

Battle in the Mind Fields

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[S]cience textbooks (and too many of the older histories of science) refer only to that part of the work of past scientists that can easily be viewed as contributions to the statement and solution of the texts' paradigm problems. Partly by selection and partly by distortion, the scientists of earlier ages are implicitly presented as having worked upon the same set of fixed problems and in accordance with the same set of fixed canons that the most recent revolution in scientific theory and method has made seem scientific.

Thomas S. Kuhn, *The Structure of Scientific Revolutions*

Nous naissons déterminés et nous avons une petite chance de finir libres. Nous naissons dans l'impensé et nous avons une toute petite chance de devenir des sujets. Et ce que je reproche à ceux qui invoquent à tout va la liberté, le sujet, la personne, etc., c'est d'enfermer les agents sociaux dans l'illusion de la liberté qui est une des voies à travers lesquelles s'exerce le déterminisme. De toutes les catégories sociales, la plus inclinée à l'illusion de la liberté est la catégorie des intellectuels. C'est en ce sens que Sartre a été l'idéologue des intellectuels, c'est à dire celui qui a entretenu l'illusion de l'intellectuel "sans attaches, ni racines", comme disait Mannheim, l'illusion de l'auto-conscience, l'illusion que l'intellectuel peut maîtriser sa propre vérité. Et je pense que dans le refus forcené que certains opposent à la philosophie, dans la haine qu'ils opposent à la sociologie, il y a ce refus de découvrir l'intellectuel enchaîné dans des déterminismes : ceux qui tiennent aux catégories de pensée, aux structures mentales, aux adhésions et aux adhésions universitaires qui sont d'ailleurs beaucoup plus déformatrices que les adhésions politiques. Je pense que les universitaires sont beaucoup plus menés par les intérêts académiques que par les intérêts politiques, etc. Autrement dit, je pense que c'est à condition de s'approprier les instruments de pensée et aussi les objets de pensée que l'on reçoit que l'on peut devenir un petit peu le sujet de ses pensées ; c'est à dire on ne naît pas le sujet de ses pensées, on devient le sujet à condition, entre autres choses—je pense qu'il y a d'autres instruments ; il y a aussi la psychanalyse, etc.—de se réapproprier la connaissance des déterminismes. Je pense que je fais exactement le contraire de ce qu'on me fait dire.

Pierre Bourdieu <http://www.sociotoile.net/article24.html>

In the field of economic and political philosophy there are not many who are influenced by new theories after they are 25 or 30 years of age.

John Maynard Keynes *The General Theory of Employment, Interest, and Money*, 383-4.

L'étonnant n'est pas que l'intellectuel partage l'esprit du temps. C'est qu'il en soit la proie, au lieu de tenter d'y ajouter sa touche.

Franc ois Furet. *Le pass  d'une illusion*. p. 517.

Definition of the notion of matrix presupposes the notions of bondage and freedom, basic to which is the notion of bound occurrence.

Willard Quine, *Mathematical Logic*, p. 297.

Alles Gescheite ist schon gedacht worden.

Man muss nur versuchen, es noch einmal zu denken.

Johann Wolfgang von Goethe: *Wilhelm Meister*, Band II, *Betrachtungen im Sinne der Wanderer, Kunst, Ethisches, Natur*.

Chapter 27

Thomas Kuhn : A new look at revolutions in science

27.1 Kuhn's thunderbolt

In 1962, Thomas Kuhn published a book that was soon to add a new word to the vocabulary of academic English: *paradigm*. The book was *The Structure of Scientific Revolutions*, and in it Kuhn proposed a picture of scientific development that may not have been without precedent in the world of the history and philosophy of science, but his presentation was fresh and new and and it felt revolutionary. It struck a resonant chord in the academic world at large in the 1960s, and was a book that simply everybody had to read.¹ Kuhn proposed that the cumulative development of knowledge was only one part of the advance of scientific knowledge, and (he seemed to be suggesting at the same time) not the most exciting part. The path of science was *not*, in Kuhn's words, an ever growing stockpile of techniques and knowledge. It was history that taught us this, but if history reveals it, then there could hardly be too much point in adopting a philosophy of science in which the central image is a stockpile that is growing. That's just not how science works. Never has, never will.

Kuhn's message was simple and appealing, and fit the spirit of the '60s to a T. It was a message that was built around three terms: *paradigm*, *scientific revolution*, and *normal science*. We could reduce it all to just a trio of slogans: no paradigm, no science; scientific revolutions are the only places to be; and normal science is important, but pretty boring, really, unless you really like that sort of thing. But Kuhn said it much, much better than that. And he

¹We noted in fn. above that Kuhn spent a year at the Center at Stanford writing this book, along with a stellar cast of scholars.

swept people off their feet.

27.1.1 paradigm

Kuhn's first proposition was that a discipline emerges into the status of a science, out of a pre-scientific past, at the moment that a paradigm arises to dominate a field. Pinning down exactly what Kuhn meant by *paradigm* turned out to be a major project. Kuhn's own explanation of how he came to choose the term *paradigm* is interesting, and helps us understand a bit better what he had in mind.

Some years after the publication of *Revolutions*, Kuhn explained where his use of the term paradigm came from, and his explanation cannot fail to pique the interest of a linguist:

If [scientists] accepted a sufficient set of these standard examples, they could model their own subsequent research on them without needing to agree about which set of characteristics of these examples made them standard, justified their acceptance. That procedure seemed very close to the one by which students of language learn to conjugate verbs and to decline nouns and adjectives. They learn, for example, to recite *amo, amas, amat, amamus, amatis, amant*, and they then use that standard form to produce the present active tense of other first conjugation Latin verbs. The usual English word for the standard examples employed in language teaching is "paradigms," and my extension of that term to standard scientific problems like the inclined plane and conical pendulum did it no apparent violence. p. xix. – Thomas Kuhn *The Essential Tension* (1979) [?]

Thus one of the senses, and chronologically the first sense, that Kuhn intended for the term *paradigm* was that of "exemplar": scientists would learn what are good examples of analysis and explanation. To learn a science is to learn, in the first place, all the great examples of successes in the current paradigm. But Kuhn had a much broader intent for the term as well, one which he would later describe as a "disciplinary matrix," to which we will return below. In any event, as a matter of fact, it has turned out that the way we understand the intent that lay behind Kuhn's use of the term *paradigm* is by looking at the cases in which his theory of paradigms provides us with insight into the development of various sciences: we apply his theory to his theory.

Back to how a discipline becomes a science, which is by becoming dominated by a single paradigm. Why should we use the term "dominate," and

how do we recognize paradigms (or domination, for that matter) when we see it? A single reply answers both questions. To *be* a paradigm is to be dominant, in Kuhn's eyes: when we see a discipline that has a good number of alternative perspectives on the very fundamentals of the field, then there is no paradigm, and the field is not yet at the point where it can be called a mature science. Coherent traditions of scientific research—in a word, paradigms—include Ptolemaic and Copernican astronomy, Aristotelian and Newtonian dynamics, and corpuscular and wave optics. (p. 10). That was the first message, and it seemed quite clearly to suggest that the transition from a pre-scientific stage to a scientific one, with the development of a single perspective for the discipline, was perhaps the single greatest accomplishment a researcher could hope for.

Kuhn was dismissive of the exploration that we discussed in Chapters 2 and 3 which were a large part of the early 19th century effort.

In the absence of a paradigm or some candidate for paradigm, all of the facts that could possibly pertain to the development of a given science are likely to seem equally relevant. As a result, early fact-gathering is a far more nearly random activity than the one that subsequent scientific development makes familiar. (15)

Random activity—it gets worse. Kuhn suggests we take a look at 17th century Baconian natural histories, and we will see this pre-paradigmatic effort is a “morass”.

27.1.2 normal science

For Kuhn, scientific progress was of two sorts: normal science, and revolution. Revolution? Revolution was what happened when a discipline shifted from one paradigm to another; normal science is what one did during the longer periods between changes of paradigm. In between were moments of crisis, which prepared the field for paradigm change.

The most perturbing point of all of Kuhn's account was how boring he makes normal science out to be. It is *puzzle-solving*, and if you need to have it spelled out for you, puzzles are things like jig-saw puzzles.

It wouldn't be right to say that Kuhn's idea of normal science was the same as the image he was out to overthrow, the idea of science as growing stockpile of truths and measurements. But it wouldn't be completely wrong, either. “Perhaps the most striking feature of the normal research problems...is how little they aim to produce major novelties, conceptual or phenomenal.” (p. 35). Here is what scientists who do normal science do: some do observations, and some do theory. The experimentalists observe,

first of all, those things which the current paradigm tells us have fine and detailed structure that we should look into, like the specific gravities of materials, boiling points and spectral patterns. They observe predictions of the current paradigm to make sure the predictions are correct, but these are relatively rare; Einstein's theory of general relativity had but three areas in which predictions could be tested. Finally, the experimentalists make observations to flesh out the paradigm in those areas where the paradigm leaves open more than one way to flesh it out. That's what experimentalists do.

Theorists of a paradigm do similar things, but in a theoretical way. They compute predictions; they develop new mathematical techniques that allow a theory to be applied to more and more complex patterns of reality. Newton's law of universal gravity can be stated with great simplicity, but applying it to just about any situation more complex than two point-like objects of finite mass at a finite distance demands mathematical tools that have taken centuries to develop, and the work is not over. And, finally, the theory can continue to be improved by adding to its rich articulation.

There's something a bit jarring in Kuhn's discussion. On the one hand, he makes it clear that these theoretical efforts within the paradigm of Newtonian mechanics included the life work of such giants of mathematical physics as Euler, Lagrange, and Hamilton. But at the same time, Kuhn just cannot rise to find it very exciting.

Perhaps the most striking feature of the normal research problems we have just encountered is how little they aim to produce major novelties, conceptual or phenomenal. (35) ...Bringing a normal research problem to a conclusion is achieving the anticipated in a new way, and it requires the solution of all sorts of complex instrumental, conceptual, and mathematical puzzles. The man who succeeds proves himself an expert puzzle-solver, and the challenge of the puzzle is an important part of what usually drives him on (36).

It gets worse. Kuhn tells the reader (p. 37), who undoubtedly had never worried about this before, that he no longer needs to ask "why scientists attack [puzzles] with such passion and devotion," since he has already told us that the problems of a normal scientist are much like jigsaw puzzles and crossword puzzles. The answer lies in the psychology of the individual who decided to become a scientist. "A man may be attracted to science for all sorts of reasons. Among them are the desire to be useful, the excitement of exploring new territory, the hope of finding order, and the drive to test established knowledge...Nevertheless, *the individual* engaged on a normal research

problem *is almost never doing any one of these things*(38)" (and those are Kuhn's italics). He goes so far as to suggest that scientists in less interesting fields are "the proper sort of addict." (38).

In the end, the image that Kuhn gives of the scientist engaged in normal science is of a man whose contentment in life is solidly enhanced by knowing that he can rely on a "strong network of commitments—conceptual, theoretical, instrumental, and methodology." (42). The paradigm "provides rules that tell the practitioner of a mature speciality what both the world and his science are like," allowing him to "concentrate with assurance upon the esoteric problems that these rules and existing knowledge define for him." (42). Boring.

27.1.3 anomalies and crisis

Kuhn offered a specific account of how the important changes in science—which is to say, the change from one paradigm to another—take place. They begin with some discoveries of problems for the paradigm that stubbornly refuse to go away and that are eventually recognized as anomalies. A major flow of anomalies creates a sense of panic, a crisis in the paradigm, and if a new proto-paradigm is waiting in the wings, it just might come in and dethrone the old paradigm. Scientists "may begin to lose faith" in a period of crisis, but "they do not renounce the paradigm that has led them into crisis." (77) Here is the crucial point:

once it has achieved the status of a paradigm, a scientific theory is declared invalid only if an alternative candidate is available to take its place. (77)

The scientist does not reject a theory by virtue of lack of fit between predicts and observations: there is no room for methodological logical positivists, or for Karl Popper. The scientist only rejects a theory when there is a new one to take its place: he is a serial monogamist.

Kuhn makes an odd argument in favor of this view. It is "odd" because he comes very close to making the claim vacuous. A person who rejects a paradigm without an alternative paradigm to take its place, simply because of an anomaly or two or five, is...not a scientist! (p. 78). You just can't do something like that and still be a scientist. You would be resigning from the profession.

27.1.4 philosophy?

The heart of scientific progress is thus the turmoil that accompanies the spirit of growing anomalies and mounting crisis. At such moments, Kuhn noted, “scientists have turned to philosophical analysis as a device for unlocking the riddles of their field.” (88). In periods of normal science, the trained scientist knows to keep away from philosophy. But “the search for assumptions (even non-existent ones) can...be an effective way to weaken the grip of a tradition upon the mind and to suggest the basis for a new one. It is no accident that the emergence of Newtonian physics in the seventeenth century and of relatively and quantum mechanics in the twentieth should have been both preceded and accompanied by fundamental philosophical analysis of the contemporary research tradition.” (88) Our journey in this book has certainly confirmed the notion that linguists and psychologists feeling the weight of crisis have turned to philosophy, but it is not clear that the cross-disciplinary fertilization is any less at other moments, at moments of normal science. Scientists who take philosophy seriously enough to read it are those who feel the freedom to develop non-paradigmatic modes of thinking and of speaking.²

27.1.5 revolution

Unlike the term *paradigm*, which existed, of course, in the language but whose use was restricted to a technical domain, the term *revolution* was already in wide use. Its original meaning comes from geometry and astronomy, where a revolution is a completed path through some kind of cyclic motion, like that of a planet going round the Sun, and in most cases it hardly matters where we think of the revolution as starting (and therefore as finishing): one point is as good as another, when a planet revolves in something close to circular motion. But when one views the world (human or otherwise) as a sequence of ebbs and flows, of beginnings, middles, and ends, there really is a natural starting point, and a natural ending point. By the early 18th century, a new sense had come to be attached to the term *revolution*, based on that sense.³ The

²Kuhn p. 90:

Almost always the men who achieve these fundamental inventions of a new paradigm have been either very young or very new to the field whose paradigm they change. And perhaps that point need not have been made explicit, for obviously these are the men who, being little committed by prior practice to the traditional rules of normal science, are particularly likely to see that those rules no longer define a playable game and to conceive another set that can replace them. p. 90.

³Ref to Cohen’s book on Revolution in Science, and his article that preceded it.

natural form of a revolution is to have a beginning, often an abrupt beginning; in 17th century European parlance (check date), this sharp rupture with the past was referred to as an *époque*, one that marked the beginning of a new revolution of history. By the beginning of the 17th century, the term *revolution* itself took on this meaning—notably in the description of the Glorious Revolution of 1688 in England, when the English Parliament forcibly removed the Catholic King James II from office, and replaced him with a Protestant king, William of Orange (who was James II's son-in-law, and also his nephew!). In some respects—certainly with regard to the position of the Protestants in England—this revolution was also a restoration. By the end of the 18th century, two more political revolutions would occur which would cement this new meaning of revolution: the American Revolution in 1776, and the French Revolution in 1789.

In order for a new idea to take hold in the development of a science, there must be a preliminary period during which a sense of dissatisfaction in the current view — the current paradigm — is the order of the day. The feeling that what we have today is inadequate, Kuhn argued, is an essential element in the larger development of science.

That was a remarkable idea, one that would never have risen to the surface as long as the activities of the scientist were viewed as adding to the stockpile of knowledge and nothing but that. Of course the United States was already engaged in a radical rethinking of many of its values by the early 1960s, most notably in the area of race relations and civil rights, and Kuhn's book was published at just the moment when many other aspects of American society would come to be challenged. There was rock music, the sexual revolution, and the powerful movement against the war in Vietnam. Kuhn's book could naturally be interpreted as a lesson and a moral: find the inadequacies (Kuhn spoke of "malfunction") of the current world order, make them clear, and simply doing that will prepare a discipline for a coming new way. And it is not even necessary to convince an entire discipline of the inadequacy of the present: as Kuhn noted, in the political area, that the growing sense of inadequacy of the present was "often restricted to a segment of the political community" — the vanguard, in a sense. The old guard, in turn, could not be counted on to appropriately judge the next big thing, in science anymore than in the larger world:

Like the choice between competing political institutions, that between competing paradigms proves to be a choice between incompatible modes of community life. Because it has that character, the choice is not and cannot be determined merely by the evaluative procedures characteristic of normal science, for these depend

in part upon a particular paradigm, and that paradigm is at issue. When paradigms enter, as they must, into a debate about paradigm choice, their role is necessarily circular. Each group uses its own paradigm to argue in that paradigm's defence.(page?)

And the vanguard's only weapon is the ability to persuade: which means, typically, showing why adopting the new paradigm, and leaving the old one behind, will be advantageous. Logic and experiment (citation, ch. 9) may be enough to persuade scientists during periods of normal science, but they are not enough to carry one from one paradigm to the next, because the old paradigm can always be defended from within. To move beyond the old, methods that go past logic and data will be necessary.

It is a small step from the notion that science is not always cumulative to the conclusion that revolutionary science ought to break with prior scholarship. As Kuhn put it:

the assimilation of all new theories and of almost all new sorts of phenomena has in fact demanded the destruction of a prior paradigm and a consequent conflict between competing schools of scientific thought. (Ch. 9, page ?)

He goes much further though. He virtually implores the reader to see, with him, a seamless match between intellectual revolution and political revolution. This is what he writes:

...the parallel between political and scientific development should no longer be open to doubt....Political revolutions aim to change political institutions in ways that those institutions themselves prohibit. Their success therefore necessitates the partial relinquishment of one set of institutions in favor of another, and in the interim, society is not fully governed by institutions at all. ...In increasing numbers individuals become increasingly estranged from political life and behave more and more eccentrically within it. Then, as the crisis deepens, many of these individuals commit themselves to some concrete proposal for the reconstruction of society in a new institutional framework. At that point the society is divided into competing camps or parties, one seeking to defend the old institutional constellation, the others seeking to institute some new one. And, once that polarization has occurred, *political recourse fails*. ...the parties to a revolutionary conflict must finally resort to the techniques of mass persuasion, often including force.

Yes, that is Kuhn speaking about *political* revolutions—and now he says,

The remainder of this essay aims to demonstrate that the historical study of paradigm change reveals very similar characteristics in the evolution of the sciences.

For everyone who felt that the description of political revolution matched their sentiments precisely about the changes that the United States was about to face, the idea that the history of science was a perfect match to political revolution came as a thunderbolt, and to most readers, as a very welcome one.

the unbearable fuzziness of science

We have focused so far on the three concepts that were central to Kuhn's vision of science: paradigm, normal science, and scientific revolution. But there was another aspect of his account which was perhaps even more important and in the long run influential, and which raised the hackles of far more readers. Kuhn pointed out that the task of understanding the historical development of science required that we employ fuzzy concepts and questions that have no real and definite answer. And he did this by asking very simple questions, questions that everyone would naturally agree are not only reasonable but completely unavoidable if we are to say something about the history of science at all. Who discovered oxygen? Too hard to say which person? then *when* was it discovered? When was the principle of the conservation of energy discovered? Still too hard? Kuhn laid bare the fact that the terms we use (both relatively concrete terms, like *oxygen*, and relatively abstract terms, like *the law of the conservation of energy*), may be perfectly clear to us now, in 1962 or the early 21st century, but to ask when these things were discovered is really to ask when our current understanding of them became clear, and that's not only hard to say, it's also typically not something that is localizable in space and time.

Kuhn studied the various agents and events that historians associate with the discovery of oxygen, and discussed it both in *Structure* and in a paper published the same year [?]. The problem with deciding who discovered oxygen, and when it happened, is that discovering oxygen is a bit like discovering the New World: if you think you have found India when you get to North America, have you discovered the New World? What if you arrive at an island 75 miles from North America and have no idea where you are: have you discovered the New World? We can debate the answers to those questions, but the problem is a good deal worse in the case of oxygen, because at the time

when oxygen was being discovered, the middle of the 1700s, scientists did not understand that there were either atoms or elements, as we understand those terms. Worse yet, the “discovery” of oxygen was intimately tied up with the shift taking place in chemistry at the time regarding the very character of burning and heat. The phlogiston theory of fire, and other things, dating back to the 1660s, claimed that there was a substance, phlogiston, which was released during burning. The theory was overturned by Lavoisier’s work in the 1770s, which introduced an account involving *caloric*, a competitor to the phlogiston-based theory. Lavoisier’s perspective stood on its head a number of central views of the chemistry of his day, and it allowed him to see, and to claim, that there was something “in” air which was essential for combustion to take place—that something being oxygen, which he saw was only a part of what constituted our atmosphere. In the end, Lavoisier’s caloric theory of heat was replaced in the 19th and 20th centuries by the theory of heat as kinetic energy of atoms and molecules, but Lavoisier took enough strides to the way of thinking that we have today that we are willing to agree that he had indeed identified oxygen for what it was.

But Kuhn reflects on what it takes to count as discovery of something, something like oxygen. It can’t be a matter of holding a sample of it: anyone holding a sealed empty bottle has a healthy sample of oxygen inside (even if there is more nitrogen than oxygen). You must understand something about what you have if we are to say you have discovered oxygen: you must understand at the very least that there were several different things that all appear as odorless and colorless gas, and it would be much better still if you had a fool-proof way of obtaining that oxygen on demand. Kuhn notes that in 1774, Priestley had in fact (“in fact” means from our modern perspective) produced oxygen with a reliable method, but he did not distinguish it conceptually from carbon dioxide. Lavoisier, a few short years later, did distinguish it from CO_2 , and he called it *oxygen*.

The story is more interesting and more complex, but when all is said and done, Kuhn’s point is unmistakable: the discovery of oxygen, like most important events in the history of science, can only be sharply and clearly identified if we make assumptions that are gross oversimplifications of the historical reality.

27.1.6 the invisibility of revolutions

There is one aspect of Kuhn’s account of the progress of science that more than any other flies in the face of what we have seen over the course of our study. Kuhn knew that he needed to be on the defensive when he said that the most valuable part of science proceeds not through accretion and

accumulation but through revolutionary movements that discard as well as contribute. This does not appear in the textbooks, and does not appear in most of the histories of science that Kuhn knew, so he knew he had to provide an explanation for that.

His explanation was based on the interest that the sciences, and the sciences themselves, have in maintaining the picture (inaccurate though it may be) of a cumulative process at their core.

But those of us in the mind sciences find just as great a commitment to revolutionary scientists denying the continuity and the cumulativeness that the scholar who looks for it will find.

new section

Kuhn's perspective on the history of science proved to be a major challenge to the current views on the philosophy of science. We have seen the evolution from a positivist and verificationist view of science that was strong at the beginning of the century, to a view heavily influenced by Karl Popper and falsificationism, the view that science consists of daring hypotheses that are never proven, but often tested against the implacable Nature that surrounds us.

In a sense, Kuhn shared with Popper a concern for how to characterize science from non-science, but their approaches to this problem were very different. Popper made no bones about it that his clear cases of non-science were Marxism and Freudian psychology, while Kuhn's concern was with the evolution of non-science into science: if there are sciences today, and were no sciences 1,000 years ago, when did science arise, and what can we see in the record that shows that an essential transformation has taken place? Kuhn described this change as a development from a pre-scientific condition, followed by a scientific revolution, to a period of normal science; but periods of normal science would henceforth always be followed by additional scientific revolutions. A discipline that has undergone just one revolution, and has its first paradigm, is a discipline that has reached what Kuhn calls scientific maturity. Along with scientific maturity comes (though whether this is by definition or not is not clear) a greater coherence in the theoretical beliefs of the discipline. (source?) ⁴⁷⁴

The Kuhnian view of science offered two quite different perspectives—though *metaphor* might be a more appropriate term — for linguistics and psychologists reflecting on their disciplines. Some questioned whether their field had yet matured past the pre-scientific stage, and many reached the

conclusion that it had not; others questioned whether their field was ready for, or already participating in, a scientific revolution.

In Kuhn's view, a science that was well characterized as a paradigm would encourage its members to effect normal science, but would inevitably be subject to a build-up of difficulties for the paradigm, and results that were inevitably challenges to the paradigm. Eventually the mass of these challenges would lead to intellectual dissatisfaction, and the appearance of a crisis, which would make a revolution possible, which in turn would lead to a new paradigm and a new conception of normal science based on the new paradigm.

And what is the status of problems, of annoying facts that don't seem to work correctly as far as the current paradigm is concerned? Kuhn's account was ambivalent on this—or could, at least, be read this way. On the one hand, simply noting a problem does not count as an example of normal science. Normal science means solving problems with the tools at hand, provided by the current paradigm. But noting problems does not constitute providing a new paradigm either—far from it. So on one reading, Kuhn's model of science leaves no room in science for finding insoluble problems.

But that reading of Kuhn leaves something important out: the role of the accumulation of nasty problems that the current paradigm is unequipped and unable to account for.

In much the same way, scientific revolutions are inaugurated by a growing sense, again often restricted to a narrow subdivision of the scientific community, that an existing paradigm has ceased to function adequately in the exploration of an aspect of nature to which that paradigm itself had previously led the way. In both political and scientific development the sense of malfunction that can lead to crisis is prerequisite to revolution. Chapter 9.

It's only in the strictest of senses, then, that the discovery and cataloging of problems for the current paradigm plays no direct scientific role—and this strictest of senses is quite misleading. When these problems become serious enough to begin to make scientists worry about their theories, at first just for a moment and then for longer and longer periods, the scientists are preparing themselves (probably without their own knowledge) for a coming crisis, to be followed by the introduction of a new paradigm.

Early Feyerabend on Kuhn

Paul Feyerabend wrote a letter to Kuhn while *Structure* was still in draft form, in which he wrote,

What you are writing is not just history. *It is ideology covered up as history.* Now please, do not misunderstand me....[I do not] pretend that in history a nice distinction can be drawn between what is regarded as a factual report, and what is regarded as an interpretation according to some point of view. But points of view *can* be made explicit....Nobody will think that the history of crime justifies crime, or shows that crime possesses an inherent 'reason' or an inherent morality of its own. In the case of the sciences or of other disciplines [for] which we have respect the situation is much more difficult and the distinction cannot be drawn with equal ease. But in these cases it is of paramount importance *to make the reader realize that it still exists.* You have not done so. Quite on the contrary, you use a kind of double-talk where every assertion may be read in two ways, as the report of a historical fact, and as a methodological rule. You thereby take your readers in....I do not object to your belief that once a paradigm has been found a scientist should not waste his time looking for alternatives but try working it out....What I do object to most emphatically is the way you present this belief of yours; you present it not as a *demand*, but as something that is an obvious consequence of historical facts. Or rather, you do not even talk about this belief, you let it as it were emerge from history as if history could tell you anything about the way you *should* run science.⁵

Kuhn's book: is it ideology covered up as history? On the face of it, that's a serious charge, though the irony of it is that it comes from the only other philosopher of science of Kuhn's generation who would eventually have a break-through smash hit in the mid 1970s, *Beyond Method*, which would often be miscited and misremembered as a plea for the principle that *Anything Goes!* And if anything goes, then why shouldn't Kuhn's word go as well? But *Anything goes* was not Feyerabend's view of the philosophy of science: it was too important for that, and we will come to this in due time.

If Feyerabend were still alive to reprimand us, we would not dare to say what must be said nonetheless: his charge should not have been that Kuhn covered ideology up as history, but that what Kuhn wrote was ideology through and through—if by ideology, we mean the selective and insidious

⁵Source: Two letters of Paul Feyerabend to Thomas S. Kuhn on Draft of *Structure*, ed. by Paul Hoyningen-Huene, *Stud Hist Phil Sci* 26,3 (1994) 354-5. Cited by Rupert Read, in On Wanting to Say, "All we need is a paradigm" *Harvard Review of Philosophy*, Vol 9, 2001.

mixture of what is and what ought to be for strategic ends.

From the point of view of linguistics and the mind sciences, this is precisely the effect of Kuhn: he shaped a perspective on disciplinary work that called attention to itself as a revolutionary movement, a vanguard of the future that will soon be, and a movement that believed it understood better than anyone else what the meaning was of staking a claim to the scientific treatment of the mind. Kuhn's view authorized a young generation to declare an insurgency against an older generation, or so it seemed at least, and at many times and many places, that is a powerfully attractive opportunity.

There is no better way to sum up in a brief phrase what it was that the visionaries of generative grammar and what would eventually come to be known as the cognitivist movement took as their model: they were the *vanguard* of a new and better science of mind, and as a vanguard, it was their sworn duty to lay open the inadequacies in every sense of the term of the rotten regime that currently held sway.

Bourdieu

Kuhn was not one to link accounts of scientific revolutions with social shifts, as Bourdieu was. Bourdieu, presenting Kuhn's account, wrote [12]:

Le mérite de Kuhn...est d'avoir attiré l'attention sur les ruptures, les révolutions...il ne me paraît pas proposer de modèle cohérent pour expliquer le changement. Bien qu'une lecture particulièrement généreuse puisse construire un tel modèle et trouver le moteur du changement dans le conflit interne entre l'orthodoxie et l'hérésie, les défenseurs du paradigme et les novateurs, ces derniers pouvant se trouver renforcés, dans les périodes de crise, par le fait que les barrières tombent alors entre la science et les grands courants intellectuels au sein de la société. J'ai conscience d'avoir prêté à Kuhn, à travers cette réinterprétation, l'essentiel de ma représentation de la logique du champ et de sa dynamique.
p. 36-7.

Keith Percival 1976 argued against the appropriateness of Kuhn's term paradigm for our understanding of the history of linguistics. His primary concern was the lack of uniform assent among linguists to the generative perspective: "it is not a conceptual framework shared by all the members of the profession." (289). Percival points both to linguists who are not at all generativists, and to the varying positions to be found within the group of linguists who identify themselves as generativists. He is, in general, quite

unsympathetic to casting Kuhn's account over linguistics — and why not be?

On Kuhn's account, normal science consists largely of puzzle solving.

27.2 McCawley 1976

From Madison Avenue Si, Pennsylvania Avenue, No!

The notion of scientific revolution, popularized particularly by Thomaks Kuhn (1962), has figured in much discussion of the recent history of linguistics. Kuhn's ideas, however, have often been grossly misunderstood; for example, there is a deplorably common tendency to form an unholy synthesis of Kuhn's notion of revolution with the previously standard view othat science develops cumulatively, which yields the popular but totally unwarranted view that scientific revolutions are always for the better. I note in passing that Chomsky's conception of the history of linguistics commits him to the view that there have been scientific revolutions for the worse in linguistics and psychology (e.g., the 'neogrammarian revolution' and the 'behaviorist revolution'). (p.223 of book version, *Adverbs...*)

27.3 Did generative grammar constitute a Kuhnian revolution?

See Newmeyer 1986 on this subject:

It was once uncontroversial to refer to a 'Chomskyan revolution' in linguistics. Commentators took it for granted that the publication of *Syntactic structures* [17] by Noam Chomsky in 1957 ushered in an intellectual and sociological revolution in the field—a revolution that deepened with the following decade's work by Chomsky and his associates. The term 'Chomskyan revolution' has appeared in the titles of articles (Searle 1972) and book chapters (Newmeyer 1980); and an historian of linguistics has written that the work of Chomsky "fully meets [the philosopher Thomas] Kuhn's twin criteria for a paradigm [in science]" (Korner 1976:709). Even Chomsky's professional opponents have acknowledged the revolutionary nature of his effect on linguistics. G. Sampson, who feels (1980: 163) that 'the ascendancy

of the Chomskyan school has been a very unfortunate development for the discipline of linguistics', nevertheless writes (130) that 'Chomsky is commonly said to have brought about a "revolution" in linguistics, and political metaphor is apt.' R. Longacre, an individual who has a quite different orientation to grammar from Chomsky's, writes (1979:247) that 'the field was profoundly shaken by him', and has identified the essence of the Chomskyan revolution (a term which he uses without surrounding quotes) as its commitment to the construction of an explanatory linguistic theory. [66]

Newmeyer goes on to say that Koerner has now changed his mind: "(1983: 152)'upon closer inspection, the term "revolution" does not properly apply to TGG,' and he cites others (in particular, Stephen O Murray, R. Antilla, B. Gray 1976, Hill 1980. But Newmeyer does indeed believe that *Syntactic Structures* began a Kuhnian revolution:

Chomskyan theory represents a revolutionary approach to the study of language, and one whose revolutionary content was present in explicit form in *Syntactic Structures* [17]. Moreover, I will argue that, sociologically as well as intellectually, the field has undergone a Chomskyan revolution. Paradoxically, however, the sociological transformation of the field has not been accompanied by a corresponding success on the part of generative grammarians in achieving institutional power.' I will demonstrate that—far from being comfortably seated on the throne after their successful 'palace coup'—generativists, as they compete for adherents with linguists of other persuasions, find themselves well outside the walls of the palace.

Newmeyer saw two revolutionary themes in *Syntactic Structures*[17]. The first is the "conception of a grammar as a theory of a language, subject to the same constraints on construction and evaluation as any theory in the natural sciences":

Prior to 1957, it was widely regarded—not just in linguistics, but throughout the humanities and social science—that a formal yet non-empiricist theory of a human attribute was impossible. Chomsky showed that such a theory was possible. Indeed, the central chapter of *Syntactic structures*[17], 'On the goals of linguistic theory', is devoted to demonstrating the parallels between

linguistic theory, as he conceived it, and what uncontroversially would be taken to be scientific theories. Still, *Syntactic structures*[17] would not have made a revolution simply by presenting a novel theory of the nature of grammar; the book had revolutionary consequences because it was NOT merely an exercise in speculative philosophy of science. Rather, it demonstrated the PRACTICAL possibility of a non-empiricist theory of linguistic structure: half the volume is devoted to the presentation and defense of a formal fragment of English grammar.

The second revolutionary theme was the placement of syntax at the center of grammar. Wrote Newmeyer: “The revolutionary importance of the centrality of syntax cannot be overstated. Phonological and morphological systems are essentially closed and finite; whatever their complexity or intrinsic interest, their study does not lead to an understanding of a speaker’s capacity for linguistic novelty, or to an explanation of the infinitude of language.” (p. 3)

“[E]arlier accounts,” Newmeyer wrote, “had typically excluded syntax from langue [the realm of systematic grammatical accounts] altogether,” specifically mentioned Saussure and the Prague school in this regard. We have seen (see sections XX above) that such a view is quite seriously at odds with the facts; Chomsky’s thesis advisor, Zellig Harris, had been developing transformational grammar for nearly twenty years at the time of *Syntactic Structure*’s publication. Newmeyer conceded this; how could he not? He wrote: “Z. Harris, it is true, had begun in the late 1940’s to undertake a formal analysis of intersentential syntactic relations (see Harris 1957),” and it is remarkable that Newmeyer fails to refer to Harris’s theory as transformational grammar. But even if Harris had developed useful or important tools for understanding intersentential syntactic relations, his philosophy of science was too out of date to allow him to understand the consequences of what his work actually meant for linguistic theory, according to Newmeyer: Harris’s “empiricist commitment to developing mechanical procedures for grammatical analysis led him to overlook what the study of these relations implied for an understanding of linguistic creativity.” (3-4).

Still, Newmeyer concedes that the idea of a generative grammar employing transformational rules was not revolutionary:

1. No partisan of Chomskyan theory has ever suggested that the proposal of a generative grammar embodying transformational rules constituted, in 1957, a revolutionary break with past practice.

Since Newmeyer himself had written that “the placement of syntax at the center of grammar” was a revolutionary theme whose importance cannot be

overstated, it is hard to know what Newmeyer's point was in (1). Perhaps he meant that the centrality of syntax was revolutionary, but the practice of transformational generative grammar (as opposed to our understanding of it) was not revolutionary, given that Harris was already practicing it, and Newmeyer was quite explicit in his understanding that

Transformational rules are not central to Chomskyan theory, nor have they ever been regarded as an innovation of the theory...Chomsky has always...credited Harris with originating them (6)

and Newmeyer refers to a specific place in *Syntactic Structure* where Chomsky credits Harris.

Even the idea that a grammar could be understood as a fully formal object “had been in the air for several years” (5), though of course, as we have seen, the idea had begun to be worked out in fully explicit fashion in the Carnap-Lesniewski-Bar Hillel genealogy; Newmeyer cites Harris 1954 and Hockett 1954 [47].

Newmeyer also gives Chomsky's revolution in linguistic credit for the revolution that took place in psychology as well:

The fact that *Syntactic structures* was syntax-centered lay at the foundation of the interdisciplinary revolution that it initiated. Consider its effect on psychology. Psychologists had certainly taken an interest in pre-Chomskyan structural linguistics; indeed, J. B. Carroll had written (1953:106): ‘From linguistic theory we get the notion of a hierarchy of units ... It may be suggested that stretches of any kind of behavior may be organized in somewhat the same fashion.’ Yet the approach to language to which Carroll referred, by granting primary position to phonology or morphology, offered little to an understanding of language processing or more general aspects of verbal behavior. As a consequence, the results of structural linguistics were completely ignored in Skinner's *Verbal Behavior* (1957), and were given only limited attention in the major pre-Chomskyan survey of psycholinguistics, Osgood & Sebeok 1954. But shortly after Miller et al. 1960 had revealed to the community of psychologists the implications for the structure of human behavior latent in Chomsky's theory of syntax, the ‘psycholinguistic revolution’ (Greene 1972: 11) was well under way. (p. 4)

Chomsky's impact on philosophy was “equally profound,” Newmeyer argued.

The effect of *Syntactic structures* on philosophy was equally profound. Although the two major schools of mid-20th century philosophy—logical empiricism and ordinary language philosophy—were preoccupied with problems of language, they paid scant attention to structural linguistics. But Chomsky’s syntax-centered approach, with its implications for limitless yet rule-governed creativity, had initiated a dialog among philosophers even before he had called attention to the ‘Cartesian’ properties of the theory (cf. Putnam 1961, Chomsky 1962, Bar-Hillel 1962, Scheffler 1963).⁶

Newmeyer appears to have been unaware of the profound impact of logical positivism on Chomsky’s conception of grammar, as we have seen. (ref.)

From Newmeyer’s perspective, generative grammar is an advance within the fold of structuralist grammar:

Chomsky’s revolution was a revolution within structural linguistics—one which profoundly altered our conceptions of the nature of linguistic structure, and opened the way to an understanding of how its nature bears on the workings of the human mind.(5)

Newmeyer agreed with Hymes and Fought about what the most important point was in Chomsky’s proposal, which Hymes and Fought identify as:

2. Chomsky’s true argument with the Bloomfieldians was with regard to the kind of evaluation procedure, the kind of formal justification of a linguistic analysis, or linguistic theory, that should be followed. To the criterion of theoretically possible induction, he opposed the criterion of theoretically definable simplicity (generality). (p. 180, Hymes and Fought).

Where did this idea come from?

Newmeyer contended that the point identified (2) came down to “the very nature of linguistic theory.” (6).⁷

⁶We have seen X examples of classic papers in philosophy already in which the importance of grammatical structure for philosophy is underscored: for example, in Ajdukiewicz’s (1935) “On syntactic coherence,” which notes that “the problems of linguistic structure [are] the most important problems of logic (this term being taken broadly so as to cover metatheoretical enquiries as well). Among these problems the one that has the greatest significance for logic is the problem of syntactic coherence.”

⁷“No issue is as important as the relevant criteria for theory evaluation, since a radically revised evaluation procedure entails a theory with a radically revised ontological basis. To abandon a procedure based on induction, and to adopt one based on generality, is to break from past practice at its most fundamental point; it requires on

27.4 Was the cognitive revolution a Kuhnian revolution

Greenwood

The movement from behaviorism to cognitivism that is often characterized as the cognitive revolution is not best represented in terms of a Kuhnian “paradigm shift” (Lachman, Lachman, & Butterfield, 1979; Palermo, 1971; Weimer & Palermo, 1973) in which one theoretical paradigm gives way to another under the pressure of an empirical anomaly or set of anomalies (Kuhn, 1970). The various anomalies that eventually faced behaviorism, such as the “discovery” of biological limits on conditioning (Breland & Breland, 1961; Garcia & Koelling, 1966), and doubts about the ability of conditioning theory to accommodate linguistic performance (Chomsky, 1959; Lashley, 1951), did not result in the abandonment of the central principles of operant or classical conditioning theories—the core theoretical elements of the behaviorist paradigm. Moreover, behaviorists continued to maintain their in-house journals, their own APA division, and a sizable professional membership (Leahey, 1997). Nor were these recognized anomalies the primary stimulus for the development of cognitive theories in the 1950s, which was provided by outside developments in artificial intelligence and the computer simulation of cognitive abilities (Baars, 1986; Gardner, 1985). p. 2

Certainly, the relation between behaviorism and cognitive psychology is not best represented as a conflict between competing and exclusive theoretical paradigms, on analogy with historical conflicts between, for example, the physical theories of Newton and Einstein in the early twentieth century, or between wave and particle theories of light in the early nineteenth century. The evidence that favored and led to the adoption of Einstein’s theory and the wave theory of light appeared to demonstrate the general inadequacy of Newtonian theory and the particle theory of light, and thus led to their complete rejection by most scientists. Yet nobody—not even dedicated cognitivists—seriously imagined that either the anomalies noted above, or their theoretical biological and cognitive resolutions, demonstrated the general inadequacy of theories of classical or operant conditioning. These recognized anomalies, and their theoretical biological and

cognitive resolutions, only led to a delimitation of the scope of explanations in terms of conditioning (albeit long overdue), and the extension of underdeveloped biological and cognitive explanations to those domains for which conditioning theory had proved to be inadequate. p. 3

...The cognitive revolution is also not best represented as a revolution in terms of a paradigm shift with respect to attitudes towards theories, in the sense of a shift from an instrumentalist to a realist conception of theories, that is, from the treatment of theories of cognitive and biological states and processes as mere linguistic instruments that facilitate the integration and prediction of empirical laws, to their treatment as theoretical references to putatively real cognitive and biological states and processes.⁶ Although this is the usual historical account advanced by those in the cognitive science community (Baars, 1986), and popularized by Jerry Fodor (Fodor, 1975), it is of doubtful validity.

In defense of this, Greenwood quotes Tolman 1932:

For the behaviorist, “mental processes” are to be identified and defined in terms of the behaviors to which they lead. “Mental processes” are, for the behaviorist, naught but inferred determinants of behavior, which ultimately are deducible from behavior. Behavior and these inferred determinants are both objectively defined types of entity. (Tolman, 1932, p. 3)

27.5 Chomsky on Kuhn's impact

Chomsky wrote:

I should also mention work on history and philosophy of science, which has begun to furnish a richer and more exact understanding of the manner in which ideas develop and take root in the natural sciences. This work—for example, that of Thomas Kuhn or Imre Lakatos—has gone well beyond the often artificial models of verification and falsification, which were prevalent for a long time and which exercised a dubious influence on the “soft sciences,” as the latter did not rest on the foundations of a healthy intellectual tradition that could guide their development. It is useful, in my opinion, for people working in these fields to become familiar with ways in which the natural sciences have been able to

progress; in particular, to recognize how, at critical moments of their development, they have been guided by radical idealization, a concern for depth of insight and explanatory power rather than by a concern to accommodate “all the facts”—a notion that approaches meaninglessness—even at times disregarding apparent counterexamples in the hope...that subsequent insights would explain them. These are useful lessons that have been obscured in much of the discussion about epistemology and the philosophy of science. [18] [Language and Responsibility p. 73]

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...if I were still in a Sellar and Yeatman mood I would unhesitatingly describe this as the Worst Thing that has happened to the historiography of twentieth century linguistics; not, of course, because of what Kuhn said, though one had to be pretty naive if one could not see that his concepts of science and history were highly controversial; nor because I do not believe that the main stream of American linguistics changed course at this time; but because it led so many of Chomsky’s supporters to make events fit Kuhn’s revolution. But we will not understand it unless we realise that the impact of Kuhn’s book became a part of its history, and partly obscured its real origins. p. 28