Tradition & Discovery

The Polanyi Society Periodical

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Preface

This is a special issue of *TAD* devoted to discussion of the work of Polanyi and its connection (and disconnection) with the work of Thomas Kuhn. Those who have followed this journal over the years know that this topic has popped up again and again. Marty Moleski, the surviving Polanyi biographer, wrote the lead article ("Polanyi vs. Kuhn: Worldviews Apart") and, after discussion with others, I invited Struan Jacobs, Aaron Milavec, Dick Schmitt, and Maben Poirier to comment on Moleski's article and the general topic of Polanyi and Kuhn. All of the commentors have long pondered--and most have published essays here or elsewhere treating--the topic.

Let me add only one interesting historical note that readers might want to factor into the rich discussion in this issue, a note that I gleaned in a recent phone conversation with Richard Gelwick (1/14/07): Thomas Kuhn was on the faculty at the University of Californnia, Berkeley, when Michael Polanyi gave the McEnerney Lectures there in February, 1962. It is possible that he attended these lectures. At the least, Gelwick reports that in either later 1963 or early 1964 he had a most interesting lunch with Kuhn to talk about Polanyi's ideas. It was a lunch that extended to four hours.

Please pay special attention to some of the material in the first pages of this issue. You will find there minutes of the last official meeting of the Polanyi Society, the financial report, as well as the call for papers for the 2007 annual meeting. Also notice in "News and Notes" that there is included the first notice of two major Polanyi conferences, one at Loyola University in Chicago and the other in Hungary at the Budapest University of Technology and Economics. There will be additional information about these important upcoming 2008 events.

Phil Mullins

Tradition and Discovery is indexed selectively in *The Philosopher's Index* and *Religious and Theological Abstracts* and is included in the EBSCO online database of academic and research journals.

NEWS AND NOTES

"*Personal Knowledge* at Fifty": 2008 Polanyi Society Conference

Mark your calendar now to reserve June 13-15, 2008 for participation in a Polanyi Society sponsored conference at Loyola University, Chicago. *Personal Knowledge* was published in May, 1958 and this conference will celebrate this event and provide an opportunity to reappraise Michael Polanyi's *magnum opus* and its philosophical agenda in terms of developments in philosophy, science and the globalization of culture. A call for papers will be posted on the Polanyi Society web site in the spring of 2007 and will be included in the next issue of *TAD* (33:3 in July, 2007).The conference will be modeled on the 1991 and 2001 Polanyi Society conferences at Kent State University and Loyola, Chicago. There will be a few plenary speakers as well as parallel sessions in which conference participants present and discuss papers with others interested in the session's particular topic. This will be a conference that builds in many opportunities for discussion as well as a trip for those interested to the archival Polanyi Papers at the Regenstein Library of the Univeristy of Chicago.

Marty Moleski, S.J. (moleski@canisus.edu) and Phil Mullins (mullins@missouriwestern.edu) are co-chairs designated by the Polanyi Society Board of Directors to organize this conference. Please send any suggestions for the conference to Moleski and/or Mullins. Although this event at Loyola should be a modestly priced academic conference, it is clear that the Polanyi Society will need to raise some dollars to support some conference elements. It is also important to attract to this conference some younger scholars with interest in Polanyi. Particularly if you have ideas about sources of support and ways to attract younger scholars, send these along to Moleski and Mullins.

"Reconsidering Polanyi": 2008 Conference of the Michael Polanyi Liberal Philosophical Association

The Michael Polanyi Liberal Philosophical Association and the Department of Philosophy and History of Science at the Budapest University of Technology and Economics are sponsoring a conference in Budapest that will be held June 26-28, 2008. The conference has the title "Reconsidering Polanyi" and will provide an opportunity for scholars to discuss Polanyi's ideas from many different perspectives. The call for papers for this international conference will be included in the July, 2007 issue of *TAD*. Proposals/ abstracts will be due by September, 2007. The conference registration is 30 EUR. Accomodations for conference participants will be available both on campus at the Budapest University of Technology and Economics and in nearby hotels. Benedek Láng (conference@filozofia.bme.hu), a member of the conference planning committtee, can provide additional information about the conference if you need that before the publication of the call for papers.

Electronic Discussion List

The Polanyi Society supports an electronic discussion group that explores implications of the thought of Michael Polanyi. Anyone interested can join. To join yourself, go to the following address: http://groups.yahoo.com/group/polanyi_list/join. If you have difficulty, send an e-mail to Doug Masini (masini@etsu.edu) and someone will see that you are added to the list.

Upcoming *Appraisal* Conference "Macmurray and Polanyi: Two Personalist Philosophers"

The annual SPCPS (*Appraisal*) confererence scheduled for March 30 & 31 at Nottingham has been cancelled. However SPCPS is spponsoring a one-day conference jointly with the John Macmurray Fellowship under the title, "Macmurray and Polanyi: Two Personalist Philosophers", on Saturday, March 3rd (10.30-4.30) at the Friends' Meeting House, St Giles, Oxford. For information contact Richard Allen at *rt.allen@ntlworld.com.*

Patrick Novak presented a paper titled "A Polanyian Understanding of the Colossian Parenesis" in the "Disputed Paulines" section of the Society of Biblical Literature annual meeting in Washington D. C. in November, 2006. His paper argued that Polanyi's epistemology provides a key in understanding Paul's intent to form disciples through the Colossian parenesis. Most work in Biblical Studies, character ethics, and theology have examined the what of parenetical material; this paper attempts to show the how. Novak reports that though a few in the audience were acquainted with Polanyi's name, few if any had an understanding of his epistemology and how it might be related to the discussion of biblical material.

Struan Jacobs, "T. S. Eliot and Michael Polanyi on Tradition in Literature and Science," *The Dalhousie Review*, 86:3 (Autumn 2006): 373-388.
Abstract: T. S. Eliot and Michael Polanyi produced important and influential theories of tradition in their respective fields of literature and science. This article describes their theories with particular reference to the concept of order, each man viewing tradition as an indispensable condition of order in modern culture. Similarities between the two theories of tradition are noted.

Polanyi SocietyFinancial Statement
September 1, 2005 – August 31, 2006

Beginning Bal. Checking		$1685.25
Income		
	Membership dues, gifts	$3100.00
Expenses		
	Annual meeting	$ 200.00
	TAD (31, #3+32, #1)	-$1417.55
	TAD (32, #2)	$ 998.96
	Annual meeting	$ 145.00
Total expenses		$2761.51
Year End Checking		$2023.74

Beginning Bal. Savings		$2394.33
Income		
	Interest	$ 18.11
	Gift (for travel)	$300.00
Expenses		-0-

Year End Savings
$2712.44

John V. Apczynski,
Treasurer

Minutes of Polanyi Society Annual Meeing of November 18, 2006

1. The meeting was called to order at 11:25 AM by President Walt Gulick.

2. The slate of nominees proposed by the Board were elected unanimously:

Wally Mead as President
Marty Moleski renominated to the Board and continuing as Secretary
Jere Moorman as Treasurer
Esther Meek renominated to the Board
Tony Clark nominated as member of the Board
Dick Moody nominated as member of the Board
Zhenhua Yu nominated as member of the Board
Paul Lewis continuing as Vice-President

3. Gulick expressed thanks to those leaving and to those joining the Board.

4. The floor was opened to discussion of topics for next year's Annual Meeting in San Diego:

— Invite Bill Coulson to participate; he is a longtime leader of a Polanyi study group in San Diego, co-author of *Man and the Science of Man* with Carl Rogers, and co-editor of the Studies of the Person textbook series with Rogers.

— Polanyi and Missiology

— Invite Durwood Foster to discuss the connection betwen Tillich and Polanyi, emphasizing Tillich's article which he suggested to Polanyi as esspecially helpful, "Participation and Knowledge and Consultation: Problems of an Ontology of Cognition."

--Research being done on Polanyi by those receiving Travel Fellowship to attend the Annual Meeting.

5. The question was raised about having more sessions and/or a different schedule for next year.

— Dinner at the hotel, then the open PolanyiSociety meeting? A "box lunch" for dinner—simple, inexpensive, adequate, unobtrusive?

— How best to arrange the time for the board meeting? It has been before the Friday session the last few years, but this causes problems with timing. Perhaps meal times should be protected for convivial discussion, not business. Meals are also an important time to get to know young scholars. Perhaps the board could meet at 6:00 PM, with a light meal or "box lunch" afterward.

— Saturday and Sunday morning meetings instead of Friday night and Saturday morning?

— An additional meeting on Friday afternoon?

6. The question was raised as to whether dues should be increased and perhaps should include a rate for retired persons and a listing of those individuals who contribute more than the regular rate. The Board will work on and refine a new dues schedule.

7. Mead reported on his efforts to have *The Tacit Dimension* reprinted by Eerdman's or Wipf and Stock.

8 Mullins reported there is now available on the Polanyi Society web site a downloadable audio file of the 1966 Polanyi-Rogers dialog. There is also a text file of this dialog posted on the site.

9. Downloadable copies of current issues of *TAD* are posted at the same time the paper editions are published. Mullins reported that there are currently about 50 persons across the wrold who receive e-mail notification when a new electronic version of *TAD* has been posted. Members are invited to send e-mail addresses to Mullins of anyone who might be interested in receiving notice about the posting of a new *TAD* issue. Mullins suggested that eventually the Society may, for economic reasons, consider publishing only a web version of *TAD*.

10. It was suggested that the responses to papers presented at the Annual Meeting continue to be posted on the electronic discussion list.

11. Dale Cannon reported that there are six members working on a Polanyi Reader. It may be ready for publication by the spring of 2007.

12. Wally Mead expressed appreciation to Leila Kees for housing and transporting several Society members and recipients of the Travel Fund, and urged the Board to thank her officially.

13. The job of managing the Travel Fund will continue to be handled by Wally Mead. He will set up a separate account to deposit funds and disburse them.

14. Ann Herbert Scott's generous donation of $7513.68 to create a fund in honor of her late husband, William Taussig Scott, was noted with gratitude. Richard Gelwick and Phil Mullins in consultation with her and the Board are to develop criteria for using this donation.

15. Work on a Templeton grant is in limbo until such time that the Polanyi Reader might be available for use.

16. In his farewell Presidential remarks, Walt Gulick noted his gratitude to the financial angels who have supported the Society in recent years, especially Jere Moorman and Wally Mead, and thanked David Rutledge for all of his support during the years that Gulick acted as President of the Society.

Respectfully submitted,

Martin X. Moleski, SJ

2007 Polanyi Society Annual Meeting

Call for Papers

San Diego will be the site for this year's annual meeting of the Polanyi Society. As usual, we will have sessions on Friday and Saturday, November 17 and 18, but our format will be somewhat different this year. The Polanyi Society is now an official "Related Scholarly Organization" of the American Academy of Religion, and our sessions will be listed as part of the regular program of the AAR in addition to being separately listed. This year we are planning to have an additional meeting on Friday afternoon, and we anticipate that the AAR will grant us space as they always have.

The first session is planned for Friday afternoon from 3:00 to 5:00 and will focus on Polanyi's understanding of the person. The thematic paper for this session will be presented by William Coulson and is entitled "On Having Misread Polanyi's Theory of Personal Knowledge." Coulson is the co-editor with Carl Rogers of *Man and the Science of Man*, (1968) which features two papers by Polanyi plus contributions from Rogers, Jacob Bronowski and others. The dialogue between Rogers and Polanyi from this book is now available on the Polanyi Society web site as both an downloadable audio file and a text file. We are seeking to co-sponsor this session with the Person, Culture, and Religion Group. The call is for respondents to Coulson's paper who may also wish to offer constructive work in a Polanyian theory of the person. Presumably if the Person, Culture, and Religion Group agrees to co-sponsor the session, they would provide one respondent and the Polanyi Society the other; if the session is not jointly sponsored, there may be more than one formal response from a Polanyi Society member..

A second session will be offered Friday night. Paper proposals are solicited for this session on any topic relevant to the ongoing work of the Polanyi Society. Special interest has been expressed for work dealing with a Polanyian perspective on cognitive neuropsychology, and this would presumably complement Coulson's emphasis on humanistic psychology.

Our session from 9:00 to 11:30 Saturday morning will feature theologian Durwood Foster's reflections on Paul Tillich's little known 1955 paper entitled "Participation and Knowledge and Cognition: Problems of an Ontology of Cognition." In their dialog in 1963, Tillich suggested to Polanyi that he might find much that is useful in this paper. The paper will soon be available on the Polanyi Society web site. We have offered to co-sponsor this session with the North American Tillich Society. Again, we will need either one or two respondents depending whether the NATS accepts our offer. As soon as information is available about whether one or two of this year's annual meeting session will be co-sponsored it will be posted on the Polanyi Society web site.

Review of proposals will begin Monday, April 2, but early submissions are appreciated. Proposals for serving as a respondent should not only briefly indicate the relevant background and interests that qualify the individual to respond, but also describe any constructive interests that the respondent might hope to develop. Proposals dealing with cognitive neuropsychology or other topics for the second session should ideally be 300 to 500 words long, but full-length papers are also welcomed. Send proposals to Walter Gulick, wgulick@msubillings.edu (phone 406 657-2904).

Polanyi vs. Kuhn: Worldviews Apart

Martin X. Moleski, SJ

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 Knolwedge*; 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.[1] It seemed to me that Polanyi's description of changing interpretative frameworks[2] 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.[3] 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.[4]

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.[5]

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."[6] 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.[7] 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.[8]

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.[9] 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*.[10] 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.[11] (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."[12]

No other correspondence between Polanyi and Kuhn has yet surfaced; in Polanyi's "Key to Correspondence Files,"[13] 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,"[14] 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.[15]

> 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."[16] 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.'[17] 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."[18] 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.[19]

> 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*.[20]

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."[21] Polanyi seems to have noticed it and (I believe) strongly disagreed with Kuhn's view of science. In *The Tacit Dimension*,[22] 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*,[23] 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.[24]

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.[25]

It has been said that the strongest force known to scientists is fame. Polanyi himself portrayed scientitific 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."[26] 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*.[27] 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.*)].[28]

Polanyi was somewhat distressed by Daly's claim that Kuhn had done "independent research" on these themes. On February 15, 1967, he wrote Poteat:

> 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.[29]

Poteat 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 Poteat.
>
> 2. He was familiar with *Science, Faith, and Society*[30] and *The Logic of Liberty*[31] 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*.[3] 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[33] 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.[34]

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').[35] 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."[36] 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"[37] 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.[38]

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."[39] 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.[40]

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"[41]—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.[42] 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"[43] 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;[44] 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."[45]

In an article written in 2002, Struan Jacobs argues that it is "no coincidence"[46] 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[47] 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."[48] He thoroughly demolishes Kuhn's claim to have derived it from reading Ludwig Fleck's work, *Genesis and Development of a Scientific Fact*.[49] 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.[50] 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,'[51] 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]. ..."[52]

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 'Weltanschaung 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*,[53] 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.[54]

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"[55] 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

[1]"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.

[2]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).

[3]*The Structure of Scientific Revolutions*, second edition, enlarged (Chicago: University of Chicago Press, 1970), x.

[4]"Self-Emptying Knowledge," 25.

[5]*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).

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

[7]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.

[8]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."

[9]Kuhn to Poteat.

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

[11]Kuhn to Poteat.

[12]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.

[13]RPC, box 44, folder 7.

[14]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.

[15]Kuhn to Poteat.

[16]In the second edition of *Structure,* Kuhn's question about truth is found on page 170.

[17]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.

[18]RPC, box 46, folder 7.

[19]*Structure*, 171.

[20]*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).

[21]*Road*, 307.

[22]New York: Doubleday and Company, 1966.

[23]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).

[24]RPC, box 6, folder 4; Nov 12, 1963.

[25]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," *Massachussetts 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.

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

[27]Edited, with an introduction, by Thomas A. Langford and William H. Poteat (Durham: Duke University Press, 1968).

[28]*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*.

[29]RPC, box 6, folder 9.

[30]Chicago: University of Chicago Press, 1946.

[31]London: Routledge and Kegan Paul, 1951.

[32]Chicago: University of Chicago Press, 1969.

[33]"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.

[34]March 19, 1968; RPC, box 16, folder 2.

[35]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."

[36]*Conceptual Foundations of Scientific Thought: An Introduction to the Philosophy of Science* (London: Macmillan, 1968) 507.

[37]*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.

[38]August 13, 1970; RPC, box 8, folder 13.

[39]18 (1972) 61-91.

[40]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.

[41]*The Times Higher Education Supplement*, July 26, 1974, p. 11; RPC box 24, folder 4.

[42]*Road* 255-323.

[43]*The Thomist* 53 (1989) 259-279.

[44]*Road*, 307.

[45]*Scientific Change*, 380.

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

[47]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.

[48]*Social Epistemology* 16 (2002) 166.

[49]Translated by Thaddeus J. Trenn and Frederick Bradley, edited by Trenn and Robert K. Merton (Chicago: University of Chicago Press, 1979).

[50]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.

[51]"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.

[52]*Worldview: The History of a Concept* (Grand Rapids: Eerdmans, 2002) 195.

[53]Cambridge: Harvard University Press, 2001.

[54]These, of course, are the last words of *The Tacit Dimension*, 92. *Personal Knowledge* ends on a similar note.

[55]See footnote 41.

Michael Polanyi and Thomas Kuhn: Priority and Credit

Struan Jacobs

ABSTRACT Key Words: framework, scientific controversy, logical gap, incommensurability, scientific revolution

The article argues that Polanyi was a likely source of influence on the theory of science that Kuhn developed in his The Structure of Scientific Revolutions *(1962). The striking similarity between Kuhn's idea of incommensurability and Polanyi's rendering of scientific controversy in* Personal Knowledge *is featured here, and is used to expose a tension between Polanyi's notions of scientific controversy and unfolding truth.*

Marty Moleski's topic of relationship between the ideas of Michael Polanyi and Thomas Kuhn boils down to a question about truth and the historical record. An important episode of intellectual history is involved: of all the scholarly books published in the last fifty years, few have had an influence to rival that of Kuhn's *The Structure of Scientific Revolutions* (1962, hereafter *Scientific Revolutions*).[1] Moleski's knowledge of the Polanyi archives is second to none, and reading his scholarly article, "Polanyi vs. Kuhn: Worldviews Apart," has enhanced my understanding of relations between Polanyi and Kuhn and their ideas. I had strongly suspected, and have argued in print,[2] that Kuhn borrowed ideas from Polanyi without acknowledging their source, but it never occurred to me that Polanyi had the same suspicion. In his published comments on Kuhn, as for example in his contribution to the Crombie volume,[3] and in the "Background and Prospect" that he added to the second edition of *Science, Faith and Society*,[4] I took Polanyi to be expressing the thought that Kuhn had *independently* arrived at positions similar to his own and that he was not at all displeased by the convergence. Moleski corrects this interpretation of Polanyi, showing that Polanyi questioned whether the convergence was coincidental and indeed, not to put too fine a point on it, wondered whether Kuhn had been intellectually dishonest in the matter.

What I want to do in the following comments is, influenced by Moleski's comparison of Kuhn and Polanyi, rethink their relationship in light of the available evidence. My hope is, as Moleski notes in his footnote 8, to write further on this topic, depending on whether the MIT Library accedes to my request for permission to reproduce Kuhn's significant letter to Professor W. Poteat of February 1967. I will proceed by reflecting on some leading questions. I begin with a wide-angle account of the interests of Polanyi and Kuhn by noting a few important differences and the many similarities in the ways in which they think about scientific research.

How Do Polanyi and Kuhn Envision Scientific Research?

A. Some Differences in Conception

The object of scientific research is, for Polanyi, to make new, true discoveries of aspects of nature, and discoveries, as he sees it, may overturn existing knowledge. For Kuhn, a sharp distinction has to be made between two kinds of science: "normal" and "extraordinary." Kuhn sees most scientific research ("normal science") as assuming, and extending, currently received knowledge which exists in the form of a "paradigm." Kuhn's paradigms in effect *present* normal scientists with "puzzles," whereas Polanyian scientists draw from

personal knowledge in order to *choose* problems. "In choosing a problem," Polanyi argues, "the investigator takes a decision fraught with risks."[5] Compared with puzzle solving in Kuhnian normal science, making a Polanyian problem-solving discovery looks to be a less structured affair, calling for acumen and audacity on the part of the individual scientist. Further differences between Polanyi's analysis of science and that of Kuhn will be dealt with below, particularly those bearing on the question of truth.

B. Resemblances in Outlook

The similarities between Kuhn's thought in *Scientific Revolutions* (1962) and Polanyi's thought particularly, but not only, in *Personal Knowledge* (1958) are many. Scientific research is conducted in a *community* (singular in Polanyi's view, multiple in Kuhn), possessing a large measure of *autonomy*, with members reaching *consensus* – the condition of stable knowledge - on most topics in their work. Structure in the community and its members' systematic engagement in scientific research are grounded in *tradition*. Research typically *assumes* rather than criticizes traditional belief. Much of the scientist's knowledge of how to do research is *tacit*, having been acquired by observation and emulation of the master practitioners to whom students are apprenticed. The doctoral apprenticeship is a subordination of the mind of the student to *authority*. An important part of education in science consists in students "*working out concrete problems*" with the aim of acquiring the skill "of converting a language, which [the student] … so far had assimilated only receptively, into an effective tool for handling new subjects"[6] and solving further problems. There is no Method of Science (e.g., verificationism or falsificationism). Scientific research relies on maxims, knowledge of which is for the most part tacit. Psychological ideas of *Gestalt* and of Jean Piaget are useful in analyzing processes underlying science, as for example perception, learning and discovery. Scientific researchers *solve problems*, and research is conducted within *conceptual frameworks* that have been handed down through education. A conceptual framework in science includes a worldview, presuppositions, theory, vocabulary, concepts, ontology, facts, precepts, values, and standards. The scientist's *observation* is conditioned by his framework. *Anomalies* arise in the course of research when scientists' observations conflict with predictions that have been deduced from the laws and theories embedded in the framework. Scientists seldom look on anomalies as falsifications. A divorce exists between successive conceptual frameworks in science, Polanyi describing it as a "logical gap" and Kuhn as "incommensurability." When scientists relinquish one framework of belief for another, they undergo a *conversion*. Complementary opposed forces of *conservation* and a drive for *originality* operate in science. Kuhn refers to the "essential tension" between "tradition and innovation" and Polanyi comments on the "purposive tension" between "commitment" to tradition on the one side, and innovation or "originality" on the other.[7] Of criteria ("accuracy, simplicity …") of what constitutes valuable knowledge, Kuhn writes that their "effectiveness …does not …depend on their being sufficiently articulated to dictate" scientists' choices; Polanyi writes similarly regarding "intrinsic interest," depth ("systematic interest"), and "certainty and precision" ("accuracy").[8]

Can so many similarities all be coincidental?

How Might Polanyi Have Contributed to Kuhn's Theory?

A. Evidence Kuhn Knew Polanyi's Work

The accumulated evidence strongly implies that Kuhn was acquainted, and perhaps conversant, with Polanyi's view of science before he sent the text of *Scientific Revolutions* off to his publisher. I construe the elements of the case as follows.

(a) Kuhn was reading Polanyi by the early 1950s, with certain writings of Polanyi being included in J. B. Conant's "General Education and the History of Science" course at Harvard.[9] A Junior Fellow at Harvard (1948-1951), Kuhn began lecturing in the history of science in 1951, and taught in Conant's course from 1951 to1956 when he left to take up a position at Berkeley.[10] Interestingly, one of Kuhn's teaching colleagues in Conant's course, Leonard K. Nash, later to be appointed to a chair of chemistry at Harvard, published a deeply thoughtful book on the theory of science, *The Nature of the Natural Sciences* (1963), a year after Kuhn's *Scientific Revolutions*. Whereas Kuhn included only a single fleeting reference to Polanyi's thought in the body of *Scientific Revolutions*, Nash cites Polanyi frequently (and usually approvingly) – more often, indeed, than he does virtually any other scholar.[11]

(b) Kuhn heard Polanyi give a lecture at Palo Alto late in 1958. Moleski indicates that they may have discussed matters at this time.[12]

(c) The paper, "The Essential Tension," delivered by Kuhn at a conference in 1959, presents a number of ideas that are redolent of Polanyi.[13]

(d) As Kuhn acknowledges, he had read at least some of *Personal Knowledge* when he produced his paper for a conference at Oxford in July 1961.[14]

B. Kuhn's Theoretical Debts and His Awareness and Acknowledgement of Debts to Polanyi

Might Kuhn have been aware of similarities between his theory and that of Polanyi and, if so, did he draw his readers' attention to them? Might he have been aware that he was in Polanyi's *debt* for certain of his ideas and, if so, did he acknowledge this? These are difficult questions moving beyond the issue of Kuhn's familiarity with Polanyi's writing; they require careful examination of particular Kuhn texts. Kuhn was *aware* that his theory was similar to Polanyi's in at least one respect, and he pointed this out in his writings. However it is difficult to tell whether Kuhn realized or suspected that he might have been *indebted* to Polanyi for this similarity (and others). Through most of his career, Kuhn acknowledged no debt to Polanyi. Below I list and comment upon the Kuhn texts that lead me to these conclusions.

<u>1959 conference paper, "The Essential Tension" (reprinted in Kuhn 1977)</u>

Kuhn presents ideas that are reminiscent of ones in Polanyi's *Personal Knowledge* (and other writings of Polanyi), although Polanyi is not mentioned in the paper. Among the ideas are tacit knowledge, anomalies as ubiquitous in science, the role of authority in the training of the scientist, scientific community, scientific research as embedded in tradition.

<u>Oxford conference of July, 1961 (Kuhn's paper "The Function of Dogma in Scientific Research," and Polanyi's "Commentary," published in Crombie 1963)</u>

David Naugle paraphrases Kuhn as acknowledging in this text that "Michael Polanyi's ideas of 'personal knowledge' and the 'tacit dimension' led Kuhn to develop his celebrated doctrine of the paradigm."[15] As Moleski rightly points out, Naugle has misread the relevant passage in Kuhn. Kuhn wrote that: "though 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*" idea of (and term) "paradigm." Kuhn's notion of paradigms is, in this instance, that of "the particular model achievements from which …the members of a scientific specialty *learn* to practise their trade. This they do partly by precept and rule but at least equally by

the practice-problem-solving."[16] Kuhn suggests, as Moleski appreciates, that he *independently* lit upon an idea that resembled Polanyi's idea of tacit knowledge, not that Polanyi's presentation had led him to the discovery of paradigms as a part of science.[17]

The Structure of Scientific Revolutions, 1st edition, 1962, p. 44 n. and text

Here Polanyi is described by Kuhn as having "brilliantly developed a very similar theme" to Kuhn's that "the existence of a paradigm need not even imply that any full set of rules exists." Kuhn's note cites chapters V and VI of *Personal Knowledge* as containing Polanyi's "theme." Again, tacit knowledge is the single idea that Kuhn says he shared with Polanyi without suggesting he obtained it from Polanyi.

In the "Preface" of *Scientific Revolutions* Kuhn acknowledges thinkers who helped him on the way: Stanley Cavell, James B. Conant, Paul K. Feyerabend, Ludwik Fleck, John L. Heilbron, Alexandre Koyré, A. O. Lovejoy, Anneliese Maier, Hélène Metzger, Emile Meyerson, Ernest Nagel, Leonard K. Nash, H. Pierre Noyes, Jean Piaget, W. V. O. Quine, Francis X. Sutton, Benjamin Lee Whorf, and Gestalt psychologists. Polanyi's name does not appear in Kuhn's "Preface."[18]

"A Function for Thought Experiments" (1964) in *The Essential Tension*

Observing that for scientists to dwell on "anomalies when they are first confronted is to invite continual distraction," Kuhn remarks in a footnote: "Much evidence on this point is to be found in Michael Polanyi, *Personal Knowledge* (Chicago, 1958), particularly chap. 9."[19] Again, the impression given by Kuhn is that he formed this view independently and then found that he shared it with Polanyi.

The Structure of Scientific Revolutions, 2nd edition, 1970, "Postscript," p. 191

Besides the citation in the note in the body of the book (see above), Kuhn refers to "tacit knowledge" as Polanyi's "useful phrase,"[20] including it in the title – "Tacit knowledge and Intuition" - of section four (pp. 191-198) of the "Postscript" of *Scientific Revolutions*.

The Structure of Scientific Revolutions, 3rd edition, 1996

The only difference between this and the second edition is the inclusion of an index (prepared by Peter J. Riggs). Notwithstanding that Polanyi is twice mentioned in the book (pp. 44 n. and 191), his name is not included in its Index.

"Discussion," 1995, published in *The Road Since Structure* (2000)

Moleski reproduces pertinent passages from this text in his article. Kuhn shows himself to be confused and confusing on Polanyi and Polanyi's thought.[21] Kuhn recalled that when he heard Polanyi's lecture at Palo Alto in 1961 he was still engaged in writing *Scientific Revolutions*. According to Kuhn, he enjoyed Polanyi's lecture and it may have assisted him to form "the idea of paradigm."[22] Kuhn noted that Polanyi's writing had formed a part of Conant's course, and that he "liked it quite a lot … Polanyi" Kuhn recognized "was certainly an influence."[23] Kuhn also recalled in this "Discussion" that, when writing *Scientific Revolutions*, he had glanced at the recently published *Personal Knowledge*, only to decide that he "must not read this book now" because it would force him "to go back to first principles and start over again" and he "wasn't going to do that."[24] There is a solecism in these recollections of Kuhn, for how could he have decided that Polanyi's book would force him to return "to first principles and start over again" if, as he suggests, he had not studied its content. For all Kuhn knew, he might have found that Polanyi's understanding of science was congenial to him, and a source of ideas that he would wish to use! Indeed, in this "Discussion," Kuhn acknowledged that Polanyi was an

undoubted influence on his thinking, which is a debt that he had studiously refrained from acknowledging in the past. Moreover, whereas Kuhn now denied having studied *Personal Knowledge* while he wrote his *Scientific Revolutions*, we recall his comment in the note in *Scientific Revolutions* as showing that he was sufficiently *au fait* with the contents of Polanyi's book as to be able to praise the "brilliant" development of the concept of tacit knowledge in two of its chapters.

To What Extent Is Kuhn's Revolution Motif Comparable to Polanyi's Notion of Scientific Controversy?

It is illuminating to trace the development of Kuhn's ideas about scientific revolutions in connection with Polanyi's discussion of the role of conceptual frameworks, logical gaps and conversion in science. Below I outline that development.

Kuhn's Concept of Scientific Revolution in 1957 (*The Copernican Revolution*)

As Westman (1994) has pointed out, Kuhn suggests in his *Copernican Revolution* that facts "exist independently of concepts. Theories can organize facts, but they do not constitute observations or facts" and, rather than being destroyed by scientific revolutions, phenomena that have been observed are "simply reorganized using different concepts."[25] Kuhn himself suggests that once "phenomena" (observed facts) have been revealed in science, and scientists have established "order" in "fields of experience," these represent "*permanent*" accomplishments.[26] Science has been practiced as a "continuous tradition," enabling the successors of Newton "to explain the phenomena first elucidated by Newtonian concepts, just as Newton [could explain the] phenomena previously elucidated by Aristotle and Ptolemy." Whereas "phenomena" form "a cumulative class through the history of science," concepts and explanations are, according to Kuhn, "repeatedly destroyed and replaced."[27] This is a conservative view of scientific revolution compared to that which appears in Kuhn's *Scientific Revolutions*. It does not include the idea of conceptual mutation that Kuhn would designate from 1961 as "incommensurability."

Polanyi's Concepts of Framework, Logical Gap and Conversion

Polanyi began addressing these and related topics in the 1940s, in works that form tributaries that flow into his *Personal Knowledge*. He sees languages as infused with worldviews. The "interpretative framework" of science is embodied in, expressed through, and supported by the language of science, representing an "idiom of belief."[28] Very shortly after his Gifford Lectures, Polanyi cites, in a 1952 article in *British Journal for the Philosophy of Science* (3:11: 217-232 and later incorporated in *Personal Knowledge*) Zande witchcraft, Marxism, and psychoanalysis as further examples of interpretative frameworks. The worldview of science is implicit in the grammar and vocabulary of science.[29] Language shapes thought as well as being an instrument for expressing thought, claims Polanyi. The vocabulary itself he explains as a particular "theory of all the subjects" and their "recurrent features" that can be discussed in the language.[30] The language enables only certain questions to be asked. Answers that are given to these questions serve to confirm the worldview that the language embodies. According to Polanyi, frameworks of belief cannot be evaluated from within. Using a language to question the worldview that is embodied in the language gives rise to self-contradictions. Only when he has relinquished one language for another, can an agent critically assess the worldview of the first language.[31]

Scientific discovery can ignite controversy, in Polanyi's view. Discovery may alter not only the content of scientific knowledge but the values and the methods of science. In a scientific controversy, supporters of a new framework of belief strive to wrest value for it away from the established framework. Polanyi's examples of controversies in science include the Copernican versus the Ptolemaic frameworks, Pasteur's view of yeast in fermentation as a cellular living organism versus Wöhler, Liebig and Berzelius' viewing it as a chemical compound, Freud's psychoanalytic theory and the frameworks of its opponents, and van't Hoff's theory of optically active carbon compounds and its rejection by Kolbe in light of his anti-speculative view of chemistry.[32]

In *Personal Knowledge* Polanyi argues that scientists are typically committed to the prevailing framework of belief, uncritically accepting orthodoxy. A Polanyian framework of belief in science is (as are frameworks of belief outside of science) able to explain most, but never all, of the phenomena in its field, adherents being prepared to set aside such phenomena as their framework is unable to explain ("anomalies") in the expectation that their framework will eventually be able to explain them or else to explain them away as illusory.[33]
The framework undergoes a programmatic elaboration; its concepts are modified to accommodate objects that are unique in some of their details. Polanyi refers to this as the "tacit art" of denotation, citing Urey's alteration of the meaning of the term "isotope" to include deuterium in its denotation.[34] Anomalies can eventually put a question mark against a framework. Polanyi observes that scientists may be swayed by "the loose ends of current thought" to replace one framework with another, notwithstanding that the new framework may have anomalies of its own.[35]

Polanyi may look on a major new discovery in science as a new framework of belief (or else as significantly altering an existing framework). Polanyi describes a new framework and an existing one as being separated from each other by a "logical gap." He means that reasoning and evidence that are produced in "*one* framework of interpretation" are valueless to adherents of the other framework. Advocates of the new framework are unlikely to receive a sympathetic hearing from the supporters of orthodoxy.[36] In effect, the orthodox are being asked to learn a new language, which they will refuse to do since they take the language to be meaningless.

The "logical gap" between conceptual frameworks involved in a controversy in science is, for Polanyi, analogous to the separation that exists between a problem in scientific research and the discovery by which it is solved. No rule of logic can lead an inquirer from his problem to its solution. The gap can only be crossed heuristically, by the inquirer leaping from the known to the unknown, his discovery being a feat of illuminative originality. For Polanyi, the logical gap means that the new framework has no logical relation (entailment, inclusion, contradiction, disjunction) with the established one.[37] The discovery irrevocably "changes the world as we see it. …My eyes have become different; I have made myself into a person seeing and thinking differently."[38] Adherents of the new framework "think differently, speak a different language, live in a different world."[39]

The scientist requires a conceptual framework as a condition of making "sense of experience" and each framework incorporates a unique view of reality.[40] Polanyi takes supporters of frameworks that are separated by a logical gap to live in the same "material universe," of which their frameworks give different pictures. Existing unformulated in the discoveries that scientists are mentally incubating at any time, premisses are fundamental to any framework in sustaining, while being affected by, methods, standards of quality, problems,

evidence, concepts, facts and relations that are taken by scientists to be credible and convincing, and by Polanyi as interdependent.[41]

In view of these differences between logically dissociated frameworks, Polanyi infers that supporters of one framework in science can never demonstrate their propositions to their opponents. "Assertion can be made only *within* a framework;" there being no vantage point that is external to, and independent of, any framework.[42] Arguments from the premisses of one framework appear as "specious" to the opposition.[43] One side's predictive successes are irrelevancies to those on the other side of a logical gap. There is no framework-independent position from which, or method by which, to adjudicate in a controversy between dissociated Polanyian frameworks. Choice between frameworks is never compelled by facts and arguments, being a *conversion* that is mostly actuated by "intellectual passions."[44]

Kuhn's Position in 1962 (*The Structure of Scientific Revolutions* and its subsequent editions)

In each discipline of science, argues Kuhn in *Scientific Revolutions*, research is intermittently plunged into a crisis, disrupted, and then gets underway again in a reconstituted form. Scientific knowledge cannot accumulate long term under these conditions.[45] Acceptance of a new paradigm (*qua* theory/framework) is, for Kuhn, *at the expense* of one that has sustained research in a scientific community up to that time. Kuhn describes each paradigm as presenting a unique view of the world. Many of the paradigm/theory's *names* are taken over from the past, but their meanings and "conceptual networks" are new, with the terms being applied to different objects or to old objects that have been ascribed with "new properties" and behaviour.[46] The paradigm poses new problems ("puzzles") and issues new standards of solution. There are new methods, instruments, and "manipulative procedures," while old instruments that continue in use are likely to "yield different concrete results," manipulations and measurements from what they yielded in the past.[47]

There is "a sense," Kuhn believes, in which each paradigm in science is "constitutive of nature." The sense is one in which scientists with a new paradigm "see the world …differently," respond "to a different world."[48] Kuhn adds that while scientists in the aftermath of a scientific revolution continue to look "at the same" world they "*see* different things."[49] In certain passages Kuhn suggests that a new paradigm changes the world from that with which its adherents used to deal as followers of the last paradigm, but this is Kuhn rhetorically exaggerating in order to get his point across. He gives a more measured statement of his view in affirming that "The world does not change with a change of paradigm, [but] the scientist afterward works in a different world."[50] For Kuhn, the scientist does not apply his ideas to "raw sense data."[51] The data and observations are not fixed by the combination of the world and the scientist's perceptual apparatus. Observations are not neutral in regard to paradigms and are not reinterpreted in light of paradigms.[52] Perception and data are conditioned, Kuhn argues, by the physical world, perceptual apparatus *and* paradigm *together*.

Kuhn takes the effect of paradigm change on a scientist's perception to be analogous to a gestalt experience – "same retinal impressions" while seeing "different things"[53] – with the difference that in science there is no "external authority" or "standard," which for psychological experiments happens to be the "experimenter" who assures the subject that *this in fact is what* he has been "looking at."[54] With a new paradigm, the scientist sees objects "differently from the way he had seen [them] before."[55] There has occurred a "transformation" or "shift of vision" Kuhn considers, notwithstanding that the scientist who has lived through a scientific revolution continues to look "at the same world."[56]

As accounts of the world, Kuhnian paradigms are "incommensurable." In various ways (e.g. disagreeing about theories and facts, "about what is a problem and what a solution," about concepts and meanings, methods and standards), the dialogue between supporters of different paradigms is "slightly at cross-purposes" with them "talk[ing] through each other."[57] Paradigm choice cannot be effected exclusively by "logic and experiment."[58] Dramatizing the extent and depth of change involved when one incommensurable paradigm replaces another, Kuhn refers to the upheaval as a "scientific *revolution*."

Did Kuhn Know of Polanyi's Ideas about Frameworks, the Logical Gap, Controversy and Conversion?

Polanyi did not develop a theory of patterned scientific development in the way that Kuhn did with his thesis of eras of normal science ending in revolutionary upheavals. Polanyi's purpose in explaining logical gaps and scientific controversies was to show that choice between frameworks in science is - contrary to those theorists who present science as simply "objective" - influenced by many deeply personal factors and these are components of whatever might be regarded as rational and empirical judgment. However, Polanyi's ideas of logical gap, controversy and conversion are remarkably close to the view of scientific revolution that Kuhn expressed in his *Structure of Scientific Revolutions*. Kuhn never refers to this similarity. Was he aware of it? In 1995, he claimed he had made a point of *not* reading *Personal Knowledge* when he was composing *Scientific Revolutions*.[59] As I indicated above, this sounds more than a little strange, given that in *Scientific Revolutions* itself Kuhn praised Polanyi's brilliant development of "a very similar theme" (tacit knowledge and its practical mode of acquisition) in *Personal Knowledge*, "particularly [in] chaps. v and vi."[60] As also noted above, Kuhn's "Essential Tension" paper of 1959 is redolent of Polanyi's ideas, and Kuhn referred to *Personal Knowledge* in his paper at the Oxford Conference of July 1961. Kuhn's citation in *Scientific Revolutions* of chapter 6 of *Personal Knowledge* is of particular note, given that this is a chapter in which Polanyi discusses, not only tacit knowledge, butthe logical gap, conversion and other ideas that bear on the issue of "scientific controversy."[61] Taken together, these matters provide strong reason to believe that Kuhn came across Polanyi's coverage of the "logical gap" between frameworks in science while he was preparing his paper for Oxford, which is to say in the first half of 1961 if not earlier, and that he may have read it when he was preparing his conference paper of 1959. Moreover, Kuhn may have heard Polanyi discuss this topic at Palo Alto in 1958, and Polanyi claims he discussed "at considerable length" with Kuhn "his project of the book which was to come out in 1963" in the passage cited by Moleski in footnote 40.

To What Degree May Polanyi and Kuhn Be Seen As Realists?

Polanyi assuredly was a *realist* in the sense of recognizing a physical world or "material universe" as existing independently of, while being an object of, and source of information for, our cognition.[62] Moreover, Polanyi believed in the *reality of truth* and other ideal ends. These two themes of realism come together in Polanyi's proposition that the aim of science is to discover *true* knowledge of the *material* world.[63]

Kuhn was a realist in the first of these senses. He affirmed the existence of an objective physical world, while arguing that the scientist's conceptualization, description, knowledge and perception of this world, and his research activity in it, are paradigm-dependent and are fundamentally changed in the course of a scientific revolution. Truth, however, plays no part in Kuhn's analysis of science. Kuhn mentions truth on only two pages

in the body of *Scientific Revolutions*.[64] He does so in the immediate context of describing the development of science as evolutionary, denying that science progresses toward some end state, as for example "one full, objective, true account of nature."[65] In his "Postscript – 1969," added to the second edition of *Scientific Revolutions*, Kuhn writes that "One often hears that successive theories grow ever closer to, or approximate more and more closely to, the truth." This, he explains, refers to a theory's "ontology, to the match …between the entities with which the theory populates nature and what is 'really there'."[66] Such a notion of truth is untenable for Kuhn because there is "no theory-independent way" to ascertain what is "'really there'," while the very idea that a theory's ontology might correspond to "its 'real' counterpart in nature" strikes Kuhn as void.[67] Historically, there is no obvious "direction of ontological development" in the succession of paradigms in science; to the contrary, the ontology of "Einstein's general theory of relativity is closer to Aristotle's than either of them is to Newton's."[68] Kuhn realizes there will be readers who would describe him as a cognitive relativist, although he denies that he is one. If, however, his theory is relativist, the fact remains, so far as Kuhn is concerned, that it has not omitted "anything needed to account for the nature and development of the sciences."[69]

Polanyi affirms that truth "exists by itself," and that it is unitary, not plural nor relative.[70] Truth he sees as lying "in the achievement of a contact with reality – a contact destined to reveal itself further by an indefinite range of yet unforeseen consequences."[71] Polanyi writes by way of illustration: "I believe accordingly – in view of the subsequent history of astronomy – that the Copernicans were right in affirming the truth of the new system, and the Aristotelians and theologians wrong in conceding to it merely a formal advantage …Copernicanism could well have been a source of truth …even if it had been false. But the Copernican system did not anticipate the discoveries of Kepler and Newton accidentally: it led to them *because* it was true. In saying this we are using the term 'true' to acknowledge the indeterminate veridical quality of Copernicanism."[72]

Polanyi's thinking on the subject of scientific change displays different tendencies. There are passages in *Personal Knowledge* in which, as I have just indicated, Polanyi suggests that scientific knowledge is progressive as regards the truth. Thus, "Dalton's atom proved a mere shadowy prefiguration of its successor, the atom of Rutherford and Bohr. Once more it was proved – and this time on a vast scale – that a scientific theory, when it conforms to reality, gets hold of a truth that is far deeper than its author's understanding of it."[73]

One gains a different picture in Polanyi's account of "Scientific Controversy" (150ff.) in *Personal Knowledge*. Here his ideas suggest that developments in scientific knowledge may be discontinuous. Separated by a logical gap, frameworks, we recall Polanyi as explaining, offer "a new vision of reality;"[74] terms, concepts, facts, and modes of reasoning are *peculiar* to frameworks, and upholders of each framework occupy "a different world."[75] Different vocabularies "divide men into groups which cannot understand each other's way of seeing things and of acting upon them."[76] Their aesthetics, valuations, and scales "of interest and plausibility" are different.[77] It is not possible for scientists adhering to different frameworks to meaningfully *compare* their views for the accuracy of their predictions, the scope and depth of their explanations, and their empirical-informative content. They cannot demonstrate any proposition to each other. In the final analysis it is, as Polanyi says, "conversion" rather than reasoned analysis that moves scientists to adopt a new framework.[78] These are ideas of Polanyi that foreshadow and, I have suggested, represent a likely source of, Kuhn's theory of incommensurability.

Endnotes

*Advice and assistance that I have received from Professors Phil Mullins and Walt Gulick mean that this article is more polished than it would otherwise have been. I am most grateful for their interest in the study, and the encouragement they have given me to improve its analysis.

1. Steve Fuller, *Thomas Kuhn: A Philosophical History for Our Times* (Chicago: University of Chicago Press, 2000), 1.

2. Struan Jacobs, "Polanyi's Presagement of the Incommensurability Concept," *Studies in History and Philosophy of Science* 33 (2002), 105-120.

3. Michael Polanyi, "Commentary," *Scientific Change*, ed. A. C. Crombie (London: Heinemann, 1963), 375-380.

4. Michael Polanyi, "Background and Prospect," *Science, Faith and Society* (Chicago: University of Chicago Press, 1964), 12, 13.

5. Michael Polanyi, *Personal Knowledge* (London: Routledge & Kegan Paul, 1958), 124. In his *Logic of Liberty*, 51-52, and 89, however, Polanyi gives a rather different impression as to the degree of difficulty that scientists encounter in trying to discover good problems for their research.

6. Polanyi, *Personal Knowledge*, 125 emphasis added.

7. Thomas Kuhn, "The Essential Tension: Tradition and Innovation in Scientific Research," (1959), in Kuhn, *The Essential Tension* (Chicago: University of Chicago Press, 1977), 225-239. Polanyi, *Personal Knowledge*, 120; *Logic of Liberty*, 39-40.

8. Kuhn, *Essential Tension*, 330; Polanyi, *Logic of Liberty*, 55; *Personal Knowledge*, 135ff.

9. Thomas Kuhn, et al., "A Discussion with Thomas S. Kuhn," *The Road Since Structure*, eds. J. Conant and J. Haugeland (Chicago: University of Chicago Press, 2000), 296.

10. Kuhn et al., "Discussion with Thomas S. Kuhn," *Road Since Structure*, 289.

11. Leonard K. Nash, *The Nature of the Natural Sciences* (Boston: Little Brown, 1963); Robert K. Merton, "The Sociology of Science: An Episodic Memoir," *The Sociology of Science in Europe* (Carbondale: Southern Illinois University Press, 1977), 88.

12. Moleski, "Polanyi vs. Kuhn," 9-10. Kuhn's book, *The Copernican Revolution*, first appeared in 1957, not, as Moleski reports on page 9, in 1959.

13. See note 7 above for bibliographical details.

14. Kuhn, "Discussion," *Scientific Change*, 392.

15. Moleski, "Polanyi vs. Kuhn," 21 quoting David K. Naugle, *Worldview: The History of a Concept* (Grand Rapids: Eerdmans, 2002), 198.

16. Kuhn, "The Function of Dogma in Scientific Research," *Scientific Change*, 392-393 emphasis added.

17. See also *Essential Tension*, xix, where Kuhn, referring to his preparation of "The Essential Tension" essay "early in 1959," makes a similar claim about his having independently discovered the concept of tacit knowledge. One infers from the text of his paper for the Oxford conference of 1961 that Kuhn had read Polanyi's "perceptive and challenging" *Personal Knowledge*, or important parts thereof, before or as he produced the paper. See Kuhn, "Function of Dogma," *Scientific Change*, 392.

18. See Kuhn, *Scientific Revolutions*, 1st ed., viii-xiv. Also relevant, but not entirely consistent with the acknowledgements in the "Preface" of *Scientific Revolutions*, are Kuhn's recollections of his intellectual debts in the "Discussion" of 1995 that is reproduced in *Road Since Structure*, 274ff.

19. Kuhn, *Essential Tension*, 262 and note.

20. Kuhn, *Scientific Revolutions*, 2nd ed., 191.

21. Moleski, "Polanyi vs. Kuhn," 17; Kuhn et al., "Discussion," *Road Since Structure*, 296-297.

22. Kuhn et al., "Discussion," *Road Since Structure*, 297.

23. Kuhn et al., "Discussion," *Road Since Structure*, 297.

24. Kuhn et al., "Discussion," *Road Since Structure*, 296.

25. R. Westman, "Two Cultures or One," *Isis*, 85 (1994), 83.

26. Thomas S. Kuhn, *The Copernican Revolution* (Cambridge, Mass: Harvard University Press, 1959 (first published 1957)), 264 emphasis added.

27. Kuhn, *Copernican Revolution*, 265; also Kuhn, "Essential Tension" (1959), reprinted in Kuhn, *Essential Tension*, 226-227.

28. Polanyi, *Personal Knowledge*, 288.

29. Polanyi, *Personal Knowledge*, 94, 112, 289.

30. Polanyi, *Personal Knowledge*, 80.

31. Polanyi, *Personal Knowledge*, 47, 286ff.

32. Polanyi, *Personal Knowledge*, 150-160.

33. Polanyi, *Personal Knowledge*, 13, 293.

34. Polanyi, *Personal Knowledge*, 105-106, 111-112.

35. Polanyi, *Personal Knowledge*, 18.

36. Polanyi, *Personal Knowledge*, 151.

37. Critical of this part of my interpretation, Gulick has suggested to me that Polanyi looks on the frameworks of Newton and Einstein, and on other such successive frameworks in science, as *related* by subsumption. Having been provided with no reference by him, I am unsure as to which passage(s) in *Personal Knowledge* Gulick has in mind. In reading the pertinent pages 14 and 15 of *Personal Knowledge*, however, my impression is that Polanyi's Einstein *subverts*, rather than subsumes, Newton's theory. It is *discontinuity* between the frameworks that Polanyi is surely underlining on those pages.

38. Polanyi, *Personal Knowledge*, 143.

39. Polanyi, *Personal Knowledge*, 151.

40. Polanyi, *Personal Knowledge*, 60; 150.

41. Polanyi, *Personal Knowledge*, 59, 130, 135,152-153, 158-161, 165, 167, 170, 201.

42. Polanyi, *Personal Knowledge*, 60.

43. Polanyi, *Personal Knowledge*, 158.

44. Polanyi, *Personal Knowledge*, 152. Gulick has advised me that "a lot more could be said about the commonly shared tacit powers embedded in intellectual passions." Quoting from *Personal Knowledge* (189), he argues that Polanyi believed that, by way of these "powers", mathematicians accomplish a '"tacit bridging of the logical gaps internal to every formal proof."' This fact, for Gulick, "defend[s] Polanyi against charges that he constructs incommensurable realms." I would point out, however, that Polanyi continues the same passage in *Personal Knowledge* by affirming the occurrence of "radical conceptual inventions opening up altogether new vistas" in mathematics (189). Citing Cantor's proofs in the mathematical theory of aggregates, Polanyi describes how they "traversed a logical gap across which only those willing to enter into their meaning and capable of grasping it could follow him. Reluctance or incapacity to do so caused divisions among mathematicians, similar to those which arose between van't Hoff and Kolbe on the subject of the asymmetric carbon atom, or between Pasteur and Liebig on that of fermentation as a vital function of yeast. Hadamard describes how he and …Lebesgue, finding themselves on opposite sides of this dispute, were compelled to recognize the *impossibility of understanding* each other" (190 emphasis added).

45. Kuhn, *Scientific Revolutions*, 92, 139. These references to *Scientific Revolutions*, and those that follow, are to its 2nd edition unless otherwise indicated.

46. Kuhn, *Scientific Revolutions*, 102, 109; also 103, 105, 128, 144.

47. Kuhn, *Scientific Revolutions*, 111, 130, 142; also 126, 129.
48. Kuhn, *Scientific Revolutions*, 110, 111, 121.
49. Kuhn, *Scientific Revolutions*, 120, 129 emphasis added; also 111, 117, 118.
50. Kuhn, *Scientific Revolutions*, 121; also 150.
51. Kuhn, *Scientific Revolutions*, 96.
52. Kuhn, *Scientific Revolutions*, 120, 121, 126.
53. Kuhn, *Scientific Revolutions*, 127.
54. Kuhn, *Scientific Revolutions*, 114, also 120.
55. Kuhn, *Scientific Revolutions*, 115, 128.
56. Kuhn, *Scientific Revolutions*, 116, 118, 119, 129.
57. Kuhn, *Scientific Revolutions*, 109, 141, 148.
58. Kuhn, *Scientific Revolutions*, 94.
59. Kuhn et al., "Discussion," *Road Since Structure*, 296-297.
60. Kuhn, *Scientific Revolutions*, 44 n.
61. Polanyi, *Personal Knowledge*, 150-160.
62. Polanyi, *Personal Knowledge*, 47.
63. This is a superficial rendering of Polanyi's views. As Mullins has reminded me, Polanyi's understanding of reality is broad and distinctive. For example, his reality includes the objects of mathematics (*Personal Knowledge*, 186).
64. Kuhn, *Scientific Revolutions*, 170, 171.
65. Kuhn, *Scientific Revolutions*, 171.
66. Kuhn, *Scientific Revolutions*, 206.
67. Kuhn, *Scientific Revolutions*, 206.
68. Kuhn, *Scientific Revolutions*, 207.
69. Kuhn, *Scientific Revolutions*, 207.
70. Polanyi, *Personal Knowledge*, 305.
71. Polanyi, *Personal Knowledge*, 147.
72. Polanyi, *Personal Knowledge*, 147.
73. Polanyi, *Personal Knowledge*, 43 text and n. 1; also 104, 147, 153, 165, 276-277.
74. Polanyi, *Personal Knowledge*, 150.
75. Polanyi, *Personal Knowledge*, 151.
76. Polanyi, *Personal Knowledge*, 112.
77. Polanyi, *Personal Knowledge*, 135.
78. Polanyi, *Personal Knowledge*, 151.

WWW Polanyi Resources

The Polanyi Society has a World Wide Web site at http://www.missouriwestern.edu/orgs/polanyi/. In addition to information about Polanyi Society membership and meetings, the site contains the following: (1) the history of Polanyi Society publications, including a listing of issues by date and volume with a table of contents for recent issues of Tradition and Discovery; (2) a comprehensive listing of Tradition and Discovery authors, reviews and reviewers; (3) digital archives containing many past issues of Tradition and Discovery; (4) information on locating early publications not in the archive; (5) information on Appraisal and Polanyiana, two sister journals with special interest in Polanyi's thought; (6) the "Guide to the Papers of Michael Polanyi", which provides an orientation to archival material housed in the Department of Special Collections of the University of Chicago Library; (7) photographs of Polanyi; (8) links to a number of essays by Polanyi.

Public Recognition, Vanity, and the Quest for Truth:
Reflection on "Polanyi vs. Kuhn"

Aaron Milavec

ABSTRACT Key Words: Thomas Kuhn, Robert Merton, Freud, James Conant, J. B. Rhine, Hilary Putnam, recognition, vanity, unconscious borrowing, unspecifiability, unaccountable element, ESP, paradigm shifts, sociology of knowledge, anticipated fruitfulness, phenomenology of discovery, scientific hierarchy, embodied knowing

After commending Moleski for his excellent study, I focus attention on three areas that merit further clarification: (a) that Polanyi's quest for public recognition was legitimate and not the effect of a runaway vanity, (b) that Kuhn's straining to define his dependence upon Polanyi was blocked by the unspecifiability clouding the discovery process and by his (mistaken) notion that Polanyi appealed to ESP to explain the dynamics of discovery, and (c) that Kuhn's success in gaining public recognition for his paradigm shift is understandable (just as is Polanyi's relative failure). In the end, I list five areas wherein Kuhn's account of scientific revolutions could be substantially improved by joining forces with Polanyi.

To begin, I want to give my great applause to Martin X. Moleski for having so carefully uncovered the threads of evidence regarding the question of dependence of Thomas S. Kuhn upon Michael Polanyi. Moleski taught me many things that I did not know, and he did a superior job of producing a tapestry that incorporated those things that I did know. Hence, he is to be highly commended for his painstaking sleuthing.

Moleski's study repeatedly exposes Polanyi's impulse to receive due recognition for his published ideas. Moleski also raises the question as to whether Kuhn is more dependent upon Polanyi than he was ever willing to acknowledge. My own reflections on these issues will be grouped under three rubrics: (a) Polanyi's quest for recognition; (b) whether Kuhn depended upon Polanyi; and (c)Polanyi's interesting hypothetical conjecture, picked up by Moleski, "If I join forces with Mr. Kuhn . . . ."

Polanyi's Quest for Recognition

Society is ever intent upon giving its applause and its awards to originators and, at the same time, to pour its scorn upon plagiarists. Having said this, one can also acknowledge that it must have been a keen disappointment to Polanyi (and to many of Polanyi enthusiasts, myself included) that his thought was largely overlooked and politely bypassed by the educated elite in general and by the philosophers of science in particular. His letter of February 15, 1967, to Poteat 9cited in Moleski, 13) is especially lucid in this regard. Polanyi expresses his hesitancy to accuse Kuhn of unattributed borrowing because he himself is unsure on this matter. While not overtly expressed, there can be little doubt that Polanyi was aware of the potential harm that could fall upon him if he publicly accused Kuhn. He could easily be judged as the spoilsport bent upon robbing Kuhn of his justly deserved recognition, or worse, as the false incriminator driven by his runaway vanity.

Moleski never asks whether the quest for recognition was a moral failing in Polanyi (and even in his disciples as well). One must ask whether Polanyi's quest for recognition conceals a dangerous "vanity" that gets

fixated on social status rather than on the advancement of knowledge within one's profession. In principle, the advancement of knowledge and public recognition are entirely compatible. Polanyi himself notes that the eureka-experience that crowns years of hard work on a perplexing problem has the effect of encouraging the pioneer to publish his/her results and to gain due recognition for having enriched others by his/her endeavors (*PK*, 142-145). What may appear, upon first sight, as "vanity" or as the "unbridled hunger for recognition," consequently, might be merely the outer expression of "the inner need for assurance that one's work really matters" (Merton, 270):

> The need to have accomplishment recognized, which for the scientist means that his knowing peers judge his work worthwhile, is the result of deep devotion to the advancement of knowledge as an ultimate value. Rather than necessarily being at odds with dedication to science, the concern with recognition is ordinarily a direct expression of it (Merton, 270).

Hilary Putnam (b. 1926) is a well-known and well-published American philosopher and mathematician. He was enamored by logical positivism during his doctoral studies, yet he later went on to advocate various forms of modified realism and social activism. In his analysis of Kuhn, Putnam wrotre in 1974: "I believe that I anticipated this view about ten years ago when I argued that some scientific theories cannot be overthrown by experiments and observations *alone*, but only by alternative theories" (Putnam, 69). In making this claim, Putnam did not try to establish his priority nor did he infer that Kuhn was influenced by his earlier work. Nonetheless, Putnam exhibits the normal disappointment that a creative individual feels when he perceives that some of his "unpopular" ideas of ten years ago have made it big time due to the enterprising efforts of a comparatively young and inexperienced philosopher. If one reads between the lines of Moleski's study, one finds that Polanyi, like Putnam, must have experienced Kuhn's easy success in gaining public recognition for many "unpopular" ideas that they held in common as a bitter pill to swallow.

Whether Kuhn Depended upon Polanyi

What can we say about Kuhn's seeming inability to recall and to publicly acknowledge the depth of his dependence upon Polanyi? Might this be an instance of Kuhn wanting to enlarge his own claim to originality while defeating the suspicion that he may have borrowed (and disguised) ideas gained from Polanyi? Possibly. The danger here, however, is to imagine that Kuhn himself might have some absolute clarity on this issue and that he intentionally disguised his dependence.

To begin with, recall that Polanyi put forward the recognition that a Gestalt perception allows only a subsidiary awareness of the particulars. Polanyi enlarges upon this same model for helping understand how the process toward the emergence of a pioneering discovery takes place as a string of successive hunches that have been guided, all along the way, by dwelling in the problem while the manifold clues were held in subsidiary awareness. As a result, the discovery that crowned the straining toward a resolution, comes as a sudden Gestalt (or a series of Gestalten). Even upon reflection, a discoverer cannot exhaustively explain (even to him/herself) why the route chosen seemed more plausible than all the others and how the eureka-experience showed up (if and) when it did. This is what Polanyi refers to as "the problem of unspecifiability" (*PK*, 62; "unaccountable element" 1962, 1; "indeterminacies" 1968, 27-30).

More to the point, "If a set of particulars which have subsided into our subsidiary awareness lapses altogether from our consciousness, we may end up by forgetting about them altogether and may lose sight of them beyond recall" (*PK*, 62).

Applying this to Kuhn, he could, of course, deliberately lie about his borrowing from Polanyi. But, setting this prospect aside for the moment, we must allow that even when Kuhn strains to replay his route to discovery, he cannot entirely reconstruct in detail the lines of influence. Thus, in reading his response, we hear him acknowledge that he read Polanyi but that he was not forcefully impressed by him—thus, giving him the impression that he did not and would not borrow from him. Is this authentic remembering? Probably. Is it at the same time inauthentic since it covers up unconscious borrowing? Possibly.

The history of science knows of many instances of "unconscious borrowing" (Merton, 272-278). Scientists sometimes even make discoveries that "seemed to come to them out of the blue [but] had actually been formulated by them years before [and later found in a journal, a lecture, a conversation with a colleague] and then forgotten" (Merton, 276). Sigmund Freud, the master of the unconscious, puzzled over the fact that, as a young boy, he had read Ludwig Börne's study of "free association" as an active agent in creative writing. Freud acknowledged that at the time he was pioneering his use of "free association" in psychotherapy, he still had the volume of Börne on his bookshelf, yet "he could not remember the essay in question" (cited in Merton, 275). This inability to remember, however, can sometimes be a clue to a psychological block; hence, it does not preclude the possibility that Freud did make use of Börne's "free association" in his own pioneering therapy. Upon reflection, Freud generalized his position as follows:

> A scientific worker may sometimes ask himself what was the source of the ideas peculiar to himself which he has applied to his material. As regards some of them, he will discover without much reflection the hints from which they were derived, the statements made by other people which he picked out and modified and whose implications he has elaborated. But as regards others of his ideas, he can make no such acknowledgments; he can only suppose that these thoughts and line of approach were generated—he cannot tell how—in his own mental activity, and it is on them that he bases his claim to originality (Freud, cited in Merton, 275).

Within this context, it makes perfectly good sense for Kuhn to acknowledge some dependence yet to be unable to precisely define it since it was so inconsequential or incongruous with his main lines of thought. In the free-flowing interview in 1994, Kuhn thus recollects the following:

> 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[1] 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.[2]

This does not appear to me to be a cover-up. Moleski is suspicious; yet, given the lapse of time and given the uncertainty still felt around the topic, I would judge that there is no falsehood here. In fact, I could safely apply

Polanyi's own words from an earlier inquiry, "I am sure that Kuhn was acting in good faith and might himself be anxious to clarify this matter" (cited in Moleski, 14).

Robert K. Merton did an extensive study of two hundred sixty four instances of "multiples"—i.e., cases where scientists working independently made similar discoveries in complete or relative ignorance of the work of the other. Since the sociology of science gives the greater recognition to the originator, Merton notes that little attention has been given to the study of multiples. For our use here, it seems apparent that "multiples" testify to the understandable high incidence that independent minds tackling the same perplexing problem within the shared intellectual environment might indeed arrive at similar solutions. Polanyi, himself, drew attention to this phenomenon:

> Two scientists faced with a similar set of facts will often hit on the same problem and discover the same solution to it. Coincident or nearly coincident discoveries by independent investigators are quite common. . . (*SFS*, 35).

If one gives due weight to multiples, then the act of discovery might be seen as an emergent possibility that resides within the group of like-minded individuals puzzling over the same constellation of problems. When one lets go of the notion that the act of discovery is always the incomparable "gift of genius," it might then be possible to consider multiples as the inevitable phenomenon that is bound to emerge among professional practitioners over the course of time. Multiples, moreover, indicate how the disposition to accept a novel discovery is already tacitly disseminated within the community poised to receive it. It also indicates how discoveries can be made and published without receiving any suitable notice; later, when a more renowned investigator arrives at the selfsame discovery, then and only then does public acceptance and recognition arrive for the one who first published.

Multiples invite quite a different perspective on the Kuhn-Polanyi affair. In fact, the prevalence of multiples encourages us to see Polanyi and Kuhn as arriving at many parallel lines in resolving the problem posed by the emergence of "scientific revolutions." Granted there are significant differences, Polanyi gave greater weight to the phenomenology of discovery; Kuhn gave greater weight to the sociology of recognition. Kuhn undoubtedly learned something from Polanyi but was entirely put off by what seemed to him, his reliance on "something very like ESP"[3] in scientific discovery. Hence, Kuhn was unsympathetic to the overall thesis of Polanyi, never read *Personal Knowledge* beyond what was required by his course work, and, accordingly, was authentically unable to recognize Polanyi as making a seminal contribution to his work in *The Structure of Scientific Revolutions*. On the other hand, Kuhn amply recognizes James B. Conant as having given him his start in the right direction.[4] Kuhn, therefore, is not opposed to showing gratitude where it is due.

"If I join forces with Mr. Kuhn . . . ."

Michael Polanyi had already celebrated his seventieth birthday when he first met Thomas Kuhn in July of 1961. Polanyi was asked to respond to a paper by Kuhn who had flown from Berkeley to Oxford to take part in "The Symposium on the History of Science." Kuhn entitled his paper, "The Function of Dogma in Scientific Research." Clearly this paper was a first draft of how Kuhn proposed to use his own studies in the history of science and the sociology of knowledge to reframe and to reformulate the static issues of verification and falsification that then dominated the program of logical positivism. Polanyi not only had a chance to hear

Kuhn's proposal but to support and to contribute to it. Polanyi's fifteen-minute verbal response to Kuhn's paper began with these promising words:

> The paper from 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 (Polanyi, 1963, 375).

Unfortunately, however, Polanyi and Kuhn never did properly join forces. Two years later, Kuhn published *The Structure of Scientific Revolutions* and brought his revolutionary notion of "paradigm shifts" into the mainstream of discussions on the history and philosophy of science. But Kuhn's impact did not stop there. By the late 60s, educators, behavior scientists, sociologists, political scientists, and artists were all reading Kuhn and borrowing his notion of "paradigm shifts" in order to account for how their respective fields sustained traditions of personal knowledge and personal performance skills that underwent changes during the course of history. In all of this, Kuhn made only a marginal use of Polanyi.

Kuhn gained acceptance in wide circles in part because he harmonized with the prevailing intellectual climate of the 60s. The fields of psychology (especially behaviorism), sociology (especially the sociology of knowledge), and the philosophy of science (especially logical positivism) had been taken over by the distrust of explaining matters by appeals to interior states.[5] This, in my mind, is a key reason why Kuhn was received so easily while Polanyi was not. Kuhn, it must be remembered, remarked on multiple occasions that Polanyi made appeals to ESP when it came to explaining how discoveries were made and accredited. ESP is a polite word for "internal states of mind" that cannot be examined or verified and thus have no bearing upon the social arbitration as to whether a novel scientific paradigm was to be accepted or rejected.[6]

Here is precisely where Kuhn needs Polanyi. In the crudest terms, the sociology of knowledge of the 60s put forward a world in which primary and secondary socialization were used to explain how "common sense" was culturally determined and how the "scientific consensus" was maintained. In such an atmosphere, the social world of science was imagined to be populated by "symbolic universes" (theories) that were promoted and maintained by "conceptual machineries" (the scientific hierarchy):

> It is important to stress that the conceptual machineries of universe-maintenance are themselves products of social activity, as are all forms of legitimation. . . . Specifically, the success of particular conceptual machineries is related to the power possessed by those who operate them. The confrontation of alternative symbolic universes [Copernicus vs. Ptolemy] implies a problem of power—which of the conflicting definitions of reality will be "made to stick" in the society. . . . Which of the two will win, however, will depend more on the power than on the theoretical ingenuity of the respective legitimators. . . . He who has the bigger stick has the better chance of imposing his definitions of reality (Berger and Luckmann, 109).

Faced with such a "social construction of reality" (the title of the book), Polanyi and Kuhn were both rebels. Polanyi believed that the apprenticeships undertaken by aspiring physicists did serve to conform them into community-accredited forms of thinking and habits of judging; yet, the active scientist remains free to challenge, correct, and reform parts of the system. Kuhn, aware of his critics, tried to argue that proponents of

a new paradigm argue for their altered system on the basis of its anticipated fruitfulness "to solve problems presented by nature" (Kuhn, 1970, 205) and as "a prelude to the possibility of proof" (Kuhn, 1970, 199). Moleski is quite helpful in noting that Polanyi had great difficulty with Kuhn on this very point and intimated that his resolution was "just plain nonsense" (Moleski, 16).

My own dissatisfaction with Kuhn comes directly from my ability to see (a) how Polanyi provides a greater depth of understanding to what they hold in common and (b) how Polanyi resolves issues that Kuhn cannot overcome within his system. In my earlier work (Milavec, 1993), I specified five areas of difficulty. I would summarize and revise them now as follows,

1. Kuhn's system forces him to project a crisis[7] at the root of every paradigm change as the stimulus for searching for alternatives. Polanyi would offer Kuhn his own critique of the heuristic value of doubting (*PK*, 269ff) which might allow Kuhn to speak more accurately as finding "intellectual dissatisfaction" rather than a "crisis" at the origin of inquiries into alternative paradigms.

2. Kuhn offers no explanation as to how scientists caught up in a crisis can initiate an explorative quest that can turn up attractive alternatives without being sent off into a maze of dead ends. At one point, Kuhn suggests something akin to a rudimentary phenomenology of discovery (Kuhn, 1970, 89f, 122f); yet, on the whole, Kuhn was burnt by accusations of using mystical mumbo-jumbo and, accordingly, he preferred the Darwinian model of random mutations and survival of the fittest.[8] Polanyi provides a sophisticated phenomenology of discovery that functions to insure that, even though the creative process cannot be exhaustively delineated, at least it can be perceived as having an internal direction from beginning to end (*PK*, 142-159; TD, 3-33; Polanyi, 1967).

3. Kuhn seems quite satisfied that "perceptual transformations" which take place in Gestalt experiments have something to indicate about (a) how the existence of one paradigm blocks its alternative and (b) how the jump can be made from one paradigm to another (Kuhn, 1970, 112ff). He also rightly notes that paradigm shifts are irreversible (Kuhn, 1970, 114) but does not have the analytical apparatus to determine precisely why this should be the case. Polanyi would bring to Kuhn a much greater depth of analysis here.

4. Once a pioneering discoverer does come forward with his novel paradigm, it appears to me that Kuhn fails to provide any credible grounds whereby any other scientist might want to join him. Kuhn rightly notes that "if a paradigm is ever to triumph it must gain some first supporters, men who will develop it to the point where hardheaded arguments can be produced and multiplied" (Kuhn 1970, 158). Careful thinkers such as Imre Lakatos accuse Kuhn of suggesting that the social structure of paradigm changes reduces the quest for truth to "a band wagon effect" (Lakatos, 181; cf. response of Kuhn, 1970, 259-266). Polanyi would bring to Kuhn a much more sophisticated understanding of the intellectual gap that divides the adherents of conflicting paradigms and help define the grounds whereby adherents might embrace a new paradigm even prior to any experimental verification.

5. Kuhn remains entirely skeptical that a winning paradigm can ever be equated with "a better presentation of what nature is really like" (Kuhn, 1970, 206) or that "successive theories . . . approximate more and more closely to the truth" (Kuhn, 1970, 206). In brief, humans can never get outside of their bodies in order to compare "reality" as such with their "ideas" of reality. "Truth" and

> "reality" are thus endangered species since, according to the sociology of knowledge, these are merely the linguistic tokens for sanctifying the reigning theories in the scientific hierarchy. But this is precisely where Polanyi begins—by taking bodily indwelling as the natural and the correct position for all knowing.[9] Based upon this, Polanyi examines the use of scientific theories as an extension of the indwelling within embodied knowing. Polanyi provides a foundation for a provisional realism that is the *sine qua non* for successful scientific research and a healing antidote to the British empiricism that has eroded our ability to accredit our bodily sensations.[10]

All in all, Polanyi had a decisive edge over Kuhn. Polanyi was a productive physical chemist and had nearly two hundred research papers to his name. Kuhn had none.[11] Kuhn received a doctoral degree in physics from Harvard in 1949, but near the end of his studies he immersed himself in the history of science and, for eight years (1948-1956), taught a revolutionary undergraduate program designed by James Bryant Conant, president of Harvard (1933-1953). His "General Education in Science" program explicitly aimed to give future policy makers a more interesting and more accurate social understanding of science by immersing thems in historical case studies wherein scientists disagreed over matters of scientific theory. When Kuhn was denied tenure at Harvard in 1955, he set himself to publishing the very thesis that he had been formulating while teaching. *The Copernican Revolution* came out in 1957 and *The Structure of Scientific Revolutions* appeared in 1962. In the preface, Kuhn speaks frankly about the importance of his experiences at Harvard:

> A fortunate involvement with an experimental college course treating physical science for the non-scientist provided my first exposure to the history of science. To my complete surprise, that exposure to out-of-date scientific theory and practice radically undermined some of my most basic conceptions about the nature of science and the reasons for its special success (1970, v).

Then, while still in his 40s, popularity overwhelmed him. Over a million copies of *The Structure of Scientific Revolutions* were sold in sixteen different languages. His book moved the popular imagination from seeing scientists as drones bent upon accumulating more precise data and more accurate theories to intelligent human beings locked in passionate conflict with each other over things that mattered deeply to them. Kuhn was a dynamic lecturer, and he had a compelling story to tell—one that went right back to his own conversion story that began when he taught Conant's (now defunct) experimental courses in the history of science.

Conclusion

At the end of his essay, Moleski remarks that "all that is good in Kuhn's position is found in Polanyi" (Moleski, 21). This is not a statement to be refuted since it is properly an act of admiration uttered by a devoted disciple. Can we not expect that a true disciple always understands and appreciates his master better than his rivals and, as a consequence, pushes for his excellence despite his comparative obscurity? Surely! So I will not subject Moleski's affirmation of admiration to a close scrutiny.

On the other hand, I do not share Moleski's suspicion that Kuhn was more dependent upon Polanyi than he was willing to acknowledge. My discussion above has endeavored to show that the issue of dependence does not have the clarity that Moleski seems to imply. The critical point for me, however, arrived when I read in Moleski's study how Kuhn, in the very act of straining to recall his dependence upon Polanyi, repeatedly emerged with his aversion to Polanyi's endorsement of ESP. Even in the free-flowing interview a year before

his death in 1996, Kuhn was still (wrongly, of course) associating Polanyi's account of the discovery process with the bogus claims of "extrasensory" powers. In part he said, "I kept feeling terrible at those points where he [Polanyi] spoke as though extrasensory perception was the source of what scientists did." Then, a moment later, Kuhn admitted, "When I did try to read *Personal Knowledge*, I discovered that I didn't like it." Thus, in the end, Moleski's excellent study inclines me to register how Kuhn's aversion to Polanyi blocked him from relying upon Polanyi. Similarly, even unconscious borrowing appears only as a remote possibility.

In the final section of my paper, I summarized the soft spots of Kuhn's exposition when contrasted with that of Polanyi. In fairness to Kuhn, however, the relative simplicity of his "paradigm shifts" (coming out of Conant's case studies) and the acceptability of "paradigm shifts" by social constructionists (who would accuse Kuhn of not being a purist because he did occasionally refer to "interior states"[12]) was key to his success. Understanding this takes Moleski's investigation another step further for the disciples of Polanyi. How so? To begin with, there is no need to envy Kuhn's success, for the best of Polanyi was inaccessible to him. Conversely, the relative "failure" of Polanyi is intimately tied up with the rich sophistication of his thought and his persuasion that "interior states" (faith, tacit skills, intellectual passions, guiding intuitions, imagination) were decisive for making discoveries in science. It is no surprise, accordingly, why Kuhn (beyond a few footnotes) decided to distance himself from Polanyi. Likewise, it is no surprise that Polanyi cannot be explained in Kuhnian categories.

I cannot blame Kuhn for his limitations; I can only applaud him for how well he carried off a revolution in the popular imagination. Many of Kuhn's soft spots are due to his settling for simplistic generalizations. Yet, I cannot entirely fault him for this since it was the very simplicity of his themes that enabled them to be communicated effectively in an hour lecture. Most of all, not being a creative scientist himself, he could not be expected to be personally aware of many of the processes known tacitly by working scientists such as Polanyi. In the last line of his essay, Moleski faults Kuhn for having no taste for "purposes which bear upon eternity." For myself, however, I am inclined to say of him what my daughter learned almost as a refrain in her preschool—"He did the best that he could."

Endnotes

[1] Polanyi uncritically accepted the "extrasensory" and "psychokinetic" experiments that were pioneered by J.B. Rhine (d. 1980) in the 1930s. Unfortunately, Polanyi then proceeded to describe the workings of "scientific intuition" by linking it to the much more obscure and "extrasensory perception" (*SFS*, 35-38, 60; *PK*, 166). "ESP has yet to be demonstrated to the satisfaction of the scientific community and is often called a pseudoscience" (Sarah G. Stonefoot and Clyde Freeman Herreid, "Extrasensory Perception—Pseudoscience? *A Battle at the Edge of Science*," http, //www.sciencecases.org/esp/esp_notes.asp). Knowing this, it becomes understandable how Kuhn was repulsed by Polanyi's endorsement of ESP and, even a year before his death, he continued to associate Polanyi's account of the discovery process with the bogus claims of "extrasensory" powers.

In December of 1963, Polanyi was invited to write a new introduction for the University of Chicago Press reprint of *SFS*. In this introduction, Polanyi described how, in retrospect, he subsequently abandoned any hope of relying upon ESP to illuminate the discovery process despite his initial enthusiasm for this line of thinking,

> The testing hand, the straining eye, the ransacked brain, may all be thought to be laboring under the common spell of a potential discovery trying to emerge into actuality. I feel doubtful today about the role of extra-sensory perception in guiding this actualization. But my speculations on this possibility illustrate well the depth that I ascribe to this problem (*SFS*, 14).

James A. Hall narrates how, as a psychiatric intern at Duke University, he made it possible for Polanyi to present his ideas at a Grand Rounds meeting of the psychiatry department in 1964. Shortly thereafter, J.B. Rhine, whose institute of parapsychology was located in property adjoining Duke's east campus, invited Hall to accompany him to a public lecture given by Polanyi. Hall regarded Polanyi's notion of tacit powers as very favorable to Rhine's work and undoubtedly shared his insights with Rhine prior to the lecture itself. But then Hall reports that "instead of the appreciation I expected, Dr. Rhine had an immediate antipathy to Polanyi's thought for reasons that I still do not understand" (Hall, 16).

More recently , the magician, James Randi, has made it his mission to expose the hoax perpetrated by persons such as Uri Geller who claim to be able to perform remarkable feats due to their psi powers. Randi found that trained physicists examining Uri Geller while he bent spoons with only his powers of mind were easily hoodwinked into crediting his powers. When Randi later duplicated Geller's feats and then explained how he did it, the physicists quickly discovered how they had been artfully distracted from what they ought to have seen. The Amazing Randi has offered a one-million-dollar prize to anyone who can show, under proper observing conditions, evidence of any paranormal, supernatural, or occult power. To date, Randi has been able to see through every one of the would-be claimants for his prize. See James Randi, *The Truth About Uri Geller* (Buffalo, NY: Prometheus Books, 1982). See http //skepdic.com/geller.html for further details.

[2] Cited in Moleski, n. 41, p. 18

[3] This telling revelation comes from Moleski's paper. Kuhn is clearly influenced by the logical positivism and the sociology of knowledge of his day wherein interior states that defy public examination were categorically ruled out as having any bearing upon how scientists evaluated the merit of scientific discoveries. Kuhn, consequently, had strong reasons to distance himself from Polanyi, especially since scholars like Mark W. Wartoksky were inclined to lump together Kuhn and Polanyi as "emphasizing the subjective and irrational components in the contexts of scientific observation" (cited in Moleski, 15). See also Moleski's citation of Richard D. Whitley for a more pronounced instance of this confusion (pp. 16-17 and n. 39). While Kuhn included two footnotes in the expanded 1970 second edition of *The Structure of Scientific Revolutions* lauding Polanyi, it would appear that Kuhn might have regretted this since it seemingly gave more ammunition to his critics. This would help explain why Moleski found a distinct trend on the part of Kuhn to distance himself from Polanyi.

[4] I thank Moleski for bringing to my attention that the first edition of *The Stucture of Scientific Revolutions* was dedicated to "To James B. Conant, Who Started It." To this, I add that Kuhn invited Conant to write the preface to his first book, *The Copernican Revolution*. Conant willingly accepted. In both instances, Kuhn recognized his true mentor.

[5] See n. 3.

[6] It is interesting to note that Hilary Putnam, a philosopher who rigorously rejected the logical

positivism of his youth in favor of a modified realism, judged Kuhn as "radically subjectivistic" (Putnam, 70) when it came to explaining how a new paradigm gains acceptance over a prevailing paradigm.

[7] Another problem with Kuhn's appeal to a "crisis situation" is the growing body of evidence that some discoveries take place and go on to gain widespread recognition without evoking a crisis situation. Kuhn himself makes reference to cases wherein a "discovery through accident" opens up a whole new field of inquiry without there being any prior paradigm. Kuhn highlights the case of X-rays (Kuhn, 1970, 57).

[8] Moleski brings this out very nicely in his paper. Polanyi, of course, rebuffed the notion of making progress through the accumulation of blind mutations in both biological evolution and scientific development. Random mutations and survival of the fittest may have been satisfactory themes in the sociology of knowledge; yet, for Polanyi, they had no explanatory power whatsoever when it came to accounting for "emergence."

[9] Polanyi allows that all bodily perceptions are projections of interior states but, at the same time, he insists that such projections are spontaneous, necessary and appropriate since they are causally related to a reality that is making its presence felt "out there." Heuristically, such projections are sense-giving and productively guide human interaction within one's environment. Working scientists who project the meanings that they discover while dwelling in their paradigms are similarly functioning spontaneously, necessarily and appropriately. Due to the "semantic aspect" of embodied knowing, the integrated meaning of the clues that originate within the organism appears "out there," i.e., at the focus of one's attention.

[10] The empirical school of British philosophers took great delight in undermining the reliability of the senses when it came to determining the actual nature of things. They did this under the mistaken conviction that science had disclosed the true (metaphysical) nature of reality that could be used to correct the mistaken judgments based upon our senses. According to this norm, the senses all suffered the terrible inadequacy of projecting bodily sensations (the so-called "secondary qualities") onto things to which they do not properly apply. The vinegar is not "sour"; the acidic interaction on the surface of the tongue simply registers this "sour sensation." The bottom of the well is not "black"; the absence of reflected light makes any object appear black. This rock is not "heavy," its mass is being pulled toward the much larger mass of the earth.

[11] Moleski says, at one point, that "both Polanyi and Kuhn left their work in chemistry and physics to take up philosophy" (p. 21). This should not be taken to mean that Polanyi's mature scientific research over more than twenty years is somehow comparable to Kuhn's studies in theoretical physics without publishing a single research paper.

[12]Kuhn made social constructionists uneasy when he spoke of "the transfer of allegiance from paradigm to paradigm" as "a conversion experience" (1970, 151). Later, in explicating this, Kuhn states that "a decision of that kind can only be made on faith" – "faith that the new paradigm will succeed with the large problems that confront it, knowing only that the older paradigm has failed with a few" (1970, 158). This faith Kuhn calls "personal and inarticulate" (1970, 158). For a discussion of these points, see Gutting, 7-12, 18.

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

Richard Henry Schmitt

ABSTRACT Key Words: Charles Darwin, Thomas Kuhn, Michael Polanyi, paradigm, normal science, evolution, scientific theory, scientific practice
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.[1] 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."[2] 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.[3] 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."[4] 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 <u>really</u> 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).[5] 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 a 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 say, 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),[6] 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."[7] 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

[1]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).

[2] “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.

[3]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.

[4]Thomas S. Kuhn, “Preface” to *The Essential Tension* (Chicago: University of Chicago Press, 1977), x.

[5]*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.

[6]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.

[7]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.

The Polanyi - Kuhn Issue

Maben W. Poirier

ABSTRACT Key Words: Polanyi, Kuhn, politics, social sciences, education, training
This brief sketch affirms my agreement with Martin Moleski's essay ("Polanyi vs. Kuhn: Worldviews Apart") *on the relationship of Polanyi to Kuhn. In this piece, I raise issues that revolve around the relationship of Polanyi to Kuhn in the field of the social sciences, and more especially in the field of politics.*

Despite the long lapse of time since I last wrote formally on this subject, my views have not changed. Having read Martin Moleski's thoughtful piece with interest, particularly as it relates to the behind-the-scenes correspondences and exchanges of Kuhn and Polanyi—information that was not available to me in 1989 when I published "A Comment on Polanyi and Kuhn"[1]—I am more convinced than ever that it is a mistake to associate Polanyi's thinking with Kuhn's thinking. These two men were, it seems to me, without a doubt, engaged in very different and, I believe it fair to say, inimical undertakings, and Martin Moleski's article confirms it in a way that I could not in 1989. Consequently, my belief is still that (a) Kuhn did not understand Polanyi's thinking with respect to *personal knowledge* and the role that it plays in relation to the advancement of knowledge in general, and, in particular, in regard to the revelation of the *real* and the *true*, (b) that Polanyi was deeply committed to revealing the *real* and the *true*, which was something that was not of concern to Kuhn (indeed, it was something that he thought was impossible, given that, for him, there was no real and no true, and he makes this patently clear), (c) that Kuhn's thinking took place entirely within the confines of what Polanyi called "explicit knowledge," and as a consequence, there is nothing like Polanyi's "tacit dimension of knowing" in Kuhn's thought,[2] (d) that no matter how one interprets Kuhn's notion of the "Gestalt shift," in no sense is it equivalent to anything that Polanyi might have meant by the expression "tacit knowing,"[3] (e) and finally, like Martin Moleski, I still believe that Polanyi, unfortunately, but likely, saw Kuhn as a much-needed ally in his struggle against positivist empiricism.[4] It seems to me that this was a serious mistake on Polanyi's part, a mistake which, while understandable under the circumstances, has done Polanyi more harm than good. It has led many to want to meld Polanyi's thinking with that of Kuhn, and presumably with the thinking of other contemporary relativists as well.[5] Polanyi was not a relativist or conventionalist, and Professor Moleski has, it seems to me, done a good job of informing us why we should not accept this overlapping of Polanyi and Kuhn. I cannot but agree whole-heartedly with him on this point.

In this short piece—in which I concern myself with matters that are of interest to political philosophers, as well as others, I hope—I want to draw attention to an issue which rarely gets mentioned in comparisons of Polanyi's thinking with that of others, and, in particular, with that of Kuhn. Very briefly, I want to shed a little light on the different understandings of *who man is* for Polanyi and for Kuhn, since this reveals a great deal about the differences between the two.[6] Allow me to begin this reflection by speaking about *who man is* for Kuhn, since it is by far the easier to render, and is also perhaps one of the more shallow description of who man is that I can think of. As we will see, Kuhn's account of who man is speaks volumes about the underpinning of Kuhn's ideas, which, I believe, are ultimately to be found in the Enlightenment and in the intellectual and political history of the late nineteenth and early twentieth centuries.

To the best of my recollection, nowhere does Kuhn speak explicitly about who man is. However, this does not mean that we are left in the lurch or that we have little to go on when it comes to constructing a picture

of who man is for Kuhn. Kuhn does say enough about related matters in his writings for us to extract a reasonably accurate picture of who he thinks man is. More importantly, the implications of what he has to say are maybe not as innocuous as it might at first seem when one takes into account the overall architecture of his argument. Not unlike other thinkers of the modern era, Kuhn did not see man as a single entity, inasmuch as he alternatively characterised man (a) as being an extremely creative being with a charismatic and very persuasive personality, and (b) as being a pedestrian labourer in the vineyard of knowledge.[7] The first category, of course, is the category into which "great scientists" fall—the Newtons and the Einsteins in the history of science—the scientists who, through the sheer power of their creative imagination, bring forth a new vision (i.e., a new paradigmatic order) around which they construct a world, proselytise in its favour, and eventually succeed in imposing it on the scientific community, usually by converting the more youthful members of the community to their way of seeing things. The older members of the scientific community, being less adaptable and having invested more of themselves in "the old vision," remain usually with the old vision.[8] Facts, of course, play no role in the "shift" from the old to the new vision. Facts, as Kuhn never tires of telling us, are *theory-laden*, that is to say, facts are created by the "theory" that one espouses, and, hence, are theory and ultimately paradigm-specific. The theory, the paradigm, conceptual framework, etc.—however one decides to call the "vision" of the "great scientist"[9]—creates its own world of "facticity," such that there can be no appeal to the facts to arbitrate disputes between or amongst visions. Indeed, visions are wholly incommensurable with one another, as Kuhn also repeatedly informs us, and disputes between visions cannot be arbitrated, unless, of course, there is a more general vision residing at a higher level of abstraction that can serve as the common ground on which to base the arbitration, and then this more general vision is itself wholly arbitrary (and this is so even if there are two more general vision, with no still more general vision above them). The vision brought into being by the creative imagination of a "great scientist,"—who, by definition, must be a mesmeriser more than what we traditionally mean by a "great scientist," since he foists his vision upon the members of a scientific community with nothing but his charismatic personality, given that there is no supporting evidence—is in the nature of a hermetically sealed jar which contains all of the contents needed to furnish a world. And so, once one has opted into a hermetically sealed jar, be it new or old, and assuming that it is of the highest level of abstraction, one cannot access the contents of another jar at a similar level of abstraction. In fact, if one has recently shifted jars, one's old jar is nothing other than a piece of furniture within one's new jar, and it (the old jar) exists according to the exigencies of the new jar. Of course, it is all important that we understand that the shift that takes place when a presumably young scientist converts from an old view to a new view is accompanied by something like amnesia, such that memories of an earlier order are not really memories at all, but elements of the present order presented to us as if they were memories of the past.[10] Kuhn speaks of this shift as being analogous to a Gestalt-shift. But this is not quite accurate, if we assess this shift by the standards of the overall argument that Kuhn advances, for it is always possible to shift back and forth when confronted with a Gestalt-image, and thus compare the elements of one form with the elements of another. Think here of the classic Gestalt-image of the young girl and the old woman. But this shifting is precisely what ought not to be possible, if we credit Kuhn's argument to the effect that visions, theories, and paradigmatic orders are incommensurable. And so, the shift has to include something like amnesia. In fact, Kuhn's notion of incommensurability demands it.

The second type of human being—i.e., the pedestrian labourer in the vineyard of knowledge—is essentially an "under-labourer," if I may be permitted to use John Locke's expression.[11] He or she is the one who devotes himself or herself to the task of cleaning-up those out-of-the-way places and obscure corners that are part of the vision of every "great scientist." He or she is the one who, convinced of the correctness (whatever that may mean under the circumstances) of the vision of his illustrious leader, elucidates and illuminates those blank-spots that the "great scientist" left in the shadows for want of time and perhaps energy when he proclaimed

his or her vision. The under-labourer is unquestionably schooled in the exigencies of the new vision. But he differs from the "great scientist," not because he shows less ability to read the true and the real, since there is no true and no real for him or anyone else to read. And he certainly does not differ from the "great scientist," his leader, because he is less able to read the facts, for facts are, as we know, theory-laden or theory-dependent. He differs from the "great scientist" because he is less daring, less charismatic, less creative, and less visionary. He is after all an "under-labourer." He is well intentioned, he likely is scrupulous in his judgements, and he is committed to the advancement of knowledge (again, whatever that signifies in the Kuhnian context), but he simply has none of the political skills that are essential for him to persuade his fellow scientists of the value of their espousing a new vision, namely, his vision. And so, for Kuhn, the "under-labourer" is more like a "good house-keeper," while the "great scientist" is like a great modern politician, namely, a creator of conventionalities for us to live by, rather than being what we traditionally understand by "great scientists," i.e., a revealer of the true and the real. What all of this means, of course, is that Kuhn has reduced man to being either a mesmeriser, of which there are few in number, or a mesmeriser's acolyte, of which there are many. And the precise location in which an individual person finds himself or herself will depend on his or her daringness, his or her assertiveness and his or her capacity to take action in the face of meaninglessness.

Now, how do we translate this into the study of the social sciences, and more particularly into a set of recommendations for the study of social and political reality? Not much ingenuity is required in order to do this. An increasingly large number of social scientists have adopted a Kuhnian perspective since the appearance of *The Structure of Scientific Revolutions* in the early 1960s. Not unlike Kuhn's great natural scientist, the great political leader, it is held, is not a someone who seeks to bring about a way of life that privileges the true and the good for his community—at least, not since the beginning of the modern era has this been the case. Rather, the great political leader is he or she who, with the assistance of his under-labourers, seeks to impose an order, namely, his or her order—an arbitrary order, to be sure—on the political community over which he or she rules, and if it turns out to be liberal democratic, then so much the better, since this will likely occasion less friction. But if it is something other than liberal democratic, then, so what? There is no true, i.e., right, and no good way for us to live with one another. There are only conventions to guide us, and while it is true that conventions that favour predictability and stability are better than those that do not, sometimes they are impossible to achieve and we have to settle for what is practicable.[12] Of course, no one wants to say this out loud. In fact, it may even be a good idea to hide it from public view. Leo Strauss (and his followers, the Straussians) seemed to think so. But the overwhelming arbitrariness of all public order is well known to the few who know such things.[13] [14] Now, obviously, from this perspective, absolutely any order is better than no order, and, before everything else, action in the face of meaninglessness ("meaningless" is the pre-paradigmatic or inter-paradigmatic chaos) is better than hesitancy and inaction. Act to bring about order, any order, and then allow yourself to reflect on the order that you have brought about, for prior to your acting, there is nothing for you to reflect on or think about. "*On s'engage, puis on vois*," said Napoléon. Now, where else have we heard this? Could it be that we have read it in the writings and sayings of some of the great tyrants of modern times?[15] [16]

Turning now to Polanyi, we can say with little fear of being wrong that Polanyi's conception of *who man is* is almost completely at odds with that of Kuhn both as regards its insight into the character of who man is and the implications that flow therefrom. At a very basic level, Polanyi does not divide the scientific community into two groupings, one small group composed of *creative* cognoscenti, i.e., "the great(s)" in the world of the natural sciences, and a much larger group made up of under-labourers. Nor does he speak to us about how "great scientists" differ from the rest of us in terms of their creativity, or about how their less assertive and less creative acolytes, namely, Kuhn's "under-labourers," mop-up after them. Polanyi knows that scientists

are not engaged in *creation*, but in *discovery*. But, more importantly, Polanyi speaks about the oneness of the structure of the knowing process, and by implication of the human species. The scientific community, and by implication, the human community generally, cannot be fragmented into two or more segments. Whether we be speaking of the knowing process at the level of simple perception or at the level of the most abstract of disciplines, it is one, Polanyi informs us. Whether we be concerned with questions of simple sense perception, nuclear physics or Italian Renaissance history, the same bi-dimensional structure of knowing is in play. All human beings know in exactly the same way. We know things focally or explicitly in the sense that at a given moment in time a certain thing is at the centre of our attention. It stands before our consciousness as an object (an otherness) to our subjectivity. It is known to us, or experienced by us, as being apart from us or at a distance from us (Polanyi speaks of it being *distal*). However, at the same time that we know a certain thing explicitly and experience it as being at the forefront of our mind and at the centre of our attention, so to speak, we know other things tacitly or subsidiarily, in the sense that we experience them as if they were part of us, that is, of us as the experiencing subject, so that if asked the question: on which side of the divide do these tacitnesses reside, on the you or no-you side of the divide? All of us would be inclined to want to respond, *on the me side of the divide*. They are *not* experienced as being at a distance from us, or as being at the forefront of our mind, or as objects of our attention. Rather, they are experienced as dwelling within us, as being rooted, and sometimes deeply rooted, within us. Of course, we can move most of the things that are tacit to the forefront of our mind, and have them become focally present to us. However, when we focus our attention on something that was formerly tacit, we cannot know it, i.e., experience it, as we knew it when it was tacit for us. We now know it explicitly. Simply put, we cannot call the tacit, in its tacitness, to the forefront of our mind, and have it remain tacit. When something is at the centre of our focus, it cannot be tacit. It is, of necessity, explicit. Now, I just stated that "we can move *most* tacit matters to the forefront of our minds," which implies that some tacit matters cannot be brought forward into the focal field. Indeed, this is the case. Some tacitnesses, with the passage of time, become so deeply rooted within us that they cannot be resurrected into explicitness. They have become us, and we them, to such a degree that it can be said of these tacitnesses that they constitute part of our very identity as Tom, Dick or Harry.[17] Now, the point to my saying this is that, with the passage of time, the person who masters a discipline, let us say, one of the natural sciences, social sciences or humanities, does more than know it in the conventional sense of the term "know." That is, he or she knows more about this discipline than he or she can recollect into explicitness. The subject in question, or more accurately aspects of the subject, are not things that stand explicitly before the consciousness of the knower as an object to the knower's subjectivity. Rather, they are tacitly present to the knower's consciousness; indeed, so deeply rooted in that consciousness that they are experienced as being one with the knowing subject. Polanyi says, in such instances, that "we know more than we can tell." In fact, the truth is that the knower incarnates the subject's aspects (Polanyi speaks of *indwelling*), in the sense that they have become him or her, and he or she has in part become the them, such that the subject and its aspects have fused with and modified "the way of being in the world" of its practitioner, and made of its practitioner a master of that field. Of course, the consequence of this is that we, of necessity, have to say of that subject that it does not reside within books, as many of our contemporaries might be inclined to say. Rather, it resides in the ways of being of its greatest practitioners.[18]

Of course, the point to all of this is that, with time, the connoisseur of any subject ("the sage" is the expression that the Taoist would have used to speak of Polanyi's connoisseur) is that person who has undergone "*une déformation professionnelle*," a deformation that is at once a deformation and a reformation such that he or she no longer is the person he or she was prior to his having undergone his or her apprenticeship.[19] And the most remarkable thing of all, according to Polanyi, is that it is this *déformation* that enables the person, who has allowed himself or herself to experience it, to make contact with what is real and what is true, not in all areas

of scholarship, to be sure, but in that area in which he or she has been "deformed." The idea is that subsidiarisation and indwelling, for Polanyi, does not relativise or historicise the knowing process, as many who root their understanding of knowing in Enlightenment thinking are inclined to want to argue,[20] but becomes instead the means of transcending relativism and historicism and of allowing us to get at aspects of the real and the true, which is something that the Taoist, Chuang Tzu, understood very well. And so, evidently, according to Polanyi, calling forth the real and the true is not a function of implementing a method, or of reading the records of the sages so as not to make a mistake or in order to get a head-start. That is the manner of understanding of a technician. Calling forth the real and the true into explicitness is a function of bringing to bear on a problem that is of interest *all that one has come to be at a given point in one's life*, and then making a decision about what is real and true. Hence the importance of the Taoist wheelwright wanting to know if the sages are alive, for, if they are alive, we can almost hear the unknown wheelwright say to the Duke, "then tell me Duke, where do they live, for I want to see them *be*. I want to be in their presence, and see for myself how a sage comports himself, since it is the comportment (the way of being) of a sage that makes a sage a sage." In short, the wheelwright wants to see how the sages have deformed and transformed themselves.[21] Now, Polanyi, in perfect Taoist form, although somewhat less poetically, in *Science, Faith and Society*, speaks to us about the same phenomena—the phenomena of being alive and of being a sage—in connection with Nineteenth and early Twentieth Century physics, chemistry, etc. He informs us that there was something like an "apostolic succession" in the field of i.e., physics, when each individual person in a long line of soon to be famous physicists, spent time with a famous predecessor, not primarily in order to receive explicit information from his great predecessor about how to do physics, for he was already explicitly knowledgeable in the field, but in order to absorb the way of being of the great predecessor in the field, i.e., in order to acquaint himself with the character and the comportment of a great physicist, i.e., a great *sage*, or in the language of the Indian sub-continent, of a *mahat'ma*, for Polanyi too knows the importance of the question "are the sages alive?"[22]

This brings me to say a few words about education, which, it is said, is something that should be available to all in a modern liberal democracy. Based on the foregoing, we must understand that if education has something to do with the acquisition of knowledge of what is real and what is true, then it is not true that knowledge of the real and the true is equally available to every human being, in the sense that all human beings are capable of acquiring such knowledge given the opportunity to attend school, etc., at least, not according to Polanyi. Polanyi makes it clear that some people are quite incapable of arriving at the knowledge of the real and the true either (a) by virtue of the fact that, though, in principle, *open to their reception*, they have, through no fault of their own, developed few or none of the requisite tacitnesses in any recognised field of study, or (b) by virtue of the fact that they are *closed to the reception* of the requisite tacitness as a consequence of their having fallen victim to a blindness that results from ideological thinking.[23] One's capacity to know the real and the true is a function of one's being *open*, which in turn is related to who one *is* as a human being, in the deepest sense of the word "is." And so, while *training* may be the great equaliser amongst men, inasmuch as it liberally distributes procedural know-how across a wide spectrum of people, *education* is definitely not an equaliser of men, according to Polanyi. Education is, in fact, the great differentiator of men, inasmuch as it separates us from one another in the most profound sense imaginable. Education is about the transformation of our *self*, a transformation that takes place in a multiplicity of rich and wholly unpredictable ways depending on one's antecedents, that is to say, on the history of one's embodied knowledge ranging from the elementary levels of simple perception to the most demanding levels imaginable for those who are able to acquire such knowledge.[24]

Now, what does this lead us to say about Kuhn's understanding of who man is versus Polanyi's understanding of who man is? For one thing, it teaches us that Kuhn had a rather superficial understanding of

who man is compared to Polanyi. In fact, it hardly ranks as an understanding of who man is. Kuhn's human being lacks density, i.e., lacks a full panoply of elements that lead to his having identity. For Kuhn, one is either a creator or an under-labourer, and a serious account of how one becomes either is missing, except for the fact that a creator identifies himself by means of his inflated view of himself and by means of his charismatic character and persuasive abilities, while an under-labourer defines himself by his plodding disposition. But do not expect Kuhn to go into these matters more deeply than that. Polanyi, on the other hand, presents us with a rich picture of who man is, a picture that very much accords with our experientially based understanding of this matter. It is a picture that measures man and the society of which he or she is a part against a standard that is knowable and real, and that is not the product of an arbitrary creative imagination, which sets no standard at all, other than the meanderings of the capricious mind of a wilful person, who need not even be "in his or her right mind."[25] Now, were this all that Polanyi did, it would be sufficient. However, Polanyi did more than that. He provided us with the means for intelligently critiquing the foundations of the social and political order in which we find ourselves in these late modern times. He provided us with a theory of knowledge that is also a theory of education, capable of drawing a distinction between training and education, a sorely needed ability in these times when "higher education" is under attack. He awakened us to the need to combat ideological thinking in all of its expressions, but particularly in the philosophy of science, where, if sane thinking is to prevail, scientism cannot be allowed to become more important than science.

Since I agreed with what Professor Moleski had to say in his interesting paper, my aim in this too brief piece was to draw attention to the implications of Polanyi's versus Kuhn's thinking for the social sciences, and more specifically for the study of politics. And one of the things that I have learned from Polanyi is that the pursuit of order in the field of politics is infinitely more worthy, interesting and rewarding when it is seen not as an opportunity to deny the real and the true in order to create an arbitrary order and system of control over a population where it is presumed that neither would exist otherwise, but as an opportunity to bring about the conditions that will make it possible for individual persons to realise, as best they might, their potential as responsible human beings and citizens living in conformity with what is real and what is true as it is discovered by an active community of explorers. Unfortunately, I have to report that the trend in the social sciences, and more specifically in political science, at the moment, is in the opposite direction. It is to ignore Polanyi's recommendations, and accept that we, as a community, move in the direction of the dehumanisation and infantilisation of the person, such that gradually we will no longer find it possible to speak knowingly of "the citizen," but instead we will accept to speak of subjects, of populations, etc., and where, when we do speak of "the citizen," "the citizen" will have become synonymous with "the population."

Endnotes

[1] *The Thomist* 53 (1989) 259-279.

[2] What I mean here is that Kuhn's understanding of how scientific knowledge emerges makes no reference to anything that could be seriously conceived of as "tacit knowing." Kuhn's understanding of the growth of scientific knowledge unfolds entirely within the confines of the Enlightenment project, that is, within the confines of what Polanyi means by "explicit knowledge," and this includes Kuhn's understanding of "paradigmatic shifts." In short, Kuhn will always be a late Enlightenment thinker for me, whereas Polanyi will never be someone who was completely "at home" in the Enlightenment setting.

[3] In fact, I should go further than that and say that Kuhn's notion of "Gestalt-shift" ought not to be associated with Polanyi's understanding of "tacit knowing," since it is nothing more than a mystification of an easily described experiential reality called "the tacit," which Polanyi illuminated quite well. Unable to shed light on how new

knowledge comes to be, Kuhn simply confounds his reader with a magician's trick, which is actually in contradiction with his notion of "incommensurability," since the elements of a Gestalt image are always comparable across the shift. In fact, it is their very comparability that draws our attention to the Gestalt image in the first place.

[4] I speak of "positivist" empiricism in order to highlight the distinction between non-ideological British empiricism, a form of empiricism with which Polanyi agreed, and ideological "positivist" empiricism, that Polanyi rejected outright. It is noteworthy here that in *The Structure of Scientific Revolutions* (1962), Kuhn countered positivist empiricism with an ideological position of his own, namely, relativist subjectivism. It was around this time, I suspect, that Polanyi saw Kuhn as a potential ally in his struggle against positivist empiricism. Unfortunately, Polanyi apparently may not have spotted the ideological character of Kuhn's thinking at this time. All he saw was an ally. In any case, in the "Postscript 1969"—after being challenged by the positivist empiricists to acknowledge that his thinking undermined *their* understanding of scientific thinking, which, it seems, Kuhn curiously thought was synonymous with scientific thinking—Kuhn espoused the positivist empiricist position. In subsequently published works, i.e., *The Essential Tension*, Kuhn seems to have moved back to his original position as expressed in the pre-postscript version of *The Structure of Scientific Revolutions*.

[5] Aspects of Kuhn's thinking are frequently associated with the thinking of N. R. Hanson (*Patterns of Discovery*, [1958]), Stephen Toulmin (*Introduction to the Philosophy of Science* [1953] and *Insight and Understanding* [1961]), and Paul K. Feyerabend (*Against Method* [1975]).

[6] For the benefit of those who may not be engaged in the study of the social sciences, I draw attention here to the fact that there is a large and growing body of literature on the applications of Kuhn's thinking to the study of the social sciences, and some of this literature gives cause for concern. Characterising regional, linguistic and cultural communities, historical eras, and ultimately even individual persons themselves as incommensurable paradigmatic orders may seem to be an appropriate way to render these complex orders and the communications breakdowns amongst them during the modern era. But are these complex orders ever as incommensurable as Kuhn and Kuhnians would have us believe? Are the breakdowns ever as complete as the term "incommensurability" implies, and is force and violence necessarily the only efficacious means for opening up a seemingly unreachable paradigmatic order? In this regard, consider Samuel P. Huntington's "clash of civilizations" thesis. Cf., Samuel P. Huntington, "The Clash of Civilizations," *Foreign Affairs*, Vol. 72 no. 3, (Summer 1993) p. 22-28. Huntington does not mention Kuhn or Kuhn's incommensurability thesis, and I am not claiming that he even had Kuhn in mind when he wrote his famous paper, and later his book. However, if we pay attention to Huntington's language, and particularly to his almost single-minded focus on fundamental differences amongst civilisations, it is rather clear that he understands civilisations as insulated wholes creating serious barriers to cross-civilisation communication and the peaceful settlement of disputes. As a result, one is entitled to wonder if he is not responding to the same climate of opinion that Kuhn was responding to when he penned his thesis. By contrast, the explicit application of Polanyi's thought to the study of the social sciences is, it must be said, very much a minority concern, and, when Polanyi is referred to in social science literature, interest in him is fortunately not driven by the need to systematise one's thinking. Indeed, it most often comes about as the result of the exercise of common sense and prudential judgement, both of which are rare commodities in the social sciences during these ideological times. In short, one cannot conceive of a Polanyi-influenced social scientist advancing a view that would be as imprudent as the Huntington thesis, which, by virtue of its epistemological presumptions, leaves little room for political negotiation, and, as a consequence, is a little too given to speaking about the inevitability of the clash of civilisations.

[7] Marx did not see man as having a single identity. There was alienated man (i.e., all of mankind to date) and "new communist man." Likewise, Hitler spoke of non-Aryan man and Aryan man. And, although they would certainly not agree with the designations assigned by Kuhn, some of Leo Strauss' followers (but maybe not Strauss himself) appear also to be of the view that man is not a single entity. The "philosopher," according to some Straussians, is not a being like the rest of mankind. He is a being who appears to reside on a different plane, and he has available to him insights that mere mortals do not and cannot have.

[8] It needs to be stated here that Kuhn does not hold that the older members of the scientific community are wrong, from the scientific perspective, because they refuse to adopt the new vision. There is neither right nor wrong in the world of science, according to Kuhn, because there is no *real*. The older members of the scientific community

are simply being traditional, which they have every right to be, if they feel so inclined. See *The Structure of Scientific Revolutions*, p. 206

[9] Note that for the very same reason that older members of the scientific community are not wrong for rejecting a new "theory," the "greatness" of the "great scientist" is not a function of his or her getting things right. There is no such thing as "getting things right" because there is no *right*. The greatness of a "great scientist" is always a function of his or her persuasive abilities, abilities which he or she brings into play when he or she proselytises in favour of his or her vision. The growth of scientific knowledge is, of necessity, based on effective propaganda, according to Kuhn.

[10] It should be noted here that if Kuhn's view is accepted, there can be no such thing as "the past" (i.e., history) or the future, understood in the conventional sense. The past and the future can only be understood by Kuhn and his followers as categories within the present.

[11] See John Locke's use of the term "under-labourer" to refer to *something's or someone's playing a supportive subsidiary role to something or someone else* in various places in his famous work *An Essay Concerning Human Understanding* (1689-90).

[12] Some contemporary scholars assert that Machiavelli's prince was one such creative genius. He literally created a world and maintained it by the exercise of *virtu* (strength and will-power), until *fortuna* got the better of his creation.

[13] It is not my intention here to suggest that there is some sort of connection between Kuhn and Strauss. There is not. Strauss was not a relativist, and he was most definitely not a historicist.

[14] The "few who know such things" are those whom many Straussians call "philosophers." Plato was one of them, according to Straussians.

[15] In a manner that was entirely consistent with the ideas of the movement to which he belonged, Joseph Goebbels is reported to have once said: "Whenever I hear the word 'culture,' I go for my gun."—by which he apparently meant to signify that man, in the chaotic context in which he always finds himself in society, ought to act first and think later. Acting has the effect of creating order, i.e., "culture." Once order is created, then we can debate and discuss. See also Ernst Nolte, *Three Faces of Fascism* (New York: Holt, Rinehart and Winston, 1965), Chapter III.

[16] In a very interesting piece on Italian fascism that deals with the necessity and priority of acting in a context of meaninglessness, H. Stuart Hughes writes: "In a philosophy of action, the theoretical premise came first. In philosophy as action, the order not only was reversed; action itself *became* philosophy." See H. Stuart Hughes, "Action as Philosophy: The Void in Italian Fascism," *The Journal of Value Enquiry*, XXIX, (1995), p. 368. We might want to ask ourselves if, for Kuhn, 'action' (i.e., creation), in a climate of meaninglessness, *becomes* science.

[17] Were it possible—which it is not—calling forth these tacitnesses into explicitness would be as if we were becoming transparent unto ourselves. (It was this sort of total transparency that I had in mind earlier when I spoke of the Enlightenment project in n. 2.)

[18] There is a famous Taoist parable that captures the essence of what Polanyi appears to have had in mind here. The author of the parable is reported to have been the great Taoist sage and scholar Chuang-Tzu (c. 369-286 B.C.), and the parable is known in English by the title "The Duke and the Wheelwright." Below is an English translation of the parable provided us by Arthur Waley. While reading it, have in mind what it means to be an educator:

> Duke Huan of Ch'i was reading a book at the upper end of the hall; the wheelwright was making a wheel at the lower end. Putting aside his mallet and chisel, he called to the Duke and asked him what book he was reading. "One that records the words of the Sages," answered the Duke. "Are those Sages alive?" asked the wheelwright. "Oh, no," said the Duke,"they are dead." "In that case," said the wheelwright, "what you are reading can be nothing but the lees and scum of bygone men." "How dare you, a wheelwright, find fault with the book I am reading? If you can explain your statement, I will let it pass. If not, you shall die." "Speaking as a wheelwright," he replied, "I look at the matter in this way; when I am making a wheel, if my stroke is too slow, then it bites deep but is not steady; if my stroke is too fast, then it is steady, but does not go deep. The right pace, neither slow nor fast, cannot get into the hand unless it comes from the heart. It is a thing

that cannot be put into words; there is an art in it that I cannot explain to my son. This is why it is impossible for me to let him take over my work, and here I am at the age of seventy, still making wheels. In my opinion it must be the same with the men of old. All that was worth handing on, died with them; the rest, they put into their books. That is why I said that what you were reading was the lees and scum of bygone men." (Attributed to Chuang Tzu, and taken from Arthur Waley, *Three Ways of Thought in Ancient China*, who in turn took it from a text given in Huai-nan Tzu, XII.)

[19] Note that the word "apprenticeship" is just another term for "subsidiarisation."

[20] The propensity of Enlightenment thinkers and their descendants to assert that rooting knowing in the person leads to relativism and historicism is, of course, misguided. But, it is more than just misguided. It reflects a gnostic tendency at the heart of modern thinking, namely, a belief that there can be absolute knowledge, disincarnate knowing and knowledge, knowledge without a human knower, and that this knowledge is available to human beings if they know the incantation, which is *method*.

[21] Note here that the wheelwright does not ask our question, that is, the question that the great majority of us today would ask, namely, "Please tell me, what do the records say?" No. The wheelwright does not care about what the records say or do not say. The records are of no importance. Asking that question is asking the "critical" question, and he is not interested in having the "critical" question answered. He asks the pre-critical, or is it post-critical, question, the question that we today rarely, if ever, think of asking. He asks the Duke if the sages are alive. That is the post-critical question, and that is the question that absolutely floors the Duke. It is a question from which the Duke does not and cannot recover, and he does not even know it until the wheelwright reaches the end of his story. The Duke even has the nerve to threaten the wheelwright with death for being too alive, until it likely dawns on him that he is searching for guidance in the detritus of the dead where no guidance is to be found, and, as a result, he himself is more dead than alive.

[22] See Michael Polanyi, *Science, Faith and Society*, (Chicago: The University of Chicago Press, 1946), p. 44. When Polanyi speaks of "apostolic succession," he is not waxing poetic, as some of us may be inclined to think. He is reminding us of the fact that in the west too, there was once a time when the sage and the *mahat'ma* was revered because of who he was, and he is also telling us that that is still how it is within the world of the natural sciences, despite the fact that we have been brought up to think otherwise.

[23] We who reside in liberal régimes are inclined to think that knowing the real and the true is just a matter of someone exercising appropriate wilfulness. This is not so. Aristotle is especially illuminating on this point. In discussing tyrannical régimes, Aristotle informs us, in the *Politics*, that it is not just the tyrant who is a tyrant in a tyrannical régime. All who live under the rule of a tyrant become mini-tyrants, and are no better than tyrants. Every person living under the rule of a tyrant becomes obsequious with respect to all who are above him in the social hierarchy, and that same person is a veritable ogre with respect to his underlings. The tyrant corrupts the *psyché* of everyone who lives under his rule, the weak as well as the presumed strong. And when the tyrant, for one reason or another, disappears from the scene, the ordinary person who lived under his rule does not suddenly allow his better side to show through. Nor does he suddenly discover morality and become a genuinely decent person. He has no better side and good is completely foreign to him. The truth is that he remains who he was under the tyrant. In fact, if given the opportunity, he would take over from where the tyrant left off. We should not be deluded about this, Aristotle informs us. Indeed, that is why it is next to impossible to transform a tyranny into a liberal régime in the short or medium run of things. One has first to rebuild character from the ground up before that can happen. Parenthetically, this is why "bringing democracy to Iraq" was an ill conceived, not to say impossible, task. Creating a liberal democracy involves a great deal more than the imposition of a constitutional arrangement of offices. It is essentially a matter having to do with developing character and a way of being that sustains and is sustained by a practice of truthfulness and justice.

[24] The distinction between *education*, on the one hand, and *training*, on the other, is not a distinction that is frequently made in our day, and even when it is made, its implications are often not well understood. More often than not, the words "education" and "training" are used interchangeably. At a very general level, the distinction might best be defined in the following way: *An education is concerned with the transformation of the self, whereas a training*

is about the acquisition of a skill which can be sold, or the products of which can be sold, but it is not primarily about the transformation of the self. And yet, it is a great deal more than getting the distinction right that is important, particularly as it relates to the field of politics. The truth is that politics can only exist amongst the educated, and rarely exists amongst the trained. The fact is that it is this distinction between education and training that lends credibility and meaning to the words "citizen" and "citizenship," and further refines their opposition to words like "a people," "a national of," "a resident of," "a subject of," "a Canadian," "an American," etc. A "citizen" is someone who, by virtue of his or her education, is not part of a body of people whose future is managed by the state. He or she is not someone who is ignorantly shunted from one niche in the economy to another as openings are opened and closed at the behest of various forces that seem beyond control and hard to identify. A citizen is not someone who is told, on leaving university, that he or she can expect to undergo retraining at least once, and perhaps twice, in the course of his or her working career, so rapid is the pace at which we are discovering new information. An educated person does not need to be re-educated after leaving university, for he or she has the wherewithal, and if not the wherewithal, then at least the means to obtain it, for as long as he or she lives. A citizen is someone who, as a consequence of his education, has a good sense of the overall workings of the community of which he or she is a part, and he or she can think for himself or herself about the important issues that need addressing in that community. A citizen does not want the state to do the thinking for him or her. A citizen does his own thinking because he can think. On the other hand, it is not just a matter of happenstance that the modern state is slowly but surely ruining education and substituting training for education. What modern state would want an educated citizenry interfering in what it perceives as its affairs and thus complicating its management of the state? And what better way for the state to assure itself that this will not take place than by focussing on training its nationals as against educating its citizens? In this connection, we should give thought to the writings of that old-fashioned American social critic Albert Jay Nock, who is still a delight to read. But let us also be aware that because of his focus on personal knowledge, Polanyi also is a profound critic of the direction in which the modern state is heading in regard to these matters.

[25] Being in one's "right mind" means a great deal to Polanyi. What does it mean to Kuhn? Can we even speak of one's being in his or her "right mind" when we speak of Kuhn? What part does being in one's "right mind" play in Kuhn's thinking, where being wilful is more important than being right?

Submissions for Publication

Articles, meeting notices and notes likely to be of interest to persons interested in the thought of Michael Polanyi are welcomed. Review suggestions and book reviews should be sent to Walter Gulick (see addresses listed below). Manuscripts, notices and notes should be sent to Phil Mullins. Manuscripts should be double-spaced type with notes at the end; writers are encouraged to employ simple citations within the text when possible. MLA or APA style are preferred. Because the journal serves English writers across the world, we do not require anybody's "standard English." Abbreviate frequently cited book titles, particularly books by Polanyi (e.g., *Personal Knowledge* becomes *PK*). Shorter articles (10-15 pages) are preferred, although longer manuscripts (20-24 pages) will be considered. Consistency and clear writing are expected. Manuscripts normally will be sent out for blind review. Authors are expected to provide an electronic copy as an e-mail attachment.

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REVIEWS

Joel R. Primack and Nancy Ellen Abrams. *The View from the Center of the Universe.* New York: Riverhead Books (Penguin), 2006. Pp. 386. ISBN 1-59448-914-9. $26.95, hardback.

This is one of the consistently most interesting, insightful, and powerful books I have encountered.

Often cultural cosmologies, which tell about the significance of human life in the order of things, are distinguished from scientific cosmologies, which describe the origin and nature of the universe based on evidence assembled in astronomy and astrophysics. Rarely does a cosmological vision have room for both cultural and scientific concerns. *The View from the Center of the Universe* is an exception. It is thoroughly grounded in the new scientific vision of the cosmos that has emerged in the past twenty years or so, yet it is sensitive to the need humans have for world orientation through myths, and it offers a compelling case for the significance of humans in cosmic evolution. It is one of those rare books having the power to transform one's understanding of self and world.

Primack's scientific credentials are solid. He is one of the developers of the theory of cold dark matter and has served in leadership roles in prominent professional organizations. His wife, Nancy Abrams, is a gifted writer, lawyer, and consultant to governments on scientific matters. The book is a product of a course they have co-taught at the University of California, Santa Cruz, for a decade. Their writing reflects their concern to be intelligible to non-specialists, yet their footnotes often introduce the more technical aspects of the topic at hand and point to a rich reservoir of further resources. Their lucid prose is augmented by helpful illustrations and symbols. Four of the key drawings are available on the web at http://viewfromthecenter.com, and other helpful supplements to the book are also available there.

In a way, the aim of the Primack and Abrams is very close to what Michael Polanyi attempted to achieve in *Personal Knowledge*. In reaction against the false objectivism of his time that eliminated human significance, Polanyi described human involvement in all knowledge and set human achievement in cosmic evolutionary perspective in Part Four. Primack and Abrams do not concentrate on epistemology as Polanyi did, but like Polanyi, they attend to our religious longings while setting their vision on solid scientific ground. Their aim is "not only to help people understand the universe intellectually, but also to develop imagery that we can all use to grasp this new reality more fully and to open our minds to what it may mean for our lives and the lives of our descendents" (8).

The book is divided into three parts: Cosmological Revolutions, The New Scientific Picture of the Universe, and The Meaningful Universe. The first part chronicles the evolution of cosmology from thinking in terms of a flat earth, to the medieval notion of the heavenly spheres in which human existence is central, to the post-Newtonian notion of a vast realm void of meaning. The authors describe the fecund cosmological imagination of the ancient Egyptians with genuine appreciation for "the attitude that *multiple non-dogmatic interpretations* of the cosmos are more inspirational than a single arbitrary story" (48). They also attend to Hebrew and Greek cosmological myths, but perhaps the shamanic worldview of the Huichol Indians garners their greatest appreciation among early flat-earth myths. Their stories "cultivate a sense of kinship, of organic connection, with the universe itself" (35), a connection that has unfortunately been severed in recent centuries.

In recent years many people have called for a new meaning-supporting myth embracing science – Loyal Rue, for instance, or Thomas Berry and Brian

Swimme. Typically the story of evolution has been highlighted as at the core of the needed myth, not the more inclusive vision of the cosmos featured in the work at hand.

> Without modern scientific cosmology, no people, no matter how wise, creative, and good, can create a mythic language through which the universe can speak to our global, science-based culture. We need to work together to achieve a cosmic perspective that can inspire a vision powerful enough to master the technological forces that threaten our survival. Whatever myth might emerge, if it is science-based, it won't stand still. As long as the universe expands, the myth must absorb, be tossed out by, or else be enfolded in larger understandings. No myth is for all time, but mythmaking is. (36)

In light of the subject matter of this issue of *Tradition and Discovery*, it is interesting to note that Primack and Abrams, when describing the history of scientific thought, strongly oppose the relativism associated with Thomas Kuhn and his postmodern successors. The authors contend that, with few exceptions, revolutionary "scientific theories do not have to overthrow their predecessors except in the earliest stages of science when a scientific theory is replacing earlier ideas that were not well supported by evidence" (24-25). New scientific theories encompass older theories by defining the limits within which the older theory is true. In emphasizing the progress of science toward ever-larger truths, the authors are again in harmony with Polanyi's vision.

Part Two sets forth the contemporary vision of the cosmos with greater clarity than any other work with which I am familiar. This cosmological vision requires conceptualization that lies well outside our earth-evolved intuitive patterns of thinking. The Big Bang is described as a phase transition from eternal inflation, a phenomenon that is hypothesized to exist "beyond" our realm of space-time. Much of the density of our universe is constituted by dark energy, a force within space that expands it at rates beyond the speed of light. As a second member of the "Double Dark" theory, dark matter provides the gravitational attraction that has led to the formation of stars and galaxies. Only about half of one percent of all matter and energy constitutes the visible universe with which our senses potentially have some direct contact.

While the cosmos seems to be a very strange place, the authors take great pains to show that it is possible, and indeed vitally important to our survival, for humans to feel a deep connection to this cosmos. In many respects, humans are said to occupy a central place within the evolving cosmos. "The only place beings with a consciousness like ours can ever feel ourselves *belonging* to the universe is at the center. But the longing to be central is not what makes us central: the structure of the universe makes us central" (272). We are roughly midway between the micro and the macro worlds when these are analyzed in term of orders of magnitude. Ours "turns out to be the only size that conscious beings like us could be. Smaller creatures would not have enough atoms to be sufficiently complex, while larger ones would suffer from slow communication" (161) – and the consequent inability to respond as individuals quickly enough to survive certain environmental hazards. Humans also live on a planet at the midpoint of our planet's career when it offers maximum hospitality for complex life. Its relative stability and maintenance of correct temperature to allow life to evolve indicates we exist at an extraordinary point in cosmic space and time. We necessarily "live at the center of our Cosmic Spheres of Time" (271), which also is the peak period for learning about distant galaxies now beginning to disappear over the cosmic horizon. Finally, we live at a crucial ecological moment when it is imperative that we develop a responsible, humble vision of our impacts on the earth, a vision that can be enhanced by cosmic insights and metaphors.

Primack and Abrams make a brief but powerful pitch for the importance of a scale sensitive vision of things. At every few increasing orders of magnitude,

the increased complexity makes for emergent features that are different in kind than their components. Many thinkers fall into Scale Confusion (for example, applying Newtonian physics to the whole universe) or Scale Chauvinism (of which reductionism is one variety), thought that typically uncritically projects concepts that are useful at the human scale but inadequately relate to reality at other scales. Rather than think only in parochial terms, "one's thinking should always be on a *larger* scale than one's actions if those actions are to be meaningful. To act wisely globally, we must think cosmically" (252). At the top of our needs as a people now is to apply scale-appropriate cosmic metaphors so as to foster sustainable prosperity that stretches beyond the mundane here and now. "This planet is so diverse that the way to deal with global problems is not to impose global solutions but to cultivate the *common ground of a large-scale goal* and encourage small-scale, decentralized solutions, appropriate to different situations, created by different kinds of people inspired by that goal" (265).

In this review I can only begin to suggest the richness of *The View from the Center of the Universe*. Not everyone will be convinced that humans inhabit the extraordinary place in the cosmos the authors argue for. Indeed, they quite openly proclaim that they only provide a basis for seeing human existence as meaningful, but that individuals can equally choose despairing or authoritarian views of existence. They never claim to present an artistically compelling myth that might capture our imaginations, but they do think they provide the grounds for meaning-rich myth making that has the great advantage of being grounded in what is real. In my view, they successfully realize this aim; they do provide "*the opportunity to see everything afresh through a new cosmological lens"* (297).

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Francisco J. Ayala. *Darwin and Intelligent Design*. Facets Series. Minneapolis: Fortress Press, 2006. Pp. xi + 116. ISBN: 0-8006-3802-6. $7.00. paper.

Initially invoking the names of Thomas, Augustine, and John Paul II, Ayala enters into current debates about Intelligent Design in order to establish that (1) religious faith and science are not incompatible with one another and (2) that Intelligent Design is both bad science and bad religion that preys upon faithful people of good will. In making his case, Ayala largely proceeds chronologically, devoting the first chapter to an appreciative but critical discussion of William Paley's argument from design, faulting it most seriously for dismissing the extent of imperfections in nature (8). Ayala then spends four chapters in a comparatively detailed exposition of evolutionary theory. He summarizes Charles Darwin's account of evolution by natural selection (chapter 2), sets out additional evidence for the theory (chapter 3), provides an overview of human evolution (chapter 4) and examines more closely the mechanism of natural selection (chapter 5). Ayala is especially keen to make the point that evolution does not happen randomly but incrementally via natural selection, a process that preserves adaptive changes in organisms. As an example, Ayala offers a plausible account of how the complex eye emerged over time rather than as the result of a designer's work (66-7).

This example hints at what is to come, for having spent several chapters on evolution, Ayala turns to Intelligent Design (ID), devoting but a single chapter, the sixth, to that theory. Therein Ayala identifies the major proponents of ID and responds to their claims. First, he attacks the way proponents of ID juxtapose "theory" with "fact," thus distorting the use of the term by scientists (72-77). Next, he attacks claims that the intricate complexity of living creatures, especially that of the eye, bacterial flagellum, and blood coagulation, is too much to be explained by chance (80-85). Finally, he presses the issue of oddities and deficiencies in nature that argue against intelligent design (85-89).

Ayala concludes with a chapter devoted to the topic of belief. Intending to show that faith and science do not have to conflict, he finds support in the writings of key figures in Christian history (Augustine, A.H. Strong, Pius XII and John Paul II), as well as statements made on the topic by mainline Protestant denominations (PSUCA and ELCA), and the Central Conference of American Rabbis. He then turns to a brief history of court cases dealing with creationism and ID; notably, supporters of creationism and ID have lost them all. He concludes by explaining how science is methodologically naturalistic and therefore, the excesses of some scientists aside, cannot speak meaningfully about religious beliefs and their implications.

Promising a cogent, rigorous argument that establishes his pair of theses, Ayala delivers a more historical reflection that gives an inordinate amount of attention to evolution, with more philosophical reflections appended to the end. The relative lack of attention to ID gives the appearance of bias (deserved or not) and leaves the book open to such criticism. That criticism itself may not be fair, however. His treatment of evolutionary theories is clear and accessible to a popular audience that likely needs to be more informed about exactly what evolutionary theories claim than what ID is about. Furthermore, although his treatment of ID is comparatively brief, his rejoinders succeed in large part because of the groundwork laid in the previous chapters on evolution.

Conceptually, the most problematic claim Ayala makes is that religion and science can be easily compartmentalized. While there is some truth to the claim that religion and science are radically different discourses, Ayala fails to acknowledge that this "solution" to the conflict between science and religion has its own problems. First, it cannot adequately explain why conflict between science and faith seems so intransigent and occurs so frequently. Secondly (and perhaps related to the first), it takes a very un-Polanyian position that does not acknowledge that faith or belief of some sort underlies even scientific knowing. Regardless, the book will serve as a useful, concise, and lucid introduction to theories of evolution, as well as a rejoinder to the claims of ID, even if it begs additional philosophical and theological questions.

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Brian D. McLaren. *A Generous Orthodoxy*. Grand Rapids: Zondervan/Youth Specialties, 2004. Pp. 297. ISBN: 0-310-25747-6. $19.99, hardback (also available as a 2nd edition, 2006. ISBN 0-310-25803-0. $14.99, paper).

Brian McLaren has become a respected leader in the Emerging Church movement, a loosely-allied "community" that is rethinking Christianity in and for a post-modern culture. Holder of an MA in English from the University of Maryland, McLaren taught college English from 1978-1986, when he became founding pastor of Cedar Ridge Community Church in the Baltimore-Washington area, a position he held until 2006. He now devotes his time to writing and speaking on a variety of topics related to the emerging church (for more biographical information, see www.brianmclaren.net). The author of several books, McLaren in *A Generous Orthodoxy* represents a confession of faith that he thinks is both faithful to the wider Christian tradition and appropriate for a post-modern age.

The book is divided into two sections, the first of which addresses the topic, "Why I am a Christian." Herein, McLaren recounts "the seven Jesuses I have known," from that of conservative protestants to the Jesus of liberation theologians (summarized effectively in a chart on pp. 64-5). He then explores some of the implications of that variety, from which he concludes that Jesus needs to be saved from what Christians have done to him (101). In the second section of the book, McLaren describes the kind of Christian he is, using a dazzling and provocative array of terms. He describes himself as missional, evangelical, post/protestant, liberal/conservative, mystical/poetic, biblical, charismatic/contemplative, fundamen-

talist/ Calvinist, Anabaptist/Anglican, Methodist, catholic, green, incarnational, depressed-yet-hopeful, emergent, and unfinished. While the juxtapositions may not always make intuitive sense, what remains constant is McLaren's driving desire to, in his words, "find a way to embrace the good in many traditions and historic streams of Christian faith, and to integrate them, yielding a new, generous, emergent approach that is greater than the sum of its parts" (18).

Throughout the book, McLaren writes in a clear, non-technical language that demonstrates a high degree of fluency with the Christian tradition in its various incarnations. He has obviously done substantive reading on church history and history of Christian thought. His often playful prose embodies the kind of irenic spirit and generosity that he thinks should characterize an authentic orthodoxy, which does not mean that he is not critical of various expressions of Christianity. For example, he criticizes both ultra-conservative and liberal versions of Christianity while at the same time remains generous in the sense of being willing to learn from both (Chapter 8). Overall, McLaren comes across as a master bricouleur who wants to overcome the dichotomous thinking so characteristic of the modern world.

If there is an intellectual failing in his work, it is that which plagues most works of bricolage. McLaren does not always identify the principle(s) of selection by which he identifies the good of various Christianities, although he sometimes appeals to "the spirit of Jesus" (120) or the limits of language to communicate God's nature (151). Neither does he articulate in detail how disparate convictions can be held together coherently. For example, he notes that what connects Anglicans and Anabaptists is that in different ways they have all resisted modernity (212). That is an intriguing observation, but are their ways of resisting compatible with one another? These "faults" may represent fuzzy thinking on his part (a typical criticism of post-modern works), or a failure of imagination in people too deeply formed by modernist ways of thinking and seeing (a typical post-modernist rejoinder to critics), or some combination of these and other factors. I wonder, however, if there might be another more Polanyian explanation, i.e., that McLaren knows more than he can say.

Regardless, I bring up Polanyi because this book will be of interest not only to those who follow contemporary developments in Christianity and who are interested in the phenomenon of post-modernism, but also to those who follow the work of Michael Polanyi. Why? Because McLaren's work explicitly draws from Polanyi at some points and shows affinities with Polanyi's ideas at others. Furthermore, I suspect that greater use of Polanyi would strengthen McLaren's responses to those who complain because they do not know how to categorize him.

McLaren explicitly draws from Polanyi's discussion of tacit knowing to treat orthodoxy as the "internalized belief" by which many Christians live and from which they attend to the world (32-33). He again mentions Polanyi by name when he reflects on what it might mean to allow Jesus to be a master teacher. Here, McLaren adapts Polanyi's ideas to describe religious traditions as practices or ways of life or apprenticeships through which we develop personal knowledge (87).

Besides these explicit references to Polanyi, McLaren shows affinities to Polanyi at two points. The first is in the terminology that McLaren uses to describe himself. McLaren explicitly calls himself post-critical instead of post-modern (18). Although from the context it is not clear that he uses the term as Polanyi does, the choice of language is intriguing. The context suggests that post-critical means for McLaren an attempt to form a new whole out of the mixed bag of the past—which does not altogether seem to be what Polanyi means by the term (and is not exactly consistent with McLaren's own stated aim not to seek a "blended" Christianity but a hologram Christianity [66]). Nevertheless, McLaren does seem to be post-critical in the Polanyian sense in that he wants to combine some of the pre-critical, fiduciary dimensions of traditional, orthodox Christian faith with the results of modern historical consciousness (see for

example his discussion of orthodoxy, 28-30). In the end, while McLaren may not use the term exactly as Polanyi does, he appears to embody the post-critical spirit.

Secondly, McLaren's chapter on emergence echoes' Polanyi's (and Teilhard's) understandings of the emergence of the noosphere. McLaren, drawing explicitly from the work of Ken Wilber, argues that reality is multi-leveled so that new levels "embrace and build on" rather than exclude earlier levels with the result that human consciousness emerges out of previous levels (279-280).

One point at which McLaren might benefit from closer attention to Polanyi is on the topic of indwelling. McLaren is correct, I think, to treat religious traditions, at their best, as entities in which we dwell tacitly and from which we attend to and understand the world. What is missing from his discussion, however, is any attention to the other part of that dynamic, i.e., breaking out. We indwell in order to break out, a point Polanyi makes explicitly about religious worship (*PK*, 198) and a point that McLaren embodies, even if he does not articulate it. His generous orthodoxy does not passively indwell what most would consider traditional orthodoxy. Instead, the latter is something out of which he breaks, without at the same time severing his ties to it.

In the end, McLaren's *A Generous Orthodoxy* provides a thoughtful, articulate account of an emerging way of thinking about Christian faith that addresses important philosophical, theological, and historical issues in a way accessible and inviting to lay audiences. Of particular interest to readers of *Tradition and Discovery*, the book demonstrates a suggestive way in which Polanyi's ideas can be used to understand the dynamics of religious life. At its best, the book models the generous and ecumenical spirit it advocates, even if it does not provide the detailed argument that philosophers and theologians might prefer. As a kind of mediating theology, it remains to be seen whether a generous orthodoxy can survive the centrifugal forces that H. Richard Niebuhr suggests inevitably undo all such theologies. Regardless of what their ultimate fate might be, however, McLaren's confessions are well worth dwelling in for the present.

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