

# Polanyi vs. Kuhn: Worldviews Apart

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**ABSTRACT** Key Words: Michael Polanyi, Thomas Kuhn, philosophy of science, epistemology of science, metaphysics of science, relativism, paradigm, interpretative framework, disciplinary matrix, truth in science, *Personal Knowledge*, *Structure of Scientific Revolutions*, worldviews.

*Michael Polanyi's work has often been conflated with that of Thomas Kuhn. This article shows that although Polanyi and Kuhn both conceded the similarities in some aspects of their accounts of science, both were critical of the other's position. The key to a correct understanding of the tensions between the authors and their views is to recognize the clash of worldviews within which their philosophies of science were constructed.*

This article focuses on three related issues: the extent to which Thomas Kuhn's notion of paradigm may have been indebted to Polanyi's *Personal Knowledge*; the view that Polanyi and Kuhn borrowed from each other's work; and the need (in my view) to differentiate Polanyi's post-critical and metaphysical realism from Kuhn's relativistic tendencies.

In 1991, while working on the theme of intellectual conversion in Polanyi, I noticed some similarities between Polanyi and Kuhn.<sup>1</sup> It seemed to me that Polanyi's description of changing interpretative frameworks<sup>2</sup> strongly resembled Kuhn's description of paradigm change. At a superficial level, I thought, one might integrate Kuhn and Polanyi by finding a way of correlating their guiding metaphors. A paradigm might be recognized as the embodiment or the symbol of an interpretative framework; it is possible that an interpretative framework could include several paradigms, in the sense of model problems, the notion of paradigm that Kuhn first suggests.<sup>3</sup> The notion of paradigm has escaped from that narrow meaning, however, as Kuhn notes in the 1969 "Postscript" to *Structure* (181-182). In the larger sense of the word, "paradigm" covers much the same ground as "interpretative framework"; both of them: create jargon; identify significant data; suggest canonical interpretations of the data; divide one school from another; define formal operations for practitioners; are surprisingly fruitful, even when wrong; explain the history of science in a satisfying fashion; depend on commitment; exhibit a tacit/articulate structure.<sup>4</sup>

Kuhn's failed effort to substitute "disciplinary matrix" for some meanings of "paradigm" confirms this interpretation of the two positions, since the images suggested by "matrix" and "framework" are so much alike:

What do [the members of a community of specialists] share that accounts for the relative fullness of their professional communication and the relative unanimity of their professional judgments? To that question my original text licenses the answer, a paradigm or set of paradigms. ... For present purposes I suggest 'disciplinary matrix': 'disciplinary' because it refers to the common possession of the practitioners of a particular discipline; 'matrix' because it is composed of ordered elements of various sorts, each requiring further specification. All or most of the *objects of group commitment* that my original text makes paradigms, parts of paradigms, or paradigmatic are constituents of the disciplinary matrix, and as such they form a whole and function together.<sup>5</sup>

Neither Polanyi's "interpretative framework" nor Kuhn's "disciplinary matrix" have sold well in the marketplace of ideas. These hulking seven-syllable compounds have been driven offstage by "paradigm" and its

partner, “paradigm change,” terms which found a surprisingly large and enthusiastically receptive audience well beyond the bounds of the history and philosophy of science.

The success of Kuhn’s terminology is illustrated by the fact that Richard Gelwick, the first person to write a Ph.D. dissertation on Polanyi and a founder and leader in the Polanyi Society, used Kuhn’s terminology to analyze Polanyi’s accomplishments: there are ten entries associated with “paradigm” in the index, but no entries for “framework” or “interpretative framework”; Chapter 3 discusses Polanyi’s philosophy as “A New Paradigm.”<sup>6</sup> Gelwick does draw the same parallel between Polanyi and Kuhn that I do: “the affinity between [Kuhn’s] notion of the centrality of a paradigm in scientific practice and Polanyi’s demonstration of the role of a Gestalt-like framework of beliefs that shape and guide scientific discovery” (128).

In the conclusion of my 1991 paper, I argued that “There seems to be no larger horizon in Kuhn comparable to [the] metaphysical vision of Polanyi. It seems that everything Kuhn understands about paradigms can be mapped into Polanyi’s notion of interpretative frameworks, but not everything in Polanyi’s position finds a correlative structure in Kuhn’s” (27). I have not found any reason to modify this view in my last fifteen years of reading and reflection; in this essay, I present additional material that further substantiates my earlier conclusion.

In the spring of 1998, I interviewed Thomas F. Torrance in Edinburgh, Scotland, as part of my work on the Polanyi biography. He told me that Polanyi had, in effect, accused Kuhn of plagiarism and that Kuhn had conceded that he had, in fact, been influenced by Polanyi. In 2002, I read the whole of the Regenstein archives and found some interesting material on how Polanyi viewed Kuhn, but did not find the letter from Kuhn to which Torrance referred. I eventually found a copy of the letter in William Scott’s files with an annotation in Monika Tobin’s handwriting that suggests that Scott had acquired it from the Regenstein archives.<sup>7</sup> I do not know why the letter is not there now, although it may be that it was removed because it was not direct correspondence between Polanyi and Kuhn but a copy of a letter from Kuhn to William H. Poteat.<sup>8</sup>

In the first section of this paper, I will try to give a chronological overview of the remarks that Polanyi and Kuhn made about each other’s work. In the second, I will very briefly consider the debate about the significance of the similarities and differences between the two authors.

### **“If I Join Forces with Mr. Kuhn”: An Unfortunate Alliance**

In the fall of 1958, Polanyi gave a talk at the Behavioral Sciences Center in Palo Alto which Kuhn attended.<sup>9</sup> Polanyi, at age 67, had just published *Personal Knowledge*. Kuhn, age 36, had finished *The Copernican Revolution*, due to be published in the next year, and was laying the groundwork for *The Structure of Scientific Revolutions*.<sup>10</sup> Kuhn was familiar with *Science, Faith, and Society* (1946) and *The Logic of Liberty* (1951) because of his work in a course on the history of science at Harvard with James Bryant Conant, who held a doctorate in chemistry.<sup>11</sup> (The first edition of *Structure* was dedicated “To James B. Conant, Who Started It”; the dedication did not reappear in the second or third edition.)

There may have been some personal dialogue between the older and younger scholar during Polanyi’s visit to Palo Alto or shortly afterward. In 1961, Kuhn sent Polanyi an offprint of “Measurement in Modern Physical Science” with the inscription, “With thanks for past kindness and hopes for a present critique! TSK.”<sup>12</sup>

No other correspondence between Polanyi and Kuhn has yet surfaced; in Polanyi's "Key to Correspondence Files,"<sup>13</sup> there is no reference to Kuhn nor any other letters or notes in the files.

In all likelihood, the main dialogue between Polanyi and Kuhn about *Structure* took place at the Symposium on the History of Science at Oxford University, July 9-15, 1961, at which Polanyi replied to Kuhn's paper, "The Function of Dogma in Scientific Research,"<sup>14</sup> an abstract "in a drastically condensed form" from the soon-to-be published *Structure* (347, fn). Kuhn portrayed himself as an ally in Polanyi's fiduciary program:

Above all, those concerned with the importance of quasi-dogmatic commitments as a requisite for productive scientific research should see the works of Michael Polanyi, particularly his *Personal Knowledge* (Chicago, 1958) and *The Logic of Liberty* (London, 1951). The discussion which follows this paper will indicate that Mr. Polanyi and I differ somewhat about what scientists are committed to, but that should not disguise the very great extent of our agreement about the issues discussed explicitly below. (fn, 347)

Kuhn again complimented Polanyi in his reply to Polanyi's critique of "The Function of Dogma":

I doubt that Mr. Polanyi is well pleased with my notion of paradigm, and I know that many members of the Symposium were not. It therefore seems worth emphasizing that, although I have only recently recognized it as such, Mr. Polanyi himself has provided the most extensive and developed discussion I know of the aspect of science which led me to my apparently strange usage. Mr. Polanyi repeatedly emphasizes the indispensable role played in research by what he calls the 'tacit component' of scientific knowledge. This is the inarticulate and perhaps inarticulable part of what the scientist brings to his research problem: it is the part learned not by precept but principally by example and practice. (392)

Learning by precepts embodied in examples (apprenticeship) seems to have been what Kuhn most vividly remembered Polanyi talking about in Palo Alto.<sup>15</sup>

Polanyi accepted Kuhn's invitation to see themselves as allies in the same struggle:

The paper by Mr. Thomas Kuhn may arouse opposition from various quarters, but not from me. At the end of it he says that the dependence of research upon a deep commitment to established beliefs receives the very minimum of attention today. I could not agree more; I have tried in vain to call attention to this commitment for many years. I hope that if I join forces with Mr. Kuhn we may both do better. (375)

Polanyi acknowledged the profound agreement between himself and Kuhn: "A commitment to a paradigm has thus a function hardly distinguishable from that which I have ascribed to a heuristic vision, to a scientific belief, or a scientific conviction. ... I have also identified these commitments with the holding of the premises of science" (375). When Polanyi described the common ground that he found between himself and Kuhn, he showed how he understood Kuhn's notion of a paradigm:

[The account given by Kuhn] tears open and leaves open the main questions concerning the nature of scientific method and the foundation of scientific knowledge. The affirmation that *commitment to a framework of accepted beliefs* is indispensable to the pursuit of science

contradicts the current view which the founders of the Royal Society expressed 300 years ago by their motto *nullius in verba*—we accept no authority. (379; emphasis added)

For Polanyi, then, Kuhn’s concept of paradigm means “a commitment to a framework of accepted beliefs.”

While Polanyi saw himself and Kuhn as engaged in a somewhat similar work, he judged that Kuhn’s contribution fell far short of what was really needed: “I can accept the excellent paper by Mr. Kuhn only as a fragment of an intended revision of the theory of scientific knowledge. Otherwise it would not only fail to answer the questions it raises, but appear altogether to ignore them” (380). Polanyi evidently thought that his own effort in *Personal Knowledge* to revise the theory of scientific knowledge was beyond criticism on this score.

At some point in or after 1962, Polanyi photocopied the last three pages of *Structure* (170-172) and made a number of marginal remarks. Kuhn suggests that in coming to a “more refined” view of progress in science, “we may ... have to relinquish the notion, explicit or implicit, that changes of paradigm carry scientists and those who learn from them closer and closer to the truth.”<sup>16</sup> Polanyi replied in the margin: “*Truth*. !!!!!!! This *really* needs analysis.” In a notebook whose date I could not determine, Polanyi recorded this comment: “Notes on ‘Sociology of Knowledge.’<sup>17</sup> Professor *Thomas Kuhn* discovers ‘dogmatism’ in science is indispensable; but changes in dogmatic teachings suggest to him that science may not be moving towards greater truth; rather ‘one damned thing after another’ as Darwin has taught us to understand organic evolution.”<sup>18</sup> This is almost certainly a comment on Kuhn’s effort to portray the progress of science on the model of Darwinian evolution:

Does it really help to imagine 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? If we can learn to substitute evolution-from-what-we-do-know for evolution-toward-what-we-wish-to-know, a number of vexing problems may vanish in the process. Somewhere in this maze, for example, must lie the problem of induction.<sup>19</sup>

The process described in Section Xii as the resolution of revolutions is the selection by conflict within the scientific community of the fittest way to practice future science. The net result of a sequence of such revolutionary selections, separated by periods of normal research, is the wonderfully adapted set of instruments we call modern scientific knowledge. Successive stages in that developmental process are marked by an increase in articulation and specialization. *And the entire process may have occurred, as we now suppose biological evolution did, without the benefit of a set goal, a permanent fixed scientific truth, of which each stage in the development of scientific knowledge is a better exemplar.*<sup>20</sup>

Late in life, Kuhn complained that this suggestion had not received sufficient attention: “I would argue very strongly that the Darwinian metaphor at the end of the book is right, and should have been taken more seriously than it was; and *nobody* took it seriously. People passed it right by.”<sup>21</sup> Polanyi seems to have noticed it and (I believe) strongly disagreed with Kuhn’s view of science. In *The Tacit Dimension*,<sup>22</sup> written at the peak of his powers as a philosopher, Polanyi clearly expressed his commitment to the pursuit of truth in science: “The discoverer is filled with a compelling sense of responsibility for the pursuit of a hidden truth, which demands his services for revealing it” (25).

Polanyi and Kuhn both served on the Board of Editorial Advisers for *Minerva*, edited by Edward Shils. The first issue of the new periodical appeared in the fall of 1962, very shortly after the Oxford symposium on scientific change, and advertised itself as “An International Quarterly Review of the Social, Administrative, Political and Economic Problems of Science and Scholarship”; Polanyi’s essay on “The Republic of Science” appeared in that first volume (54-73). Whatever editorial work Polanyi and Kuhn may have done for Shils, it does not seem to have produced any further dialogue between the two men. In his 1963 “Background and Prospect,” a lengthy prologue to the second edition of *Science, Faith, and Society*,<sup>23</sup> Polanyi made two rather neutral references to Kuhn, mentioning him as one of ten “later writers whose conclusions overlap my own” (12) and noting Kuhn’s treatment of paradigmatic discoveries (13).

That same year (1963), Polanyi wrote to Gerald Holton, a professor of physics at Harvard, enclosing an offprint from the Oxford symposium on scientific change:

I had a discussion with Thomas Kuhn about his paradigmatic discoveries when he spoke about Dogmatism in Science at Oxford in July 1961. ... I would link your study of the thematic coefficient of science in a similar manner to my own enquiries on the premises of science, its suppositions about the general nature of things which serve as its heuristic vision, etc. I think I would come nearer than you do to Koyre’s formulation in attributing to these ideas a bearing (or intended bearing) on reality. ...

You will see that I criticized Tom Kuhn for not taking up the epistemological difficulties arising from the acknowledgment of dogmatism as he called it. *Personal Knowledge* was of course principally concerned with an attempt to answer this question.<sup>24</sup>

The next year (1964), Polanyi wrote William T. Scott expressing some anxiety about not getting the credit he deserved for his groundbreaking work:

Thanks very much for your account of my writings about science. I do not want to accuse others who have said similar things more recently of not having thought of them for themselves, but I am always worried that if I say nothing about my priority I shall be accused of plagiarizing others who wrote on similar lines later. I don’t think this can be altogether avoided, but I wanted just to share my thoughts with you as a friend.<sup>25</sup>

It has been said that the strongest force known to scientists is fame. Polanyi himself portrayed scientific choices as being guided by a proper form of self-interest: “The line the scientists must choose turns out, therefore, to be that of greatest ego-involvement; it is the line of greatest excitement, sustaining the most intense attention and effort of thought.”<sup>26</sup> In his scientific career, Polanyi was often embroiled in debates about who should get credit for priority in making key discoveries, concerning both his own work and that of his colleagues. Polanyi’s concern about not being given due credit for his philosophical accomplishments came to a head when reading Cahal B. Daly’s manuscript, “Polanyi and Wittgenstein,” which was being prepared for *Intellect and Hope: Essays in the Thought of Michael Polanyi*.<sup>27</sup> While comparing Polanyi and Wittgenstein in their use of Gestalt psychology in the sixth section of his essay, Daly makes an extended commentary on similar themes found in *Structure*: “Thomas S. Kuhn, in independent research into the history and epistemology of science, provides remarkable confirmation of Polanyi’s interpretations and, at the same time, unexpected

support for my comparison of Polanyi with Wittgenstein.” Daly then identifies seven points of similarity; like Polanyi, Kuhn:

1. Stresses the role of tradition, consensus, authority, within the scientific community as important determinants of research and results in science [SSR, 4-6, 18-20, 103, 148-151, 157-158, 163-165, 167-169].
2. Stresses the “tacit” character of demarcatory rules and methodological principles in science [46-49, 54-55, 110].
3. Calls attention to the place of intuition, personal insight, in scientific discovery [121-122, 131-134].
4. Describes the importance of trained perception in scientific observation, in terms that recall Polanyi’s frequent analyses of “connoisseurship” in science [110-111].
5. Finds an important place in scientific verification for aesthetic categories and for “faith” [156-157].
6. Shows how science is modified by the high-level “quasi-metaphysical commitments” with which it is associated in different periods and from which it can only with difficulty be distinguished [41; cf. 4].
7. [Interprets] revolutions of scientific theory in terms of “perceptual shift” or “paradigm shift” [84-85, 117-119]. [Kuhn] invokes in this connection Wittgenstein’s celebrated simile of the figure that can be seen alternately as a rabbit’s head or as a duck’s [85, 110-111. Compare Wittgenstein, *Philosophical Investigations*, II xi (193 *et seq.*).<sup>28</sup>

Polanyi was somewhat distressed by Daly’s claim that Kuhn had done “independent research” on these themes. On February 15, 1967, he wrote Poterat:

I hesitated for a long time whether to mention this matter to you, but I think I ought to do so.

The contribution by the Rev. Cahal B. Daly draws an interesting parallel between certain ideas in *Personal Knowledge* and similar ideas to be found in *The Structure of Scientific Revolutions* by Thomas Kuhn. ...

This parallelism has been mentioned to me before, last time by Lon Fuller at our meeting in Bowdoin. He asked me how I could account for this communication of ideas by Kuhn without reference to their publication by me. I answered that Kuhn probably felt that he would be misunderstood as agreeing with my general position, if he quoted parts of my analysis of science. I think this explanation may be largely to the point, though perhaps I put it forward to some extent in order to avoid the embarrassment of having to express my disapproval of Kuhn’s action.

I am writing to you about this today because I am supposed to comment on the contributions to your volume at some stage and [the] Daly manuscript explicitly says that Kuhn came to these conclusions independently (see last line on page 42 [of Daly's manuscript]). Now I do not want to have to deny this, nor would I think it right to sanction it, as I would be taken to do if I commented on these essays and did not challenge it.

Personally I doubt that Kuhn was quite independent in his relation to *Personal Knowledge* and my previous work, which all the way back to *Science, Faith and Society*, contained many of these ideas, but I do not want to say this, if for no other reason, because I am not sure about the fact. In any case, it would be important to know whether Kuhn himself believes that he has been independent or else influenced in some degree by my writings.

I would suggest, therefore, that this matter should be clarified. It would be quite simple for the Rev. Daly to inquire of Kuhn. It might also be that you yourself would regard it of interest to know whether Kuhn's work was in these respects quite independent or perhaps less independent.

I am sure that Kuhn was acting in good faith and might himself be anxious to clarify this matter.

I am afraid this is where I have to leave it myself so far as your publication is concerned. If nothing happens I shall not raise the issue.<sup>29</sup>

Potyat evidently wrote Kuhn on February 22; Kuhn replied on February 28. Struan Jacobs, who has written detailed analyses of the intersection between Kuhn and Polanyi, hopes to publish this correspondence in full. The points that Kuhn makes in his reply are:

1. He is doubtful about how to answer the question posed by Polanyi and Potyat.
2. He was familiar with *Science, Faith, and Society*<sup>30</sup> and *The Logic of Liberty*<sup>31</sup> from his work in James Bryant Conant's course at Harvard on the history of science.
3. He disagreed with Polanyi's extrapolation from freedom in science to the political sphere.
4. He disliked Polanyi's reliance on "something very like ESP" in scientific discovery.
5. He consciously chose not to mention Polanyi in his preface to *Structure*. Polanyi's writings only provided him "with encouragement not with significant substance."
6. Polanyi's talk at Palo Alto on apprenticeship may possibly have helped Kuhn develop his notion of "paradigms in the sense of concrete examples of scientific achievement" and he might therefore owe Polanyi "a major debt."
7. Daly's assertion of Kuhn's independence from Polanyi should not be "allowed to stand."

In the event, Poteat did not require Daly to change his text. Despite Kuhn's admission, however equivocal, that he might be greatly indebted to Polanyi, Poteat allowed Daly's assertion that Kuhn was independent of Polanyi to remain in the article.

In the spring of 1968, Marjorie Grene was preparing the collection of Polanyi's essays that were eventually published as *Knowing and Being*.<sup>3</sup> Grene and Polanyi disagreed about which essays should be included in the collection. In making the case that only Polanyi's best work should be republished, Grene noted how the recognition of similarities between the epistemologies of Polanyi and Kuhn could lead to the suppression of the deep differences in their philosophical vision:

The reason I am fuming just now is: in the B.U. Studies '64-6 I find a reference in [Marx W.] Wartofsky's paper<sup>33</sup> in which he first uses Kuhn's acknowledgment of indebtedness to you to call Kuhn's position (selbstverständlich) "fideistic obscurantism" and then proceeds to *discover*, for himself, that what scientists are *really* doing is to try to find out the structure of reality—this is 3 or 4 pages that could have come straight out of *PK*—this sort of thing makes me so furious, I don't want to be a party to publishing statements of yours that are less than the absolutely plainest and clearest you can do.<sup>34</sup>

Wartofsky had conflated Polanyi's worldview with Kuhn's:

The task at hand is not to describe the relation between metaphysics and science, but to explain it. Popper fails to answer this question; Agassi and Kuhn fail to ask it. In Kuhn's case, with respect to his debt to Polanyi, we might guess that the prospective answer lies in Polanyi's account of what he has called 'heuristic vision' (which Polanyi himself suggests is like what Kant means by 'paradigm').<sup>35</sup> If it does, then I would judge that *this* alternative to positivism lies in the direction of a total abandonment of rational objectivity in science, and substitutes instead a voluntarist obscurantism. ... Unless a more adequate account is forthcoming, of something we might call 'scientific realism' which does not simply take metaphysics into account, but copes with it seriously, we are left with the alternatives of Polanyi-Kuhn fideism; or else, with a reform within the framework of positivism ... (148-150)

In a later work, Wartofsky again identified Polanyi and Kuhn as "emphasizing the subjective and irrational components in the contexts of scientific observation."<sup>36</sup> Whatever publicity Polanyi may have gained by being associated with Kuhn, it came at the cost of having his philosophical vision overshadowed by Kuhn's more relativistic, anti-metaphysical outlook.

Polanyi's letters late in life show that he was not mollified by Kuhn's equivocations about whether or not he had profited from his acquaintance with Polanyi's work. In 1970, three years after his fruitless correspondence with Poteat, Polanyi reiterated his claim to priority in a letter to Donald T. Campbell, a professor of psychology at Northwestern University. He began by noting that "Genius in Science"<sup>37</sup> sums up his position, basing itself mainly on quotations from *Science, Faith, and Society* (1946, 1964).

I felt that now that this work [i.e., *Science, Faith, and Society*] will be twenty-five years old when my paper ["Genius in Science"] reaches its public and I myself will be eighty at that time, it would be appropriate to take this occasion for declaring my claims which have been



ignored consistently in the literature of professional philosophy. ... I shall not go into details, but will mention as an example Kuhn's *Structure of Scientific Revolutions* (1962). I would say that its content largely repeats, without reference to their origins, the ideas I have developed in my previous books. If you have a copy of "Intellect and Hope" by Langford and Poteat, you will find on page 161 a whole list of 'confirmations' of my ideas by Kuhn. As to the rest of this book, it seems to me that most of it is nonsense. The reputation which Kuhn has earned is comparable only with that of Karl Popper whose writings, so far as they deal with science, seem to me just plain nonsense. So you see ... I have been ... alienated from the philosophic literature about science. ... What I think more broadly about my relation to the philosophy of science, I wish to explain only to you and shall not say it in public.<sup>38</sup>

A year later (1971), Polanyi made essentially the same criticism of Kuhn in a letter to Paul Halmos, a professor of sociology at University College, Cardiff, and the founder of the *The Sociological Review Monograph*, which had published Richard D. Whitley's article, "Black Boxism and the Sociology of Science: A Discussion of the Major Developments in the Field."<sup>39</sup> Whitley made a number of points that may have irked Polanyi:

Accepting Kuhn's sociology of science [M.D.] King presumably also accepts his epistemology according to which science cannot be said to progress over paradigm changes since there are no criteria common to the two paradigms. ... [The sociological import of Kuhn's position] is based on the role of tradition in sociological research, the practical but unformulated and non-rational styles of scientific work which are passed from Master to Apprentice by mimesis (cf. Polanyi, 1964 [Science, Faith, and Society]; 1966 [The Tacit Dimension]: 1-26, 61-80). (68)

Kuhn, in the postscript to the 1970 edition of his *Structure of Scientific Revolutions*, appears to allow for some element of rational choice in paradigm changes. Nevertheless, sociologists continue to see such changes as religious conversions or gestalt switches with no epistemological rationality, possibly because they wish to be able to explain all aspects of scientific change in sociological terms. (72)

While Polanyi had been saying similar things previously, at a time when interest was growing in the sociology of knowledge and the sociology of science Kuhn offered an intuitively appealing framework. (78)

By talking of authority, intellectual and social, Kuhn has begun to outline a mechanism of the production and alteration of scientific ideas. In asserting the impossibility of comparing the truth content of paradigms, however, and the concomitant psychological conversion process between paradigms, Kuhn has denied the possibility of progress in scientific knowledge since there can be no universal criterion for evaluating scientific knowledge across paradigms. Although in his later papers Kuhn asserts that progress does occur, he does not outline what such progress is or how it occurs (1968; 1970a; 1970b). Particularly he does not explain how science progresses if the truth content of a paradigm is incommensurable with that of its successor. In fact, Kuhn appears to follow Polanyi in relying on the collective wisdom of the scientific elite to ensure 'correct' decisions for progress will be made (Polanyi 1964 [Science,

*Faith, and Society*]; 1962 [*Personal Knowledge*] 216-222). This position suggests that the search for a universal criterion of knowledge will a priori fail. Knowledge is then defined purely conventionally by whoever happens to be a member of the elite at the time. (79)

In his letter to Halmos, Polanyi clearly wished to differentiate his position from Kuhn's while still laying claim to priority in developing some of the concepts that had made Kuhn famous:

My first book was a sequence of three essays published in 1946 and is now available as a paperback under its original title *Science, Faith and Society*. My main contributions after that were in the *British Journal for the Philosophy of Science* and in two books, *The Logic of Liberty* in 1950, and the outcome of my Gifford Lectures of 1950-51 published as *Personal Knowledge: Towards a Post-Critical Philosophy* in 1958. All these works and some later contributions as well were well-known to Kuhn, with whom I discussed personally at considerable length his project of the book which was to come out in 1963. ... The main point ... is that I have been critical of Kuhn's work for years before he published it and still regard it in this light.<sup>40</sup>

Joining forces with Kuhn brought Polanyi decidedly mixed results. On the plus side, Polanyi gained some recognition for his work insofar as it resembled Kuhn's position. In the Regenstein archives, there is a copy of Ron Johnson's newspaper article, "On Scientific Knowledge," which lauds Kuhn for inspiring a revolution in the philosophy of science. One sentence of the article is heavily underlined in black ink: "Michael Polanyi, after years of crying in the wilderness, has seen his arguments emphasizing the intuitive and tacit nature of the scientific 'craft' become more widely circulated and accepted"<sup>41</sup>—the implication, of course, is that Polanyi's work was being read not on its own merits but as a consequence of being associated with Kuhn's philosophy of science. The downside of this kind of "success" was that Polanyi's convictions about the metaphysical foundations of science and personal knowledge vanished when his work was viewed through a Kuhnian lens.

Kuhn did give Polanyi some credit in the second, enlarged edition of *Structure* (1970). In the text, Kuhn writes that "the existence of a paradigm need not even imply that a full set of rules exists" (44). To this he attached a footnote:

Michael Polanyi has brilliantly developed a very similar theme, arguing that much of the scientist's success depends upon "tacit knowledge," i.e., upon knowledge that is acquired through practice and cannot be articulated explicitly. See his *Personal Knowledge* (Chicago, 1958), particularly chaps. v and vi.

In the Postscript, Kuhn again praises Polanyi:

For [a man who did not know even the problems] the generalization could begin to function only when he learned to recognize ... something, prior to the law, about the situations that nature does and does not present. That sort of learning is not acquired by exclusively verbal means. Rather it comes as one is given words together with concrete examples of how they function in use; nature and words are learned together. To borrow once more Michael Polanyi's useful phrase, what results from this process is "tacit knowledge" which is learned by doing science rather than by acquiring rules for doing it. (191)

The next section of the Postscript (191-198) is entitled “Tacit Knowledge and Intuition.” Other than the phrase “tacit knowledge,” I am not confident that Polanyi would find much of his own understanding of tacit knowing reflected in these pages.

In October 19-21, 1995, Aristides Baltas, Kostas Gavroglu, and Vassiliki Kindi conducted an autobiographical interview with Kuhn in Athens, Greece.<sup>42</sup> Kuhn was 73 and would die the next year. In this interview, Kuhn gave an account of how Polanyi did and did not influence him that seems somewhat difficult to square with his letter to Poteat and the compliments tendered in the second edition of *Structure*. As in his letter to Poteat, Kuhn admitted that Polanyi’s presentation in Palo Alto may have helped him with his key insight into paradigms, but seemed to have changed his mind about how brilliant Polanyi had been in his discussion of tacit knowledge:

Now, a question I don’t know the answer to—this is a point at which my work is often linked to Polanyi’s. Polanyi came to the Center that year and gave a lecture on tacit knowledge. I liked the lecture all right, and it’s possible that it helped me to get the idea of paradigm, although I’m not sure. There is no great reason why it should have, because tacit knowledge was also propositional knowledge in some sense or other. ... We need to find something ... that’s not propositional ... (296)

Then Kuhn claims not to have finished reading *Personal Knowledge*:

We did read some Polanyi in the Conant course. Conant introduced him to the course, and I liked it quite a lot—I don’t remember just what it was, except that I kept feeling terrible at those points where he spoke as though extrasensory perception was the source of what scientists did. I didn’t believe that. That ... gets into the tacit knowledge thing also. I don’t know. But Polanyi was certainly an influence. I don’t think a great big one, but it was helpful to me to have him out there. In that connection, another story—two books that came out while I was trying to write *Structure*. One of them was Polanyi’s *Personal Knowledge* and another was Toulmin’s *Foresight and Understanding*. Particularly with *Personal Knowledge*, I looked at it and said, I *must* not read this book now. I would have to go back to first principles and start over again, and I wasn’t going to do that. ... Later, when I did try to read *Personal Knowledge*, I discovered that I didn’t like it. I never got through that early bit about statistics, which seems to me just way off, quite wrong. (296-297)

The elder Kuhn seems to have decided that there was no “major debt” to be paid to Polanyi after all. Despite the common ground that they may have recognized in their first exchanges, by the end of their lives, neither wanted much to do with the other.

### **Appraisals of the Polanyi-Kuhn Connection**

In 1989, Maben Walter Poirier wrote “A Comment on Polanyi and Kuhn”<sup>43</sup> in which he opposed those like Imre Lakatos who saw both Polanyi and Kuhn affirming “truth by consensus” (260-261):

For Polanyi, truth in general, and in the natural sciences in particular, is understood to be a fundamentally correct insight into the real, as it is independent of human thought processes.

... Truth, for Polanyi, is not to be found in the collective aspirations of the community of scientists, or of its leading members, as seems to be the case for Kuhn. It resides in the judgment of a scientist, who, because of his feel for a particular subject, correctly claims that here is the real. ... Polanyi is a philosophical realist, and not a radical relativist like Kuhn. (261)

Kuhn never accepted the charge that he was a relativist;<sup>44</sup> he saw himself as “a Kantian with moveable categories” who reinvented the British logical empiricist tradition (264, 321). Kuhn certainly did not share Polanyi’s explicit affirmation of the role of truth in the scientific framework. He criticized Larry Laudan for upholding “the traditional view of scientific progress, closer and closer to the truth, absolutely dropping the problems [I had] pointed out. From my point of view, that’s very bad stuff!” (321). Kuhn turned down an invitation to testify against creationists who were using *Structure* in support of their case: “I didn’t think there was any way in the world in which somebody who didn’t quite believe in Truth, and getting closer and closer to it, and who thought that the essence of the demarcation of science was puzzle solving, was going to be able to make the point. ... I thought I would do more harm than good” (322).

In my view, Kuhn’s resistance to being identified as relativist was merely rhetorical, not real; science solves “puzzles” decisively by rejecting some solutions and keeping others through an implicit appeal to the way things really are. Kuhn’s reluctance to affirm the role of truth in the progress of science leaves him with a very stunted epistemology. Poirier shows that, on this point, Polanyi and Kuhn were antagonists, not allies:

Polanyi is ... guided in his search for the truth in science by a universal criterion, namely, reality[.] ... For Polanyi, it is commitment to the real (to the universal criterion) that serves as the reference point, that keeps scientists within the straight and narrow, and not arbitrary decision-making by the sanctioned authorities. ... It is the scientist who decides [when the essential conditions for implementing approved procedures have been realized]. Likewise, it is the scientist who determines the future course, when approved procedures are themselves in dispute. This is Polanyi’s point all along, and to the extent that scientists are committed to reveal the real, there is absolutely nothing lawless, arbitrary, mystical or autocratic here. (262-263)

Poirier then gives what is, for me, a most plausible suggestion about how and why Polanyi failed to separate himself from Kuhnian relativism:

Michael Polanyi may have inadvertently contributed to the confusion. Having spent a good part of his life struggling against the very school of thought which was to offend Kuhn and his associates, and forced, in a manner of speaking, to accept help and support in this struggle from wherever he could find it, Polanyi may not have been as careful as he should have in maintaining the separation between himself and his comrades-in-arms. (264)

To transpose Polanyi’s comments on Kuhn at the Oxford symposium, bringing out the hostility that Polanyi only expressed in notes to himself or in private correspondence, the part of Polanyi’s work that is “barely distinguishable” from Kuhn’s is merely a “a fragment” of Polanyi’s “intended revision of the theory of scientific knowledge.”<sup>45</sup>

In an article written in 2002, Struan Jacobs argues that it is “no coincidence”<sup>46</sup> that some parts of Kuhn’s work are virtually indistinguishable from Polanyi’s:

Among the few thinkers to comment on Polanyi's effect on Kuhn is MacIntyre<sup>47</sup> who states that Kuhn was 'indebted' to Polanyi for his account of science but that 'Kuhn nowhere acknowledges any such debt' ... [a] thinly veiled allegation that Kuhn had plagiarized from Polanyi. (107-108)

Jacobs holds that "Polanyi presaged Kuhn and Feyerabend's motif of incommensurability. ... one of the major metascientific themes in recent decades" (108). However, unlike Poirier, Jacobs seems to prefer Kuhn's view of the incommensurability of paradigms and criticizes Polanyi for not recognizing the flaw in his understanding of conflicting interpretative frameworks: "Polanyi's thinking on the logical gap between conceptual frameworks runs counter, however, to his anti-relativist strand. Frameworks on either side of a logical gap are not contradictories; they do not make incompatible claims about the same subject matter" (118).

In his article on "The Genesis of 'Scientific Community,'" Jacobs does not raise the issue of plagiarism, but demonstrates in a very powerful fashion that Polanyi must be given credit for coining the term and exploring its ramifications "for the best part of 20 years before Kuhn began employing it."<sup>48</sup> He thoroughly demolishes Kuhn's claim to have derived it from reading Ludwig Fleck's work, *Genesis and Development of a Scientific Fact*.<sup>49</sup> In a phrase reminiscent of his comments on Polanyi's influence on his thought, Kuhn said, "I have more than once been asked what I took from Fleck and can only respond that I am almost *totally uncertain*" (158). After surveying the development of the term in Polanyi's work, Jacobs implies that Kuhn ought to have realized the true source of the phrase:

Kuhn, we have seen, did not obtain the term 'scientific community from Fleck whose relevant expression—translated as 'thought collective'—denotes a considerably wider class of social groups. The possibility that Kuhn coined the term independently cannot be excluded but, by the same token, Kuhn knew his Polanyi, so Polanyi may well have been Kuhn's source for the term.<sup>50</sup> Even if Kuhn coined 'scientific community' independently, Kuhn's reading of Polanyi may have served to confirm the expression's value in his mind. (165)

Jacobs then shows that there are striking differences between the way the term functions in Polanyi and Kuhn:

When Polanyi wrote on the scientific community, some 20 years after having coined the term, he did so under the rubric of 'The Republic of Science,'<sup>51</sup> a phrase coined by the French political philosopher, Bertrand de Jouvenal. In this later essay, Polanyi describes the scientific community as a model of a liberal society. Kuhn and Fleck, on the other hand, lay greater emphasis on orthodoxy and conformity, and it surely tells us something about the complexion of Kuhn's *Structure of Scientific Revolutions* that the word 'freedom' never appears in the body of the work. (166)

Polanyi, I am sure, would be very grateful for the way Jacobs' meticulous articles establish Polanyi's priority in "the emergence of a genuinely social perspective on science" (157), although I suspect that he might disagree with Jacobs' tendency to undervalue his realism. It also seems to be a stretch to say that Polanyi was the only person wrestling with the reality of the interpretative frameworks by which we make sense out of the world. As so often happens, that question was "in the air" at that time, and Kuhn deserves full credit for introducing the term that has dominated the market ever since.

In the same year that Jacobs was carefully differentiating Polanyi's stance from Kuhn's, David K. Naugle illustrated the more common tendency to conflate the two:

There is a remarkable confluence between Polanyi's understanding of the tacit character of the scientific enterprise and Thomas Kuhn's revolutionary concept of the 'paradigm.' In fact, Kuhn credits Polanyi with leading him to his notion, as he explains in an address at a 1961 symposium entitled "The Structure of Scientific Change" at Oxford University: "Mr. Polanyi himself has provided the most extensive and developed discussion I know of the aspect of science which led me to my apparently strange usage [of paradigms]. ..."<sup>52</sup>

Naugle then proceeds to consider how Kuhn capitalized on Polanyi's insights observing that

Michael Polanyi's ideas of 'personal knowledge' and the 'tacit dimension' led Kuhn to develop his celebrated doctrine of the paradigm, thereby inaugurating what Edwin Hung has called the 'Weltanschauung Revolution.' (198)

This is too loose a reading of Kuhn's text. Kuhn does not say that Polanyi's discussion led him to his insight into paradigms; instead, Polanyi is only given credit for discussing that "aspect of science" which was Kuhn's inspiration. In other words, Kuhn says that he made his breakthrough by thinking about the same aspect of science that Polanyi did; he does not, in fact, give Polanyi credit for bringing that aspect of science into view for him. Even more unfortunately, in a book dedicated to demonstrating the centrality of worldviews to the whole of human life (xi), Naugle does not seem to notice the profound differences between Polanyi and Kuhn's worldviews.

In *Return to Reason*,<sup>53</sup> Stephen Toulmin says that everybody is keen to be "the 'Newtons' of social theory" (47) or the "Newtons of the human sciences" (55). Both Polanyi and Kuhn left their work in chemistry and physics to take up philosophy because they felt that there were important discoveries to be made in the human sciences. The surprising coincidence of their views on frameworks or paradigms (possibly caused by unconscious plagiarism on Kuhn's part) seems less important to me than the divergence of their worldviews. Polanyi's reflections on dwelling in and breaking out of interpretative frameworks are only a fragment of his philosophical vision. The part of his vision that he shares with Kuhn takes on an entirely new meaning when it is embedded in the whole of Polanyi's vision of reality:

Men need a purpose which bears on eternity. Truth does that; our ideals do it; and this might be enough, if we could ever be satisfied with our manifest moral shortcomings and with a society which has such shortcomings fatally involved in its workings.

Perhaps this problem cannot be resolved on secular grounds alone. But its religious solution should become more feasible once religious faith is released from pressure by an absurd vision of the universe, and so there will open up instead a meaningful world which could resound to religion.<sup>54</sup>

From my point of view, all that is good in Kuhn's position is found in Polanyi, while there is no trace in Kuhn whatsoever of Polanyi's orientation toward purposes which bear upon eternity. Polanyi's worldview goes far beyond Kuhn's in its orientation toward truth as a metaphysical prerequisite for the progress of science. This

is a necessarily vague concept, operating primarily in the tacit dimension, in the background of our awareness, that compels us to seek contact with reality and measure all of our efforts against this highest standard and broadest interpretative framework. Because of his empiricist outlook, truth is not something that can appear in Kuhn's system—it is not something that can be “observed” impersonally. We recognize its force within us as the ground of our intellectual passions which motivate us to take issue with conflicting interpretations of the world around us.

I confess that I am not an unbiased observer of the contest between Polanyi and Kuhn. After immersing myself in the story of Polanyi's life, it seems to me that I can feel his anguish at seeing a limited and inadequate philosophy of science sweep the field, bring Kuhn the accolades and fame that Polanyi never enjoyed in his own lifetime. I cannot prove that it was he who underlined the sentence about him spending “years ... crying in the wilderness”<sup>55</sup> but I believe it was his own hand, wearied with age, weakened by the condition that robbed him of his powers of speech, and wordlessly expressing his frustration that it was Kuhn, and not he, who was hailed as the man who had revolutionized the world's understanding of science.

## Endnotes

<sup>1</sup>“Self-Emptying Knowledge: The Tacit Apprehension of Mysteries in Science and Religion,” in *From Polanyi to the Twenty-First Century*, edited by Richard Gelwick (Biddeford, Maine: The Polanyi Society, 1997; 414-434); revised and republished as “Self-Emptying Knowledge: Michael Polanyi's Vision of the Moral Foundations of Scientific Revolutions,” *Appraisal* 1 Supplement (1997) 22-29; references here are to the latter version.

<sup>2</sup>Polanyi's notion of an “interpretative framework” is itself nested in an interpretative framework. In “Religious Meaning in Polanyi's *Personal Knowledge*” (*Polanyiana* 2:4 [1992] / 3:1 [1993] 75-82), Phil Mullins has shown that there are a cluster of associated terms not very clearly defined or distinguished in Polanyi's work: “articulate systems” (76-77), “articulate frameworks (77-78), theories or “conceptual frameworks” (78), “heuristic vision” (78, 80), “mental dwelling places” (80). “In *Personal Knowledge*, it is already clear that different frameworks seem to require a different degree (or perhaps a different kind) of participation. ... Accreditation of all such frameworks and the specifications of the relationships among the variety of frameworks available was what most concerned Polanyi” (79).

<sup>3</sup>*The Structure of Scientific Revolutions*, second edition, enlarged (Chicago: University of Chicago Press, 1970), x.

<sup>4</sup>“Self-Emptying Knowledge,” 25.

<sup>5</sup>*Structure*, 182; emphasis added—Kuhn's use of “group commitment” in this passage echoes a major theme in *Personal Knowledge* (cf. Chapter 10, “Commitment,” 299-324).

<sup>6</sup>*The Way of Discovery: An Introduction to the Thought of Michael Polanyi* (New York: Oxford University Press, 1977), 55-82.

<sup>7</sup>Scott seems to have found the letter in Regenstein Polanyi Collection (RPC), box 6, folder 9. The RPC is housed in the Special Collections Section of the Regenstein Library at University of Chicago; all material drawn from the RPC in this article is used by permission.

<sup>8</sup>February 28, 1967. Because of his longstanding interest in and publications about the Polanyi-Kuhn connection, Struan Jacobs is seeking permission to publish the whole of Kuhn's letter. I will refer to this letter as “Kuhn to Poteat.”

<sup>9</sup>Kuhn to Poteat.

<sup>10</sup>Thomas S. Kuhn, *The Road Since Structure: Philosophical Essays, 1970-1993, with an Autobiographical Interview* (Chicago: University of Chicago, 2000) 295.

<sup>11</sup>Kuhn to Poteat.

<sup>12</sup>The offprint is preserved in RPC box 54, folder 4. The article appeared in *Isis* 53 (1961) 161-193. There are no references to Polanyi in the article and no comments by Polanyi on the offprint itself. There is no way to reliably date the inscription on the offprint.

<sup>13</sup>RPC, box 44, folder 7.

<sup>14</sup>In *Scientific Change: Historical Studies in the Intellectual, Social, and Technical Conditions for Scientific Discovery and Technical Invention, from Antiquity to the Present*, ed. A. C. Crombie (New York: Basic Books, 1963); Kuhn's article is pp. 347-369; Polanyi's reply is pp. 375-380; Kuhn's response to Polanyi is pp. 391-395. Page numbers in the next few paragraphs are all to this reference.

<sup>15</sup>Kuhn to Poteat.

<sup>16</sup>In the second edition of *Structure*, Kuhn's question about truth is found on page 170.

<sup>17</sup>Struan Jacobs and Phil Mullins note that this is a term associated with Karl Mannheim, another Hungarian exile and long-time friend of Polanyi. For some details on their friendship, see William T. Scott and Martin X. Moleski, *Michael Polanyi: Scientist and Philosopher* (New York: Oxford University Press, 2005) 41, 45, 194-196, 242. For an in-depth discussion of their relationship and Polanyi's criticism of Mannheim's views as a sociologist of knowledge, see Struan Jacobs and Phil Mullins, "Michael Polanyi and Karl Mannheim," *Tradition and Discovery* 32:1 (2005-06) 20-43.

<sup>18</sup>RPC, box 46, folder 7.

<sup>19</sup>*Structure*, 171.

<sup>20</sup>*Structure*, 172-173; emphasis added. Polanyi copied part of the final sentence that I have italicized in some notes on Kuhn that are discussed more fully below (RPC box 24, folder 4).

<sup>21</sup>*Road*, 307.

<sup>22</sup>New York: Doubleday and Company, 1966.

<sup>23</sup>Chicago: University of Chicago Press, 1964. It should also be noted that Polanyi consistently emphasized the importance of the transcendent ideal of truth for science and other dynamic orders in his early writing. See for example "The Growth of Thought in Society," *Economica* 8 (1941).

<sup>24</sup>RPC, box 6, folder 4; Nov 12, 1963.

<sup>25</sup>William T. Scott's private correspondence, December 22, 1964. The letter to Scott was written shortly after the two men were introduced to each other; Scott had written a fair-minded review of *Personal Knowledge* ("Polanyi's Theory of Personal Knowledge—A Gestalt Philosophy," *Massachusetts Review* 3 [1962] 349-368) that Polanyi deeply appreciated. Scott's correspondence will eventually be made available in the Scott archives at University of Nevada in Reno.

<sup>26</sup>"The Republic of Science," in *Knowing and Being*, edited by Marjorie Grene (Chicago: University of Chicago Press, 1969) 52.

<sup>27</sup>Edited, with an introduction, by Thomas A. Langford and William H. Poteat (Durham: Duke University Press, 1968).

<sup>28</sup>*Intellect and Hope*, 161. I have re-formatted Daly's paragraph and numbered the points of comparison for clarity; parenthetical references in the quotation are to the 1962 edition of *Structure*.

<sup>29</sup>RPC, box 6, folder 9.

<sup>30</sup>Chicago: University of Chicago Press, 1946.

<sup>31</sup>London: Routledge and Kegan Paul, 1951.

<sup>32</sup>Chicago: University of Chicago Press, 1969.

<sup>33</sup>"Metaphysics as a Heuristic for Science" in *Proceedings of the Boston Colloquium for the Philosophy of Science, 1964-1966: In Memory of Norwood Russell Hanson* (Boston Studies in the Philosophy



of Science) by Colloquium for the Philosophy of Science, R. S. Cohen, and Marx W. Wartofsky (Dordrecht: Reidel, 1967) 123-172.

<sup>34</sup>March 19, 1968; RPC, box 16, folder 2.

<sup>35</sup>Wartofsky provides a reference here to the OUP symposium, *Scientific Change*, 375. The key sentence on that page is: "A commitment to a paradigm has thus a function hardly distinguishable from that which I have ascribed to a heuristic vision, to a scientific belief, or a scientific conviction."

<sup>36</sup>*Conceptual Foundations of Scientific Thought: An Introduction to the Philosophy of Science* (London: Macmillan, 1968) 507.

<sup>37</sup>*Encounter* 38 (January, 1972) 43-50. Also in *Society, Economics and Philosophy: Selected Papers*, edited and introduced by R. T. Allen (London: Transaction Publishers, 1997) 267-281.

<sup>38</sup>August 13, 1970; RPC, box 8, folder 13.

<sup>39</sup>18 (1972) 61-91.

<sup>40</sup>September 14, 1971; RPC, box 10, folder 2. Polanyi seems to have mistaken the publication date of *Structure*, which appeared in 1962, not 1963; the monograph was originally written for "Fundamentals of Unified Science: International Encyclopedia of Unified Science," edited by Otto Neurath and Rudolf Carnap, 2:2 (Chicago: University of Chicago Press, 1962). Neurath and Carnap were logical positivists from the Vienna Circle.

<sup>41</sup>*The Times Higher Education Supplement*, July 26, 1974, p. 11; RPC box 24, folder 4.

<sup>42</sup>*Road* 255-323.

<sup>43</sup>*The Thomist* 53 (1989) 259-279.

<sup>44</sup>*Road*, 307.

<sup>45</sup>*Scientific Change*, 380.

<sup>46</sup>"Polanyi's Presagement of the Incommensurability Concept," *Studies in History and Philosophy of Science* 33 (2002) 105.

<sup>47</sup>Alasdair MacIntyre "Epistemological Crises, Dramatic Narrative and the Philosophy of Science," in G. Gutting (Ed.), *Paradigms and Revolutions* (Notre Dame: University of Notre Dame Press, 1980), 67.

<sup>48</sup>*Social Epistemology* 16 (2002) 166.

<sup>49</sup>Translated by Thaddeus J. Trenn and Frederick Bradley, edited by Trenn and Robert K. Merton (Chicago: University of Chicago Press, 1979).

<sup>50</sup>Jacobs has a footnote here in which he says that "Kuhn had certainly read Polanyi's book [*Personal Knowledge*] by July 1961" and perhaps even earlier. The material in the first part of this paper shows that Kuhn was not willing to admit ever having finished reading *Personal Knowledge*. The same material does confirm Jacobs' view that "Kuhn knew his Polanyi" from having used *Logic of Liberty and Science, Faith, and Society* in Conant's course at Harvard.

<sup>51</sup>"The Republic of Science, Its Political and Economic Theory," *Minerva* 1 (1962) 54-73. As noted in the first part of this article, Kuhn was also on *Minerva*'s editorial board at this time.

<sup>52</sup>*Worldview: The History of a Concept* (Grand Rapids: Eerdmans, 2002) 195.

<sup>53</sup>Cambridge: Harvard University Press, 2001.

<sup>54</sup>These, of course, are the last words of *The Tacit Dimension*, 92. *Personal Knowledge* ends on a similar note.

<sup>55</sup>See footnote 41.