

# Darwin, Kuhn, and Polanyi: A Comment on "Polanyi vs. Kuhn: Worlds Apart"

Richard Henry Schmitt

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*This article extends Moleski's discussion (in "Polanyi vs. Kuhn: Worlds Apart") of the worldviews of Kuhn and Polanyi in two ways: by considering an evolutionary view of science as proposed by Kuhn, and by evaluating Kuhn's notion of "paradigm change" compared to Polanyi's work on scientific practice.*

Martin X. Moleski, SJ, has again done a service to the community of scholars interested in Polanyi. Here he brings together interesting material on the relationship between Michael Polanyi and Thomas Kuhn; often he is bringing new materials to light. I am particularly interested to learn details of what Polanyi thought was wrong with Kuhn's early work and, on the other side, what Kuhn thought was wrong with Polanyi's work. The crucial objections from Polanyi, aside from those about priority, were that Kuhn did not acknowledge the commitment to truth within the scientific community and that he did not address the epistemological problems that arise in understanding that commitment. Kuhn's objections seem more diffuse, but the one that shocked me most was his off-handed idea that Polanyi relied on "something very like ESP" in his explanation of scientific discovery - a misunderstanding that apparently arose from Polanyi's misleading analogy in 1946 between ESP and anticipation, intuition, and coincidence in scientific discovery.<sup>1</sup> I also wonder whether Kuhn really understood the connections and experiences that lie behind "Polanyi's extrapolation from freedom in science to the political sphere."

Of course both of them would deny, and sometimes did explicitly deny, that such criticisms properly stated their respective positions. There was nothing extra-sensory about Polanyi's understanding of human interaction: on the contrary, he is very concrete about how humans learn skills and embody that learning, about how they emulate and empathize with others, about how they can focus and shift their attentions, about how they orient themselves to authority and community. And, on the other side, Kuhn seemed to see by 1969 how the "initial formulation" of his viewpoint had led to "gratuitous difficulties and misunderstandings" and how claims for paradigms as concrete puzzle-solutions had led to "controversies and misunderstandings ... particularly for the charge that I make of science a subjective and irrational enterprise."<sup>2</sup> Such an admission disarms any suspicion that he meant it that way.

So, we have this clash of views, these mutual misunderstandings, along with the questions about priority and influence, and the acknowledgement of others. As it stands this story has importance to scholars of the history of science in the mid-twentieth century. But we also have to think about what it means for our work going forward, including our work as scholars. Along those lines, I want to extend Moleski's analysis in two ways: I want first to look at the argument presented in these texts over a Darwinian approach to progress in science, and, second, to investigate what Kuhn's notion of "paradigm change" gets us compared to Polanyi's work.

I am interested in the first topic because I would have to agree with Kuhn that we should take some kind of Darwinian or evolutionary metaphor seriously - if that comparison is understood correctly. Here we need

to disentangle the issues, and try to understand what is at stake in Polanyi's marginal comments to the last few pages of *Structure*, where - by Kuhn's own admission at the time - the word "truth" makes a surprisingly belated appearance.

I am interested in the second topic because it seems to me that Kuhn's argument in *Structure* has serious limitations. Kuhn's analysis of "normal science" - by which he means, at least sometimes, tradition and convergence of thinking - is essentially represented by textbooks; and he is right to suggest that textbooks, which attempt to present science within a pedagogically clear but necessarily fixed framework, will on occasion need "revolutionary" revision, rewriting from a fresh "paradigm". But that is really a different issue from the questions that Polanyi worked at, including how we get to such a textbook-rewriting moment working along within a tired old paradigm. I will argue that Polanyi was much more concerned with the inner, embodied experience of being a scientist, not just theories and schema in the head; this was something that he had experienced over a long part of his life. Kuhn, though he got his Harvard PhD in Physics, was employed between his dissertation (1949) and publication of *Structure* (1962) as a Harvard junior fellow, a Guggenheim fellow, and a teacher of the history of science at Harvard and Berkeley - perfectly legitimate endeavors - but not working as a scientist doing physics research.<sup>3</sup> Indeed the initial insight about scientific revolution seems to have come to Kuhn in 1947 when he interrupted his physics project to "prepare a set of lectures on the origins of seventeenth-century mechanics."<sup>4</sup> Thus the contrast in worldview between Kuhn and Polanyi reflects a contrast in their work experience as well as differences in framework. To me, this means that Polanyi's work has a value to my own work, as it would have had to Kuhn. But he did not seem to appreciate or acknowledge that when he conceived his idea of revolutions in scientific frameworks. Kuhn, in *Structure* at least, uses paradigm change to argue against a very different view of science from Polanyi's view.

### 1.) A Darwinian approach, fitness to survive, and truth

First let us look at what Kuhn says in those last few pages of *Structure*, where - as Moleski tells us (p. 11) - Polanyi writes in the margin, "This really needs analysis." At this point, Kuhn had written, "We may ... have to relinquish the notion ... that changes of paradigm carry scientists ... closer and closer to the truth" (170).<sup>5</sup> Much of what follows in *Structure* is Kuhn's construction of a Darwinian approach, as Moleski illustrates (p. 11) with two quotes, one containing a specific sentence that Polanyi focused on. In that sentence, Kuhn suggests that each successive stage in the development of scientific knowledge "may have occurred, as we now suppose biological evolution did, without the benefit of a set goal, a permanent fixed scientific truth ..." (172-173) - that is, without a single overarching paradigm producing an unbroken progressive march toward the ultimate goal.

Kuhn claims that the "most significant and least palatable of Darwin's suggestions" was his abolition of teleological explanation, citing Asa Gray's struggles with this problem (172). This leaves something to be desired, but Kuhn's real point is to raise a rhetorical question: "What could 'evolution,' 'development,' and 'progress' mean in the absence of a specified goal? To many people, such terms suddenly seemed self-contradictory" (172). In fact Darwin did work this out, in part because he worked within a tradition of "uniformism" - and perhaps Kuhn knows he did and is just leaving the exercise to students. Darwin did even acknowledge the possibility that there might be a God with a plan acting as the final cause, though he thought it presumptuous of us to think we could understand or explicate that plan. Primarily he made observations of existing geological formations, both in the inorganic and the organic kingdoms of nature, and he proposed

explanations of the processes that led to those results "by reference to causes now in operation." He most definitely did not think that evolution proceeded by means of revolutions or catastrophes; in fact, as Eldredge and Gould argued, he even slighted the possibility of "punctuated equilibriums". Nature does occasionally make leaps - or at least take a fall - contrary to Darwin's favorite Latin tag: *Natura non facit saltum*. Here Darwin was following the lead of his mentor, geologist Charles Lyell. But, while Darwin believed in the uniformity of natural causes over time, he also added significantly to our knowledge of the extent and diversity of natural variation; his experience on the *Beagle* showed that this was far greater than Lyell's evidence had suggested.

The critical point for us is that Darwin focused on particular, proximate causes rather than some final, though inscrutable, cause. In the case of nature, his theory about the origin of species depends upon naturally occurring variation, reproduction and inheritance, the struggle for existence that arises from geometric increase, and the resulting gradual but ineluctable selection of fitter individuals during periods of change. He did not know how inheritance occurs, what gave life the capacity to reproduce, or anything much about the causes of variation, but all of those things were clearly present in the record even without knowing their specific causes. His theory was about how natural selection would explain the origin of species. He shows that no intervention is required by a Supreme Being following Her Fixed Plan, just the processes of nature as we can observe them occurring today.

Darwin did admit that chance plays a part, in the kinds of variation that occur, in the changing environmental conditions that determine fitness, and in impeding or allowing the geographic distribution of a successful variation; and this was troublesome to many. Variations occur as the result of processes unknown to Darwin, and regularities in its operation were only the subject of speculation. But in the case of geological changes, they are - as Lyell had shown - clearly the results of observable processes such as sedimentation and erosion, the accumulation of shells and fossils, the growth of coral, earthquakes, volcanic eruptions, and glaciers. This is not chance, though the timing may be random. Natural selection is neither chance nor random, but a regular, law-governed process: it always applies over a long time the criteria of fitness in reproduction and survival, adaptation within the existing, often changing, polity of nature. True, what constitutes fitness in nature may not suit human tastes or understanding. Still, in the case of variation under domestication - more directly analogous to selection in scientific communities - it is clear that there are criteria of selection used by breeders, but no single overall plan or purpose that they universally follow. They all depend upon variation, reproduction, and inheritance, though some breeders will select pigeons, say, for speed, some for plumage, some for tumbling, etc. In contrast, nature always selects individuals having even a marginal advantage in survival and reproduction. So, for Darwin, attention shifts from God's master plan to admiration for the way in which the complexity and perfection of the natural organic world could result from the working out of such simple laws, causes still observably in operation and used by humans in domestication to produce startlingly divergent organisms, beginning from a few simple forms or even from one single form of living creature.

Where did Polanyi stand on applying an evolutionary model to science? He did not insist that science is unchangeable, or that it proceeds directly and inevitably toward perfection. Nor did he believe that science was only conceptual. He did insist that it was a practice and a community of practitioners. He did insist that there are diverse purposes, principles of selection, in operation within scientific communities, and that a commitment to truth is the ultimate determinant of the survival of variations, great and small, between theoretical explanations. He was, however, opposed to any inflexible paradigm, against seeing science as dogma - even in periods of "normal" science. And he opposed the imposition of any overall final plan from outside of science or any single purpose from within. Nothing here is incompatible with a Darwinian approach. In fact, where Kuhn

suggests paradoxes between "evolution" and "development" and proposes "revolution" as the major mechanism of evolution, Polanyi talks about proximate causes, about how new theories arise out of existing practice, and about how they are judged within the scientific community. I suspect that he was not annoyed about Kuhn's use of evolution per se, but that he had questions about how a scientific community would operate as a scientific community if it were to relinquish that commitment to truth as a test ineluctably applied, at least when temporary obstacles are overcome. Scientific training and practice and the community of practitioners ultimately determine the "fitness" of any scientific theory, and Polanyi observed in some detail how these processes operate "by causes now in operation" either in a period of "normal" or of "revolutionary" science.

So, Polanyi did not object to a Darwinian approach. His problem was with Kuhn relinquishing a commitment to truth as the measure of the adaptiveness of theories and practices. For Polanyi, one could forego teleology without relinquishing a search for truth. Giving Kuhn the benefit of the doubt, I accept that he did not really intend to suggest that the development of science was possible in the absence of some conformity to purpose and to practice. He did not say outright that science is fundamentally a subjective and irrational enterprise, though he may not have been unambiguously clear on this point. He only said that we did not need teleology, that we did not need to hypothesize a final cause, an idea that he had likely once held himself, and one that his undergraduates may have brought regularly to class, indeed one suggested by their textbooks, but one that other professionals in the history of science had already given up.

## 2.) Textbooks, paradigms, and working as a scientist

In the concluding sentence of *Structure*, Kuhn wrote, "Since [the evolutionary view of science developed here] is also compatible with close observation of scientific life, there are strong arguments for employing it in attempts to solve the host of problems that still remain" (173). Let us judge Kuhn's approach and Polanyi's on this basis.

Looking again at *The Structure of Scientific Revolutions* I have three reactions, which can be organized in a graded series. First, there is that annoying term, *paradigm*, that has been used - and abused - so often since the book appeared. Second, there is the feeling that we are dealing with a particular set of phenomena, not a complete approach to the history of science but only part of it: sometimes there is the crisis over the existing textbook formulation of a set of scientific problems, and this leads to a revolutionary replacement by a new formulation. There is, after all, some legitimacy to this pattern if we do not push it too far, or if we take it as the narrative for an introduction to scientific endeavor for those who have no direct experience with it. Then third, as I look at Kuhn's text in detail, I begin to wonder whether Thomas Kuhn himself understood it that way: as an approach that raised problems without giving them any single final answer, and that addressed difficulties, anomalies, in a specific early positivist view of scientific theories. So, perhaps Kuhn's questions were overtaken by the popular success of his coinage. I cannot entirely convince myself of this third hypothesis, but I see some evidence. There is no way to come to a definitive conclusion without getting deeply into Kuhn-'*Lehre*' (as Stephen Toulmin calls it),<sup>6</sup> but it is a useful hypothesis if it causes us to look carefully at what Kuhn was trying to accomplish.

My first reaction is of course to the clichés that "paradigm" and "paradigm change" have become, used to lend gravitas to every new marketing angle. Some of this was undoubtedly outside of Kuhn's control. It is of significance here only because it is a negative factor in my reaction. Kuhn's own definition simply says that a paradigm is an achievement that is "sufficiently unprecedented" to attract a following and "sufficiently open-

ended" to require further problem-solving (10). Both of these are requirements for any kind of evolutionary survival, indeed for any effective cause. He further says that such an achievement will provide models for "law, theory, application, and instrumentation" and that it will have "conceptual, observational, and instrumental applications" (43). But it is hard not to read "paradigm" as principally conceptual, as in his example from optics: light is first material corpuscles, then transverse wave motion, finally quantum-mechanical entities (photons) exhibiting characteristics of both. Such scientific conceptualization is by definition disembodied from the tacit experience of scientists.

My second reaction is more serious and, I hope, less dependent on popular distortions that may have crept into Kuhn's terminology. Kuhn's topic seems to be not a general theory for the history of science, but a special theory about revolutions that affect the presentation of science in textbooks. His story goes like this: "normal science" despite its ultimate shortcomings proceeds efficiently, delaying expensive retooling and working on a defined set of problems in the way that a factory produces widgets. In fact "normal science" proceeds in just the way that Kuhn is out to discredit as the overall explanation of science: it makes incremental improvements toward a final goal. But, faced with anomalies, normal science reaches a crisis, and scientists suffer feelings of uncertainty. Continuous progress is broken at such points.

Thus Kuhn presents a kind of "catastrophist" hypothesis: sciences repeatedly suffer from crises that are relieved only by revolution. Surely this is part of the story, just as revolutions play a role in political history, and sometimes - only rarely - these revolutions are conceptual. Necessarily, the frameworks of a science as presented in textbooks will be fixed, especially in retrospect. Necessarily, change will then appear as discontinuous, theoretical frameworks incommensurable, even though they may arise from a continuous process of tacit knowing. Such a historiography will then conveniently divide the history of science into epochs, and this has a pedagogical value. But this will disguise the processes in continuous operation: as Lyell observed about the catastrophist hypothesis in geology, "we see ... a desire manifestly shown to cut, rather than patiently to untie, the Gordian knot."<sup>7</sup> In this case, the Gordian knot involves how scientists could arrive at any achievement sufficiently unprecedented and sufficiently open-ended working within a tradition that enforces an element of apparent arbitrariness and that drastically restricts the possible observations. That such inflexible systems will ultimately suffer some sort of crisis, even collapse, is not at all hard to understand. Clearly, there then will be discontinuities in the overall conceptual framework of particular sciences over time. But we still need to unravel the processes of exploration and discovery, of transmission of the skills of observation and the making of effects, of variation in theoretical constructs, indeed of the encouragement of divergence in thinking, in matters both internal and external to the scientific endeavor, that are in operation. We need to look for the dynamic processes, and see that the crises and revolutions are merely epiphenomena.

Looking at Kuhn's text, I begin to suffer qualms about my criticisms. Was Kuhn perhaps aware of all, or at least some, of this? Was his presentation of "normal" and "revolutionary" science intentionally didactic, even a bit provocative? Did he actually intend a special, and not a general, theory of science? In *Structure*, he says that he has appropriated "paradigm" from the word for an accepted model or pattern, as for the conjugation of a group of regular Latin verbs. Six years before that, in the preface to *The Copernican Revolution*, he says that students in a General Education course on science will learn about these technical facts and theories "principally as paradigms rather than as intrinsically useful bits of information" (xi). So, was "paradigm change" really just a means to a pedagogical end? Who exactly was the intended audience?

Without doing a systematic search, I do find indications that Kuhn intended *Structure* as merely an outline of part of his position, as an argument against a particularly narrow and doctrinaire view of science, and

itself as an effort to force a new paradigm on the basis of which a new sort of history and philosophy of science would come into view. In the preface, Kuhn admits that he says "nothing about the role of technological advance or of external social, economic, and intellectual conditions" even though he also says, "One need ... look no further than Copernicus and the calendar to discover that external conditions may help to transform a mere anomaly into a source of acute crisis" (x). The problems with the Copernican revolution come up again later: "Though immensely important, issues of that sort [i.e., those outside of the breakdown of normal technical puzzle-solving] are out of bounds for this essay" (69). In Kuhn's first chapter, he admits that "Section XIII will ask how development through revolution can be compatible with the apparently unique character of scientific progress. ... this essay will provide no more than the main outlines of an answer, one which depends upon characteristics of the scientific community that require much additional exploration and study" (8). He also recognizes that this is a historical study that is itself seeking conceptual transformation, and that his own attempts to apply various intellectual dichotomies to his subject have made those distinctions "seem extraordinarily problematic" (8-9). And the same puzzlement arises again in the final section: it is clear that we can reject the simple notion that "there is some one full, objective, true account of nature and that the proper measure of scientific achievement is the extent to which it brings us closer to that ultimate goal" (171). But how then are we to understand progress in science and how does the model of crisis and revolution help us to unravel that process?

In the middle of all this, Kuhn tells us that a century ago he could have "let the case of the necessity of revolutions rest at this point" but that today he needs to address an interpretation of scientific theories "closely associated with early logical positivism and not categorically rejected by its successor," which "would restrict the range and meaning of an accepted theory so that it could not possibly conflict with any later theory" (98). We should take this very seriously, given that the essay was published in the *International Encyclopedia of Unified Science*, which began as a logical-positivist project. Kuhn is confronting this interpretation because it redeems and revitalizes the old idea that science progresses by incremental improvement, that it is merely puzzle-solving and the accumulation of facts. Now, if all that Kuhn is trying to do is to force us to abandon a doctrinaire view of science, to recognize that scientific frameworks are useful but can get frozen into dogma, to accept the necessity and the historical fact of revolutionary changes in framework, and to adopt fresh models and patterns of explanation when they are better adapted to the circumstances, okay: I can go with that. But then he brings us just to the point where the interesting questions begin. The catch is - and Kuhn knew and acknowledged this (though perhaps too fleetingly) - others were already past that point. Kuhn's argument was not with the likes of Michael Polanyi, Stephen Toulmin, Norwood Russell Hanson, and certainly not with the Thomas Kuhn who wrote *The Copernican Revolution*. Instead he is arguing either with a naïve sort of undergraduate who believed in teleological progress or with a positivist sort of philosopher who believes that theories always exhibit a logical inclusiveness of preceding theories - to Kuhn their notions about science suffer the same inadequacy. *The Structure of Scientific Revolutions* offers an argument against views of science that see it as progressing toward a unitary goal. It does not say much about the processes by which it does evolve, diverge, and select, even though Kuhn was aware that there was already a literature on that subject, with a lot to say.

When I think about Michael Polanyi's work, undertaken after a lifetime of active scientific practice, I am struck by the historical circumstance that Polanyi already had a lot of answers to Kuhn's questions in *Structure*, to the seeming contradiction of "evolution" and "development", to the paradox of having a commitment to truth without the need for a single fixed goal as in teleological explanations. Polanyi explains how both technological advances and other external changes in the intellectual climate produce new variations;

and he shows that this is built into the tacit knowing of practitioners. He shows how "inheritance" works through commitments to the scientific community and by the process of training and apprenticeship within science. He explains how scientific advances are not just a matter of conceptual change, not just shifts in mental framework, but a process of continuing adaptiveness in practice and in judging the results. He does not rely on final causes, but shows us that personal knowledge is the result of causes still in operation, a series of small adjustments that sometimes can produce a wholly new framework with a whole new set of problems in its wake, but without need of Divine intervention.

In the end, I am left with my own experience of applying Polanyi's useful insights. They provide a rich story of how humans within a scientific community explore and engage in knowing; how they adapt their concepts to what they find; how those concepts lead to new techniques, new facts, and new concepts; and how the activity and the skills are passed from generation to generation. Polanyi and others were already providing answers to the didactic questions that Kuhn raised in *Structure*. That would explain to some extent why Polanyi joined forces with Kuhn, and also what Kuhn saw, though perhaps only superficially, in the work of Polanyi. In Kuhnian terms, we can see this as indicating a difference of worldviews, and we could heighten the contrast and thereby make them into competing paradigms within a revolutionary period in the history of science. Yet, in Polanyian terms, we can see this as arising from differences in their working experiences and in the purposes that each had in writing. Kuhn is reporting on his own realization that science does *not* progress by increments toward a single account of nature as its final cause, a point he was making to undergraduates at Harvard as early as 1947. Polanyi is reporting on his own lifelong personal experience with the processes by which science *does* operate, its modes of variation, inheritance, adaptation, divergence, and selection.

In *The Structure of Scientific Revolutions* (1962), Thomas Kuhn raised questions about scientific theories. By then, others looking at scientific practice - Michael Polanyi among them - had answers.

## Endnotes

<sup>1</sup>Michael Polanyi, *Science, Faith and Society*, 2d ed. (Chicago: University of Chicago Press, 1964): 35-38. The analogy appears in the 1st ed. (1946), but Polanyi states in his "Background and Prospect" for the 2d ed. that he is "doubtful today" about this notion (14). Certainly ESP plays no such role in *Personal Knowledge* (1958). So, it is likely that the early familiarity with Polanyi that Kuhn mentions in his letter to Poteat (1967) may have ended prior to the publication of *Personal Knowledge* (see Moleski's article, this issue, pp. 12-15).

<sup>2</sup>"Postscript 1969," *The Structure of Scientific Revolutions*, 2d ed. (Chicago, University of Chicago Press, 1970), 174, 175. Citations throughout are to the 2d ed. unless otherwise noted.

<sup>3</sup>This is not a criticism of the statement in the last paragraph of Moleski's article where he correctly points out that both Polanyi and Kuhn shifted to philosophy because of a similar interest in the human sciences, without comparing their prior working experience.

<sup>4</sup>Thomas S. Kuhn, "Preface" to *The Essential Tension* (Chicago: University of Chicago Press, 1977), x.

<sup>5</sup>*Structure*, 2d ed.; in the 1st ed., paging within the final section (XIII) is the same but starts on p. 159 rather than 160; so, this appears one page earlier, on p. 169, in the 1st ed.

<sup>6</sup>Personal communication, a characteristic turn of phrase. This shorthand for study of a person's views derives from the conventional title for a book of teachings in German, e.g. *Musiklehre* as the title of a textbook on music. But here the subject would be Kuhn's theories and doctrines rather than, say, the history of science.

<sup>7</sup>See Stephen Toulmin and June Goodfield, *The Discovery of Time* (New York: Harper & Row, 1965), 163-170. The quote from Lyell appears on p. 168.