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The accidental rebel: Thomas Kuhn and *The Structure of Scientific Revolutions*

Preface:

This tale is one that has changed in the telling. I should have expected such an outcome, but it didn't hit me until I had reached a certain critical mass of understanding (or confusion). The story of Thomas S. Kuhn and *The Structure of Scientific Revolutions* played out in my mind like the unraveling of a personal murder mystery. The story seemed simple at first, and the players seemed to be of a manageable number. As I unearthed clue after clue, interrogated (figuratively) witness after witness, I slowly began to realize that the so-called "simple" story was one that involved a huge number of players with unique and often conflicting viewpoints, and the interplay of their voices historically--the preeminence of certain voices, and the silencing (both intentional and unintentional) of other voices--has lead to where we are now, and to where *I* am now, writing a paper on Thomas S. Kuhn, wondering how his work, the much-cited *Structure*, has become mostly unquestioned--a veritable black box which has influenced entire fields of study (including my own). *Structure* itself is the primary suspect of this investigation, but there are others, and with this paper, I hope to speak for the victims.

There are numerous ways of reading *Structure*, as Kuhn himself came to realize as his book gained in fame, and not all of them were equally charitable. Within the Philosophy of Science community, especially, Kuhn's work was initially regarded with disdain or dismissed altogether. Despite those early criticisms, however, *Structure* has mostly come to be revered (in academia as well as the public sphere) as being

revolutionary and of massive importance, shifting the intellectual focus away from dead-end approaches to more inclusive and relevant modes of understanding science in relation to society as a whole.

It is important to note, however, that critics of Kuhn, though marginalized, never disappeared completely. A few years back, far from the hallowed halls of Science Studies, in a Philosophy of Science seminar[1] offered within the *biology* department at UC Davis (where I worked), I wrote a short essay defending the ideas of Karl Popper against the critiques of Kuhnian theory. Reading *Structure* more carefully in 2001 and being further exposed to dominant (mainstream) conceptions of what it all meant, I found myself at further odds with the book and with the most common interpretations of it. This paper is meant to clarify my objections to both.

Philosophical arguments never really go away; they're just shuffled around, pushed into dark corners or thrust into the limelight, depending on the circumstances. As I have discovered in my research, critics of Kuhn were more numerous and vocal than I expected. As will be discussed in the main body of this paper, Karl Popper, for example, proved to be a formidable intellectual opponent to Kuhn, and Kuhn had to develop complex arguments against Popper and his allies (*after Structure was published* in 1962). Within the fledgling Social Studies of Science context of the late 70s and early 80s, however, the debate between Kuhn and his critics was mostly ignored as idiosyncratic, outdated, and secondary to the importance of *Structure* itself (which only contained a handful of pages discussing Popper's logical empiricism).

[1] The seminar (Plant Biology 223) was entitled *Science: Revelation, Discovery or Invention?* Kent J. Bradford was the instructor.

In recent years, historical-minded critiques and analysis of Kuhn have once again come to the fore, most notably in the form of Steve Fuller's *Thomas Kuhn: A Philosophical History of Our Times* (2000). Is it a coincidence that I've chosen to write a critique of Kuhn at this point in time, or is it indicative of a certain feeling that is "in the air"? While I certainly cannot claim to be wholly original in my critique of Kuhn--as I have discovered too many critics who argued brilliantly before me--the perspective I will speak from is the one I am most familiar with: that of a natural scientist with a very specific (and ever-increasing) exposure to the world of Science and Technology Studies.

On the Social Construction of Kuhn

The goal of this paper is to address the following questions: What was Thomas S. Kuhn trying to do when he wrote *Structure*? In what context was it written? How did particular individuals or groups initially respond to the book, both positively and negatively, and what were the reasons behind both types of responses?

Thomas Kuhn's "*The Structure of Scientific Revolutions*" is considered one of the most important academic books of the 20th Century. It radically influenced the way in which the lay public and experts alike viewed the practice of science, it spawned new programs of studying science, and its jargon was appropriated by vastly disparate fields--from politics to art to anything else you can imagine.

As I read and discussed *Structure*, and read other people's interpretations and criticisms of it, including Kuhn's own thoughts, it became more difficult to know which single interpretation of that book, if any, was the most satisfactory. The experience clearly illustrated to me that when we read things, we see exactly what we want to see (or have been socialized to see). Or as Thomas Kuhn might say, our learning proceeds

according to our paradigm, our theoretical framework, and if what we read is counter to the paradigm, we tend to blame the faulty evidence and not the paradigm itself. As there are so many different frameworks from which to approach *Structure*, I came across many disagreements as people tried arguing *Structure*'s flaws and merits. Amidst the debates, there were numerous breakdowns in communication, but I think critical understanding *did* emerge from the dialogue, making my efforts to follow the complex arguments and counterarguments worthwhile in the end.

When I read *Structure*, I found it had many interesting and relevant observations, but I was also puzzled at some of its conclusions. Kuhn's descriptions of the persistence of theories and the difficulties of overturning established paradigms resonated well with my personal experiences within science. On the other hand, Kuhn's thoughts on the nature of progress in science were alien to me. Kuhn used an evolutionary metaphor to make his point on this matter. I will discuss this metaphor as one example of Kuhn's method of presenting science—a method that some of his readers have considered overly ambiguous, as we will discuss shortly.

On the evolution of knowledge

One of Kuhn's most provocative ideas expounded upon in *Structure* and other essays is the notion that science does not proceed towards truth. The absolute truth about nature, according to Kuhn, can neither be obtained nor approached by science. This is an unusual viewpoint that almost seems at odds with his discussion of scientific 'progress.' According to Kuhn, such progress occurs as scientific theories become more articulated and better and more matched to nature (i.e. as puzzles are solved). Although this notion of progress seems to indicate that scientists *can* devise more accurate representations of

nature (thereby approaching the truth of it), for the purpose of argument, we will agree with Kuhn in this instance. As a biologist however, I have to question his (apparently flawed) use of an evolutionary metaphor to explain away science's quest for truth. Allow me to explain…

It is interesting that Kuhn brings up the Darwinian mode of evolution at all. The modification of species over time by natural selection is a slow and gradual process. This is directly at odds with Kuhn's portrayal of fundamental theories and ideas changing over time as not being gradual but *revolutionary* in nature. Darwinian gradualism more appropriately symbolizes a slow accumulation of changes (which includes loss as well as gain of characteristics) in knowledge. Given the opportunity (had *Structure* been written later), Kuhn would have been better off equating his model of paradigm shifts to the more modern conception of Punctuated Equilibrium[2] being the significant mode of evolutionary change.

More troubling, perhaps, is Kuhn's characterization of the scientific process as being analogous to the non-purposive process of evolution. Kuhn rightfully points out that evolution, despite common misconceptions, is not a goal-oriented process. There is no "progress" in evolution, only change over time. Species at the end of an evolutionary line are not "better evolved" than their ancestors, nor are they "more advanced"; they are simply different. Kuhn asserts that the scientific process is no different; there is no "progress" in science in terms of approaching the truth of nature, and more modern conceptions of nature are not better or more advanced than any conceptions that came before them. The main problem with this analogy is that science, unlike evolution, *is* a

[2] Gould S.J., and N. Eldredge. 1977: Punctuated equilibria: the tempo and mode of evolution reconsidered. Paleobiology 3, pp. 115-151.

purposive and goal-oriented process (more akin to *artificial selection*, as described by Darwin, than natural selection). As opposed to acting without preexisting motivations, scientists *do* seek the truth. [Self-proclaimed 'scientists' who seek to fabricate information for their own benefit are referred to as confidence men, not true scientists.] Having better and more advanced conceptions of nature (that approach the truth of nature) is indeed the goal of scientists. Whether or not science has sufficient epistemological means to achieve such a goal is another topic of debate altogether (discussed in the realm of philosophy, from Hume to Kant to Popper), and certainly not one that Kuhn resolves by claiming that science is non-progressive like evolution. Kuhn's brief treatment of science making mistakes and regressing (rather than progressing) seems counterintuitive to the notion (that is taught to students of biology, even) that we can learn from our errors, and that such errors increase, not decrease, our knowledge in the long run.

As a description of science and scientists, therefore, Kuhn's evolutionary metaphor leaves much to be desired. In light of the many good (descriptive) points he makes about science in *Structure*, this part of the book seems decidedly out of place. One might attribute its inclusion as being the result of a mental slip, but I would make the assertion that Kuhn had a particular motive that we need to examine. If we view the metaphor as a *prescriptive measure*, and not purely descriptive, we start to get a handle on what Kuhn was trying to do with *Structure*.

Kuhn's ambiguous agenda

Perhaps the biggest problem I had when reading *Structure* is the one Kuhn himself mentions in the postscript, where he answers his critics who complain that the

text alternates (ambiguously and without warning) between modes of description and prescription[3]. That is: describing what scientists do versus prescribing what they should be doing. Concerned about its "propagandistic potentialities", Paul Feyerabend voiced his criticism of Kuhn's ambiguity:

> **Whenever I read Kuhn, I am troubled by the following question: are we here presented with *methodological prescriptions* which tell the scientist how to proceed; or are we given a *description*, void of any evaluative element, of those activities which are generally called 'scientific'?[4]**

Is Kuhn simply trying to give us an objective and value-free description of science? The text is certainly descriptive in its tone. Paraphrasing Kuhn: 'The scientist does this and that.' In the postscript to *Structure*, Kuhn briefly discusses the prescriptive undertone of his work, but (in response to critics such as Feyerabend) justifies the prescription as logically following from his description of science.

> **The preceding pages present a viewpoint or theory about the nature of science, and, like other philosophies of science, the theory has consequences for the way in which scientists should behave if their enterprise is to succeed.[5]**

Feyerabend and other philosopher critics of Kuhn may or may not have been impressed by Kuhn's brief explanation of why he is allowed to logically derive the 'ought' from the 'is'. Without jumping into the vagaries of non-traditional logic, however, we can at least conclude that for Kuhn's prescriptive argument to make any sense, his 'descriptive' viewpoint or theory about the nature of science *must* include a standard of judgment regarding what 'successful' science is.

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[3] *The Structure of Scientific Revolutions*, p. 207
[4] Feyerabend in *Criticism and the Growth of Knowledge*, edited by Lakatos.
[5] *The Structure of Scientific Revolutions*, p. 207

For Kuhn, what is successful science? *Structure* never explicitly tells us what Kuhn is thinking along these lines. Is *Structure*'s undertone of prescription indicative of Kuhn's subconscious desire for science to be exactly the way he described it? To find an answer, I went back to other of Kuhn's writings to see if those were not as ambiguous. I was surprised to find that, in those writings, he was very clear about his normative position.

Science as implicitly conservative

In Kuhn's later essays where he argues with critics, his viewpoint on what good scientists should be doing is significantly less ambiguous than what he presents in *Structure*. One of Kuhn's pre-*Structure* papers, entitled "The Essential Tension"[6], is an excellent introduction to what he's about to do in *Structure,* and a post-*Structure* paper, entitled *Comments on the Relation of Science and Art,* gives us a sense of what Kuhn feels he has established. In these two papers, Kuhn clearly lays out what he thinks makes a good versus bad scientist.

Briefly summarizing Kuhn's main thesis in *Structure*, normal science proceeds according to the prevailing paradigm until it reaches a crisis, at which point a revolution occurs, and a new competing paradigm takes the old one's place, and then normal science proceeds once again. Normal science takes the current paradigm to be true no matter what, and normal scientists therefore are puzzle-solvers. Normal scientists are not interested in making new theories or challenging the current ones, but are instead interested in finding confirming instances of the current paradigm, finding better tools, being more accurate in numerical measurements, etc. According to Kuhn, their goals are

[6] *The Essential Tension: Tradition and Innovation in Scientific Research*, collected in *The Essential Tension: Selected Studies in Scientific Tradition and Change* (1977). Originally presented at *The Third (1959) University of Utah Research Conference on the Identification of Scientific Talent*.

necessarily conservative, non-critical, and non-innovative, and it follows that their work has the same flavor.

> **Under normal conditions the research scientist is not an innovator but a solver of puzzles, and the puzzles upon which he concentrates are just those which he believes can both be stated and solved within the existing scientific tradition.**[7]

Any revolutionary innovation in science, therefore, is a rare by-product of the normal scientific tradition. It is interesting to note that Kuhn does *not* dismiss the important role of the innovator, but considers such a person to be more important within the *applied* sciences than within *basic* science. Addressing the difference, Kuhn says:

> **…most of you are really in search of the *inventive* personality, a sort of person who does emphasize divergent thinking…In the process, you may be ignoring certain of the essential requisites of the basic scientist, a rather different sort of person…**[8]

Kuhn's viewpoint then (that is not immediately clear in *Structure*), is that the ideal scientist is *not* the revolutionary extraordinary scientist, but the mundane normal scientist--who does not seek to advance new theories and is primarily a puzzle-solver. For Kuhn, science is unique and privileged by virtue of the fact that it has paradigms and the goal of puzzle-solving based on those paradigms. Kuhn uses this argument to distinguish science from art, for example[9]. Although both scientists and artists have what some may call paradigms and puzzle-solving traditions, artists use puzzle-solving as a *tool* to aid in the production of their final works (such as what is presented in a museum). According to Kuhn, science operates in exactly the opposite manner. Scientists do not use their puzzle-

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[7] *The Essential Tension*, p. 234
[8] *The Essential Tension*, p. 239
[9] *Comments on the Relation of Science and Art* (1969), collected in *The Essential Tension: Selected Studies in Scientific Tradition and Change* (1977). Originally published in *Comparative Studies in Society and History*

solving to change old theories or to produce new ones. Instead, scientists take existing theories for granted and use them as tools, the end product of their enterprise being puzzle-solving.[10]

When the normal scientist is confronted with evidence that the current paradigm may be mistaken, he or she tends to ignore that evidence, blaming it on experimental error instead of critically investigating the anomaly. Even in the face of overwhelming evidence against them, well-established paradigms die hard, and in Kuhn's point of view, this conservatism is exactly what we want. Scientists *should* be 'normal', not innovative. Solving puzzles is the goal, not new theories.

> **…innovation itself need not be a prime value for scientists, and innovation for its own sake can be condemned, Science has its elite and may have its rear guard, its producers of Kitsch. But there is no scientific avant-garde, and the existence of one would threaten science. In scientific development, innovation must remain a response, often reluctant, to concrete challenges posed by concrete puzzles.[11]**

For Kuhn, protocol and consensus is the most important aspect of scientific practice. Although Kuhn makes the claim that scientists are trained as such, I can offer at least the singular disproving exception that is my own experience as a scientist--where I was educated to value scientific progress over protocol.[12] Of course, Kuhn does not deny that science can have some sort of progress, though he described that progress in a somewhat unusual way[13]. Some have argued that Kuhn's prescribed scheme of how scientists achieve progress is not good enough. Those arguments might never be settled,

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[10] Paul Feyerabend is one critic who was not impressed by Kuhn's use of puzzle solving as a criterion of science. Feyerabend argued that organized crime would be a science according to Kuhn's criterion of demarcation. *Criticism and the Growth of Knowledge* (1970) pp. 199-201

[11] *The Essential Tension*, p. 350

[12] If it is not already being done on a regular basis, it would be interesting to poll scientists and record their opinions on the relative importance of innovation versus conservatism in their field of study.

[13] See this paper's discussion of Kuhn's evolutionary metaphor.

but in revisiting them, I became aware of Kuhn's intent, thereby allowing me to read *The Structure of Scientific Revolutions* in a whole new light.

A Cold War Conception of American Science

Before going into critical responses to Kuhn, it will benefit the reader to understand some of context behind Kuhn's prescriptions. As explained by Steve Fuller[14], Kuhn's writing can be placed within a Cold War context. When he was at Harvard, Kuhn's mentor (who he respected tremendously) was James Conant, who was the president of the University from 1933 to 1953. Conant was one of the so-called "action-intellectuals" who, like his close colleague, Vannevar Bush, was an organizer and promoter of wartime and postwar American science. Conant's agenda[15] was to bolster the strength of American science, i.e. against the communist threat. He wanted pure science to be left alone to do its own thing, with industry and government ready to accept its advice, and with little feedback going in the opposite direction. American science, according to Conant, should not be controlled by external forces (such as potentially infiltrating communist forces[16]) and should not be held accountable. Most of his arguments were political in nature, but Kuhn, who taught courses for Conant took the argument further by looking at science and how it "works" and then proposing (in *Structure*) a cyclic internally-regulated mode of how science proceeds.

With this context in mind, Kuhn's use of an evolutionary metaphor to describe his ideal of science--proceeding without a purposive force to guide it--makes more sense.

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[14] *Thomas Kuhn: A Philosophical History for Our Times (2000)*

[15] A good example of Conant's views on science and science education in American culture can be found in his foreword to *The Copernican Revolution* (Kuhn, 1957)

[16] The fear of communism and of being labeled a communist and its effects on discouraging externalist accounts of science in the 1950s is discussed by Ziauddin Sardar in *Thomas Kuhn and the Science Wars* (2000)

Kuhn's ideal scientists are dedicated puzzle solvers, and are more or less untouched by potentially corrupting and *coercive* political, economic, and other social concerns. Furthermore, as Kuhn idealized them, the scientists should not have to work according to the demands and wishes of business and the state. According to Kuhn, the problems studied by *applied* scientists and inventors are "largely determined by social, economic or military circumstances external to the sciences",[17] but Kuhn deliberately excluded that brand of (externally-influenced) science from his analysis and defense (of basic science).

With the starting assumption that (basic) science works well, Kuhn noticed that scientists routinely did not behave as older notions (of how scientists work) would have predicted[18], so he formalized the behaviors he saw and found a way to say they were exactly what science needs in order to flourish. Although some have seen *Structure* as a criticism of science and a backlash against the pervasiveness of dominant paradigms, it is clear that Kuhn was really out to *promote* science and encourage the use of singular dominant paradigms in basic scientific research.

Critical Responses to Kuhn

In his defense of science, Kuhn did not use *Structure* to discuss the other thinkers at the time who were also discussing what scientists need to be doing[19], and the motivations of *those* thinkers were more along the lines of "scientists need to be doing things better" instead of Kuhn's "scientists are doing things good enough as it is". These philosophers were the ones who most heavily criticized Kuhn early on, Karl Popper and

[17] *The Essential Tension*, p. 238
[18] *The Structure of Scientific Revolutions*, p vii
[19] Fuller suggests that Kuhn only had a modest understanding of Popper's brand of logical empiricism at the time of *Structure*'s publication, and that *Structure* was less a response to Popper and more of a response to the older logical positivist tradition he had been exposed to as an undergraduate at Harvard in the early 1940s. *Thomas Kuhn: A Philosophical History for Our Times*. p 391

Paul Feyerabend being the most notable. Briefly stated, their reaction to Kuhn was essentially: "*This* is the science you want to promote?" The disagreement was so fundamental, Popper did not consider Kuhn's science to be science at all, and Kuhn considered Popper's view of science (a variation of logical empiricism, with falsificationism replacing verificationism) to be describing rare and comparatively uninteresting exceptional cases within science, and not representative of science proper. Many commentators have pointed out that Kuhn's description of science is more fitting with the historical facts than Popper's science, and even as a scientist, I can see that Kuhn was right to describe certain historical attitudes the way he did. What the commentators failed to understand, however, was that the logical empiricists did not write history, and that was not their intent. They often saw the same historical trends that Kuhn saw, but to them, the observed prevalence of unsatisfactory "normal science" (akin to metaphysics) was exactly why they needed a "scientific method". We see some of this attitude in the writing of A.J. Ayer, one of the later (and more progressive[20]) proponents of Logical Positivism.

The philosophers in Popper's camp presented their arguments criticizing Kuhn (including the ones discussed above) at an International Colloquium in the Philosophy of Science in 1965, backed by the British Society for the Philosophy of Science, the London School of Economics, and the International Union to History and Philosophy of Science. Their papers, and Kuhn's response, were published in *Criticism and the Growth of Knowledge* in 1970 (edited by Lakatos).

The most interesting criticism of Kuhn in that volume was the one presented by Paul Feyerabend, who was still considered a Popperian at the time. Even as he criticized

[20] Progressive in that he set out to distinguish between notions of "strong" versus "weak" verifiability.

Kuhn's argument, questioned the ambiguity of Kuhn's presentation style, and was sufficiently horrified by Kuhn's idea that scientists should seek to maintain dominant perspectives and achieve consensus instead of encouraging critical debate, he also defended one aspect of *Structure*. Feyerabend approved of what he read Kuhn to be saying regarding the distinctly *irrational* nature of science, and as such, suggested that science might best be performed as an irrational enterprise[21]. Kuhn called Feyerabend's unusual 'defense' of his work "not only absurd but vaguely obscene"[22] and counter to his main point. Feyerabend, in his later works *Against Method* and *Science in a Free Society*, decided that if irrationality was so important, there's no reason to limit oneself to science, and science should not be considered any different from art appreciation. Feyerabend was certainly a black sheep in the Philosophy of Science, but his viewpoints bring us closer to the way some of the Social Sciences view *Structure*.

A Revolution in his Name

In trying to defend science, Kuhn inadvertently presented it in a way that was easily attackable, such as by Feyerabend and Rorty[23]. Despite the fact that Kuhn clearly stated that *Structure* said "nothing about the role of technological advance or of the external social, economic, and intellectual conditions in the development of the sciences" and noted that such things, while interesting, "would not…modify the main theses developed in this essay", STS writers such as Sandra Harding cited Kuhn as having started the trend of looking at the powerful external influences on science and science's embedded-ness in a larger social fabric, ultimately leading to a constructivist approach to science studies. According to Harding, with the publication of *Structure*,

[21] *Criticism and the Growth of Knowledge* pp. 227-288
[22] *Criticism and the Growth of Knowledge* p 264
[23] "Untruth and Consequences," The New Republic, July 31, 1995, pp. 32-36.

> **…new approaches to science and technology studies began to flourish…pursuing the call of the social historians to show the integrity of events and processes in the history of modern science with their historical eras.[24]**

Yet Kuhn, while unashamed to utilize sociological observations, was neither a traditional sociologist nor a sociologist of science. In response to one of Popper's critiques, he wrote:

> **If he means that the generalizations which constitute received theories in sociology and psychology (and history?) are weak reeds from which to weave a philosophy of science, I could not agree more heartily. My work relies on them no more than his.[25]**

Sal Restivo was one of the early figures in STS to point out the fact that Kuhn was *not* the revolutionary the field had made him out to be[26]. Kuhn's impact had been made, however, and Kuhn himself had already been canonized as a pivotal figure in the birth of the Social Studies of Science.

In an interview late in his life, Kuhn recalled being invited by various student protest groups (in the 60s) to speak at their functions, and how they were grateful to him for having pointed out what paradigms were so that they (the students) knew how to do without them. Awkward situations like those forced Kuhn to routinely explain to people that his view was actually profoundly conservative, and not revolutionary in its intent.

> **I thought I was being—I want say badly treated—badly misunderstood. And I didn't like what most people were getting from the book.[27]**

If Popper's demarcation criterion (of science) allowed anyone of a critical rationalist bent to be considered a scientist, Kuhn's criterion was simply that science is

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[24] *Is Science Multicultural?* (1998) p vii
[25] *The Essential Tension* p 235
[26] "The Myth of the Kuhnian Revolution" (1983)
[27] *The Road Since Structure* (2000)

whatever scientists do, such that only those who belong to the scientific establishment are truly scientists. However, instead of insulating science from outsiders, both in terms of social criticism and 'non-scientists' wanting to 'get in on the act', Kuhn unwittingly brought about the opposite effect with *Structure*.

In one sense, *Structure* was used to bring science down from its privileged position of inquiry. In another sense, it was used to bolster and justify various social sciences, the social study of science in particular. As many other fields attempted, such as art, some social sciences adopted their own paradigms and puzzle-solving traditions in hopes of making themselves equal to science. Although *Structure* portrayed science as being internally-regulated and isolated from society, those within the Social Studies of Science took the opportunity provided by *Structure*'s popularity to jump in and demonstrate its own usefulness in unpacking the myths of science (some of which I wonder are intentionally created for dramatic purposes).

Science and Technology Studies as a self-secure field of study

In conclusion, Kuhn was vastly misunderstood, but should not be blamed overmuch for those misunderstandings. After all, we see what we want to see, and we read what we want to read; Kuhn probably could not have prevented that. Furthermore, Social Studies of Science was already in the air when *Structure* made its splash; *Structure* was merely a catalyst.

Over the years, the field of STS has proven its value many times over; it no longer needs *Structure* as justification, and it turns out that such a usage was a bastardization of the book's intent anyway.

It should also be noted that the tradition of the philosophers of science and the brand of logical empiricism emerging in the 60s was not really destroyed by *Structure*. The philosophical debates I read looked far from resolved, and who knows if such things ever are? If anything, *Structure* obscured the importance of that line of philosophical inquiry by mostly ignoring it, which is even more deadly than criticism (as Bruno Latour rightly points out). That 'lost' discourse deserves reexamination from an STS perspective.

It is my hope that the Social Studies of Science (and technology) can proceed without feeling the need to justify itself based on a flawed interpretation of Kuhn. Furthermore, in addition to studying what some scientists are often forced or unfortunately sometimes trained to do (which is boring, uncritical, dogmatic, and uncreative normal science) in the short run, I hope STS will not ignore what many scientists are trying to do and are often uniquely capable of doing (which is creative, critical, revolutionary, and extraordinary science) in the long run.

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