research Council, a more active arm of the National Academy of Sciences, in order to "see a closer connection established between pure science and its applications" and in order to demonstrate the true value of pure science to all (quoted in Kline, 1995, p. 205). The engineers and the pure scientists competed to see who could crack the problem of usable sonar submarine detection first; the race was a draw (Kevles, 1971, pp. 117–123). But for Hale the crucial point was that the value of pure science, as opposed to the undisciplined dabbling of mere inventors and engineers, needed to be demonstrated.

After the war, there was some effort to reinforce the distinction and to keep pure science in its vaulted position. But not much effort was needed. Huxley and Rowland had been largely successful

in their efforts to alter our understanding of science, and to enshrine the pure science ideal. The growing ranks of industrial scientists began to proclaim the value of pure science in their efforts to be seen as more than technicians. The linear model and the pure science ideal seemed to give them more stature, as they were the ones who understood and utilized pure science, just as engineers had used it to gain ascendancy over craftsmen a generation earlier (Kline, 1995).

2.3. *Philosophers engage: Dewey vs. Russell*

By the 1920s, philosophers began to depend upon or dispute the pure vs. applied science distinction as part of their own debates over the direction of philosophy. While these debates might seem far afield from the conceptualization of science at the time, in fact they provide a fortuitous window on what people thought about science. This is because many philosophers, including both Dewey and Russell discussed here, wanted to make philosophy more scientific. Long frustrated with the lack of apparent progress and the apparently endless metaphysical debates that plagued the history of philosophy, philosophers began to look to science as a better model for doing philosophy. Making philosophy scientific was thought to ensure that philosophy would indeed progress, as science assuredly did. The scientific philosophy movement was borne, and philosophers were called to emulate the best aspects of science.

But what understanding of science were philosophers to emulate? Here we can see what philosophers thought about the nature of science. Bertrand Russell argued that it was pure science which should serve as the guiding model for philosophy:

“But if philosophy is to become scientific... it is necessary first and foremost that philosophers should acquire the disinterested intellectual curiosity which characterizes the genuine man of science.” (Russell, 1914/1926, p. 27)

Rather than attempting to provide meaning to human life or offer a “solution to the problem of human destiny,” Russell argued that philosophy should, “when... purified from all practical taint,... help us to understand the general aspects of the world and the logical analysis of familiar but complex things.” (ibid., p. 28) Pure philosophy, like pure science, should aim for the satisfaction of understanding, and nothing else.

It might seem odd, given Russell's social and political engagement in his own writings, that he espoused such a view. But Rose-Mary Sargent has argued that, to the contrary, it was because of his social and political views that Russell took this line (Sargent, 2011). When Russell looked at the American context, he saw much of what Rowland had bemoaned 40 years earlier—a utilitarian emphasis that kept Americans from making significant contributions to pure science. Russell worried that such utilitarian leanings would “kill the pure desire for knowledge,” (Russell, 1923, as quoted in Sargent, 2011) and, even worse, lead to more applied knowledge, which he saw not in the idealistic terms of human progress described by Rowland, but rather for more mundane and less benign purposes of increased production, more destructive wars, and trivial amusements. After the horrors of World War I, to which science clearly contributed, claiming that advancing science would necessarily lead to general betterment of humanity seemed naive. It was in pure science that men like Russell could take comfort, as it aimed solely at pure understanding and not at these deleterious ends.

John Dewey, who also sought to make philosophy more scientific, disagreed with this characterization of science. Rather than shielding pure science from responsibility for the problematic impacts of applied science, Dewey argued instead that the artificial

distinction between pure and applied science was in fact the reason why so many harmful outcomes were being seen. He saw the undesirable “materialism and dominance of commercialism of modern life” as due not to “undue devotion to physical science,” but rather to the artificial divisions such as the “separation between pure and applied science.” (Dewey, 1927/1954, pp. 173–174). The separation brings with it “honor of what is ‘pure’ and contempt for what is ‘applied,’” which leads to “a science which is remote and technical, communicable only to specialists, and a conduct of human affairs which is haphazard, biased, unfair.” (ibid., p. 174) Because pure science is pursued without regard to the import for society, it can not effectively inform the needed discussions over public affairs, leading to poor social decisions. Dewey was emphatic that this way to pursue science is problematic for both society and science: “Science is converted into knowledge in its honorable and emphatic sense *only* in application. Otherwise it is truncated, blind, distorted.” (ibid., emphasis his) The implications for practicing scientists are stark: “The glorification of ‘pure’ science... is a rationalization of an escape; it marks a construction of an asylum of refuge, a shirking of responsibility.” (ibid., p. 175) Scientists should not, could not, hide behind claims that they were only doing pure science and so the impact of science on society was not part of their burden. Indeed, it was this kind of thinking that had led to there being such harmful impacts of science as were found in World War I.

For Dewey, the aim of science “is to discover those properties and relations of things in virtue of which they are capable of being used as instrumentalities.” (Dewey, 1929, p. v). Scientific knowledge is demonstrated to be scientific knowledge by the ability to help us grapple with the world, where such grappling is eminently practical. Thus, there can be no clear conceptual distinction between pure and applied science—both for epistemic reasons and because such a distinction is harmful to the society in which science functions. Scientists had to face some responsibility for their work, even if done under the guise of pure science. Dewey was arguing, however, against powerful cultural currents which he was unable to alter, particularly as the pure vs. applied distinction took on meaning within the fight over Soviet style planning and the fate of science in the West.

2.4. *The shadow of the Soviet experiment: Planning vs. pure science*

Russell ultimately won the debate with Dewey on both the proper nature of both science and philosophy. As George Reisch has emphasized, philosophers of science retreated to the “icy slopes of logic,” focusing on pure science and eschewing any practical aspects of science (Reisch, 2005). To see how Russell defeated Dewey so handily, how the ideals of pure science (and of pure analytic philosophy) became ascendant by mid-century, we need to examine the debates around science planning and the freedom of science in the 1940s. There we see a remarkable cultural development, as the pure vs. applied distinction becomes a bulwark against planning for scientific research and against totalitarianism, and any erosion or questioning of the distinction is seen as an alignment with Soviet-style Marxism.

Many scientists in the 1930s were interested in the Soviet experiment and sympathetic to Marxist ideas about science. The general Marxist view of science was that societal, economic, and practical needs had driven much of the history of science and continued to structure much of which science was done (Nye, 2011, pp. 191–192). No principled distinction between pure and applied science could be made, as all science should serve the needs of the society in which it was pursued. The pure vs. applied distinction was an illusion created by scientists’ misapprehension of their proper social function; that all science, when properly done, would

serve the social good. (ibid., p. 194) Central planning was an acceptable way to ensure this.

Many leftist scientists were sympathetic to these views, and they posed a serious challenge to the pure science ideal. In the 1930s, such leftist scientists became a powerful force in British science, and the publication of J. D. Bernal's 1939 book, *The Social Function of Science*, provided a clear articulation of the movement's views.⁵ Scientists who disagreed with the leftists became increasingly alarmed, and in 1940, a new society, The Society for Freedom in Science (SFS), was formed (Bridgman, 1944; Shils, 1947). Initially a group of 30 scientists, the society grew to over 130 members in the UK by 1944 (Bridgman, 1944, p. 55). Newly emigrated to Britain in 1933 and deeply suspicious of the Soviet experiment, Michael Polanyi was crucial to the formation and activities of the SFS (Nye, 2011, p. 204). A central feature of disagreement between the two factions was the existence of a genuine distinction between pure and applied science. While for the leftists, the distinction had little purchase on scientific practice and seemed more a self-absorbed way for scientists to eschew responsibility for the impact of their work, for the SFS, the distinction was crucial for carving out a space where the scientist could work unencumbered by the demands of society to develop scientific understanding which could then be applied for the benefit of society. Pure science was what could not and should not be planned.

Although Bernal was often thought to be a standard bearer for the leftist movement, he was by no means a pure Marxist, and in fact wrote in moderate terms of "science as pure thought and as power," implying that neither pure nor applied science on its own sufficed (Nye, 2011, p. 215). His policy recommendations included increased funding for science through a British central agency, but he suggested that

"the distribution of funds would be determined by needs both for the internal development of science, according to scientists' own estimates, and the need for particular developments in sciences on account of urgently required applications in the public interest." (Nye, 2011, p. 217)

So Bernal was not calling for the rigid central planning of science by bureaucrats. But the leftist tone of the book kept readers like Polanyi from noticing the nuances. For Polanyi, any blurring of the pure vs. applied distinction was unacceptable both because of its implications for science planning and for its suggestion of scientists' responsibilities to concern themselves with anything beyond theoretical research. Indeed, for Polanyi, any effort to direct the attention of scientists was bound to fail, for "scientific research, which is the growth of the organism, cannot be deflected from its internal necessities by the prospects of useful application." (quoted in Nye, 2011, p. 195).

The activities of the SFS resonated in the U.S. with the proposal of the Kilgore bill in 1942 as a possible model of post-war science funding. American scientists were upset with features of the bill which would have included geographic equity considerations for the distribution of funds and suggestions that science could and should be directed towards issues of societal interest (Kleinman, 1995). When Percy Bridgman critiqued the idea that science could be directed towards such ends in his presidential address to the American Physical Society in 1943 (reprinted in *Science*), he was immediately contacted by the SFS as a promising like-minded thinker (Bridgman, 1943, 1944, p. 57). Bridgman introduced the SFS to the American science community in the pages of *Science* in 1944 (Bridgman, 1944). Although one might think that one could separate conceptually the issues of how much the State should plan

scientific research from whether a pure vs. applied science distinction is conceptually coherent, the two issues were inextricably merged at the time. Indeed Bridgman's essay on the SFS triggered an intense debate in the pages of *Science* over the pure vs. applied distinction.

The first response to Bridgman's essay focused on the Kilgore Bill, and argued that the principles of the SFS were not widely held and that one could oppose the Kilgore Bill without subscribing to them. (Ross, 1944) But immediately thereafter, the focus became the pure vs. applied distinction. Alexander W. Stern wrote in heated terms that the threat to pure science was "a growing danger to intellectual freedom throughout the civilized world." (Stern, 1944, p. 356). He argued that "[t]his danger arises from the totalization and socialization of science which is growing throughout the entire world," the prime example of which was the Soviet Union. (ibid.) For Stern, such socialization would be a catastrophe of the first order, for although it might produce a world free from material want, it would undermine the "intrinsic goodness" of science, where "the pursuit of truth and the passion for understanding give a dignity and nobility to man." (ibid.) If such a value was to be protected, "pure science must remain free, autonomous, and supported for its own sake." (ibid.)

Stern's essay was objected to in subsequent issues of *Science* by those who rejected the strong pure vs. applied distinction central to the SFS position. Industrial physicist John Pearson argued that it was the quality of the investigator that mattered, not whether they were doing "pure" or "applied" science, and that both industry and academia saw a range of talent (Pearson, 1944). Science, for Pearson, was both an intellectual activity requiring integrity and a practical activity. The distinction so central to Polanyi, Bridgman, and Stern was nonsense. Eugene V. D. Robin agreed. Robin argued that the "question of 'pure' science versus 'applied' science is a question which does not have real roots in life," and that the only sense that could be made of the distinction was with respect to the "attitude on the part of the scientist doing the work rather than a basic characteristic of the work itself." (Robin, 1944). Echoing Dewey, Robin noted that in order to be science, it had to engage with the world, and thus was necessarily practical. Robin argued against those who would take an attitude of disengagement with the impact of their work, an "attitude of disregard for practical applicability." (Robin, 1944, p. 520). He disparaged those who would pursue science purely for intellectual satisfaction as doing little more than playing a game. Instead, he thought the scientist should "acknowledge his debt to all past science and human endeavor (without which his work would be impossible) and knowingly contribute his work to the betterment of the present and the promise of the future rather than smugly raise a false cry of 'purity' or 'intellectual satisfaction.'" (ibid.) Finally, chemist Jerome Alexander also wrote to criticize the pure vs. applied distinction, arguing such a distinction has a "savor of priggishness" and that "we should break down fictitious, pedagogical barriers" separating areas of science (Alexander, 1945, p. 37).

Such fighting words got strong responses. Stern re-emphasized his insistence that "science has nothing to do with usefulness." (Stern, 1945, p. 38). He warned readers that they "must be alert and guard against scientific research degenerating into rubber, oil, textile, military research." (ibid.) Again, according to Stern, the threat to Western culture could not be greater. If the distinction were given up, and the ideals of pure science lost, "the age of scientific enlightenment and culture may be succeeded by an age of technology, where comfort replaces culture, and mankind replaces man. Science must be kept free, not because of the

⁵ Shils (1947) also notes J. G. Crowther's *The Social Relations of Science* and Lancelot Hogben's *Science for the Citizen* as being important texts at the time for leftist scientists (Shils, 1947, p. 80).

material comforts and riches it will bring us—that technology can do—but for the very preservation of our civilization.” (ibid.) Socializing science, bending it to society’s ends, might make the masses (mankind) comfortable, but it would destroy the great intellectual endeavor that is science. John R. Baker, an Oxford scientist and member of the SFS, responded in a similar vein. Baker rejected Robin’s analogy with game playing, arguing that scientists pursuing pure science “consider that they serve mankind well in discovering truth.” (Baker, 1945, p. 301). Such truth serves a crucial edifying role, to be compared with “art, music, literature, and philosophy,” which are not valued for fulfilling material wants by give people richer lives. (ibid.) For scientists like Baker, Stern, and Polanyi, the distinction between pure and applied science was central to the preservation of their very civilization.

Moving into the post-war context, these starkly different views on the nature of science would find purchase in debates over science policy. The distinction between pure vs. applied science provided arguments for a particular form of government funding for science, that of funding pure science so that it may be applied by others (those with private funding). The linear model of science, dependent as it was on the pure vs. applied distinction, became the standard picture of scientific research and its impact on society. In 1945, the linear model received its most familiar formulation in Vannevar Bush’s *Science: The Endless Frontier*. Replacing the term pure science with basic science, Bush argued that it was basic science that provided the crucial raw material for applied science, and because the utility of basic science was often too far off to expect adequate private investment, public investment in basic research was essential. The eventual pay off for the public investment would be substantial, Bush argued:

“One of our hopes is that after the war there will be full employment. To reach that goal the full creative and productive energies of the American people must be released. To create more jobs we must make new and better and cheaper products. We want plenty of new, vigorous enterprises. But new products and processes are not born full-grown. They are founded on new principles and new conceptions which in turn result from basic scientific research. Basic scientific research is scientific capital.” (Bush, 1945, p. 6)

It is in order to generate the needed store of scientific capital (currently depleted by the war effort) that public funding of science was needed. This essentially utilitarian argument was a far cry from the view of pure science as culturally essential and humanistically edifying. But given the large amounts of public funding being sought for basic science, far more than was given to music, the arts, literature, and philosophy, Bush needed a different argument than the one mobilized by Stern and Baker. Although it took a while to set up, massive public funding of science, both basic and applied, was indeed initiated, propelled by Bush’s work (Kleinman, 1995).

In the post-WWII debates over science funding, the distinction between pure and applied science, and its essential connection to scientific freedom and the fight against Soviet totalitarianism was repeatedly reaffirmed. In 1947, Edward Shils summarized the main precepts of the SFS as “(1) a clear and fundamental distinction between pure and applied science, and (2) the conviction that knowledge of the laws of Nature is a good in itself.” (Shils, 1947, p. 80). The view that applied science was an autonomous activity of discovery was completely rejected—Shils insisted that “applied scientific work has produced very little insight into the laws of nature.” (ibid., p. 81) Grants to universities for research, on this view, are not to shape the direction of research, but only shape the amount of research. (ibid., p. 82) Scientists, and scientists alone, should decide where such funds are best put to use. (ibid.) Such autonomy had become rhetorically bound up with the

demands of a democratic society and the pure vs. applied science distinction.

It is in this context that a young Thomas Kuhn came to Harvard (where Percy Bridgman spent his career) to pursue his graduate studies. A key influence on Kuhn was James Conant, then president of Harvard. Conant accepted the conceptual distinction between pure and applied science of the time, although he thought they had become inextricably interlinked in practice in the 19th century (Conant, 1950, pp. 196–197). In his descriptions of the relationship between pure and applied science, Conant echoed the proponents of the pure science ideal from the 19th century and defenders of pure science in the 20th century—applied science was an application of principles and ideas discovered by pure scientists rather than an activity of discovery in its own right. A key advance in applied science and the development of technology was the joining of pure science to applied science. This generated theoretically informed applied science, which was far preferable to the trial and error “empiricism” of the tinkerer. (ibid.) But for Conant, the important conceptual work was the work of pure science, and it was in the development of new conceptual schemes, which could then be applied by others, that the true value of science was demonstrated. (ibid., p. 194)

At Harvard, Conant was championing a change in the general education curriculum that would provide the Harvard undergraduate with the crucial understanding of the nature of science so important for democratic civic life (Nye, 2011, p. 235). Conant proposed developing a series of detailed historical case studies to show the actual complexity of good scientific practice and the development of conceptual schemes in science. Kuhn switched from physics to the history of science to help Conant with the effort. What resulted from his efforts was *The Structure of Scientific Revolutions*.

3. Recovering progress

Thus, in Kuhn’s time, the distinction between pure and applied science was enshrined in funding agencies and science policy, considered an important counter to Soviet ideology, and utilized to protect scientists from political pressure or moral responsibility. It would have been difficult for Kuhn to question, let alone dissolve, the distinction. Indeed, we can see him reinforcing it in his characterization of the scientific community, when he lauds “the unparalleled insulation of mature scientific communities from the demands of the laity and of everyday life.” (Kuhn, 1962, p. 164). Science, for Kuhn, is an internally driven enterprise, with no direct thought to the broader society in which it exists. The paradigm served a crucial function in maintaining this form of scientific practice: “A paradigm...can even insulate the community [of scientists] from those socially important problems that are not reducible to the puzzle form.” (ibid., p. 37)

Dewey would surely have rejected such a characterization of the nature of science, but with the demise of pragmatism and the ascendancy of analytic philosophy (modeled on Russell’s version of scientific philosophy, emulating pure science), with the Cold War and McCarthy pressures making toxic any talk of science planning for societal needs and any concomitant blurring of the pure/applied distinction, and the scientific community still smarting from the pangs of responsibility felt after the atom bomb, Kuhn reflected the predominant cultural understanding of science as pure science, proceeding independently from any applications or societal problems. The insulating paradigm reflects the idea of science driven only by its own internal logic. In Kuhn’s theory of science, the centrality of the paradigm is a warning to those who might want to plan science—they would be undermining the organic strength of the scientific community by interfering in the

natural (internal) development of the paradigm that is central to scientific practice (See also Polanyi, 1962).

Yet, the pure vs. applied distinction is both artificial and implausible from the perspective of historical examination. It cannot plausibly be construed as a distinction about location: academic scientists do applied science (especially since the Bayh-Dole Act and other efforts to increase “technology transfer” out of academia) and industrial scientists do pure science. It cannot be construed as being about content: the history of science reveals attempts at application as providing theoretical breakthroughs and theoretical work as providing new modes of application. The linear model has been decisively rejected as descriptively inaccurate. It cannot be construed as being merely about the intention of the scientist, as an effort to shield the scientist from responsibility for the impacts of their work. Even if intention matters for responsibility, in that we are held more responsible for what we intend than what we do not, intention does not bound the scope of a scientists’ (or anyone’s) responsibilities (Douglas, 2003, 2009a). The specter of dual-use science and the controversies over how to manage it display this starkly. We can see in the history of the pure vs. applied distinction lots of strategic reasons for upholding it: to argue for monetary support, to argue for social status, to argue for freedom and autonomy without accountability. But none of these rhetorical uses withstands philosophical scrutiny.

Even recent commentators on the pure vs. applied distinction fail to provide a clear view. Donald Stokes, in a book about showing the difficulties of the distinction in practice, still thought it somehow clear conceptually. He defined it in terms of the intention of the scientist, a traditional approach. He wrote: “On any reasonable view of the goals of basic and applied research, one cannot doubt that these categories of research are conceptually different. The defining quality of basic research is that it seeks to widen the understanding of the phenomena of a scientific field.” (Stokes, 1997, p. 7). Stokes explores in his book a new category, use-inspired basic research (Pasteur’s quadrant), but one can question the very distinction, that basic research is of a different kind than applied research, that the analysis is based on. After all, what endeavors of applied research do not also seek to “widen the understanding” of phenomena? Even if one is applying theory to a particular context, the application is rarely straightforward, and the resulting insight gained deepens and widens understanding, sometimes even transforming the original theory. Applied research is not just an easy application of a theory and does hold the promise of discovery.

Without a sound philosophical reason to keep the pure vs. applied distinction, it behooves us to see how our understanding of science, and of scientific progress, might shift without it. Most obviously, rejecting the distinction can allow us to reclaim the vision of Whewell and of Dewey, of seeing science as embedded in a society which very much needs it. We can also see that Dewey was expressing something important, perhaps even an expression of a deep empiricism (not Conant’s trial and error), in noting that some application, some test, even if in a controlled context, is needed for science to be genuine knowledge. Empirical test requires some application in the real empirical world, and science requires empirical test, whether one is doing observational work or laboratory work or something in between.

Dissolving the pure vs. applied distinction also allows us to focus more on the varied contexts of use to which we put scientific

work. Concerns about contexts of use for evidence are burgeoning in philosophy of science, and rightly so as successful empirical test in one context of use does not ensure successful use in another context (Cartwright, 2012). The details of the context can matter greatly, and being alive to that issue of application can motivate both more careful testing of applications and awareness that discovery can be made in different contexts.

We can also gain a clearer account of scientific progress. With the pure vs. applied distinction removed, scientific progress can be defined in terms of the increased capacity to predict, control, manipulate, and intervene in various contexts.⁶ This is the kind of success that translates well across paradigms, that is rarely lost with theoretical change, and which matters greatly to both scientists and the public. While paradigm change can create losses in understanding or losses in explanatory unification as clear conceptual structures are swept away, what is not lost is the ability to predict phenomena and/or the ability to control aspects of the world. Theories or paradigms may come and go, but the ability to intervene in the world, or at least predict it, has staying power. We can think of explanatory frameworks and understandings lost in paradigm change (e.g. an intuitive grasp of what light is, a sense of place in the universe, a clear grasp of what makes something a species), but we are hard pressed to think of a predictive or manipulative capacity that has been lost.⁷ It is our raw facility with the world around us that increases over time, even across paradigms. What I am suggesting here is that the near universal agreement that science makes progress arises from this admittedly applicable and applied capacity.

Of course, this is not a sense of progress that can be easily quantified. It would be hard to determine whether one intervention or predictive capacity should count more than another, which predictions or interventions should have more weight, or whether particular bits of progress should be further individuated or collapsed. But Kuhn was not struggling to provide a quantitative account of progress; he was struggling to provide even a qualitative account. To give that account, we need to be able to detect the direction of change, but we are not looking for a full-blown vector. This account of progress at least succeeds to that extent.

One might think that I am reducing scientific progress to technological progress, but this would be inaccurate. Technological progress of course counts, as well it should given the tight interrelationships between science and technology, but one need not produce new technologies to produce scientific progress on this account. Because I would count increased predictive capacity as progress, areas of science that tend not to be used to manipulate material artifacts still progress.⁸ Astronomy has gained increased predictive capacity (along with increased technological capacity to see into the cosmos) and thus has progressed. Biology has developed increased predictive capacity (Williams, 1973; Winther, 2009), *pace* those who see biological progress solely in explanatory terms. This view also does not ignore the importance of explanatory efforts. Because explanations are so central to producing new predictions and implications, the traditional sense of the importance of explanation is captured even with this emphasis on the increased ability to predict (and sometimes intervene in) the world (Douglas, 2009b). Progress arises not just from new artifacts, but new understandings that allow us to engage with the world with greater success, to predict its behavior even when we don’t intervene on it.

This is a sense of progress which both scientists and the public can grasp. Indeed, what I am suggesting is that the very thing that

⁶ To be clear, while I think this is a useful rubric for scientific progress, it is not a remotely sufficient account for how one should assess scientific theories. For a start on that, see Douglas (2013). Successful prediction and/or intervention are important to assessing theories, but so too is explanatory unification. I am, however, dubious that the extent or potency of explanatory unification can be compared across the broad sweep of history for which scientific progress seems so obvious.

⁷ The only one I can think of is astrological prediction, and considering that a successful predictive practice seems a stretch.

⁸ Conversely, the challenge to gain a clear sense of progress in social science despite theoretical and practical change can be traced to the lack of sense of progress in our ability to predict and manipulate social behavior. Perhaps we would not want too much progress on this front anyway.

Rowland railed against over 100 years ago, that the public's fascination with the application of science, to enable us to do new things, to engender new capacities (for better or worse, within and without the lab) is the clearest sense we can make of the nature of scientific progress. I suspect that it is this sense of progress on which the general public authority of science ultimately depends.

Thus once we relinquish the pure vs. applied distinction, we can articulate a clear and strictly scientific sense of progress. However, it is not one with which either scientists or the public should rest content. For on this account of progress, any increase in the capacity to intervene, control, or predict the empirical world would count as progress. An increase in the capacity to predict or control the thoughts and feelings of human beings would count as scientific progress. An increased capacity to destroy human subpopulations (through, say, targeted pathogens) would count as scientific progress. Developing new heinous capacities would count as scientific progress. Unlike Rowland, we should have no illusions that greater causal efficacy, greater power of intervention, will in fact always provide a better society. If World War I created doubts about the equivalence of scientific and societal progress, World War II and the atom bomb demolished it. Science, and the powers it unleashes could indeed make society better, or it could end civilization completely.

Most scientists (I venture) would thus balk at such a strictly scientific sense of progress. Most would want to claim instead that scientific progress should not include such destructive or humanity-undermining abilities, even if they come with deeper theoretical and practical understanding of the empirical world and how to manipulate it. But if scientists want to reject the narrow sense of scientific progress articulated above, they will have to accept a more socially, ethically mediated conception of progress, one that takes into account all of science, both pure and applied. To construct a sense of scientific progress that sounds genuinely like progress, with all its positive connotations, we are going to have to embed science even more fully in society. We will need to ask which science will provide us with a better society, and, which science will perhaps undermine it.

This does not collapse scientific progress with societal progress, even if they are more closely tied together. Even if scientific capacities increase in a way judged to be good, there is no guarantee society will effectively deploy them.

Nor is it asking for theories that will make us happy. Rather it is acknowledging the power inherent in scientific knowledge and asking whether, in particular cases, it is indeed knowledge that we think will further human, and humane, ends. Under this broad umbrella, scientific progress must be judged by whether, in fact, the change in capacities is thought to be good. That can include increased understanding (even where we cannot intervene, as in the case of astronomy), assessed in part by an increased ability to predict what we will observe. As the advocates for pure science proclaimed, scientific knowledge is a *prima facie* good. But while granting this, the complex setting of society can justly, fairly, and correctly overturn this *prima facie* good for some cases. With this understanding, we can address the responsibility that comes with the development, indeed the progress, of knowledge.

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