

Tradition & Discovery

The Polanyi Society Periodical

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Preface

This issue is the last before the June 8-10 Loyola Conference; included is the final call for papers and an update on the program, as well as the call for papers for the November 2012 annual meeting. It is not too late to make a Loyola paper proposal but the deadline is approaching. The Society is still seeking donations to support scholarships for this conference. As in past February issues, there is the annual financial report and the minutes on the annual meeting of the Board. The bottom line is the Society is solvent and the Board is organizing to address several important issues that lie before us in the near future.

Mary Jo Nye's *Michael Polanyi and His Generation: Origins of the Social Construction of Science* came out last October. Ted Brown and Dick Schmitt have review essays on this interesting new book and Mary Jo Nye kindly provided a response to Brown and Schmitt.

Darcia Narvez, a psychologist, was invited to give a paper at the November 2011 annual meeting; Gus Breyspraak and Paul Lewis responded to this paper. These materials provoked a lively discussion in San Francisco; they are included in *TAD* 38:2, along with a final word of response from Narvaez.

Daniel Paksi, a young Hungarian Polanyi scholar, raises issues concerned with Polanyi's discussion of evolution and emergence; Paksi's essay challenges the criticisms that Walter Gulick and others have offered (in *TAD* and other journals) of Polanyi's account. I invited Gulick to respond to Paksi in this issue.

Finally, there is an essay on Polanyi and Karl Popper that Struan Jacobs and I wrote. The essay digs into little-known archival correspondence and closely examines some Polanyi publications and lectures from the early fifties, the period when Polanyi first made clear his differences with Popper. Also you will find two book reviews this round, one covering a widely reviewed and criticized popular book by two prominent philosophers.

Phil Mullins

Tradition & 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

A Note on Dues Payment

The Oct. and Feb. issues of *TAD* include a membership flyer and an addressed envelope to be used to mail annual academic year dues and/or to make donations to the Polanyi Society. US postage regulations require that **EVERY** copy of *TAD* mailed in the postage class used must weigh exactly the same. Thus, even if you have already paid your annual dues, you will receive these membership materials in your copy of *TAD*. Dues remain \$35 (\$25 for libraries and \$15 students), a bargain in the academic journal world. Except for those residing outside the US, members should pay dues with a check. **The Society can no longer easily and inexpensively process credit cards.** We are presently setting up a Pay Pal payment option on the Polanyi Society web page. Until the Pay Pal option is operational, subscribers unable to pay dues with a check in US dollars, should contact Phil Mullins. Note that dues and donations are handled by the Polanyi Society Treasurer, Charles Lowney, whose address is on the enclosed envelope; Phil Mullins should be contacted directly for *TAD* address changes (contact information is on the inside of the cover).

Minutes of Board Meeting November 18, 2011

Present: Voting Members: Wally Mead, Phil Rolnick, David Rutledge, Diane Yeager, Andrew Grosso, Gus Breyspraak, Dale Cannon, Esther Meek, and Charles Lowney (arrived late). *Non-Voting Members:* Phil Mullins, Jere Moorman, Marty Moleski, Walt Gulick (arrived late).

1. President David Rutledge called the meeting to order at the Daily Grill Restaurant at approximately 8:15 PM.
2. The Minutes from the Board meeting in Atlanta of October 29, 2010, were approved as distributed.

3. The Treasurer's Report was postponed until Charles Lowney arrived (see item 11 below).

4. Report on *TAD*, sent out by Phil in advance.

- The primary issue is whether or not we are going to continue to publish a paper journal — or rather, when we will cease to do so. In the long term, we will have to shift entirely over to a digital journal. But for the immediate present, what we do is OK, though postage will soon raise the rate significantly. Dale proposed that a survey be taken of the current readership to find out preferences. Digital publishing of *TAD* in MUSE and in JSTOR is being explored.

- As Phil Mullins is now retired from Missouri Western and no longer will be having the connections and privileges that he once had, Phil recommended that we need to create a committee (for perhaps a year) to oversee appointing a replacement to the current printing arrangement among other things. It is not as urgent to move the website (which likely can continue where it is for the immediately foreseeable future and, if so, would work well for Phil) as it is to move the production of a paper journal. David, as president, agreed to appoint a small committee to work with Phil to take on this task and insure an orderly transition.

- In any case, if and when we cease to produce a paper *TAD*, we need to give ample notice to subscribers and have it end on a July issue, the last of our annual cycle

5. Miscellaneous:

- Andrew suggested that the U of Chicago be approached with a proposal to have the Polanyi archives digitized. Phil commented that this would be a BIG project and probably we would need a big grant to pursue it.

- Marty added that the original Scott manuscript of the Polanyi biography is on disk and available for whoever requests it.

- Phil said that the “center of gravity” in Polanyi Studies has moved to Hungary and a new generation of students and scholars has emerged there in

connection with MPLPA (The Michael Polanyi Liberal Philosophical Association). We probably should try to work closely with them.

- Marty reported that John Polanyi has said that he plans to donate Michael Polanyi's letters and other papers that have never been accessible to the public to the University of Toronto Library.
- David reported an interest on the part of the Thomas Torrance Society in having a joint meeting on Polanyi and Torrance in celebration of his 100th Birthday anniversary.

6. Next Year's Plans:

- It was noted that the AAR Program this year has next to nothing in it about our (Polanyi Society) program this year. Several responded that it is important to get this exposure, even given the cost. The Endowment Committee was asked to take this up.
- Relating to the earlier discussion about *TAD* going all digital, the comment was made that *TAD's* counterpart in England, *Appraisal*, has a subscription-only access to new issues and to their archive of old issues. Their subscription list, compared to *TAD*, apparently has relatively few members. Marty suggested that we consider a subscription-only access, but Phil commented that this move would have some liabilities.
- We will need about \$2000 for next year's program at the Chicago AAR. In response to this announcement, Jere and Wally each pledged \$500 and Phil Rolnick pledged \$25.

7. Need for a Membership Secretary

- It was suggested that we have need of a membership secretary to alleviate some of the responsibility that Phil has been carrying. But we have up to now had little systematic record keeping of membership. Phil proposed that we hold off for a year from appointing a membership secretary, for Charles may be able to handle it without much additional work.

8. 2012 Loyola Polanyi Conference:

- Phil, Marty, Gus, and David are the Conference Planning Group.
- Costs were discussed, which have reduced significantly since the 2008 Conference, partly

because we will be meeting at the downtown Loyola campus. The proposed Registration Fee is \$185.00, which will include the Banquet.

- It looks like we will be able to cover expenses with our current budget trajectory.
- We plan to make contacts with the 2008 Conference attendees, the graduate [and undergraduate] students that attended our Meeting last year, and possibly contact relevant academic departments and graduate programs. Some volunteers may be needed to help with the emailing and/or phoning.
- It was suggested that the Endowment Committee work at trying to stimulate extra donations for the Loyola Conference. It meets tomorrow morning (Saturday 11/19) at Max's Deli at 7:30 am.

9. Travel Fund:

- Wally reported that the Travel Fund was not depleted at all this year. We have a little more than \$1100 available. He said we should double that amount.

10. Polanyi Reader:

- Walt Gulick arrived about 10 pm. He reported that the Reader has been done for 2 years, but he confessed apologetically that not much has been done in terms of follow up recently, but, possibly with some help from Dale and others, some progress should be made.

11. Treasurer's Report

- Charles arrived about 10:10 pm. He gave a quick version of the Treasurer's Report.
- At the end of the fiscal year (8/31/11) the General Fund had \$4,986.52; Travel Fund, \$1,074.69; *TAD*, \$846.33; and Endowment, \$9,950.24.
- The Report was accepted unanimously by voice vote.

12. Poteat Papers Archive

- Wally reported that the Poteat Papers have been sent to the Yale Divinity School Library and are presently being processed for archiving. There are currently about 3000 pages, including a large amount of correspondence (perhaps 40 %). What was on audio tape has been transcribed to CDs.

More is expected to be sent to the collection.
13. We adjourned about 10:25 pm.

Dale Cannon, Secretary

**Polanyi Society Treasurer's Report
Fiscal Year Sept 1, 2010 – August 31, 2011**

General Fund:

Opening balance 9/1/10:	\$4,641.52
Revenue:	\$4,570.00
Transfers from <i>TAD</i> acct:	\$4,000.00
Dues & donations:	\$ 570.00
Transfers out:	(\$2,660.00)
To travel fund:	\$ 922.50
To endowment fund:	\$1,737.50
Expenditures :	(\$1,565.00)
(includes \$1,390 Loyola Deposit)	
Ending balance 8/31/11:	\$4,986.52

TAD Account (at Missouri Western):

Opening balance 9/1/10:	\$1,429.04
Revenue(dues & donations):	\$5,827.60
Expenditures:	(\$3,127.87)
Printing:	\$1,319.75
Postage:	\$ 944.37
Supplies:	\$ 80.69
Bank Charges:	\$ 65.50
Transfers to General Account:	(\$4,000.00)
Ending balance 8/31/11:	\$846.33

Travel Fund:

Opening balance 9/1/10:	\$350.00
Revenue:	\$1,024.21
Direct donations:	\$ 100.00
Transfers in from genl acct:	\$ 922.50
Interest:	\$ 1.71
Outlays:	(\$ 300.00)
Returned Outlay:	(\$ 300.00)
Outlays:	\$600.00
Ending balance 8/31/11:	\$1,074.69

Endowment Fund:

Opening balance 9/1/10:	\$8,167.66
Donations (transferred from Genl Acct):	\$1,737.50
Interest:	\$45.08
Ending balance 8/31/11:	\$9,950.24

Overview of General, *TAD*, and Travel Accts:

Opening balances 9/1/10:	\$6,421.04
Revenue (dues, donations, interest):	\$6,499.31
Total Expenditures:	(\$4,275.31)
Transfers to Endowment Fund:	(\$1,737.50)
Ending balances 8/31/11:	\$ 6,907.54

Charles Lowney, Treasurer

Travel Assistance Available For Younger Scholars Attending Polanyi Society Meetings

For students and other young scholars planning to attend the June 8-10, 2012 Loyola Conference or the November 2012 Annual Meeting in Chicago, limited travel funding is available. Society members are urged to inform worthy candidates about this assistance. Those interested in this funding, as well as those who know of potential candidates, should contact Walter Mead (wbmead@ilstu.edu). Contributions to the travel fund are always welcome; those interested in contributing should e-mail Walter Mead. Related information about travel funds can also be found on the Polanyi Society web site.

Polanyi Society Speakers Bureau

The Polanyi Society's Speakers Bureau helps organize talks to groups by Polanyi scholars. Marty Moleski, S. J. and Richard Gelwick gave talks in 2010 at universities; Richard Moodey and Phil Mullins gave talks in summer of 2011 at a meeting in Gummersbach, Germany. If you know anyone who might be interested in a speaker, send the name and e-mail address to Phil Mullins (mullins@missouriwestern.edu). There is now a link on the Polanyi Society web page with general information about the Speakers Bureau. You will find there a précis of the talks given by Moleski and Gelwick. Several Society members have indicated interest in speaking on different aspects of Polanyi's thought. It is likely that the Society can arrange for someone nearby to provide a talk on a topic of interest.

Update on Polanyi Society Loyola Conference

The Polanyi Society conference that is scheduled for June 8-10, 2012 at Loyola University, Chicago, is taking shape. The conference will be held at the downtown Loyola conference center which is easily accessible by public transportation. This site is much like the Loyola conference center at which the 2008 Polanyi Society conference was held. Registration is online and is now open; some details about food options are still being negotiated but the registration form is on the Polanyi Society web site (<http://www.missouriwestern.edu/orgs/polanyi/> or simply polanysisociety.org/). Some travel assistance/conference registration scholarships will be available for students and/or younger scholars (see the notice elsewhere in this issue as well as Society President David Rutledge's open letter which seeks fund donations for the travel fund and other Loyola expenses).

Some plenary sessions for the Loyola conference are now set and possibilities for other sessions are being explored. Mary Jo Nye, Horning Professor in the Humanities and Professor of History Emeritus at Oregon State University, will give a talk on Friday evening June 8. Nye is a historian of chemistry with long-standing interest in (and many publications about) Michael Polanyi. Professor Nye received the History of Science Society's Sarton Medal for Lifetime Scholarly Achievement in 2006. Her new book, *Michael Polanyi and His Generation: Origins of the Social Construction of Science* (University of Chicago Press) is discussed (with a response from her) in two review articles in this issue of *TAD*. Walter Gulick, Professor Emeritus from Montana State University, Billings, will provide the Saturday, June 9 evening banquet address. Gulick had a long career at MSU, Billings, teaching in philosophy, religious studies and interdisciplinary humanities as well serving as a department chair, founding honors director and academic vice president. He was twice a Fulbright Scholar (once in Budapest where he worked on the then new journal of the MPLPA, *Polanyiana*). Gulick joined the Polanyi Society in 1972 at its inception and has been an important leader, serving as Society President, Board member and *TAD* review editor for twenty years. He has published more than thirty essays, review articles, and book reviews in *TAD* as well as many essays and reviews in other Polanyi-oriented journals such as *Appraisal* and *Polanyiana* and journals like *Zygon*. On Saturday afternoon, there will be a panel discussion focusing on issues in political philosophy. Panelists will be scholars whose writings about political theory/political philosophy makes use of Polanyi's work. It may be possible to set up one conference session in which one of Polanyi's "economics education" films made in the late thirties is shown and discussed by a panel of economists.

The 2012 Loyola conference, like the earlier conferences, will also include a number of concurrent sessions at which conference participants give papers. As of mid December, about 15 proposals have been submitted and other inquiries have been received; we anticipate that a full set of approximately 30 papers will come in before the deadline. Insofar as possible, papers will be grouped thematically. Sessions will be scheduled with ample time for discussion. The conference planning committee hopes for proposals covering a wide range of topics to which Polanyi's ideas can be related. See the call for papers elsewhere in this issue of *TAD*. This conference, like earlier Loyola conferences, will build in time for informal discussions among participants.

An Open Letter Soliciting Financial Support For The Polanyi Society

Dear *TAD* Reader,

Even a quick reading of the plans for the June 8-10, 2012 Polanyi Conference at Loyola University, Chicago outlined in this issue reveals the opportunities for scholarly enrichment this conference presents. Anyone who has attended these Loyola gatherings in the past knows how much more enjoyable they are than more typical, large academic meetings where you have to fight to access elevators or restaurants!

But the Chicago conference offers another, less visible, opportunity—a chance to support the Society’s work tangibly by making a special gift to the Loyola effort. Many Society members contribute more than dues to the Society each year, and that faithfulness, as well as an extremely Spartan operation with minimal overhead, has enabled The Polanyi Society to be one of the more active scholarly groups around. Publishing this journal, maintaining a web site (with many scholarly resources) and an electronic discussion list, sponsoring annual meetings (with peer-reviewed papers and/or invited distinguished speakers) in conjunction with the American Academy of Religion and occasional meetings with the American Philosophical Society, supporting important publications (e.g., the Polanyi biography and the re-issue of *The Tacit Dimension*), establishing the William Poteat archive at Yale, setting up a travel fund to help young scholars attend Polanyi-related meetings—all of these efforts have depended upon the energy and financial support of a relatively small society.

The Loyola conference entails certain unavoidable expenses (e.g., renting meeting rooms), as well as costs that enhance the event: (covering expenses for some plenary speakers, registration and travel support for some younger scholars, organizing a trip to the Polanyi archives). As in the past, contributions to offset special conference expenses are something we must solicit and count on. Please send a check to the Treasurer, Dr. Charles Lowney, Dept. of Philosophy, Washington and Lee University, Lexington, VA 24450 (e-mail: lowneyc@wlu.edu).

As other earlier announcements in *TAD* indicated, another giving opportunity that exists is the new Polanyi Society Endowment Fund, which aims to establish more stable, long-term funding for the Society’s activities. The endowment contains about \$10,000 at present, and we hope to double that amount in this next year. If you wish to know more about supporting the endowment (and that includes making a bequest), please contact me, since as the current Society president I chair the Endowment Committee (david.rutledge@furman.edu). Like last year, we had a challenge open until January 1, 2012 from endowment donors willing to match up to \$2000 received. We may be able to set up such a challenge later in 2012; if you are interested, send me a note. Send endowment donations (clearly marked as such) directly to Charles Lowney at the address above. Since the Polanyi Society is an IRS 501C3 organization, donations to the Loyola Conference and/or the endowment above the \$35 annual Society membership dues are tax deductible.

David Rutledge
President, Polanyi Society

2012 Polanyi Society Conference

Connections/Disconnections: Polanyi and Contemporary Concerns and Domains of Inquiry

Call For Papers

The Polanyi Society will sponsor a three-day conference June 8-10, 2012. Conference participants will have the opportunity to spend a morning (June 8) at the Regenstein Library of the University of Chicago reviewing the archival Polanyi materials held there. The conference will include several plenary speakers or panels as well as parallel sessions in which conference participants present and discuss papers with others interested in the session's particular topic. Like the Polanyi Society sponsored conferences in 2001 and 2008, this will be a conference that builds in many opportunities for discussion and is open to persons using Polanyi-related ideas in a number of fields.

Proposals are invited for papers that examine the themes of post-critical thought in the context of the new century. The following are some suggested general categories within which specific papers might be grouped. [Please do not think of these categories as a limit for submissions but as a springboard for your own reflections. The final program will reflect groupings adjusted in light of proposals submitted.]

- Polanyi As Public Intellectual: Cultural Criticism and Reorientation in the New Global Order
- Redeeming Reason: Does “Personal Knowledge” Have a Future in a Partisan World?
- Polanyi in the Light of Developments in Psychology (and vice versa)
- Polanyi’s Work in Relation to Current Accounts of Organizations, Institutions and Authority
- Doubt and Commitment in the Postmodern Environment
- Religion and Science: Polanyi and Current Discussions
- History and Philosophy of Science: Polanyi and Current Discussions
- Contemporary Politics and Economics in Polanyian Perspective (and vice versa)
- Polanyi and the Rediscovery of Embodiment
- Language, Learning and Logic—Polanyi and Current Discussions
- Trust, Truth and Conscience: Polanyian Communal Values and Contemporary Culture
- The Good Society: Polanyi and Current Challenges
- Pluralism: Does Polanyi Help Us Address Current Interest in and Problems Associated with Diversity?
- Can Polanyi Speak to a Digital Age?
- Resources in Polanyi for Theological Reconstruction in the Face of Fanaticism and Secularism
- Skills, Practice and Virtue—Polanyian Links
- Polanyi on the Importance of the Beautiful
- Polanyi’s Antireductionism and the Logic of Emergence
- Proposals for panel presentations are invited

Proposals should be 250-300 words and will be reviewed by a panel of jurors. Send an electronic copy of the proposal without your name on it as an attachment to Phil Mullins at mullins@missouriwestern.edu. In the body of the e-mail, provide a preferred mailing address (or fax number) as well a phone number of the author. The final deadline for proposals is March 30, 2012. If the proposal is for a panel, the full panel needs to be identified and one member designated as the primary contact. The panel proposal should outline why a discussion of this topic is important. Postings on the Polanyi Society web site (<http://www.missouriwestern.edu/orgs/polanyi/> or polanysisociety.org) will include additional conference information.

2012 Polanyi Society Annual Meeting

Call for Papers

The annual meeting for the Polanyi Society for 2012 is projected to be November 17, 2012 in Chicago in conjunction with the American Academy of Religion annual meeting. There will be a morning and evening session on Saturday, November 17 with two papers and responses in each session.

We invite papers on the following topics:

- Explorations of Polanyi's ontology (Tihamér Margitay's recent writing reopens this question; see his essay, as well as others in *Knowing and Being: Perspectives on the Philosophy of Michael Polanyi*, reviewed in *TAD* 37:1.
- Because 2012 is the centennial year for Thomas Torrance, we welcome a presentation focusing on Torrance and Polanyi.
- A paper that follows up on "The Participatory Turn," the 2011 paper by Jake Sherman (based on a recent book) and response by Dale Cannon (on links with post-critical philosophy).
- A paper on moral psychology that follows up the discussion initiated by the 2011 paper by Darcia Narvaez.
- A paper on educational assessment and its "fit" with Polanyian ideas.
- A paper on Polanyi as a social theorist.
- A paper on the work of Scottish moral philosopher John Macmurray.
- A paper on the recent book by Mary Jo Nye, *Michael Polanyi and His Generation: Origins of the Social Construction of Science* (Chicago, 2011).

Proposals (or general inquiries) on these or any other topics are invited. Proposals (250-300 words) should be sent as an attachment to David Rutledge (david.rutledge@furman.edu) by April 1, 2012.

Michael Polanyi and the Social Studies of Science:

Comments on Mary Jo Nye's *Michael Polanyi and His Generation*

Theodore L. Brown

ABSTRACT Key Words: Michael Polanyi, Mary Jo Nye, Weimar Berlin, republic of science, scientific authority, social construction of science, Thomas Kuhn, Robert Merton, Ludwik Fleck, J. D. Bernal.

This review offers comments on the book, Michael Polanyi and His Generation: Origins of the Social Construction of Science by Mary Jo Nye.

Mary Jo Nye's book about Michael Polanyi and his fellow pioneers in social studies of science is a joy to read. Her perspectives on the development of this field of inquiry, and her assessment of Polanyi's role are presented through an unusual organizational structure. While her book is not intended to be primarily biographical, her account of Polanyi's career, with its twists and turns, triumphs and defeats, is filled with many fascinating insights into his life and work. Nye begins by relating her own early introduction to the social aspects of science as a member of the 1960s graduate school generation studying the history of science. It was at about this time that study of the social aspects of science was ceded as having an importance in its own right, apart from science's intellectual history. Nye has aimed to show that the origins of new social conceptions of science can be traced to the scientific culture and political events of Europe in the 1930s. Michael Polanyi's story is an excellent vehicle for conveying just how those conceptions arose and evolved over time into a full-blown field of intellectual endeavor. Nye's organization of her materials is thematic rather than simply chronological, so that one finds the same temporal ground being trod more than once. For example, Polanyi's life in Weimar Berlin is the subject of Chapter 2, but his research accomplishments and challenges during the same period are covered in Chapter 3, and his contemporaneous interests in political and economic issues are treated in Chapter 5. This can at times be a bit challenging for the reader, but the end result is satisfying.

Michael Polanyi was born in 1891 in Budapest into a family living comfortably in an elegant apartment situated in a fashionable part of the city. At that time, the tenor of life in Budapest was liberal, and Jewish families such as Polanyi's enjoyed wide acceptance. However, his family's comfortable life came unraveled in stages, as his father was forced to declare bankruptcy and then died from pneumonia when Michael was 14. Michael enrolled as a medical student and received his medical degree in 1913. He served during World War I assigned to a medical hospital, but was ill for much of his service time. While recuperating, much of the time in Budapest, he was able to complete a doctoral thesis in physical chemistry. He also participated in discussion groups that dealt with ethical and religious issues, which, of course, weighed heavily on those concerned with the fate of society in light of the carnage that raged around them. His elder brother Karl was also active in these discussions.

There was great political turmoil in Hungary in the aftermath of the war. In 1919, a Christian, nationalistic regime took power, and a wave of anti-Semitism swept the country. To escape some aspects of the laws restricting the freedoms of Jews, many converted to Christian faiths. Michael Polanyi was baptized in the Roman Catholic Church, while others of his close friends chose Lutheran or Calvinist denominations.

It was clear that Hungary was not to be a hospitable place for Jews; Polanyi and most of his close friends with interests in science, including Eugene Wigner, Leo Szilard, Edward Teller and John Neumann, all destined for fame as scientists or mathematicians, undertook what Nye terms their first exile, mainly to institutions in Weimar Germany. Nye describes the struggles of the “refugee” generation in Chapter One, detailing how they had to deal with many varieties of exclusion and alienation over their careers. In the next chapter, she then doubles back, as it were, to describe in detail Polanyi’s experiences as a young scientist in Weimar Germany.

In September, 1920, Polanyi began a thirteen-year period of intense scientific activity as a member of the staff of the Kaiser Wilhelm Gesellschaft (KWG), in the Berlin suburb of Dahlem. The KWG was headed by Fritz Haber, who, despite being Jewish, was something of a scientific hero in Germany because of his contributions to the war effort in the Great War. The KWG exemplified a new way of organizing and carrying out scientific work in the universities and basic research institutions. Scientists were free to work on problems of inherent interest without regard for the immediate applications of their work to the solutions of practical problems. The guiding ethos of the new German university had been spelled out earlier by Wilhelm von Humboldt, who argued for an idea of *Wissenschaft* that embraced a disinterested, passionate and free pursuit of knowledge. It permeated the scientific community of Weimar Berlin, in which were gathered a stellar array of scientists, including Max Planck, Walter Nernst, Fritz Haber and—most notably—Albert Einstein. While participating to some extent in the frenetic cultural scene in Berlin, natural scientists for the most part withdrew from the political debates that raged around them. The sense of science as a closed community was strong, and participation in it was a heady and challenging experience for a young scientist such as Polanyi. The Wednesday afternoon colloquium at the University’s Physical Institute was regularly attended by several scientific stars who were already Nobel Laureates or soon would be. Polanyi later recalled these Wednesday colloquia as “the most glorious intellectual memory of my life.” As Nye says, “The freedom of research that he had experienced in a tightly networked community of world-class colleagues within the tree-lined precincts of Dahlem became an induplicable but idealized memory that formed the foundation for his later writings on the nature of scientific life and scientific achievement”(p.83).

It was during this period that Polanyi learned just how competitive and challenging scientific research could be. One of his primary research themes dealt with adsorption of gases on surfaces; it had been the subject of his doctoral thesis, completed in Budapest. His aim was to develop a general theory of adsorption of gases on surfaces, based on classical thermodynamic concepts. He made good progress with a model that involved multi-layer adsorption, but there arose competition from Irving Langmuir, an American who had taken his doctoral degree in Berlin with Nernst. Langmuir, employed at General Electric Research Laboratory in Schenectady, New York, rejected the commonly held assumption that adsorption was a multi-layer process, and proposed a radically new theory that the adsorption happens on a single layer. The competition went on for several years, and Polanyi modified his model over time, in response not only to new experimental evidence but also to Langmuir’s competing model. In the end, Langmuir’s approach won the day. Langmuir received the Nobel prize in chemistry in 1932 “for his discoveries and investigations in surface chemistry.” In his Nobel lecture he made no mention of Polanyi. For many years afterward, Polanyi reflected on the disappointment of what he felt was a rejection of his work. He remarked to Erika Cremer, “Whose fate is better, mine or Langmuir’s? My theory is absolutely right but not accepted. Langmuir’s theory is wrong but he is very famous . . . Langmuir is better off!” (97). In this interesting quotation, Polanyi seems to have forgotten his commitment to the notion that our understanding of nature is always contingent, that “absolutely right” and “wrong” are not appropriate terms in which to describe alternative models when each comports well with at least some aspects of the observation domain.

X-ray diffraction studies of fibers formed a second major area of research interest. Polanyi's work attracted great interest for the light it might shed on whether materials such as cellulose were composed of low molecular weight molecules stacked in regular arrays or of long, high molecular weight molecules. In 1921, he reported that the x-ray evidence was consistent with either interpretation. He was greeted with protests from the organic chemists that only the low molecular weight hypothesis could be correct; the idea of long-chain molecules had not yet taken hold. Polanyi deferred to his organic chemist colleagues on the matter. Hermann Staudinger at the Zurich Polytechnic Institute almost alone argued for the high molecular weight interpretation that eventually proved to be correct; in 1953 he received the Nobel prize for his work. Polanyi later expressed the view that he had missed an opportunity, by failing to see the importance of the problem.

Nye concludes, quite correctly I believe, that these and other experiences during Polanyi's time in Berlin formed the basis of his arguments about the ways in which scientific authority is constructed and recognition conferred in science. In later writings and interviews, Polanyi used these two and other examples from his own work to characterize the sometimes ruthless manner in which models other than the one that becomes dominant are frequently consigned to the scrap heap, even though they may contain valuable insights.

The rise of Adolf Hitler and the National Socialist Party made Germany an inhospitable place for Jews, and even Jewish scientists of the highest rank were not excluded. Haber, for example, was eventually driven from Germany, a broken man. Despite his angst at the prospect of leaving Dahlem, events forced Polanyi's hand. He accepted an appointment at Manchester University in England, and arrived there in 1933. In Chapter 4, Nye describes this phase of Polanyi's scientific life. The move was not easy, but he in due course embarked on studies of chemical reactions which he had already begun in Germany. The work on reaction mechanisms, and development of the theory of the "transition state," which Polanyi began in Germany and continued at Manchester, form the body of work for which he is most widely recognized as a scientist. Collaborative studies with Henry Eyring, who had come to study in Berlin with Polanyi as a postdoctoral student and continued his work at Berkeley and later at Princeton, and theoretical work by Eugene Wigner, his old friend from Budapest, shaped the direction of research in modern chemical dynamics. Nevertheless, from the time of their first publication in 1931, Eyring and Polanyi's work met with considerable criticism. The chemical world was not yet quite ready for the applications of new theoretical understandings to traditional chemical topics such as organic reactions. Despite initial skepticism, their theoretical model steadily gained acceptance, and Polanyi's stock rose accordingly. Why then did several visitors and collaborators in that period report that Polanyi seemed to be increasingly preoccupied with matters outside chemistry?

In Chapter 5, Nye's account of Polanyi's career takes a sharp turn away from science and into economics. She relates that his early interest in economics was a natural outgrowth of his education in Budapest, and likely due also to the influence of his older brother Karl, who became a widely recognized pioneer in economic history. In his first publication in economics, a short essay written in 1930 for *Der Deutsche Volkswirt*, Michael argued for government support of long-range, fundamental science. He also organized a Sunday evening dinner and discussion group that included his close scientific friends as well as economists. At Manchester, prompted in part by travels to Russia he wrote a long paper critical of Soviet economics that eventually was published in 1935 in *The Manchester School of Economics and Social Studies*. Nye goes on to relate Polanyi's increasing preoccupations with economic issues, arguing that they significantly influenced his sociological view of science. Nye's account of this aspect of Polanyi's career strikes me as rather lengthy, but her emphasis on his economics interests may be justified, given that economic theory is a leitmotif of "The Republic of Science", 1962, one of his best-known publications, and a presence in much else that he wrote.

Nye goes on to argue in later chapters that Polanyi's economic and political views were closely coupled to his evolving ideas of science's place in modern society.

As the 1930s wore on, Polanyi became increasingly involved in political concerns. He was alarmed by what he saw as an increasing interest in socialist planning in England. His personal history had imbued him with a strong antipathy toward heavy-handed government of whatever description. He rose to argue for an independent "pure" science not predicated on social and economic needs, with autonomy for individual scientists in choosing research agendas. During the 1930s, there was considerable discussion in England around the question of whether there was a useful distinction to be made between pure and applied science, and, if there were, what that might mean for how scientists should regard their social responsibilities. J.D. Bernal's sympathetic portrayal of Soviet science in his 1939 *Social Function of Science* infuriated Polanyi. Indeed, it may have catalyzed a shift already in progress of Polanyi's major interests away from the practice of scientific research to study of the epistemic and social aspects of science. In 1941, he became one of the founding members of the Society for Freedom in Science. He later identified the launch of the SFS as the "entrance of my career as a philosopher . . . this was the turning point of my life. At any rate the last of its turning points" (p. 207-8). The transition was complete in 1948 when Polanyi exchanged his chemistry professorship for a chair of "social studies" at Manchester.

In the later chapters of her book, Nye masterfully describes the growth of interest in the political and social implications of science, beginning in the 1930s. Chapters 7 and 8 and the Epilogue form a superb summary and synthesis of the work of those who contributed importantly to the evolution during this period of fully formed descriptions of the social nature of science. The work of these scholars involved crossing indistinct boundaries between the history, philosophy, economics and sociology of science. Polanyi was a prominent voice among many, including J. D. Bernal, Karl Popper, Robert Merton, Thomas Kuhn, Ludwik Fleck, Julian Huxley and Patrick Blackett: practicing scientists, philosophers, sociologists and historians. Nye shows how differences in political and economic outlooks, to say nothing of the usual competitions for recognition between scholars, served at times to block acknowledgements of commonalities, particularly between Bernal and Polanyi.

Scientific authority is a prevailing theme in Polanyi's views of science's place in society. He learned in the course of his own scientific career that the internal authority of science is powerful in determining what new results are accepted and thus become what he refers to as "scientific opinion", the settled judgment of the community. He had faith that, left on its own, the scientific community is capable of autonomously arriving at self-governed, optimal resolutions of contested issues. It is just this faith that allowed him to argue for scientific authority in matters of scientific import affecting the larger society. But his insistence on science's special claims to authority and autonomy left an impression of science as a sharply bounded, self-contained community. His views were criticized in the years following the publication of "The Republic of Science" in 1962 and *The Tacit Dimension* in 1966. They were rejected even more forcefully by later waves of social constructivists. Notwithstanding, as I wrote in *Imperfect Oracle: The Epistemic and Moral Authority of Science*, Polanyi's vision of science was, and still is, shared by many in the world of science. It does not fit comfortably in many respects with what we observe today of a vastly larger and more complex enterprise. Yet, he had lived the life of a practicing scientist, and knew how science works. It is still the case that every active scientist pursuing basic research is expected to adhere to norms and ideals that Polanyi, Merton and Kuhn identified as core values; that competition in research is often very keen and priority the coin of the realm. However, it was left to others who followed Polanyi and his contemporaries to delve more deeply

into the question of the fiduciary responsibility of scientists, individually and collectively, to apply scientific expertise in addressing societal needs.

One comes away from Nye's account of Polanyi's career with the understanding that his life was shaped at times by urgent necessities; that he was resourceful, ambitious and—most importantly—intellectually restless. He moved from a dedicated and highly successful pursuit of science, in arguably the most imposing scientific environment of his time, into economics and politics, and then finally, by his reckoning, into philosophy. Nye has made an excellent contribution to our understanding not only of Michael Polanyi's career, but also of the historical development of the sociology of science, the field of inquiry that he helped to define.

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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 is 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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The Personal and the Paradigmatic

Richard Henry Schmitt

ABSTRACT Key Words: Personal knowing, paradigms, sociology of scientific knowledge (SSK), Michael Polanyi, Mary Jo Nye

Mary Jo Nye's book, Michael Polanyi and His Generation, argues that the social conception of science originated in the scientific culture and political debates of the 1930s. As a historian, Nye covers the major themes of Polanyi's work, from his science research to his interest in economics and the political organization of science, concluding with the book Personal Knowledge and its reception. She reflects on the paradoxical legacy of his work (largely opposed to planning) in contemporary programs of social constructivism. Perhaps the paradox can be resolved by attending to adapting, learning persons as illustrated by Polanyi himself.

It is significant in my view that Mary Jo Nye learned of the work of Michael Polanyi from a footnote in Thomas Kuhn's 1962 *The Structure of Scientific Revolutions*, the book that introduced the terms "paradigm" and "paradigm shift" into the discussion of scientific theories. Those terms are indeed appropriate to patterns laid down in science textbooks, the materials addressed in Kuhn's study. Such paradigms necessarily undergo "revolutions" from time to time, even though the origins of the new paradigm have no such necessity. Kuhn himself admitted that "the existence of a paradigm need not even imply that any full set of rules exists" so that "much of a scientist's success depends on 'tacit knowledge,' i.e., upon knowledge acquired through practice and that cannot be articulated explicitly,"¹ citing Polanyi's *Personal Knowledge* (1958). This citation led the historian in Nye to Polanyi's work, and to Karl Popper and his "misnamed" *Logic of Scientific Discovery* (1959), and subsequently to other scientists and academics of Polanyi's generation.

For Nye, trained in the late 1960s, the basic research paradigm is the social epistemology of science. Her argument in this study of Polanyi and his generation is that "the origins of this new social conception of science lie in a historical period considerably earlier than the 1960s," namely the scientific culture and political events of Europe in the 1930s, and that "Polanyi was among the leading protagonists" in cultural debates that continued into the 1950s (p. xv).² Nevertheless, Nye recognizes that the outcome has been paradoxical, since Polanyi's arguments for the autonomy of scientific governance have been appropriated by her contemporaries to argue for more purely sociological explanations of scientific knowledge.

Let me say from the start that Nye works primarily as a historian, without a dogmatic agenda. She provides the details of Polanyi's career and affiliations in such a way that it is possible to see beyond any single, comprehensive paradigm, such as the strong program for the sociology of scientific knowledge (known by the acronym SSK). And, so, she gives us a lot to think about, especially when we consider the concept of personal knowing.

Origins of the Social Construction of Science

This is not a biography of Polanyi; rather it considers the historical origins of a certain view of science, loosely, a kind of social constructivism. Eight chapters and the epilogue are organized thematically; these are roughly chronological though not strictly so, following the course of Polanyi's personal and intellectual development. Polanyi undoubtedly saw the environment of his postgraduate scientific work, first at Karlsruhe

and then in Berlin, as the model for organizing science. Yet, in his scientific work and subsequently in his other interests and concerns, Polanyi's life was in many ways polycentric and dynamic, using different techniques and styles and adapting old concepts to new uses, as he moved from Budapest to Karlsruhe to Berlin, then to Manchester and Oxford, and as he shifted his research focus within physical chemistry, and then his primary audience and affiliations from laboratory research to economics and the organization of the scientific enterprise, and finally to philosophical reflections on his personal experience. One can view these as changes in colleagues and opponents against a shifting institutional backdrop; and one can also see personal evolution that led Polanyi to hold the theories and positions he did.

The first four chapters cover Polanyi's scientific career, taking us from Budapest to Manchester. The first chapter covers Polanyi's upbringing, but also more broadly the period after the first war and the resulting émigré status of many educated Hungarians. The second follows Polanyi's early scientific career in Germany, first at Karlsruhe, where the *Technische Hochschule* had strong ties to the main German scientific establishment in Berlin, and then to the center itself, where Polanyi worked from 1920 to 1933, ultimately at Fritz Haber's Institute for Physical Chemistry and Electrochemistry. Here we also learn about broader connections to German science and industry existing in Weimar Berlin and how it was all dismantled under the National Socialists. The next chapter covers Polanyi's work on adsorption and using X-ray diffraction to study the structure of fibers and large molecules; the reception of his work helped to form Polanyi's views of authority within science. The fourth chapter covers Polanyi's work on chemical dynamics at Berlin and then at Manchester, along with Polanyi's awakening (perhaps re-awakening) interest in social dynamics and in the relationship between empirical results and theory as experienced in laboratory science.

By the end of the 1930s, Polanyi had largely turned to concerns other than physical chemistry. The second half of Nye's book covers these aspects of Polanyi's work: economics and planning, the organization of science and its functions, politics and philosophy, and finally the book *Personal Knowledge*. They are, as the subtitle of the final chapter puts it, about "argument, audiences, and sociological engagement." In each, there are colleagues and issues, with opponents who have to be taken seriously. In the debates about economics and planning, there are Keynes, Hayek, and Polanyi's brother Karl. Applying these questions to science, the sixth chapter concerns questions of pure and applied science, the sad fate of genetic research in the Soviet Union, and engagement with the left-leaning J.D. Bernal about the function of science. The seventh chapter discusses what Nye calls "the political dimensions of post-empiricist history and philosophy of science": Karl Popper's opposition to historicism and the "enemies of the open society," Thomas Kuhn's goal of general scientific education in a democracy, and Polanyi's efforts to describe an autonomous scientific tradition and its authority. The eighth chapter concerns Polanyi's philosophical efforts during the 1950s and early 1960s: context, responses, alternative efforts and views, specifically to *Personal Knowledge*. Finally, Nye concludes with an epilogue. Here she summarizes the current paradigm for the sociology of science, calling it a paradoxical legacy of Polanyi and the 1930s generation, given Polanyi's distrust of science having a social agenda.

Paradigms, Disputes, and Interpretations

In a book covering as many topics and people as this one, any reviewer is likely to find matters that might be seen in a different light. Let me mention three things that I noticed.

First and foremost, I wonder: Is it possible that a focus on scientific paradigms puts too much emphasis on theory? Nye writes (p. 43), "Most scientists who reflected on the philosophy of science in the nineteenth and

twentieth centuries . . . described the essence of scientific knowledge as knowledge of ideas and theory. They did not emphasize practical skills and laboratory routines.” She suggests that this applies equally to England, France, Austria, and Germany. In a way, an emphasis on ideas would be guaranteed whenever you reflect on the philosophy of science. Yet Jed Buchwald—reconstructing devices used by Heinrich Hertz at Karlsruhe in the 1880s—found a marked difference between his work and that of British researchers. Having learned Helmholtz’s way of doing physics at Berlin, though it was never explicitly articulated, Hertz was interested in making and investigating new effects; his lab results often looked like failed experiments until he provided a new theory. By contrast, the British community investigating similar problems worked at their desks rather than in the laboratory, solving problems on paper.³ Of course, Nye gives us plenty of evidence about Polanyi’s research work, showing his concern for practice: building laboratories, learning new techniques (such as X-ray diffraction), balancing time at bench and desk, the “semi-empirical” method, *ad hoc* accommodations, etc. (pp. 65, 71, 105, 124, 129, 141-44). This broadens our view of research methods; it is not simply that observations come first or that theories do. Rather they co-exist in our experience in ways that remain largely unconscious.

Second, the themes of planning and economic values apply both to questions of the economy—whether and to what extent and by whom it should be planned—and to the nature of the “republic of science”. In many ways, the planning debate of the 1920s and 1930s has been overtaken by change, not only the collapse of many of the centralized, “planned” economies but huge quantitative changes that produced qualitative differences: multinational firms that are larger and more powerful than most historical empires, vastly improved techniques in information gathering and distribution, along with the unraveling of old regulatory disciplines and understandings. Two things stand out for me in the positions taken by Polanyi and Friedrich Hayek during the debates of the 1940s and 1950s. First, neither of them proposed, or even considered, anything like real “laissez-faire,” despite the occasional appearance of that term as a contrast with “planning.” Nye is not entirely clear on this; see, for example, her introduction of that term (p. 212) while discussing Edward Shils’s report on the Hamburg conference in 1953.⁴ In his report, the “methodological issues that raced like a hare across the field” were between idealists and positivists, not between planning and laissez-faire. And, despite the caricature we now get of Hayek, I believe that even he always assumed that free markets would need to be well-regulated.⁵ A second and related point, which is easy to forget: the position shared by Polanyi and Hayek recognized the nature, better: the permanence, of ignorance surrounding our experiential knowledge.⁶ This is critical. There is no sense of value, no permanent truths, that can be stated in the abstract, *ab initio*; instead there are a series of choices made *ad hoc*, largely unconsciously and without exact knowledge, somewhat provisional decisions, about which projects to pursue, which anomalous observations to ignore, which researchers to encourage and fund, etc. Hayek’s *The Sensory Order* makes an even more fundamental argument about cognitive psychology: experience itself must be mediated by previous experience, rather than by some kind of innate, inherited pattern—the argument is against certain views of Ernst Mach, also held by the Vienna Circle.⁷ Neurological structures are reinforced by repeated use, and so the sensory order is self-organizing, dynamic, and plastic, rather than routinized, static, and predictable.

In passing, let me note that Nye also contrasts Polanyi with the early Ludwig Wittgenstein (p. 263). Though his *Tractatus* probably did not directly influence Polanyi, it likely did influence Wittgenstein’s cousin, Friedrich Hayek. Nye accepts a conventional but mistaken association between Wittgenstein and the logical positivists, those in Schlick’s circle. In fact Wittgenstein’s argument was with Bertrand Russell (and by extension with the Vienna Circle) about the boundary between expression in logical analysis and consideration about values and empirical facts. So, I do not see his view as inconsistent with “tacit knowledge”; instead it addresses a different problem, the limitations of logical atomism.⁸

Finally, in these and other discussions, the similarity between Polanyi and his opponent – at least in retrospect – is emphasized. We seem to have a rear-view mirror in which objects appear closer to one another, and oppositions are viewed within the same paradigm, *Zeitgeist*, or thought-style. On p. 178 we learn about views that Michael shared with his brother Karl—which certainly did exist—but shouldn't we also work out what in Michael's experience put them in opposition? On p. 195, Polanyi sounds like historians of ideas "writing in a semi-positivist mode on the internal logic of science." In the discussion at pp. 218-22, we see how "Marxism and Liberalism meet at the social turn," i.e., what assumptions were shared between Polanyi and J. D. Bernal. At p. 279, "Polanyi sounds almost like Jean-Baptiste Lamarck but without the mechanism of the inheritance of acquired characteristics." Elsewhere, "[Karl] Mannheim in 1929 sounds much like the later Polanyi or Kuhn . . . insofar as observers . . . eradicate as error whatever deviates from their unanimity" (p. 281). Again, we are forced to consider what caused Polanyi to formulate and hold his own position, sometimes accepting a shared framework but in opposition to others who shared it. And we need to consider his negative personal experiences as well as positive ones: for example, the unimaginably dark collapse of the idyll in Weimar Berlin, or the unjust imprisonment of his niece Eva Striker in Lubianka, over which Michael and Karl disagreed. Nye herself gives us enough of these details for us to ponder this.

Paradoxes often arise because a premise misleads us from the start. In the barber's paradox, the initial premise states that "The barber shaves only those men in town who do not shave themselves." This cannot be true unless the barber is not a man, does not live in town, wears a beard, or does not have facial hair. Of course it is even more likely that the word "only" is the root of the problem, because the person asserting the claim has simply forgotten to consider the barber himself. While the problem is not fully apparent at the start, we are asked to hold a problematic, or even contradictory, proposition. In the end, we see the paradox, and we recognize that something must be missing from the premise. Is that the explanation of Polanyi's paradoxical legacy to the social constructivists?

Knowing and Adapting Persons

Nye mentions (p. 271) the criticism of *Personal Knowledge* by Gerald Holton: he found it "maddening in spots;" that it might best be considered an example of confessional literature.⁹ Actually that is a very shrewd observation. Polanyi's reflections are personal, about the nature of his experience. He presents a theory of knowledge centered on the knower. His "confessions" reveal something that he tacitly experienced that was overlooked in epistemologies limited to certifiably positive facts or explicit formal theories or textbook paradigms. When you see Polanyi's reflections in the tradition of Montaigne, they do not seem so eccentric.

Belatedly, let me make a personal disclosure. I was introduced to Michael Polanyi in quite a different way—though I must add that this was purely accidental and reflects no cleverness, indeed no planning, on my part. I wandered in like Candide, but without the benefit of a panglossian philosophy. As a teenager, I worked a few weeks one summer in the physical chemistry laboratory of Edward Bair; at the time he was developing new spectroscopic procedures and equipment to study explosive chemical reactions, looking at transition states and free radicals in the decomposition of hydrazine. As an undergraduate at the University of Chicago, I spent some time listening to pragmatic philosophers like Richard McKeon and studying social sciences with German émigrés like Gerhard Meyer, a member of the first Frankfurt School. I had the luck to hear Michael Polanyi lecture on several occasions. As a part-time graduate student, I worked with Edward Shils and wrote a dissertation with Stephen Toulmin. In other words, "social constructivism" is not directly part of my scientific heritage. On the contrary, Toulmin's "interest" in Kuhn's views (mentioned by Nye, p. 267)

was mostly negative. He used to tell me that, when Kuhn's *Structure of Scientific Revolutions* was published, it sucked the air from the room, though he also said that at the 1965 conference they were able to get Kuhn to soften many of his more extreme positions. Toulmin taught me to take a wider look around, to consider the adaptability of concepts to new circumstances. I should add that, along with Toulmin's 1959 review of *Personal Knowledge*, in which he criticized Polanyi for being outside the mainstream of philosophical discussion, one needs to consider his much more positive view in *Return to Reason* (2001).¹⁰

A further connection to Polanyi occurred in an unexpected way, through conversations with John Gedo, another émigré Hungarian but of a younger generation than Polanyi, displaced as a teenager. At the time, Gedo was a Chicago-trained psychoanalyst and scholar about artists and creativity. He told me that his generation of psychoanalysts felt restricted by the theoretical framework they inherited, but that Polanyi's work validated the use of their clinical experience when it was at odds with theory, breaking the logjam, and leading to new theoretical frameworks. Gedo documented this debt in his *Evolution of Psychoanalysis* (1999).¹¹

As a result of this personal history, I see Polanyi from a different perspective. It seems to me that his primary contribution is not to the sociology of scientific knowledge, but rather to cognitive psychology, to explaining scientific discovery and creativity, to understanding the "husbandry" of variation in scientific research and the personal engagement of the scientist making choices. I do not claim that this is the only possible perspective, or even a better one than Nye's. In some ways the legacy of Polanyi's work to cognitive theory is now such an accepted part of the background that no one much bothers to cite Polanyi when they mention tacit, non-verbal, unconscious mental operations. Here Polanyi's legacy is not at all paradoxical; for many it is fully assimilated and increasingly anonymous.

Still, no one can deny that Polanyi did work on the sociology of science, or that science is affected by society and by its social organization. Again, I think Polanyi focused on the reality of a scientist's personal experience, as he had experienced it. I suspect that I'm seeing this through Shils's eyes, but in sociological terms that reality occurs in the primary group and an individual's affiliations and orientation to that group and its traditions. Perhaps if the program for the sociology of scientific knowledge can come to terms with the knower-centered part of Polanyi's work, his legacy to them may not seem so paradoxical. Given that nothing important can be ignored by those with an open mind, they will find there is something else that must be happening to explain the evolution, change, and replacement in those otherwise all-determining social constructions. In her history of Polanyi's career, by presenting the shifting themes of his work and the choices he made, Mary Jo Nye gives us all the materials anyone needs. Just look at where the paradigms, as fixed patterns, do not explain what happened.

On Tacit Knowing

Let me add a brief note about tacit knowing. These reflections go beyond the review of Nye's book, though in part they grow out of thinking about what Ted Brown so clearly says about Polanyi's career.

Tacit knowing is not itself a full-blown methodology, and certainly not a formal theory. Polanyi was well aware of the extent of human ignorance, of the difficulty of producing reliable observations, and of the challenge in sorting the significant from the insignificant. His ideas arose in opposition both to idealism and to positivism. He knew that circumstances change, that we need to adapt. So, he gave a description of what we resort to when there are no certain facts, when old patterns and values no longer apply, when new circumstances arise, when we approach a question with a fresh goal in mind. Mostly, we are not in a position

to plan on achieving a certain result. So we do something that Charles Darwin described as “unconscious selection”¹²—looking for what works, seeking what produces a reliable result and what manifests our values and realizes our goals, whether we have articulated them or not. Tacit knowing has no single paradigm, and that is its virtue.

Endnotes

¹Thomas S. Kuhn, *The Structure of Scientific Revolutions*, (Chicago: University of Chicago Press, 1962), p. 44 and footnote.

²Page references without any other notation are to Mary Jo Nye, *Michael Polanyi and his Generation: origins of the social construction of science* (Chicago: University of Chicago Press, 2011).

³Jed Z. Buchwald, *The Creation of Scientific Effects: Heinrich Hertz and electric waves* (Chicago: University of Chicago Press, 1994), pp. 321-29.

⁴Edward Shils, “The Scientific Community: thoughts after Hamburg,” reprinted in his *The Intellectuals and the Powers, and Other Essays* (Chicago: University of Chicago Press, 1972), pp. 204-212.

⁵I am basing my observation on the essays in Hayek’s *Individualism and the Economic Order* (Chicago: University of Chicago Press, 1948), in which a regulated marketplace is an ever-present and fundamental assumption. Recently bloggers have discovered that Hayek even allowed for national insurance plans in his *The Road to Serfdom* (1944), chap. 9.

⁶For example, see Hayek’s response to Polanyi’s “Pure and Applied Science and Their Appropriate Forms of Organization,” in *Science and Freedom: the proceedings of a conference ... held in Hamburg on July 23rd-26th, 1953* (London: Congress for Cultural Freedom, 1955), pp. 53-54.

⁷Friedrich A. Hayek, *The Sensory Order, an inquiry into the foundations of theoretical psychology* (Chicago: University of Chicago Press, 1952), pp. 175-76.

⁸Ludwig Wittgenstein, *Tractatus Logico-Philosophicus* (London: Routledge & Kegan Paul, 1922). See Wittgenstein’s preface, which states his aim, and Bertrand Russell’s introduction, which discusses the difficulties this makes for Russell’s own theory.

⁹Gerald Holton, “Michael Polanyi and the history of science,” *Tradition and Discovery* 19 (1), 1992-93, p. 24

¹⁰Stephen Toulmin, *Return to Reason* (Cambridge, MA: Harvard University Press, 2001), especially chap. 11 & 12.

¹¹John E. Gedo, *The Evolution of Psychoanalysis: contemporary theory and practice* (New York: Other Press, 1999); Polanyi’s collection, *Scientific Thought and Social Reality*, is the first of twenty-five books that are discussed.

¹²Charles Darwin, in “Variation under Domestication” (chap. 1 in *The Origins of Species*), discusses methods used by breeders and cultivators to produce new and strikingly different domestic stocks by the slow accumulative action of unconscious selection, i.e. “without a distinct object in view” and largely without any explanation of variation or inheritance.

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 (Douglas.Masini@armstrong.edu) and someone will see that you are added to the list.

A Response to Theodore L. Brown and Richard Henry Schmitt

Mary Jo Nye

ABSTRACT Key Words: Michael Polanyi, economic theory, scientific autonomy, tacit knowledge, paradigms, realism, Thomas S. Kuhn, Stephen Toulmin.

This essay responds to reviews of Michael Polanyi and His Generation: Origins of the Social Construction of Science by Theodore L. Brown and Richard Henry Schmitt. Special attention is given to Polanyi's economic views, to the conception of tacit knowledge (in Polanyi's philosophy of science and in Thomas S. Kuhn's description of paradigms), and to Polanyi's commitment to scientific realism.

It is a pleasure to respond to Theodore L. Brown's and Richard Henry Schmitt's essay reviews, which offer the reader informative and thoughtful analyses of my new book. Their reviews are occasionally overlapping in content, but complementary in their different perspectives. Brown writes as a chemist and academic administrator whose chemical researches, like Polanyi's, partly have focused on reaction kinetics and mechanisms. Brown's interdisciplinary interests have led him, also like Polanyi, into the study of the history, philosophy, and sociology of science, most recently reflected in his book *Imperfect Oracle: The Epistemic and Moral Authority of Science in Society*.¹ Schmitt's principal research lies in the philosophy of science, with an emphasis on Ludwig Wittgenstein, and a longstanding interest in the social and cognitive sciences. As Schmitt writes in his review, he had personal acquaintance at the University of Chicago with some of the characters in my book, notably Michael Polanyi, Edward Shils, and Stephen Toulmin, and as Schmitt writes elsewhere, he shares with Polanyi a Hungarian ancestry.²

Brown offers the reader a finely detailed and accurate summary of the main themes and content of my book, including an overview of Michael Polanyi's life and career. I especially appreciate Brown's explicit recognition of the links that I make between Polanyi's day-to-day career experiences in chemistry and his gradual development of a philosophy of science with emphasis on community, tradition, and authority in science. In particular, as Brown writes, Polanyi developed a strong sense of science as a closed community while he was in Berlin, and this feeling contributed, along with his political and economic experiences and reflections, to Polanyi's conviction of the necessity for scientific freedom and autonomy within the scientific community. Within such a community, in Polanyi's view, scientists exercise among themselves an interlocking communal and expert authority which determines directions for research, the value of results, and recognition of scientific achievement. Brown himself employs the themes of authority and autonomy in his book *Imperfect Oracle*, where, he notes, as in his essay review, that many scientists today share Polanyi's views on these matters, despite the larger and more complex milieu today for scientific research than in the 1930s.

Brown wonders if I have given too much attention to Polanyi's economic interests, but writes that perhaps the emphasis is justified, given that economic theory is a leitmotif of Polanyi's well-known 1962 essay "The Republic of Science." Here is a point of intersection with one of Schmitt's comments. In my view, Polanyi's substantial time spent in economic work, including his essays, book and educational films, was one of his major preoccupations and not a casual interest. This economic work structured the gradual transition between his life in chemistry and his life in the philosophy of science. In this regard, Schmitt notes in his review the importance to Polanyi's thinking of debates in the 1920s and 1930s about the implementation of centralized, "planned" economies (including heated debates with J. D. Bernal). Polanyi opposed centralized

planning for national economies, and he sounded an alarm to scientists of the dangers of central planning and mandates for socially useful science not only in the Soviet Union and Germany, but potentially in Great Britain and the United States.

Schmitt is correct in reminding the reader that Polanyi's opposition to planning did not advocate a primitive "laissez-faire" approach. Rather, as I describe in my book (e.g., pp. 146, 161, 177, 203), Michael Polanyi marshaled arguments for a modified capitalist system, warning against unfair disparities in income distribution, complete separation of government from the market, and failure of mutual communal responsibility among employers, workers, and governmental agencies. Partly influenced by reading John Maynard Keynes, and partly spurred by disagreements in economic theory with his brother Karl, Michael Polanyi advocated the role of economic experts to advise government on measures to regulate the money supply in a free market, including the use of deficit spending when needed, but not including public works projects. Friedrich von Hayek and Polanyi drew upon each other's ideas, and each adopted the (liberal) view that mutual adjustments occur among freely acting individuals in economic systems and in systems of knowledge (e.g., science) with the result that productivity and order emerge spontaneously.

Schmitt writes that Hayek and Polanyi shared recognition of the permanence of "ignorance surrounding our experiential knowledge." Interestingly, by way of contrast, Brown points out an occasion when Polanyi "seems to have forgotten his commitment to the notion that our understanding of nature is always contingent," noting Polanyi's remark to Erika Cremer that his theory of surface adsorption was "absolutely right" and Irving Langmuir's was wrong. This remark on Polanyi's part highlights a theme in my book that addresses, I think, Schmitt's question about what I may mean by "paradox" in the adoption or adaptation of Polanyi's work by leaders in the social construction of science (or sociology of scientific knowledge, SSK) in the 1970s and 1980s. The theme has to do with scientific realism and scientific truth, a theme discussed at length by Brown in his books *Imperfect Oracle* and *Making Truth: Metaphor in Science* (2003).³ First, however, before returning to this theme, I want to set that stage with some comments about paradigms, sociology, and psychology, as brought up in Schmitt's review.

Thomas S. Kuhn's *The Structure of Scientific Revolutions* (1962) popularized the term "paradigm," which had been used a few years previously, with reference to patterns or ways of seeing the world, by philosophers including N. R. Hanson (1958), Stephen Toulmin (1961), and Polanyi himself (1958). Polanyi's usage occurs in *Personal Knowledge* in his dismissal of Laplacian mechanistic determinism as "the paradigm of a conception of science pursuing the ideal of absolute detachment."⁴ Kuhn wrote that he used "paradigm" in two different senses: (1) the sociological sense of the "constellations of beliefs, values, techniques, and so on shared by the members of a given community," and (2) the philosophical sense, which "is the deeper of the two," of that element in (1) that has to do with exemplars, which do not employ rules and logic, but rather tacit knowledge and "intuitions" that are "tested and shared possessions of the members of a successful group which the novice acquires through training."⁵ These two uses both are found in *Personal Knowledge*, and Kuhn in his 1962 book explicitly mentions his debt to Polanyi's explanation of tacit knowledge.⁶

Tacit knowledge became recognized as one of Polanyi's most novel contributions to the philosophical understanding of science. In Stephen Toulmin's *Return to Reason*, a reference for which I am grateful to Schmitt, Toulmin praises Polanyi's explanation of tacit knowledge for helping "to dissolve away the intellectualist accounts of the Philosophy of Science that were in fashion in the earlier part of the twentieth century," adding that tacit knowledge harmonizes with the later views of Ludwig Wittgenstein in showing "how scientific terms

and statements acquire their meaning from association with particular constellations of human situations and actions.”⁷

In *Making Truth*, Brown writes that in his demonstration of the “embodied nature of our conceptual frameworks,” Polanyi has a good deal in common with developmental psychologists and cognitive scientists. Schmitt writes in his review that he sees Polanyi’s primary contribution not as one to sociology, but to psychology, in Polanyi’s explanations of discovery, creativity and theory-choice. The link to psychology, however, was precisely one of the major points of criticism for Polanyi (and Kuhn) from philosophers of logic.⁸ And of course, this problem is a hard one. How can we account for discovery? How do individuals come to be creative? What is the role of the individual and of the community in effecting scientific change?

As I discuss in my book, Polanyi explains discovery as an operation that is not strictly logical because there is a “logical gap” between the current dominant interpretive framework and a new way of seeing things. A phenomenon that has been puzzling or worrying is resolved by crossing a logical gap in a kind of conversion, but this revelation is based in long-practiced skills and in judgment acquired through apprenticeship in a community of connoisseurs.⁹ The individual scientist’s peers may or may not adopt a new explanation depending, as Polanyi puts it, on their judgment of its accuracy, relevance, and intrinsic interest.¹⁰ Thus, the psychological dimension of scientific innovation, whether on the individual or the group level, is intimately linked with the sociological dimension of the standards and traditions of the scientific community as a whole.

Schmitt mentions toward the end of his review that the knower-centered part of Polanyi’s work is the least assimilated part of the legacy adopted in SSK. I think, however, that we need to distinguish two psychological senses of Polanyi’s “knower”: (1) the skilled connoisseur who produces “embodied knowledge” and (2) the passionate explorer who seeks “contact with a hidden reality.” The knower in the first meaning has been an object of considerable interest in SSK, but not the knower in the second sense.

The problems of realism and truth are crucial to understanding differences among Polanyi, the younger Kuhn, and the next generation of social constructionists. As I mention in my book (255), Martin X. Moleski discovered Polanyi’s annotation “Truth!!!!!!!!!! This *really* needs analysis” next to Kuhn’s statement in *Structure* that “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.” Kuhn further suggested that “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.” For Kuhn, there was no one big mind-independent world to be discovered as truth, although the varieties of knowledge acquired in different specialties were by no means arbitrary and science remained progressive in nature.¹¹ This view largely carried the day in the second half of the twentieth century among scientists and philosophers, as well as sociologists. Brown writes in *Making Truth*, for example, that “We have no grounds for believing that there exist objective, mind-independent truths awaiting discovery. Rather, statements we regard as truths about the world are the product of human reasoning.”¹²

Like many philosophers of his generation (notably Toulmin) and the next generation (for example, David Hull and Ronald Giere), Kuhn found congenial a naturalist and generally Darwinian evolutionary model of scientific innovation and scientific change. As I write in my book, Polanyi wrote of evolution as a process of emergence leading to greater complexity and achievement both in knowledge systems and in biological species (265-266, 278-279). His notion of evolution was less Darwinian and more progressive and teleological

than most contemporary views, “guided by the urge to make contact with a reality, which is . . . waiting to be apprehended.”¹³ Polanyi compared his frame of mind in this conviction to the worship of God.¹⁴

Polanyi had a commitment to realism and confidence in the power of natural science to know the world—an attitude that also informed the sociological writings of his generational peers J. D. Bernal, Karl Mannheim, and Robert K. Merton. Polanyi’s belief protected him against worries that his writings could be used to undermine the authority and autonomy of science. His remark to Erika Cremer that his adsorption theory was absolutely right captures Polanyi’s conviction that correct scientific theories reveal the reality of things, not what is effective or convenient or fashionable to think. In my book, I suggest that there is irony in the later severing of his social epistemology of science from its original metaphysical realist premises, with the result that some of his most novel insights into the way that science and scientists work have indeed been used to undermine Polanyi’s campaign for the autonomy and authority of the scientific community.

Endnotes

¹Theodore L. Brown, *Imperfect Oracle: The Epistemic and Moral Authority of Science in Society* (University Park, PA: Pennsylvania State University Press, 2009).

²As mentioned in Richard Henry Schmitt, review of John E. Gedo’s *The Evolution of Psychoanalysis: Contemporary Theory and Practice* (1999) in *Tradition and Discovery*, 27:3 (2000-2001): 41-42.

³Theodore L. Brown, *Making Truth: Metaphor in Science* (Urbana and Chicago: University of Illinois Press, 2003).

⁴Michael Polanyi, *Personal Knowledge: Towards a Post-Critical Philosophy* (Chicago: University of Chicago Press, 1958), pp. 139-140.

⁵Thomas S. Kuhn, *The Structure of Scientific Revolutions*, 2nd ed., enlarged (Chicago: University of Chicago Press, 1970), pp. 175, 187, 191.

⁶Kuhn, *Structure of Scientific Revolutions*, p. 44, n. 1.

⁷Stephen Toulmin, *Return to Reason* (Cambridge, MA: Harvard University Press, 2003), p. 183.

⁸Brown, *Making Truth*, pp. 8-9.

⁹Polanyi, *Personal Knowledge*, p. 263.

¹⁰Polanyi, *Personal Knowledge*, pp. 135-136. As I note on p. 246 of my book, Polanyi used the terms validity, profundity and intrinsic interest in *Science, Faith and Society* (Chicago: University of Chicago Press, 1964, originally 1946), and plausibility, scientific value, and originality in “The Republic of Science: Its Political and Economic Theory,” *Minerva*, 1: 54-74.

¹¹Kuhn, *The Structure of Scientific Revolutions*, pp. 170, 171; and (1969 Postscript) 206.

¹²Brown, *Making Truth*, p. 12.

¹³Polanyi, *Science, Faith and Society*, p. 35; also, e.g., *Personal Knowledge*, p. 106.

¹⁴Polanyi, *Personal Knowledge*, p. 405.

Moral Rationality

Darcia Narvaez

ABSTRACT Key Words: rationality, objectivity, virtue, moral development, knowledge, epistemology, indigenous thinking.

Although objectivity is generally considered a result of Western science and civilization, Michael Polanyi and John Macmurray offer some insights into how the received view of objectivity as impersonal and unemotional entails a deep misunderstanding of what knowledge truly is. My contention is that ancestral indigenous thinking (small-band hunter gatherers) represents truer knowledge and rationality and aligns more with Polanyi and Macmurray. I integrate these ideas with modern cognitive and neuroscience and briefly apply these expanded notions to the realm of virtue.

Objectivity is generally associated with Western traditions of science and rationality and both are associated with civilized humanity. Michael Polanyi and John Macmurray offer some insights into how these concepts and their practice are misunderstood. Indigenous thinking aligns more with Polanyi and Macmurray than the received views about objectivity and rationality. I integrate their suggestions with modern cognitive and neuroscience and apply these expanded notions to the realm of virtue.

Objectivity and Rationality

The notion of objectivity has shifted through the history of science (Daston & Galison, 2007). In tracking the history of image science, Daston and Galison point out that initially the goal was to be “true to nature” in one’s observations of natural objects. This entailed representing the *idea* of the thing, an image that didn’t represent any one specimen but the type as a whole. The great variability in how different scholars sketched the same “type” led to a desire for a more systematic representation, less mediated by varied human experience. Seeking a systematic approach, British physicist Arthur Worthington spent years in the mid-nineteenth century illustrating with a machine he invented the perfect symmetry of a droplet in various stages of flow. He watched each stage during a millisecond flash of light and created a visual taxonomy of the major outcomes. Nearly twenty years after he started, he finally was able to use a camera to make a photograph of the splash. Contrary to his extensive illustrations of symmetry from watching with the naked eye, on film every drop at every stage was imperfect. Daston and Galison conclude that Worthington exhibited a common trait seen throughout science:

over the long course of making systematic study of myriad scientific domains, the choice of the perfect over the imperfect had become profoundly entrenched... idealization had long been the governing order...What had been a supremely admirable aspiration for so long, the stripping away of the accidental to find the essential, became a scientific vice (Daston & Galison, 2007, pp. 15-16).

Worthington (as described in Daston & Galison) determined that tracking the ideal form was due to a human psychological flaw, a perceptual deficiency that biased him towards seeing symmetry. Worthington suggested that efforts to track ideal forms be rejected and replaced with a mechanical “objective view,” one where the

complexity, individuality and asymmetry of the world would be recorded, uncontaminated by human perceptual inadequacies.

The notion of objectivity evolved further to its common definition today. “To be objective is to aspire to knowledge that bears no trace of the knower—knowledge unmarked by prejudice or skill, fantasy or judgment, wishing or striving... objectivity is blind sight, seeing without inference, interpretation or intelligence” (Daston & Galison, 2007, p. 17). The practice of objectivity in its fullest sense today fuses together emotional detachment, quantification, “automatic procedures for registering data,” and “belief in a bedrock reality independent of human observers” (ibid, p. 29).

The ability to perform this kind of objectivity we now know is an impossibility because human perception is constructed by prior experience. Prior experience fills the mind with interpretive frameworks and automatic inferences, driving perception and action, so much so that what a scientist believes is important to study and how to study it is influenced by experience. However, many scientists still present themselves and their work as “objective,” as if there were no bias or mediation in the shaping of the question, the experimental design, the findings or the interpretation. Let’s presume it is only because they are unaware of additional learned filters and framings that shape what they see, interpret and conclude.¹

In scientific objectivity, emotions are to be quarantined, as if this too were possible. For centuries, it has been common to believe in the West that emotion and cognition are separable and that good thinkers keep their reasoning uncontaminated by emotion or passion (remember Kant). However, at the neurobiological level the systems are indistinguishable (Lewis, 2005). A brain system is only labeled as representing cognition or emotion based on the goal of the researcher. That is, when a particular system is activated during an experimenter’s thinking task, it is called cognition, and when the same system is activated during an experimenter’s ‘feeling’ task, it is called emotion. The framing changes, but not the neurobiology. We are taught to parse our reactions into emotions and cognitions (thoughts) but this may be an artifice as they are highly interdependent.

It’s not only the impossibility of the task that should concern us about the desire to be objective as described. We should also be concerned about the nature of knowledge that is apprehended under these conditions and how useful that knowledge is. In a way science’s aim to “establish a strictly detached, objective knowledge” demonstrates a lack of understanding of what knowledge truly is (Polanyi, 1966/2009, p. 20). Dissecting the world into particular, isolated fragments does not lead towards true knowledge but away from it. “The belief that, since particulars are more tangible, their knowledge offers a true conception of things is fundamentally mistaken” (Polanyi, 1966/2009, p. 19). Our schooling and achievement testing are based on the assumption that such detailed knowledge “makes the man.” But this is a thin superficial type of knowledge that does not necessarily lead to a good life. Instead, as Polanyi (1958) contends, true knowledge necessarily lies in our ability to use it. Knowledge is primarily personal, primarily tacit. Personal knowledge is imbued with intelligence, experience and perspective—the opposite of scientific objectivity’s goals. To eliminate the personal elements of knowledge is to destroy knowledge itself (Polanyi, 1958).

Macmurray (1962/1992) offers a more holistic understanding of objectivity that aligns with Polanyi’s perspective. According to Macmurray, ‘reasoning is primarily an affair of emotion’ and “none of our activities, not even the activities of thinking, can express our reason unless the emotions that produce and sustain them are rational emotions” (1962/1992, pp. 10-11). Macmurray points out that when we suppress and ignore emotion and emotional development, we necessarily stay in egocentric thought and cannot know the world as

it is—we cannot know reality. Instead we live in a subjective world, misunderstanding objectivity. Instead of objectivity as detached provable knowledge, objectivity means knowing *and feeling* a thing in itself, outside of an egocentric interpretation or use for it. He uses the example of love which can be subjective and irrational when it is focused on feelings of pleasure, or objective and rational when it is focused on loving the person for whom he or she is. Objectivity means apprehending something without an agenda, and in my terms, with full emotional presence. Moreover, true thoughts are those that refer properly to reality which is necessarily tied to meaning. “Objective emotion is not a mere reaction to a stimulus . . . it is an immediate appreciation of the value and significance of real things . . . emotional reason is our capacity to apprehend objective values” (ibid, p. 15). Macmurray expands Polanyi’s view of personal knowledge to include not only intellectual commitment but *emotional* commitment.

Intellectual knowledge tells us about the world. It gives us knowledge *about* things, not knowledge *of* them. It does not reveal the world as it is. Only emotional knowledge can do that . . . One cannot really know about anything unless one first knows it. Intellectual awareness is egocentric (1962/1992, p. 22).

Perhaps the distinction to be made between these two contrasting perspectives of knowledge and objectivity—the scientific received view and the correctives from Polanyi and Macmurray pointing to the personal—has to do with mental orientation. Bringing in cognitive neuroscience, humans have two orientations corresponding to the left and right brain hemispheres.² (For a recent review of research on left and right brain, see McGilchrist, 2009). We can label the received view of scientific objectivity a *left-brain objectivity*. The left brain processes information by categorizing, dissecting and separating. It cannot attend to the gestalt and prefers static or lifeless things. This form of processing information represents the most respected methods of science (emotionally-detached, control and isolation of elements—through randomized, controlled experimentation). In modern developed societies, left-brain objectivity has the highest status. “Objectivism has totally falsified our conception of truth, by exalting what we can know and prove, while covering up with ambiguous utterances all that we know and *cannot* prove, even though the latter knowledge underlies, and must ultimately set its seal to, all that we *can* prove” (Polanyi, 1958, p. 286).

The emphasis on proved knowledge and skepticism towards unproven knowledge is so strong in the Western world that Mander (1978) pointed out some time ago, that many people don’t believe that human breastmilk, evolved over millions of years with thousands of ingredients, is better than scientifically-derived infant formulas that have a few dozen, manufactured ingredients—experiments must provide proof. Research studies that cannot prove that breast milk is better for some outcome (usually over the short term), are deemed inconclusive about whether breast milk is any better for babies than formula. How is it that humanity has come to disbelieve anything that has not been proved through the scientific method? Research shows that the left brain is where language and consciousness reside for most brains and consequently seems necessarily to dominate human self-understanding of rationality. The left brain denigrates what it cannot perceive—all the tacit knowledge that is mostly subconscious and accessed by the right brain. The left brain in isolation is divorced from meaning and living things.

The second type of objectivity, *right-brain objectivity*, reflects a different mindset that Westerners seem to have a difficult time acknowledging. Right brain processing attends to the holistic, gestalt perception that embraces context and relationships within a setting. It is primarily tacit knowledge, as verbal and conscious knowledge resides primarily in the left brain. Enlisting a right-brain orientation or apprehension of the world

requires a calmness of mind and an emotional presence. The right brain attunes to the energy and life in all things. This was well described by the neuroscientist, Jill Bolke Taylor (2008), when she had a stroke on the left side of her brain, gradually losing her ability to talk and think linearly, but at the same time she felt more and more in tune with the deep meaning of things. The right-brain approach better represents indigenous thought (Jacobs, 1998) which assumes meaning, morality and spirituality in all experience. Subjectivity is assumed in all animals and even objects, not just in human beings. The interconnectedness of all life is understood and so great care is taken in decision making and action.

Perhaps the need now is to support the development of right-brain objectivity, representing tacit knowledge and emotional rationality. But how does rationality develop generally?

The Development of Rationality

Rationality is founded “in the action of biological drives, body states, and emotions.” These form “the neural edifice” of human reason (Damasio, 1994, p. 200). Greenspan and Shanker point out that from the beginning of life, emotions “give birth” to the ability to think and invent symbols; “sensory and subjective experiences... are the basis for creative and logical reflection” (Greenspan & Shanker, 2004, p. 2). Among all animals, including humans, emotions are “psychobehavioral potentials” (Panksepp, 1998) that lead to greater adaptation, a form of intelligence (Piaget, 1936/1963). In developing symbolic thinking, humans learn to transform situated basic emotions into increasingly complex emotional signaling, which eventually allows the separation of an image or desire from immediate action: hence, the birth of ideas.

Emotions, emotion systems and emotion regulation are shaped by early experience. Although some emotion systems are available at birth (e.g., fear, panic), these emotions like others are shaped by early experience when the brain is developing rapidly. At full-term birth, only 25% of the brain is developed. Thereafter brain system structure, wiring and connectivity are deeply influenced by parental care. Emotions and their ‘rationality’ are shaped by early experience beginning with events surrounding birth and patterns of experience during early life.

As with all of basic cognitive development, eventual understanding is founded on the physical experience of interaction with the environment through the “interiorization of action” (Chapman, 1988, p. 9). That is, understanding develops from initial reflexes toward more differentiated conceptual structures, moving from implicit to verbalizable understanding (Gelman & Baillargeon, 1983). But none of these are separable from the emotion systems that guide action and thinking. From repeated experience, emotions are associated with particular events and form the worldview that a person carries lifelong, particularly for social relations (Tomkins, 1965).

The understanding that tacit knowledge is predominant is finally a widespread view in psychological science. Most of what we know we cannot put into words (Keil & Wilson, 1999). Implicit or tacit knowledge systems—all sorts of them—operate on a nonverbal level most of the time (Reber, 1993). Developmentally, explicit knowledge emerges from tacit knowledge built from experience. Such tacit understanding and skill “is continuous with the inarticulate faculties of animals” (Polanyi, 1958, p. 90). Even in infants we can see develop a basic explanatory set of preverbal conceptual schemas (Keil & Wilson, 1999; Mandler, 2004). There are nearly infinite bits of constructed “common sense” tacit knowledge that underlie everyday functioning.

Indeed, implicit knowledge can never fully be verbalized. Verbalization is a subset of what is expressable (also included would be sounds and movements that express emotion and understanding). Although rationality is not equivalent to personal emotion, rationality is built from this complex personal knowledge. And most of this is tacit, as Polanyi noted. “Our mute abilities keep growing in the very exercise of our articulate powers our formal upbringing evokes in us an elaborate set of emotional responses operating within an articulate cultural framework” (Polanyi, 1958, p. 70).

In everyday life, true rationality combines both intellect (detached thinking) and emotion. We come to reason well when we have full life experience with our senses, our emotions, our bodies. Emotion and intellect develop together, function together and guide action. Both underlie our tacit knowledge and well-educated intuitions. Thus dichotomies between reason and intuition and reason and emotion are false dichotomies.

Under ancestral conditions of childrearing which fosters optimal development (Narvaez et al., in press), right-brain objectivity develops naturally as part of human nature.³ When early care is optimal (Narvaez et al, in press), one has emotional rationality available to see things as they are and flexibly respond. If early experience is traumatic or relationally non-responsive and rejecting, emotional systems become predominantly oriented to self-protection (what I call a safety ethic when applied to the socio-moral aspects of life), undermining much of the emotional intelligence that otherwise develops under good care. Unable to truly apprehend reality, one learns to use self-centered filters for self-protective purposes and “left brain” detachment.

The Practice of Rationality

Polanyi criticizes scientific objectivity for its detachment and third-person point of view. Science’s attempt to standardize, sort and categorize experience is often lauded as civilized and progressive. Asymmetry is the deep bone of natural objects and everything in nature, including every human brain/mind, is unique. Each orange, each popped corn kernel tastes different. Nature is not a factory. Only human-built machines create things that are replicas. Science seems to be seeking reality as replicas. So how do or should rational beings approach such uniqueness? *Not* through objectifying, sorting and categorizing. Standardizing, sorting and categorizing takes away from understanding the uniqueness of each thing. Indigenous peoples understand this and often refuse to cooperate in research studies that focus on hypotheticals of things that are irrelevant to their experience (Luria, 1976). Their knowledge is personal knowledge that they use every day to survive and thrive.

Real knowledge requires a first person point of view. Objectivity emerges from the experience of indwelling. “Indwelling in objects” means understanding reality from the object’s point of view. “The method is not that of *detachment* but rather that of *involvement* . . . in order to understand living things, we must dwell in our subjects of knowledge more deeply . . . we can succeed here only by a completely reflexive indwelling—a full conviviality with our subject” (Polanyi & Prosch, 1975, p. 63). The first person point of view also emerges with tool use when the tool user extends self agency through the tool in that we “pour ourselves out into them and assimilate them as parts of our own existence” (Polanyi, 1958, p. 59).

Rationality also entails the ability to take a second person point of view, what Macmurray calls seeing the “thing as it is” with full feeling and intellect. Again this is primarily tacit knowledge, not third-person detached understanding. This point of view abandons egocentric purposes and approaches the object with

openminded but also openhearted attitudes. Indigenous thinking encompasses these perspectives, offering subjectivity to all things, using a tacit seeing that comes from our animal nature. Indigenous knowledge of the surroundings involves taking the time to know an object, a setting, a situation, allowing tacit knowledge to develop and emerge.

It is my contention that the third-person, detached, left-brain objectivity is ultimately irrational. It only seems rational in the individualistic West, fostered by schooling and a harsh early life, which shut down emotion development. Although hegemonic in contemporary modern societies, left brain objectivity represents a minority perspective among peoples in the history of the world. The evolution of the Western “objective” mind has increasingly detached itself from relationships, from nature, from emotion, to emphasize reasoning, individual autonomy and control. With roots in ancient Greek thought, but also in Abrahamic religions and their inherent violence towards children (punish, circumcise, obey), the Western cultures undermine capacities for the holistic right brain objectivity described by Macmurray. As Macmurray points out, ignoring emotions and their proper development has plagued the Western world for centuries. In my view, left-brain objectivity is proving to be environmentally and socially destructive as attention is drawn away from emotional and ecological intelligence. The push to left-brain objectivity leads to extreme individualism, a plethora of psychopathologies and degradation of the natural world. (See McGilchrist, 2009 for historical review.)

Moral Understanding and a Science of Virtue

Like all vital knowledge, moral understanding is primarily tacit. It begins implicitly in the interactions with caregivers in early life. A mutually-responsive relationship builds a sensorimotor understanding of reciprocity (Lerner, 2002) and facilitates empathy (Kochanska, 2002). Good caring fosters the development of prosocial emotions systems and their connectivity to higher order centers (Schore, 1994). Our ancestral environment provided the evolved caregiving practices that facilitated the development of natural virtue through lived experience (Narvaez, in preparation; Narvaez & Gleason, in press; Narvaez et al., in press).

Natural virtue develops from tacit knowledge constructed from life experience. Moral understanding (virtue) is applied in the moment and is primarily tacit, embodied knowledge (knowing how). Virtuous behavior occurs between organisms, dynamic systems that are uniquely interfaced at any given moment. Virtue is about taking the right action at the right time in the right way. Emerging from practice and experience, “true knowledge lies in our ability to use it” (Polanyi, 1966/2009, p. 17).

Moral virtue is about living well. Piaget (1932/1965) contended that morality is the logic of action. Locke too saw moral ideals as maxims of prudence. We can see this among those who live in our longstanding ancestral lifestyle, small-band foragers (representing over 95% of human existence). In that environment, virtue developed naturally, as a continuously nurturing environment and community solidarity fostered virtue as well as survival. Virtue evolved to be rational, although these days it has become mixed up with irrationality and divorced from survival.

Moral learning often takes place through negative behavior such as one’s mistakes (Oser, 1996). When one takes an action that unforeseeably causes harm to another, it offers a chance to learn to be more sensitive in the future. We can expand this notion of learning from negative experiences to include personal experiences of discrimination and injustice. Indeed, when one is shunned or mistreated, one has the opportunity to dwell in the realm of injustice, and feel how it feels to receive unjust treatment. Social indwelling provides the insights

that lead to greater empathy. Personal experiences can expand one's imagination. Consider losing a parent. There is minimal comprehension from the outside (pre-loss). But from the inside (post-loss), one learns great empathy for others who have suffered the same kind of permanent personal loss.

Most human complex societies have moved away from natural virtue. I believe it is because of the abandonment of ancestral parenting practices concomitant with an emphasis on left-brain thinking, cultural divorce from the body and emotion, and the hierarchical structures that coerce young children to suppress their emotions, instead of encouraging full sensory development. Instead, children (and adults) are punished for their natural inclinations (e.g., instead of close physical affection, extensive separation is enforced day and night). This suppression of evolved propensities, the closing off of emotion systems and emotional intelligence, leads to the rampant and increasing ill health and psychopathology in Western cultures that is unknown in ancestral environments.

The differences between Western and ancestral upbringings are well described by the anthropologist, Colin Turnbull (1963). Turnbull contrasts his own British upbringing by nannies, emotionally distant parents and boarding school with that of the Mbuti. Whereas the Mbuti child is encouraged to develop all senses to their fullest capacities, the British child's spirit was squelched, punished, and traumatized, arriving at adolescence with an internal emptiness that is filled by the adult's prescriptions, rules and beliefs. Such is not the way to *natural* virtue.

When natural development is thwarted, natural virtue is undermined. Instead, the West has necessarily emphasized an *adopted* virtue. Adopted virtue assumes an external morality—rules designed by someone else, imposed through coercion. It is a left-brain approach that seems logical and rational by people whose natural virtue development was thwarted. It is not known tacitly but through explicit memorization, conditioning and association. Thus, people can talk about it, even though they may not know it, or know how to practice it. This type of inert, memorized semantic knowledge is “learned” without a tacit base, but few would argue that it represents true understanding.⁴

Adopted virtue lends itself to scientific experiment because it is not real virtue and is the subject of most experiments. Science lives in static, controllable variables but natural virtue does not. As science is currently configured, isolating and controlling variables, it cannot analyze natural virtue. Virtuous action cannot be randomly assigned. Virtue is about the right manner for the moment. Science does not deal with manner or moments. The gleam in the mother's eye at the moment of a child's behavior is not replicable by experiment (controlled conditions). Virtue may be part of our unprovable knowledge referred to by Polanyi (1958).

When children don't get what they need to build all brain and body systems that underlie their prosociality, they necessarily develop self-centered brains that move between threat sensitivity and emotional detachment, what I call for moral functioning a *safety ethic* and a *personal imagination ethic* respectively (Narvaez, 2008; in preparation). But there can be much psychopathology supporting these particular ethics. The undermining of natural virtue may underlie what Polanyi noted as “moral inversion,” when “skepticism drives men's moral sentiments underground, whence they emerge, combined with sadism, as a creed of salvation by violence” (1975, p. 28). Moral inversion can flourish when self doubt and cynicism are rampant, again from the undermining of natural virtue, and are channeled into a support for unfettered power. Adopted virtue can be an ideology so strong that it leads to vicious imagination (Narvaez, in preparation) and evil felt as a duty (Weil, 1947/1952).

Morality is a whole-brain phenomenon—it takes contextual awareness as well abstraction from the present moment to think about possibility (what I call communal imagination). We need the full capacities of right-brain thinking coordinated by left-brain thinking, a type of mindful morality. Anything less, and we betray our human heritage.

Conclusion

If we are going to be good scientists or scholars of any type, we need the personal knowledge of our subject. To obtain personal knowledge we need to be unburdened by misdirecting experiences. But individuals must develop their emotions and sensibilities optimally so they can trust their tacit knowledge instead of feeling the need to rely on outside proof of what is real.⁵ For the moral life, this immersed experience needs to be in real social life, not in books or other mediated experiences (although these can expand existing intuitions).

As long as science emphasizes “left-brain thinking,” with its decontextualization of features, separation of components from the whole, ignoring data that does not fit the model, virtue will remain a mystery. Virtue cannot be measured with left brain emotionally-detached analysis. However, with the employment of indwelling and right-brain understanding, virtue may be understood and lived.

Endnotes

¹For example, only Western-educated people believe that individuals can make choices that don’t affect others. Psychologists and their subjects often take this view and study individual decision making, then draw conclusions about selfish human nature.

²These comments about left and right brain differences come from studies of patients whose left and right brains were severed to prevent epileptic seizures. See McGilchrist, 2009, for a recent review of the research.

³Our human ancestral parenting practices in early life, revisions of social mammalian practices that evolved more than 30 million years ago, include natural childbirth (no interference with time, no separation of baby from mother), breastfeeding 2-5 years, constant touch, responsiveness to the needs of the child (no crying), multiple adult caregivers, free play in nature. We are studying these in my research laboratory and finding effects of each on moral development (e.g., empathy, conscience, self-regulation).

⁴Although often described as intuition, true tacit knowledge must be distinguished from false and naïve intuitions. Tacit knowledge represents well-educated intuitions built from immersed experience in a good or “kind” environment (one that gives appropriate feedback for what we are learning). Naïve intuitions, guesses based on other knowledge, often head us in the wrong direction because we do not have enough appropriate experience in a domain. Intuitions can also be misleading if we just adopt them from others, as children often do from parents or peers, or group members do about non-group members. These are false intuitions, focusing on approach/avoidance rather than the intricate knowledge of the expert. They are not our own intuitions at all. They do not form tacit knowledge but sloganeering semantic associations.

⁵Science itself needs to be more humble about describing reality, especially if it remains stuck in left-brain thinking. Understanding Polanyi’s advice would be a good start. “Owing to the ultimately tacit character of all our knowledge, we remain ever unable to say all that we know, so also, in view of the tacit character of meaning, we can never quite know what is implied in what we say” (1958, p. 95).

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Polanyi and Narvaez's "Moral Rationality"

Gus Breyspraak

ABSTRACT Key Words: Narvaez, objectivism, indigenous knowing, childrearing, right and left brain, reductionism, brain and higher cognitive phenomena.

Before raising five points for further discussion I attempt to outline the back story or meta-narrative behind *Narvaez and/or her sources and question whether it fits well with Polanyi. I then address her views of appropriate childrearing, how she conceptualizes the relationship of brain studies to higher level cognitive phenomena, her positive assessment of the indigenous mind, the widespread acceptance of tacit knowing in psychology, and whether her attacks on objectivism allow the positive role for scientific knowing that Polanyi advocated.*

I want to begin by thanking Darcia Narvaez, not just for this stimulating paper, but for her voluminous output in so many important areas of the psychology of moral development, moral education, ethics, and philosophical reflection as related to these areas. And I thank Paul Lewis who seems to have been the one who "discovered" her for this tribe that gathers annually, because of our interests in Polanyi's thought, to discuss papers.¹

There are many interesting points to explore, so Paul and I agreed to chase different rabbits. Paul knows much more about virtue ethics, not to mention about virtue itself, than I do, so I am leaving those topics for him. Also those who have read Narvaez's paper may have other questions such as questions about the compatibility and relationship of Polanyi with MacMurray that I am ignoring.

Before raising five points, I summarize what I see as a possible back story or meta-narrative underlying Narvaez's paper, though not in exactly the order she wrote it. I don't think this is the only way she would tell the story, but I do suspect that some of her sources tend in this direction.

I. Two Ways of Knowing and Being

Almost all of the italicized words, phrases, and sentences in the following eight paragraphs are taken directly from Dr. Narvaez's paper:

Once upon a time, or more accurately for a very long time (*for over 95% of human existence*), *human beings were parented appropriately in the ways of our ancestors. We developed naturally into virtuous, emotionally healthy (expressive rather than contained?), fully sensory, emotionally intelligent, and body-accepting members of communal groups. We survived and thrived by relating to the rich individual particularities of the world holistically by primarily using our right brained, largely tacit, ways of knowing. The world we came to know was infused with meaning, morality, and spirituality. We found subjectivity in all things through involvement and indwelling.*

This indigenous way of knowing and being is largely lost in the modern West's individualistic, detached, left brained objectivity of egocentric intellectual awareness. This alternative way of knowing and being is found in complex societies and results from harsh child rearing (violence toward children), suppression of

emotions, and detachment from nature. It gives us knowledge about things rather than knowledge of them.

The primary cause seems to be faulty child rearing: whether raised by nannies, boarding schools, or hierarchical, emotionally and physically distanced parents, children's spirits are squelched, punished, and traumatized. They arrive at adolescence with an internal emptiness that is then filled by the adult's prescriptions, rules, and beliefs of adopted virtue rather than the natural virtue of ancestral ways and indigenous cultures.

The pursuit of "objective" science as a cultural ideal far beyond science also has a contributing role in causing our fall; thus Polanyi's ideas get involved.

The increasing practice of third person detached, left brain objectivity had led to a Western way of knowing and being that "has increasingly detached itself from relationships, from nature, from emotion, to emphasize reasoning, individual autonomy and control."

This modern Western objective form of knowing and being categorizes, standardizes, dissects, separates, prefers lifeless and static things, seeks replicas rather than true knowledge of individual reality. It so distorts our thinking that we no longer trust our received wisdom: many don't even believe that breast milk evolved over millions of years with thousands of ingredients is better than scientifically derived formulas with a few dozen. Objective knowing is environmentally and socially destructive and leads to extreme individualism, a plethora of psychopathologies, and degradation of the natural world.

When children don't get what they need to build all brain and body systems, they develop self-centered brains that move between threat sensitivity and emotional detachment, thus undermining natural virtue and making them ripe for what Polanyi has described as "moral inversion."

Furthermore, science as currently configured will be unable to recognize or analyze the natural virtue we must recover: "As long as science emphasizes "left-brain thinking" with its decontextualization of features, separation of components from the whole, ignoring data that does not fit the model, virtue will remain a mystery. Left-brained scientific thinking can neither know nor learn to live virtue." Instead we need to support the development of right-brain objectivity to regain emotion, subjectivity, tacit knowledge, indigenous wisdom, and natural virtue.

II. Does Polanyi Fit the Logic of the Back Story?

My summary has re-arranged the order but I think not the logic underlying some sections of the paper. Her paper contains important divergences from this story to which I will return later, but I find something like this in the background—whether from some of her sources or in her own thinking. Her references reveal important works in progress that will explain and build her case in greater depth and I look forward to reading them. She employs Polanyi in several interesting ways as she tells this sad tale and probes its implications. He might well be pleased to find a major psychologist using his ideas. I find important contributions in her work.

But, perhaps because my left brain is overactive, I am not convinced Polanyi fits this particular story at some major junctures. And I have some significant reservations if this is the background story or meta-narrative informing her thinking.

III. Five Points For Consideration

Let me address five points briefly:

1) I am intrigued with her attention to “appropriate childrearing,” as the grandfather of two toddlers among other reasons, and I think this may go beyond Polanyi in helpful ways. Polanyi did not seem much interested in this type of psychological reflection about such matters, as far as I remember. He uses Gestalt, Kohler, cites much experimental data and draws from Piaget a good bit, but almost seems to assume that people develop normally. He is fascinated by how that happens. He doesn’t dig around in “relationships” or what can go wrong in our thinking when relationships get fouled up as much as he focuses on how we are built, wired, raised for discovery and meaning seeking. I wish he had been more tuned into psycho-dynamics and the psychology of interpersonal relationships in the same way that I wish he had been more “sociological.”

He is not totally unaware of detours, maladaptation you might say, as when he discusses the chicken who “pecks wildly at its fellow chicks and runs around terror stricken” (PK 210 and 295) when put in their company after having been raised in isolation. There are more than 3 dozen references to children in the index of PK. But Narvaez’s exploration the impact of early nurturing including how we can get sidetracked into destructive detours is helpful and seems to me to be an addition that Polanyi can use. I would be interested in even more examination of how “objectivism” is impacting child rearing in negative ways, if it is. That seems to be among the projects in press or on which she is working.

2) Similarly, her knowledge of and attention to what I would call the neurological correlates of some of the phenomena Polanyi describes and analyzes can also be quite fascinating. As a fan of Eric Kandel, both his wonderful autobiography/history of 20th century brain research and his excellent work with Charley Rose on that series that you all should view online if you have missed it, I want to know about more about the brain in all of this (where this stuff is going on, what chemicals are involved, etc.) In this paper she takes us into right brain left brain dimensions and in other works engages other areas of emerging cognitive science.

There are, of course, dangers here, most notably a kind of reductionism that is quite opposed to Polanyi when it is assumed that descriptions of the “correlates” at what we might call lower levels explain away the higher level phenomena. Even the great John Searle recently responded to letter from a reader who does not see how it is possible that “a bunch of firing neurons in any kind of network produce consciousness.” Searle replied: “I entirely agree that, at present, the way neurons produce consciousness remains mysterious.”²

David Barash was recently asked what was the most difficult unsolved problem in science:

I answered without hesitation: How the brain generates awareness, thought, perceptions, emotions, and so forth, what philosophers call “the hard problem of consciousness.” It’s a hard one indeed, so hard that despite an immense amount of research attention devoted to neurobiology, and despite great advances in our knowledge, I don’t believe we are significantly closer to bridging the gap between that which is physical,

anatomical and electro-neurochemical, and what is subjectively experienced by all of us .
... or at least by me.³

So I am curious about Narvaez's understanding of the relationship of these neurobiological phenomena to "higher level" cognition and emotions. She seems to be treading on thin ice in reporting "at the neurobiological level the systems are indistinguishable" when speaking of emotions and cognitions, reasoning and passions. Searle and Barash both seem to think it is only a matter of time before the mystery will be solved, but I am wondering if Lewis from whom Narvaez is drawing, thinks it already has been solved and is saying that emotion and cognition are the same because the same neurons are firing?

With Polanyi's way of thinking, as I understand it, the "mystery" will remain because it is built into the very structure of our knowing and being. Polanyi taught us that an exhaustive physical-chemical analysis of a frog will not reveal that it is a frog and that exhaustive explication of the physical-chemical properties of a machine will not reveal the purposes of the machine or even its existence as a machine. Perhaps some attention to levels is needed here to "distinguish the systems."

Also, trying to locate explicit knowledge and tacit knowledge in different hemispheres may involve different understandings of explicit and tacit from some uses in Polanyi: think of his opening distinctions in *The Tacit Dimension* between the functional, phenomenal, and semantic, and ontological dimensions. Can each of these be assigned to different brain regions? How does this mapping address the "from-to" structure of all knowing?

I am reassured by Narvaez's concluding affirmation that "morality is a whole-brain phenomenon" and that we need "the full capacities of right-brain thinking coordinated by left brain thinking." I think Polanyi would agree with her earlier statement that "true rationality combines both intellect (detached thinking) and emotion" and "... a dichotomy between reason and intuition, reason and emotion are false dichotomies." But I feel some tension between such statements and the comments on indistinguishable systems and the tacit/explicit mapping by hemisphere and I am curious if she does.

3) I also find her appreciation of what is variously described as the indigenous and ancestral mind of interest because it takes me into one of the most fascinating areas of Polanyi's thought - his puzzlement over how the scientific account is better than that of the Azande witchcraft. I hope someone will work out more of the relationship of Polanyi to Evans-Pritchard. (I can remember spending hours arguing about all of that with Ron Hall over coffee in the Great Hall at Duke when I should have been reading my assignments for theology courses.) There is much to be learned from that section of PK and from Polanyi's little essay on "The Stability of Beliefs," and I still haven't got it all sorted out.

I am concerned, though, that Narvaez, perhaps following her sources, so strongly rejects "objectivism" that she is embracing a Romanticized version of indigenous and ancestral thinking. Polanyi clearly prefers scientific accounts in many instances to indigenous thought's explanations. Does Jacobs (from whom she draws) prefer the indigenous mind's accounts over those of science? Doesn't Narvaez think that in most areas our modern Western account is not only better but true? Do we really want to go with poison oracles and being held responsible for what we do in someone else's dreams?

How do we want to sort all of that out in a way that does not fall into either trap of an objectivist scientific reductionism that misses other realities on the one hand or an overly naive romanticized view of the noble savage mind that rejects science on the other?

4) I am glad to hear that “the understanding that tacit knowledge is predominant is finally a widespread view in psychological science.” That may well be progress, depending on what psychological science understands tacit knowledge to be. Not everyone who uses the term is using it the way Polanyi developed it.

We have a long history in modernity of letting our deeply rooted objectivist assumptions slip in the back door when we open to front door with an attack on objectivism, and I would fear some of that may be happening in psychological science as it does in so many of the more popular treatments of tacit knowledge I read in newspapers and reviews. The recent flurry of discussion (on the Polanyi Society discussion list) of Collins’ book on tacit and explicit knowledge provides much discussion of questionable appropriation of Polanyi’s thought, and I would expect some similar misunderstandings can be found in psychology

This brings to mind a question for those who knew Polanyi or know of his reactions from biographical research: are there any examples of his reactions to persons who misunderstood his ideas and used them inappropriately? I have this image of him always welcoming those who wanted to apply his ideas - surely there were times he was concerned about misunderstandings?

5) And that brings me to my fifth and final point, one which I raise with some hesitation because I am speaking about sources she has used of which I am ignorant. To use a distinction Narvaez develops in earlier work, I don’t even have “recognition knowledge,” much less articulate explicable knowledge of the works from which she is drawing.

But it seems to me that a superficial reading of her essay might lead astray any one of a variety of subjectivists, as opposed to objectivists, who rightly sense something is wrong with objectivism. And I mean lead them astray into the wilderness of failing to appreciate scientific ways of knowing, in spite of the widespread misunderstanding of the nature of objectivity and how science has actually discovered what it has. I think Polanyi’s goal was to redefine, although that is too timid a term for what he attempted, the notion of objectivity - not to replace it with another form of subjectivity. He was moving toward a kind of redefinition of all of the terms, and I suspect some of those from whom Narvaez is drawing, may not have reached that level of understanding.

What appear in some of her sources to be attacks on science itself, as distinguished from a mistaken account of science that is leading us astray, seem to me to be very different from Polanyi’s project which was pro-science to the extent of having scientific knowing serve, once properly understood, as the model for much inquiry and even the basis of a free society.

Narvaez seems to recognize some of this at various points and there are several “objectivisms” in her essay which add nuance to the back story I have outlined. When she pulls all of this together in her Terry, Gifford or some other lectures, I hope she gets to that level of re-defining objectivism and all the associated terms with careful avoidance of embracing a subjectivism that is as bad as a left brained reductionist objectivism as she has described it.

In the meantime I look forward to reading her continued probing of these very important areas with the great resources, skills, and rich background she brings to the task.

Endnotes

¹Paul Lewis provides a summary of Narvaez's work in "The Emerging Comprehensive Moral Psychology of Darcia Narvaez," *TAD* 37:3 (2020-2011): 9-18.

²Letters to Editor, *The New York Review of Books*, September 29, 2011, p.101

³David Barash "The Hardest Problem in Science," *Chronicle of Higher Education Brainstorm* (online blog), Oct 28, 2011.

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Toward a Holistic Moral Rationality: Questions in Response to Darcia Narvaez's "Moral Rationality"

Paul Lewis

ABSTRACT Key Words: Darcia Narvaez, Michael Polanyi, Aristotle, Albert Borgmann, Matt Crawford, virtue, virtues, practical wisdom, moral rationality, natural.

This brief essay summarizes Darcia Narvaez's "Moral Rationality" and raises five questions for further reflection: Should we be wary of the erosion of the multiplicity of virtues in the ancient world to the modern discourse of virtue? How natural are the virtues? Upon what basis can we build a defensible account of human flourishing? Does the West have virtues that are worth preserving? What does "real" social life mean concretely?

I. Introduction

Like Gus Breyspraak, I begin by expressing my thanks to Darcia Narvaez, both for taking the time out of her prodigious commitments to prepare an original essay for us and for her willingness, even courage, to make a presentation at the Polanyi Society annual meeting; I know that it can be intimidating to speak about Polanyi to people who know his work by chapter and verse—in fact, I still feel like a neophyte most of time. I am also grateful for the opportunity to continue a conversation that we have pursued now and then over the past couple of years. Since discovering her work when I was on sabbatical in 2008, I have wanted to get friends and colleagues in a room where we could talk about her work and its implications for our own. This is now the second time I have been able to do so (the first was a conference at Mercer University last year) and I am excited about what can emerge from our discussions this morning.

The paper that Narvaez has prepared for us exhibits many of the qualities that I value in her work. As is true of most of that work, she draws from multiple disciplines to make her case; in this paper, that means primarily the natural sciences, the social sciences, and philosophy. Moreover, she does so with a sensitivity to what is needed for authentic human flourishing. My response proceeds then in two parts: a synopsis of the paper and an exposition of five points for our consideration.

II. A Synopsis of "Moral Rationality"

Narvaez begins her paper with a lucid and succinct account of the modern view of rationality as objective, detached, and focused on quantification, what she later calls a third-person construal of rationality. Along the way, she suggests that this view persists for two reasons: the biological fact that left and right brain hemispheres process information differently and the cultural proclivities of the modern west to prefer one to the other (or the whole; p. 5). Nevertheless, she recognizes that the modern view of rationality has its problems, most especially its failure to recognize the way that prior experience shapes perception, its naïveté about the relationship between reason and emotion given that much of the same neurological systems underlie these two capacities, and its superficiality (pp. 2-4).

In Polanyi and Macmurray, she finds people who suggest somewhat different but complementary alternatives to the modern view. She lauds Polanyi's emphasis on how knowledge is personal, experiential and perspectival, as well as Macmurray's emphasis on the kind of knowledge that emotions can provide, i.e., knowledge "of" rather than knowledge "about" (p. 4). She draws from both people to supplement her account of how rationality develops out of biological drives, bodily states, emotions, and early experiences such as parental care and interactions with the environment. The result is a rationality that, at its best, combines emotion and reason, as it did naturally under the conditions in which our ancestors evolved (p. 8). Authentic rationality therefore combines both "first-person" involvement with a "second-person" open-mindedness that is willing to meet the other on the other's own terms (pp. 9-10).¹

Narvaez next applies this model of a true or authentic (one might say holistic) rationality to the special case of moral understanding or virtue. Like rationality at its best, moral understanding is primarily tacit and learned from experience—especially negative experiences (pp. 11-12).² Like holistic rationality, moral understanding developed naturally in the ancestral environment and is threatened by the individualistic, emotionally-stunted west that turns morality into rote recitation or memorization that in turn fosters an ethic of security and perhaps even promotes moral inversion (pp. 13-14). Her solution for combating these failures of modern western life is to "get real," i.e., get engaged in life first hand, "close-up and personal," so to speak, rather than at a distance.

III. Five Points for Further Consideration

So then, what shall we make of this call to engage the world as agents who both think and care deeply? After all, this is a call to which I am sympathetic, as I suspect is true of all of who are reading this issue. Realizing that one can only do so much in a brief paper, I now offer five points for further reflection and conversation. The first three points seek to initiate a conversation between present and past (specifically with Aristotle), whereas the latter two ask Narvaez to expand on some of her key points.

First, I would like us to think some more about her treatment of virtue as moral understanding, language that strikes me as a rather modern view of virtue. As Alasdair MacIntyre and Thomas Spragens have noted, one of the things that modern moral philosophy has done is to erode the rich account of the plurality of virtues found among the ancient Greeks in favor of a monolithic, one-dimensional, view of moral virtue that emphasizes calculative reason.³ To be sure, at one level, Narvaez sounds much like the ancients (especially Aristotle), such as when she says that virtue is about "taking the right action at the right time in the right way" (cf. Aristotle's discussion of anger) or, that the development of virtue requires a supportive environment (the ancestral environment for Narvaez, a healthy polis for Aristotle) or "moral virtue is about living well" (cf. Aristotle's *eudaimonia*). On the other hand, I worry that this equation of virtue with understanding risks undermining her larger point about the holistic nature of our cognitive capacities and lends itself to reinforcing the view of moral reason as calculative and dispassionate. Aristotle avoids that problem by describing *phronesis* (practical wisdom) as the virtue that bridges or connects the moral and intellectual, the passionate and the dispassionate. In short, I wonder if we don't need something like Aristotle's more richly-textured account of the virtues (plural) in order to avoid the problems of modernity's emaciated understanding of moral reason.

My second point continues this conversation with Aristotle, for I would like to hear more about natural virtue. Again I wonder if this is a place where the ancients did better than we moderns. For Aristotle, there was nothing natural about the virtues, apart from the fact that human beings naturally seem to need them.⁴

The virtues, for Aristotle, function as a second nature that guides or channels our multiple capacities towards flourishing. Whereas for Aristotle what is natural seems to need direction, Narvaez uses “natural” as a term of approval for what is not artificial, adopted, or cultural. At this point, three related questions emerge for me. I wonder, first, if we can so easily distinguish the natural from the artificial. For example, human beings “naturally” seem to have propensities to develop the artificial (such as tools or even cultures). I wonder, secondly, if we can so easily label the natural “good” and the artificial “bad.” For example, if culture is what guides human capacities to fulfillment, in what sense is it “bad?” I wonder, thirdly, if nature is so monolithically good. For example, human beings “naturally” come wired with capacities for both aggressive behaviors and cooperative behaviors. Are they both good, or is the former good and the latter bad? On what basis do we make these judgments? In short, I would like to hear more about how we might tease apart what is natural from what is artificial, as well as what can serve as the basis upon which we make judgments of good and bad.

My third point is again rooted in Aristotle (and this will be my last, I promise). Again, Narvaez sounds like Aristotle when she talks about living well. As she develops her point, we can infer from the essay that a human life well lived is a life lived in harmonious relationship with others: we do not discriminate, we do not mistreat, we do not suppress emotions, etc. Instead we live in mutually supportive communities that foster individual growth. I am happy to affirm this broad sketch of human flourishing, but would like to hear more about what informs her normative vision, i.e., the convictions and commitments that guide her to affirm this vision of a peaceable kingdom. Is it just the ancestral environment or is there more to it?

My fourth point has to do with Narvaez’s treatment of western society. Polanyi certainly was critical of some facets of western life, but was not so seemingly pessimistic. While many of the evils Narvaez attributes to the west are fair enough (at least generally speaking), they do not tell the whole story. For example, I am not convinced that Abrahamic religions are inherently violent towards children. Whereas the evidence about the practice of child sacrifice in ancient Judaism is mixed, it is clear that early Christians remembered that Jesus valued children, as displayed by the episode in which he chastises the disciples for turning away the children, whom he then blesses.⁵ In short, our assessment of the west needs further refinement and so I ask, are there any virtues of the west that we need to preserve?

That point leads to my final question, which emerges from the treatment of the ancestral setting as good and the modern west as bad: how do we recover the best of the past? Narvaez suggests the answer when she calls us to engage in “real” social life. I am not sure what that means, however, and so would like to see some flesh put on those bones. I find her language reminiscent of arguments set out by Albert Borgmann and Matt Crawford, both of whom worry about contemporary tendencies to substitute a virtual world for a robust, real world. Borgmann makes his point by contrasting the rich texture of urban street life with a suburban supermarket. On the street, he says, “We smell the bread baking; we see the produce being trucked in. In a supermarket, to the contrary, all underlying distinctions and connections are consolidated and concealed. We are left with a superficial display of commodities.”⁶ Crawford echoes Borgmann when he compares being part of a crew with being part of a “team,” a term that now occurs ubiquitously in contemporary organizational life. On a construction crew, Crawford argues, “face-to-face actions are still the norm, you are responsible for your own work, and clear standards provide the basis for solidarity of the crew, as opposed to the manipulative social relations of the office ‘team.’”⁷ To make the point more bluntly, Crawford observes, “where no appeal to a carpenter’s level is possible, sensitivity training becomes necessary.”⁸ For him, real social life requires face-to-face engagement with others that is responsive to shared standards of success that lie outside the self

and are therefore more than personal preference or whim. My question then is this: Do these examples point us in the right direction for putting some flesh on the bones of the phrase “real social life?”

IV. Conclusion

When all is said and done, I want to reaffirm the value of Narvaez’s paper as an ally with Polanyi and others in the quest to provide a more holistic, human, and humane understanding of rationality in general and the moral life in particular. I am sure this work has stimulated your thought as it has mine. It has raised, for me at least, several issues for further exploration: the turn from virtues to virtue, how natural the virtues might be, the basis upon which we might build a defensible account of human flourishing, the other side of the story when it comes to western society, and what real social life means more concretely. I look forward to the ongoing conversation can take us.

Endnotes

¹Here I think of Buber’s distinction between I-It relationships and I-You. I wonder how Narvaez’s “2nd person” rationality compares to Buber’s “I-You.”

²At least she highlights the negative in this paper. Her larger body of work gives a more comprehensive account of how morality can be educated. For a summary, see my “The Emerging Comprehensive Moral Psychology of Darcia Narvaez” in *Tradition and Discovery* 27:3 (2010-2011):11-12.

³See Alasdair MacIntyre, *After Virtue* 2nd ed. (Notre Dame, IN: University of Notre Dame Press, 1984), 233 and Thomas Spragens, *Reason and Democracy* (Durham, NC: Duke University Press, 1990), 14-56.

⁴Aristotle, *Nicomachean Ethics*. Trans. Martin Ostwald (New York: Macmillan Publishing Company, 1986), 1103a, 25.

⁵Child sacrifice in ancient Judaism is a complex and contested topic. On one hand, the story of the binding of Isaac (Genesis 22:1-19) seems to mitigate against the practice, which is expressly forbidden later in the Torah (Leviticus 20:2-5). On the other hand, one might infer from Exodus 22:29-30 that God required the sacrifice of the first born. Later, Jeremiah denies that God ever considered allowing child sacrifice (19:5-6), whereas Ezekiel claims that God did in fact require child-sacrifice in order to clearly establish who was in control (20:25-26). For Jesus and the children, see Matthew 19:13-15 or its parallels in Mark 10:13-16 and Luke 18:15-17.

⁶Albert Borgmann, *Crossing the Postmodern Divide* (Chicago: The University of Chicago Press, 1992), 131.

⁷Matthew B. Crawford, *Shopcraft as Soulcraft: an Inquiry into the Value of Work* (New York: Penguin Press, 2009), 199

⁸Crawford, 157.

Restoring Personal Knowledge and Virtue: Response to Commentary

Darcia Narvaez

ABSTRACT Key Words: personal knowledge, childrearing, virtue, small-band hunter gatherer, science, objectivity, self, well-being.

The orientation to personal knowledge rather than inert, detached knowledge, may be established in early life when brain and body systems are shaped by caregiving. The restoration of social supports like ancestral parenting practices in early life may return humanity to its highest intelligence, a whole-brain eco-mindedness.

I appreciate the thoughtful and constructive comments from Gus Breyspraak and Paul Lewis. And I enjoyed the group discussion after my talk. I have tried to create an integrative response to several comments.

Inspiration and the Past

When we refer to past societies, we have to be clear about what aspect and what era of the past we are referencing. I am referring to the social environment of the small-band hunter-gatherers in which 99% of human genus history was spent. This is the period before war, before the hoarding of resources and before institutions of hierarchy. I am engaged these days in reading anthropological and explorer accounts of contemporary or historically-recent small-band hunter-gatherer societies, where prosocial behaviors, peaceableness, and psychological well-being flourished (Fry, 2006). I use this context as the baseline for discussion of the human family, society and human nature. It is not that I want us to return our societies to this very environment, but there are important insights that knowledge of our genus' typical lifestyle can provide.

In the small-band hunter-gatherer context, the stressors were/are mostly physical (food, shelter, predators) rather than social. The social environment was supportive and, from the anthropological reports of societies in these conditions, fun, leisurely and egalitarian (in cases of conflict, groups split up). In these conditions, the mind/brain grows socially and intellectually optimally, facilitating collective functioning and adaptive physical survival. Somehow in modern life we have put the emphasis on physical supports (e.g., comfort) rather than on social supports, even though the latter may be more critical for physical, social, and collective wellbeing.

The Effects of Objectivism on Child Rearing

We live and raise children in an environment that is opposite to the small-band hunter-gatherer, with relative physical comfort but with enormous built-in social distress. For example, separating a baby from mother at all is detrimental to brain and body development but we routinely cause such pain (and much more) to babies at birth. We have drifted away from the practices that foster well-being in early life when the brain is developing so rapidly (extensive, on-demand breastfeeding; constant touch, responsiveness to needs—so the baby doesn't cry, multiple adult caregivers in the midst of adult life, free play and natural childbirth). I call the lack of these characteristics “undercare” and it characterizes the lives of many USA children today (Narvaez, in preparation; Narvaez & Gleason, in press). Such practices create mind/brains that are less adaptive and

less conducive to cooperative eco-mindedness, which is a given in small-band indigenous thought. Poor early environments, as in many settled societies, lead the brain to become oriented to self-protection, to fantasize and develop all sorts of strange beliefs to keep painful reality at bay (like the Azande). The strange beliefs we see in complex societies, including in the West,¹ also correspond to the trauma that children and/or adults have undergone, perhaps through coercion of some sort, which is a rare occurrence in small-band society. The natural tendency of the mind/brain is to explain and draw causal conclusions from experience. In contrast, the reports of small-band hunter gatherers indicate that they do/did not have extensive belief systems about what they did not/cannot know directly (e.g., Everett, 2009). They don't experience the institutionalized violence and neglect that characterizes modern life from birth and don't need the extensive narratives of explanation to make meaning and generate self comfort.

Through the neglect of what children need, I think the USA has been increasingly obsessed with left-brain, detached thinking, in part because we have neglected right brain development (Schoore, 1997, 2000, 2001). When you isolate children from others in early life, the brain is forced to derive pleasure not from relationships and connection (as evolved to do) but from the objects and cognitive interactions given in the place of physical closeness and affection (teddy bears, computers, i-phones). Socio-emotionally neglected children grow up to be emotionally-detached parents, and low nurturing caregiving worsens by generation in animal studies (Meaney, 2001). Without the tacit knowledge of how to be a social animal, parents feel the need to turn to experts ("objective knowledge") to replace missing personal knowledge. And science has been messing up parenting and parenting advice for some time (see James Watson's bestselling book on parenting from 1928 where he contends that babies should be touched as little as possible). I know that it is ironic that I am a scientist arguing for paying attention to scientific evidence on these matters! But if people just took the time to observe and learn from experience, they would quickly adopt many methods of childrearing from our ancestors, as it is much more encouraging to live with pleasant children than the mis-raised children of today.

Humans evolved to find great pleasure in parenting but we've been eradicating social pleasure with each new wave of engrossing technology. Generally speaking today, parents seem more and more detached from the pleasurable emotions of parenting in early life and more and more enthralled with technology (e.g., talking on cell phones while with their young children). Perhaps because they did not receive the deep nourishment to the self in early life, many parents seem absorbed in their own self-development, unnecessarily leaving children in "stranger" daycares, the vast majority of which are documented to be suboptimal for child development (NICCHD, 2004).

Virtue is comprised of sets of situated cognitive-affective-action knowledge units that are built from immersed experience and from guided mentoring—creating embodied knowledge. These are the tasks of early caregiving. To develop full sociality, one must have the relational presence of caregivers—habitual I-You experience. Too many children have regular experiences as an It to their caregivers. This sets children up for an artificial life in which facebook substitutes for true friendship and texting is more comfortable than face-to-face dialogue. Real, lived learning becomes a foreign experience to a child raised in front of screens instead of climbing trees in self-directed play with others. Then inert knowledge taught from external authorities seems like real knowledge rather than the personal knowledge that comes from direct, full-bodied, active experience.

Am I Being Reductionistic Or Too Hard On Science?

I don't think science can solve the mysteries of life nor can neuroscience. Neuroscience can be useful in outlining some specific functions but not the emergent properties that comprise much of human functioning. And, as interacting complex, dynamic systems, humans are too individually different to follow psychological laws like the laws of physics. There is a mystery to life that can likely never be completely understood by our conscious mind (reason) although it is sometimes glimpsed by the nonconscious mind (faith).

Whereas I might behave like a scientist, using its tools and findings (and raising hackles about being reductionistic!), I am increasingly concerned about science's effects. So much damage has been done to our planetary biosystem and subsystems as a result of "scientific thinking." But perhaps it is mostly due to a left-brain intellectual science. Science as a detached method was not necessary for our small-band hunter gatherer ancestors who spent their days "indwelling" to learn and manage their habitats. Too often, science encourages Buber's "I-It" relationship instead of "I-You" with the entities in the world (thanks to Paul Lewis). Science must be grounded in both first and second hand understanding rather than the third hand accounts that pervade scientific discourse.

The detached intellectual understanding of life that Western civilization and science encourages has brought us to where we are today: wellbeing is decreasing in every age group across the USA (UNICEF, 2007; USDHHS, 1999; WHO/WONCA, 2008;); every eco-system is under duress from human activity (Millenium Eco Assessment, 2005); the planet is about to pass into irreversible and catastrophic climate instability (IPCC, 2011). We have to yet to return to what Frances Moore Lappé (2011) calls an ecomind—realizing the connection of us all to each other and to everything in nature. For too long science, religion and capitalism have been encouraging objectification, detachment and manipulation of nature to the detriment of life (Jensen, 2000).

True personal knowledge must be embedded in the natural world—how can one be a true human being without being in love with the natural entities in the environs? Crawford (2009) raises for me the question whether one can be a true human being without knowing how to carve out a life with one's own hands and abilities. Moral virtue comes about from lived experience, actual face-to-face relationships, ideally in small communities (MacIntyre, 1981) situated in a world of nature. Instead, each person must learn how to develop personal knowledge of self and the world in natural environments. As the film, *The Age of Stupid*, points out, as we face the abyss of climate instability right now, we seem to think we are not worth saving. If we can restore our sense of self as we are meant to be—deeply socially engaged and collectively mindful of life—we will indeed remember that we are worth cherishing.

Endnotes

1 For example, 5-6% of US citizens believe that they have been examined by aliens (Appelle, Stuart. *The Abduction Experience: A Critical Evaluation of Theory and Evidence. Journal of UFO Studies*, 6, 1995/96, 29–78)

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WWW Polanyi Resources

The Polanyi Society web site (<http://www.missouriwestern.edu/orgs/polanyi> or polanysisociety.org/ or polanysisociety.com/) provides information about Polanyi Society membership and meetings. The site also contains the following: (1) digital archives containing all issues of *Tradition and Discovery* and predecessor publications of the Polanyi Society going back to 1972; (2) indices listing *Tradition and Discovery* authors, reviews and reviewers; (3) the history of Polanyi Society publications; (4) information on *Appraisal* and *Polanyiana*, two sister journals with special interest in Michael Polanyi's thought; (5) a link to the "Guide to the Papers of Michael Polanyi," which provides an orientation to archival material housed in the Special Collections Research Center of the University of Chicago Library, Chicago, IL 60637; (6) photographs of Polanyi; (7) links to a number of Polanyi essays (available on the Polanyi Society web site and other sites), Polanyi's Duke Lectures (1964), as well as audio files for Polanyi's McEnerney Lectures (1962), and Polanyi's conversation with Carl Rogers (1966).

In Defense of Polanyi's Understanding of Evolution: A Response to Walter Gulick¹

Daniel Paksi

ABSTRACT Key Words: Polanyi, teleology, emergentism, vitalism, ordering principle, evolution, natural selection, neo-Darwinism.

In a recent article, Walter Gulick interprets Polanyi's teleology as the consequence of an unacceptable vitalism and his ordering principle of evolution as something which threatens his vision of the multileveled hierarchical reality of living beings. I argue that Polanyi's account of teleology entails no unacceptable vitalism. Moreover, his teleology is as necessary as his higher level ordering principle of evolution to explain the emergence of multileveled hierarchical living beings: Polanyi argues that from "primordial inanimate matter" (PK 404) shaped by the mechanical principles of physics and chemistry and neo-Darwinian principles of "evolution" no development and no multileveled hierarchical living being could come into existence. Polanyi is not a vitalist philosopher but a true emergentist, one of the best, and this is at the core of his philosophy.

Preface

In a recent *TAD* essay,² Walter Gulick, following Marjorie Grene, sharply criticizes Michael Polanyi's understanding of evolution.³ He argues that central to Polanyi's misunderstanding of evolution is the "simplistic dichotomy [that] he offers as apparently the only basis for explaining evolutionary emergence: the laws of physics and chemistry versus his higher 'active' (vitalistic) explanation" (Gulick 48). Gulick contends that "this richness [of the multileveled hierarchical structure of life] cannot be reduced to a simple opposition between the laws of physics and chemistry and *vitalistic* impulses of an individual or a process" (Gulick 48, my emphasis).

Gulick's (and Grene's) fundamental inference is correct: vitalistic processes and principles are not acceptable in evolutionary theory after Darwin. But there are no vitalistic processes or principles in Polanyi's understanding of evolution. Polanyi is an *emergentist*, and not a vitalist at all. He does not assume or claim there are any other substances—such as some kind of Bergsonian "élan vital"—over and above the physical. He speaks about only the higher, emergent levels of life and an emergent ordering principle (actually, Polanyi posits two such ordering principles, the ordering principle of life's origins and the ordering principle of evolution). That is, the richness of life cannot be reduced to simple mechanical and physical processes of natural selection and genetics as the neo-Darwinians do and as is accepted without criticism by Gulick (and Grene). The whole notion of emergence from John Stuart Mill and the British Emergentists (Samuel Alexander, Lloyd Morgan, C. D. Broad) is designed to provide a *non-vitalistic* alternative to materialism. Polanyi's account of evolution does not deny the Darwinian theory of natural selection as the true vitalistic thinkers do. However, he does, more limitedly, claim that natural selection and genetics are not sufficient to explain the development of living beings and the rise of man. More is needed, Polanyi argues, and that more is an emergent ordering principle of evolution.

Why does Gulick claim (like Grene, Clayton⁴ and others) that there are vitalistic processes and principles in Polanyi's understanding of evolution? It is not because Polanyi is incoherent as an emergentist. Instead, I believe that the answer lies in the neo-Darwinian dogma that teleology is something monstrous and is the unambiguous sign of vitalism. The famous neo-Darwinist Ernst Mayr makes such a claim.⁵ It is,

of course, possible to equate, in principle, teleology and vitalism, but Polanyi, like Aristotle, does not make this equation. Thus, to properly understand Polanyi's account of evolution one must start with his non-vitalist notion of teleology.

In my discussion below, I first show that Darwinian natural selection itself is also necessarily teleological, however, of course, not in a vitalistic sense but in a Polanyian sense, so, teleology is not something monstrous at all but a necessary notion for Darwinism as well. I then ask why Darwinian evolution does not acknowledge that it is teleological. This is the heart of Polanyi's critique of the notion of Darwinian evolution. I outline Polanyi's solution to the problem of evolution: he posits an emergent ordering principle of evolution. I show how the multileveled hierarchical structure of life (*pace* Gulick) can be "reduced" to a simple opposition between the laws of physics and chemistry and an emergent ordering principle without threatening its hierarchical richness. I conclude, finally, that in the future I hope Polanyi will be recognized as a coherent emergentist without vitalistic flaws.

1. The Treacherous Concept of Natural Selection

What is natural selection? Historically, natural selection is a mechanical explanation accounting for the evolving of new species, just as Newton's theory of gravity is a mechanical explanation for the revolution of planets. Newton's account of gravitational force widened the mechanical worldview in his field and so did Darwin's account of natural selection. At first, Newton was accused of adding an occult force into the mechanical philosophy from which a kind of teleological process followed. The accusation had merit because, in a very new sense, the Newtonian gravitational force was an end-directed process (i.e., attracting objects towards the gravitational centre—see the letters of Bentley and Newton in the Turnbull collection of correspondence as well as Thomas Kuhn's discussion, which charts the slow acceptance of the idea that gravity was innate⁶). End-directed processes were in another sense part of Aristotelian philosophy which, of course, was not at that time regarded as incorporating a proper mechanical process. However Newton's views were later accepted as a non-teleological, material account. At first Darwin was accused of offering a teleological account also, an accusation to which he responded with a comparison with the case of Newton: "It has been said that I speak of natural selection as an active power or Deity; but who objects to an author speaking of the attraction of gravity as ruling the movements of the planets?"⁷ Darwin's views later were also accepted as a non-teleological, material account describing a mechanical process. The term "teleology" came to be used only to describe *vitalistic* end-directed processes. However, for Polanyi, this restriction is not acceptable because these accusations against Newton and Darwin have merit. Before turning to this matter, however, I must clarify what natural selection actually is.

There are two necessary conditions for the process of natural selection, according to Darwin. First there must be *variants* and second, there must be *insufficient means* for living. When there are insufficient means for living, the variants must compete and an evolving process starts. It is an evolving process in which the quasi-permanent environmental resources (sunlight, nutrition, etc.) *determine* the competing processes between the variants. So, within a required interval, when the environmental factors are not occasional but are relatively constant, natural selection, on its own, moves toward an environmentally determined end; that is, in a sense, it is necessarily teleological, as the famous neo-Darwinist Francisco Ayala⁸ makes clear when he says "...the complicated anatomy of the eye like the precise functioning of a kidney are the result of a nonrandom

process—natural selection” (Ayala 35). It *must* be teleological, otherwise it could *not* serve as an explanation for any purposeful things. Darwin himself emphasizes over and over this kind of teleological feature of natural selection when, in connection with his several examples, he talks about how different species, organs and ecological systems are shaped in a specific, directed way, according to given environmental relations (e.g., Darwin 64, 165, 349-50, 401). But, of course, this sense of teleology about which Ayala and Darwin talk is *not* the same sense of teleology found in truly vitalistic philosophies.

Ayala rightly emphasizes that “the over-all process of evolution cannot be said to be teleological in the sense of proceeding towards certain specified goals” (Ayala 42). Why can the overall process not be described as teleological if natural selection is the only fundamental mechanism of Darwinian evolution? The answer is in the nature of natural selection. The second necessary condition of selection, insufficient environmental resources, is, in the long-run case of the over-all evolutionary process, not constant. The continents are moving, the climate is changing, etc. There is an inevitable changing dynamic in the overall process.

Thus natural selection *locally* is necessarily teleological but *globally* it is not. The overall process of evolution is not teleological and thus from the operation of natural selection nothing necessarily follows. As Darwin describes natural selection,

In some cases variations or individual differences of a favourable nature may never have arisen for natural selection to act on and accumulate. In no case, probably, has time sufficed for the utmost possible amount of development. In some few cases there has been what we must call retrogression of organisation. But the main cause lies in the fact that under very simple conditions of life a high organisation would be of no service—possibly would be of actual disservice, as being of a more delicate nature, and more liable to be put out of order and injured (Darwin 99-100).

Darwin himself did not use the word “evolution” in 1859. He was clearly speaking about something else, as his book’s title indicates: *The Origin of Species by Means of Natural Selection, or the Preservation of Favoured Races in the Struggle for Life*.

So, if there is only natural selection as a fundamental principle of an overall process, there is no evolution, only the continuous changing and struggling of species. This is the conclusion one must draw from the neo-Darwinian account of evolution solely in terms of natural selection. It leaves us with the patently absurd view that there is no evolution, but only random *change*. Such change is explained in detail, of course, and in a very scientific (mechanistic) and explicit way. But for Polanyi this account ignores the obvious (in a move to preserve a mechanistic perspective) and is simply insufficient. For him, there *is* apparent non-random evolution from the prokaryotes to vertebrates to human beings. The question only is by what principle.

2. The Ordering Principle of Evolution

What is Polanyi’s main argument against neo-Darwinian theory? A closer look at his argument will better clarify his understanding of teleology and evolution. Polanyi claims, “Darwinism has diverted attention for a century from the descent of man by investigating the *conditions* of evolution and overlooking its *action*. Evolution can be understood only as a feat of emergence” (PK 390).

Contrary to Gulick, who quotes this passage, the passage is not “an epistemological insight” which Polanyi “gained by adding an active component to Gestalt theory” and then “extrapolates to the theory of evolution” (Gulick 48). Polanyi in this passage focuses on ontology, the hierarchical ontology of beings created by evolutionary emergence. Polanyi describes living beings (as well as artifacts like machines) in terms of hierarchical levels which impose or are instantiated within boundary conditions; each higher level configuration operates within boundaries or boundary conditions left open (i.e., possibilities) by the lower level. Each higher level is harnessing and controlling the lower level(s).⁹

Higher level and lower level processes are logically independent. Neither can be deduced from the other. Otherwise, the lower—more fundamental—level processes wholly determine structure or organization and no higher level principles operates independently, thus controlling the possibilities left open by lower level processes. However, since the correlation of the two levels is random, this means that the higher level processes can come into play and harness lower level processes. For example, in a prokaryote cell, the elementary physical and chemical processes are harnessed by a higher level of organization. Thus two essentially different principles are operating, according to Polanyi, and the higher level is not determined by the lower. As discrete principles that are logically independent, one does not descend directly or necessarily from another, but emerges as a possibility of stable order that becomes real. From this, Polanyi concludes, “Thus the logical structure of the hierarchy implies that a higher level can come into existence only through a process not manifest in the lower level, a process which thus qualifies as an emergence” (*TD* 45).

An ordering principle of evolution operating above the laws of physics and chemistry is a necessary condition, because higher levels cannot otherwise come into existence from lower levels. It is simply logically impossible, according to Polanyi. Thus, the (non-teleological) mechanical processes and principles of the fundamental material level cannot explain the hierarchical and purposeful richness of life; a higher level emergent principle is needed which directs the teleological maturation processes.

Gulick asserts that “The flow of Part IV of PK could be purged of its teleology and progressivism without damaging its vision of an increasingly complex world of biologically-based achievement...” (Gulick 49). For Polanyi, however, the teleology of the last Part of PK is the necessary consequence of the ordering principle operating above the laws of physics and chemistry which does not “violate” or “damage” his vision of “an increasingly complex world of biologically-based achievement” (of emergent levels). On the contrary, it is the necessary condition and source of it.

Polanyi’s comment in *Personal Knowledge* on randomness is important: “Randomness alone can never produce a significant pattern, for it consists in the absence of any such pattern” (*PK* 37). As noted in the previous section, the overall process of neo-Darwinian “evolution” is a random changing process, because in the long run, the two conditions of its only fundamental principle (variants and insufficient environmental resources) are random and are determined by the occasional processes and principles of the fundamental material level (mutations and sunlight, continental drift, climate change, etc.). Polanyi argues that “random impacts can *release* the functions of an ordering principle and suitable physico-chemical conditions can *sustain* its continued operation; but the *action* which *generates* the embodiment of a novel ordering principle always lies in this principle itself” (*PK* 401).¹⁰

For Polanyi, Darwinian natural selection is the fundamental mechanical and material process of evolution, a necessary *condition* for evolution, but “evolution can be understood only as a feat of emergence,”

that is, as an *action* of a higher level ordering principle. This action has to be teleological because it is not a random mechanical process but a culminating development which creates purposeful, sentient beings, that is, higher level emergent principles harnessing the lower levels.

So, for Polanyi there is a simple *dichotomy* between *random* and *teleological* processes. From random processes such as the mechanical process of overall “evolution” no development follows but only *change* follows. Whatever development follows only from a teleological process. There is always necessarily present/involved in development a higher level ordering principle. This ordering principle, however, can be construed as a *vitalistic* force or substance like Bergson’s *élan vital*, or as an *emergent* principle like Polanyi, or as Aristotle’s *form*, or as something else. Presumably, there are several logical possibilities. But with different ordering principles, the development—that is, the teleological process—will be *essentially* different. Thus to conclude there is unacceptable vitalism in Polanyi’s teleology is, I believe, a mistake. I am not suggesting that it is impossible to argue against Polanyi’s understanding of evolution and teleology, but that to do so one has to focus not on vitalism and teleology in a narrow sense but one must argue against his concept of boundary conditions, emergence, and teleology and their logical consequences.

What in the final analysis is the ordering principle of evolution? Polanyi carefully addresses this question when he says, “And evolution, like life itself, will then be said to have been *originated* by the *action* of an ordering principle...The *ordering principle* which *originated* life [and evolution] is the *potentiality* of a stable open system...” (PK 383-4). This means that the laws of physics make possible that a random configuration of matter is given at the beginning, a configuration which can function as a basis for a stable open *evolutionary system*—like the Earth—where the developmental process of emergent evolution can be started. Although Polanyi’s theory is not extensively elaborated at this point, he makes clear that his approach to evolution is not in the least vitalist but unambiguously *system theoretical*. Polanyi’s views already are not too far from what Gulick calls for using complexity theory. But Polanyi’s views entail no teleology conceived in the vitalistic sense as Gulick understands teleology.

3. Back to the Past: It is not a Reduction

Gulick, like Grene, suggests that Polanyi misses the mark when he argues for “some additional principle to explain how emergence produces more complex biotic levels” (Gulick 47). Further Gulick contends “the principles guiding the maturation of an individual from an embryonic state to adulthood are not the same as the principles guiding the development of a species as it adapts to changing environmental conditions” (Gulick 48). Thus Polanyi “by using the same notions of maturation and achievement to describe two different levels of reality... violates his ontological insight that different levels operate according to different principles and are not reducible one to another” (Gulick 48). Polanyi thus uses the same principles to describe phylogenetic and ontogenetic emergence and these two processes are different levels of reality (Gulick 48). I am not sure on what ground Gulick makes this latter claim, but it is important to emphasize that the phylogenetic and ontogenetic are not two different levels of reality in Polanyi’s use of the notion of levels. The different levels of reality for Polanyi are elements of the hierarchical emergent structure of living beings. Clearly, some living beings have a complex ascending series of such cooperating boundary conditions. A prokaryote has fewer such levels than man; this is the reason, according to Polanyi, why man is unquestionably more developed. And this is also a reason why evolution is a feat of emergence and not only a neo-Darwinian changing process where the difference between prokaryotes and man is explained only in material complexity. (If there are no levels,

why are human cells more developed or more complex at the same material level than other being's cells?) Ontogenetic evolution is the temporal developmental process of a given individual being while phylogenetic evolution is the *temporal* developmental process of the hierarchical emergent structure of all living beings (life). Human beings are the multi-level creatures whose highest levels of achievement (i.e., the levels of speech, culture, and science) situates them for now at the end of ontogenetic evolution (but only for now--no one knows the future).

Since it is a finite temporal process, one can ask what was before this evolutionary process. Naturally, it is logically *necessary* that *before* the process the achievements of the process did *not* exist. So, there were no prokaryotes, no men, no single living beings, and, of course, no deity or no "pre-existing design" (Gulick 49). That is, there were *not* any higher emergent levels of reality but only what Polanyi calls "primordial inanimate matter" (PK 404). But also logically necessary before the evolutionary process, beyond the principles of "primordial inanimate matter" (laws of physics), there was a higher level ordering principle of evolution which made the emergent evolutionary development *possible*. This is the case, according to Polanyi, because higher emergent levels and principles cannot emerge from "primordial inanimate matter" and by the principles of fundamental physics. This is logically impossible in Polanyi's view. So, there *had to be* at least one higher level ordering principle before the first living beings as well.

However, this travel back into the past is not a reduction of the higher emergent levels of living beings to the fundamental inanimate physical substance at all. Firstly, it is not a non-temporal (synchronic) process as generally reductions are (e.g. thermodynamics to statistical physics or mind to brain processes, etc.). Secondly, it is not an explicit process as is required from reductions, but rather a tacit one. Finally, and this is my most important point, it "reduces" the living things not only into a fundamental physical substance, but *also* into a *higher level* ordering principle, which is fairly strange for a reduction.¹¹ So this travel back into the past does not "violate his [Polanyi's] ontological insight that different levels operate according to different principles" (Gulick 48). He simply provides a logically necessary explanation for the ordering principle of evolution, showing how evolution could produce the rich, multileveled hierarchical structures of living beings. According to his account, Polanyi also claims that "evolution, like life itself, will then be said to have been *originated* by the *action* of an ordering principle..." (PK 384). Unfortunately, Polanyi so little elaborated on this point that it is hard to tell what he was implying when he wrote this.¹²

Conclusion

"The rise of man can be accounted for only by other principles than those known today to physics and chemistry. If this be vitalism, then vitalism is mere common sense, which can be ignored only by a truculently bigoted mechanistic outlook..." (PK 390). As Polanyi wryly notes here, he is not acknowledging that he is a vitalist, but that he dislikes the continuous misinterpretation of his intentions. He is *not* a vitalist because he is an *emergentist*. But materialists and neo-Darwinists always confuse these two essentially different philosophical notions, and, unfortunately, Gulick does too. Polanyi claims that "all physiology is teleological" (PK 360), but he understands teleology not in the least in a vitalist sense but in an emergentist way. According to him, if someone accepts the rich, multileveled hierarchy of life, as Gulick does, she must accept the teleological nature of life and evolution too, as well as the logical necessity for a higher level emergent ordering principle at the beginning.

Endnotes

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²“That ‘Treacherous Footnote’: Assessing Grene’s Critique of Polanyi,” *TAD* 37:2 (2010-2011): 45-57. This essay is cited hereafter as Gulick by page number in parenthesis.

³For a similar discussion, see Gulick’s earlier essay “Polanyi on Teleology: A Response to John Apczynski and Richard Gelwick,” *Zygon* 40:1 (2005): 89-96.

⁴Philip Clayton, “Emergence, Supervenience, and Personal Knowledge,” *TAD* 29:3 (2002-2003): 8-19 and Philip Clayton, *Mind and Emergence. From Quantum to Consciousness* (Oxford: Oxford University Press, 2004).

⁵Ernst Mayr, *Systematics and the Origin of Species* (New York: Columbia University Press, 1942) and Ernst Mayr, *One Long Argument: Charles Darwin and the Genesis of Modern Evolutionary Thought* (Cambridge, MA: Harvard University Press, 1991).

⁶*The Correspondence of Isaac Newton*, H. W. Turnbull (ed.), Vol. III, 1688-1694 (London: Cambridge University Press, 1961): 233-256. This correspondence between the cleric Bentley (whose sermons attacking atheism were about to be published and who therefore consulted Newton to see that he was properly interpreting Newton) and Newton makes clear Newton’s theism and his skepticism about the hypothesis of innate gravity.: See also Thomas Kuhn’s discussion (*The Structure of Scientific Revolutions* [Chicago, London: The University of Chicago Press, 1996]) where he comments: “Gravity, interpreted as an innate attraction between every pair of particles of matter, was an occult quality in the same sense as the scholastics’ ‘tendency to fall’ had been. Therefore, while the standards of corpuscularism remained in effect, the search for a mechanical explanation of gravity was one of the most challenging problems for those who accepted the *Principia* as a paradigm. Newton devoted much attention to it and so did many of his eighteenth-century successors. The only apparent option was to reject Newton’s theory for its failure to explain gravity, and that alternative, too, was widely adopted. Yet neither of these views ultimately triumphed. Unable either to practice science without the *Principia* or to make that work conform to the corpuscular standards of the seventeenth century, scientists gradually accepted the view that gravity was innate” (105).

⁷Charles Darwin, *The Origin of Species by Means of Natural Selection, or the Preservation of Favoured Races in the Struggle for Life* (London: John Murray, 1872): 63.

⁸Francisco J. Ayala, “Teleological Explanations in Evolutionary Biology” in *Nature’s Purposes: Analyses of Function and Design in Biology*, eds. Colin Allen, Marc Bekoff, George Lauder (Cambridge, MA: MIT Press, 1998): 29-49.

⁹Two late Polanyi essays (originally published in 1967 and 1968) that are available in anthologies of Polanyi material provide his best account of the operation of boundary conditions: “Life’s Irreducible Structure” (KB, 225-239) and “Life Transcending Physics and Chemistry” (SEP, 283-299). I cannot here elaborate in detail Polanyi’s view but I recommend these important essays since I believe they have not been appropriately recognized as providing the key to Polanyi’s criticism of Neo-Darwinism and his alternative account in terms of emergence.

¹⁰In this approach, the randomness is clearly *only a correlation* between levels. The lower or the higher levels can be regarded as random if *exclusively put in correlation with another level*—in this case with each other—but *not if by itself*. “By saying a factor is *random*, I do not refer to what the factor is in itself, but to the relation it has with the main system” (W. Ron. Ashby, *An Introduction to Cybernetics* (London: Chapman

& Hall LTD, 1957), 259. It follows from this that *the higher level can never be a random consequence of the lower* because, given this, it ought to itself be random too, yet this is not the case. In itself it is entirely deterministic and it *has to be* so, otherwise it could not have meaning, it could not be purposeful and it could neither control nor harness lower level processes. The randomness is only a correlation of the two different structures and the principles governing these levels. If their correlation is not random, it means that the higher level *depends* entirely on the lower; thereby, there is no essential difference between them, and they are determined by *one* – lower level – principle.

¹¹Contrary to all of this, I believe it is sensible to call this process a new kind of reduction, *diachronic* reduction. See Daniel Paksi, “Emergence and Reduction in the Philosophy of Michael Polanyi, Part I” *Appraisal* 8:2 (2010b): 34-41, and “Emergence and Reduction in the Philosophy of Michael Polanyi, Part II” *Appraisal* 8:4 (Oct, 2011): 28-42.

¹²Elsewhere I have tried to sketch a possible interpretation of this passage. See Daniel Paksi, “Polanyi and Evolution,” *Knowing and Being, Perspectives on the Philosophy of Michael Polanyi*, Tihamer Margitay (ed.) (Newcastle on Tyne: Cambridge Scholars Publishing, 2010): 151-172.

(continued from p. 39)

Notes on Contributors

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Daniel Paksi (daniel.paksi@filozofia.bme.hu) recently received his Ph.D. from Budapest University of Technology and Economics where he studied Polanyi in the Department of Philosophy and the History of Science. His articles in English include “Polanyi and Evolution” in the new collection of essays titled *Knowing and Being: Perspectives on the Philosophy of Michael Polanyi* and “Emergence and Reduction in the Philosophy of Michael Polanyi, Part I” *Appraisal* 8:2 (2010b): 34-41, and “Emergence and Reduction in the Philosophy of Michael Polanyi, Part II” *Appraisal* (forthcoming).

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On the Adequacy of Neo-Darwinism: A Reply to Daniel Paksi

Walter B. Gulick

ABSTRACT Key Words: Polanyi, evolution, neo-Darwinianism, gestalt theory, randomness, significant order, emergence.

In this article I respond to Daniel Paksi's attempt to justify Michael Polanyi's claim that neo-Darwinian thought is inadequate to account for the emergence of life itself and increasingly complex forms of life because it lacks an ordering principle (which for Polanyi is the potentiality of a stable open system). I argue that presupposed in any dynamic system, of which neo-Darwinianism is an example, is some notion of stable self-organization, that Polanyi's distinction between randomness and significant order introduces an inappropriate anthropocentric bias into evolutionary theory, that neo-Darwinian thought has within itself ample resources to account for the emergence of life in all its richness, and that therefore Polanyi's (and Paksi's) critique of neo-Darwinian theory falters.

It is gratifying to have one's article taken seriously enough that a reader is willing to spend time writing a response to it. So I thank Daniel Paksi for his response. As the title of his article suggests, he defends Polanyi's theory of evolution against what he thinks is my mistaken critique. However, in my view his response highlights what is problematic in the account of anthropogenesis Polanyi gives in Part IV of *Personal Knowledge*. I feel that Polanyi's attack on neo-Darwinism is the weakest aspect of one of the 20th century's greatest works of philosophy.

The core of Paksi's critique of my views seems to center on four charges: that I (and neo-Darwinians) have a materialistic view of evolution, that I (and neo-Darwinians) deny any form of teleology and have a deterministic view, that I think Polanyi is a vitalist, and that I have an inadequate understanding of the role of emergence in Polanyi's thought. In the following discussion I shall try briefly to highlight what I see as problematic about Polanyi's (and Paksi's) criticism of neo-Darwinian thought and why the appeal to the suggested ordering principle of evolution is unhelpful and misleading. As my account unfolds, I hope it will become evident why I believe that each of Paksi's four charges is off target.

Polanyi and Paksi suggest neo-Darwinian thought postulates a mechanical system that can explain random change but not the significant change that produces emergent phenomena, and in particular, life. Here I want to point out what I think is a basic flaw in Polanyi's argumentation, a flaw not specifically pointed out elsewhere so far as I am aware. The problem centers on his notion of what is significant in "significant order" and the nature of "randomness" and "accident." It extends further to an inconsistency in his use of Gestalt theory.

The chief function of Chapter 3, "Order," in PK is to underscore the ability of humans to recognize significant order. Polanyi uses an example of white pebbles arranged in words to greet people arriving in Wales (PK 33-34), in contrast to these pebbles scattered after they had been ignored for many years. He accredits our capacity to recognize significant order in the first case and not in the second. But the significance of the spelled out rather than the scattered pebbles prioritizes as significant that which is explicated in language, that which has become explicit and focal. Why is the particular arrangement in the second case not seen as resulting from

another, perhaps more tacit, sort of significant order, the complex ordering effect of rain, erosion, the playful use of pebbles by children, wind, etc.? More generally, by limiting the significant to what is directly perceived as relevant to a line of inquiry governed by some human interest, doesn't Polanyi incidentally disestablish the associated tacit background information, which he assigns to the insignificant realm of the random and the accidental? I believe this is the case, and this step has deleterious consequences for his assessment of neo-Darwinism. What is of interest to him is the rise of life followed by the development of new species and ultimately the rise of homo sapiens; these are classed as significant, while the contextual circumstances and laws that in fact make possible these events are considered, as being merely random, not to have sufficient power to explain biological emergence.

The negative consequences of his view for neo-Darwinian thought are apparent when he denies that "the different living species [could] have come into existence by accidental mutations" (PK 35). Mutations in DNA may not be predictable via our present research capabilities, but presumably they are causal events arising in highly unusual circumstances. When Polanyi states that "no highly significant order can ever be said to be solely due to an accidental collocation of atoms," and then concludes "that the assumption of an accidental formation of the living species is a logical muddle" (PK 35), one must challenge the way he uses "accidental." Again, presumably the circumstances within which life first developed were highly organized at a certain chemical level (perhaps a concentrated mass of organic molecules struck by lightning, but at present we can only speculate). To call these circumstances "accidental" is to disqualify them because we currently have no explicit, verified understanding of the process, no comprehension of the *intrinsic* ordering principles (PK 39) involved. In claiming that the emergence of life and of new species is significant while implying that other changes (supernovae, transition of solid to liquid, etc.) are only random, Polanyi is imposing a bias upon natural processes that seems to shift the conversation away from scientific explanation toward anthropocentric meaning. An implication of this shift is that he no longer treats neo-Darwinian thought as a scientific theory in which different forces and different materials at different levels impact each other in equally valued ways such as is emphasized in ecology. Instead he narrows the examination of evolution to value as significant only those principles pertaining to "the rise of man" while regarding other evidence as random or accidental.

In his further discussion of order in the same chapter, Polanyi uses Gestalt language to distinguish between figure and background in acts of visual perception. "No feature of the background may be linked in an orderly manner to the figure. Hence all relations of the background features to the figure must be random, and this will be best safeguarded if the background is random in itself" (PK 38). Gestalt theory postulates that color, line, pattern and other figural elements are fused into perceptual recognition through an act of equilibration. In his theory of knowing, however, Polanyi adds an active element to Gestalt perceiving in order to distinguish between illusion and knowing (PK 342). An effort must be made by a subject to achieve rightness of perception. By including background knowledge and skill that enables a person to distinguish right from wrong, and by distinguishing between the subsidiary and the focal, Polanyi moves decisively beyond Gestalt equilibration and the significant/random dichotomy. That is, Polanyi emphasizes that the background subsidiaries in which we dwell and which we integrate to form focal meanings are much more than random. Through his theory of tacit knowing, he acknowledges that our indwelt background influences include the learned and the innate, the social and the skillful. These attuned tacit elements contribute in essential ways to the formation of our explicit knowledge. I believe the figure-background theory and its associated notions of what is random and accidental ought to be viewed as an unproductive line of Polanyi's thought, one to be replaced by his powerful theory of tacit knowing. An implication is that his critique of neo-Darwinian thought, based as it is on the significant/random and background/figure distinctions, should also be superseded.

Paksi claims that Darwinian natural selection is based upon two conditions: the existence of variants and insufficient means for living. But the neo-Darwinism that Polanyi felt to be insufficient contains much richer resources today than just these two conditions. I'll briefly describe five additional factors within neo-Darwinian thought that can contribute to evolutionary development and the emergent rise of new species: mutations, genetic drift, exaptation, migration and species isolation, and dynamic species-environmental interaction.

Mutations in germ line cells are heritable, and the resulting genetic changes can be harmful, neutral or beneficial to a species. When beneficial, animals (and analogously, plants) with the changed genetic makeup are liable to thrive, and their altered configuration of DNA may give them traits such that mating with their ancestor species is unattractive or infertile. The resulting speciation is not just change, but results in the *emergence* of a separate species. Genetic drift does not produce new alleles like mutations do, but bring about random change in allele frequencies that may affect the ability of a species to survive in the long run. Exaptation involves the hijacking of a trait that had survival value in one setting for a novel use. This notion (developed especially by Stephen Jay Gould after Polanyi wrote) helps explain how something like bird feathers and then wings might have developed through long states of nonfunctionality; feathers originally helped dinosaurs insulate themselves against cold weather. It is consistent with Polanyi's understanding of emergence to see birds as an emergent development beyond dinosaurs, not simply a random change. Further, when a population migrates and becomes adjusted to environmental conditions not experienced by its ancestors, genetic changes fostered by natural selection occur over time. This process, which Darwin explored in the Galapagos, may lead to the rise of new species.

The last factor of neo-Darwinian theory I alluded to, dynamic species-environmental interaction, deserves a somewhat more extended treatment, because it illuminates especially well what is problematic about Polanyi's interpretation of evolution. Polanyi speaks of a phylogenetic field that guides evolutionary development as a process of "maturation along the gradients of phylogenetic achievement" (PK 400). In essence, this claim treats species development as a pre-ordained achievement comparable to the gene-guided maturation of an individual living being, and it completely ignores the role of the unpredictable changing environmental conditions to which species must adjust. The concept of an unfolding field inappropriately imposes upon the evolutionary process as a whole a telic aspect. Paksi is right to claim that natural selection has a teleological element locally (the behavior of individual plants and animals is oriented toward survival as an end). He understands that the global process has no teleological gradient toward some specific achievement. However, Paksi is wrong to think this implies that natural selection cannot contribute to evolutionary emergence. Natural selection in concert with mutations and the other factors I have mentioned are collectively the basis of evolutionary advance, not natural selection plus an ordering principle alone.

Paksi has grounds for questioning my calling the ontogenetic and phylogenetic different "levels" of reality, but the point I am making about their fundamental difference still stands. The difference between the ontogenetic and phylogenetic is not only a matter of distinct processes. They are also distinct in terms of number, scope, and context. The differences number can make can be seen when comparing the properties of an atom of water with the properties of the trillions of atoms in, say a pond. An atom of water cannot form ice, cannot dissolve entities, etc. Likewise, the maturation of an individual animal (ontogenesis) has no necessary predictive relevance to the fate of its species (phylogenesis). In sum, Polanyi's attempt to model phylogenetic emergence on ontogenetic maturation is not good science and is misleading as an analogy.

Next, let us consider Polanyi's claim that neo-Darwinian thought is incomplete and perhaps even incoherent by itself. He thinks an ordering principle of evolution is needed to supplement neo-Darwinism and account for emergence rather than just random change. I am puzzled as to why Polanyi, who so brilliantly shows how independent actions can lead to spontaneous order, should refer to the adjustments made to an environment as "random" and leading only to change rather than a dynamic order. There is nothing random or accidental about what traits best allow a species to survive in an environmental niche. Nevertheless, what is the nature of the ordering principle he thinks is necessary? Polanyi states, as Paksi notes, "that the *ordering principle* which *originated* life [and sustains evolution] is the *potentiality* of a stable open system" (PK 383-384). Paksi quite appropriately notes that "Polanyi so little elaborated this point it is hard to tell what he was implying when he wrote this." Is this vague point sufficiently important that it justifies denying the adequacy of neo-Darwinian thought?

No, because *any* systemic explanation (like evolution) of how a dynamic process unfolds must assume the process continues until some form of stability or equilibrium is reached. The ordering principle Polanyi sees as needed is suggestive of the self-ordering drive to stability found in complexity theory. There is no single law or principle underlying all the examples to which complexity theory applies; each system achieves self-organization and stability in a way appropriate to the forces, properties, and other components of its "habitat." Polanyi's criticism of neo-Darwinian theory because it lacks a sense of self-organization is superfluous because such a drive is inherent within the assumptions supporting evolution as a systemic theory and inappropriate because neo-Darwinian thought is rich enough to account both for change and emergence.

That Polanyi critiques evolutionary theory because it does not make explicit the non-telic goal of stability it tacitly assumes¹ is both ironic and unfortunate. It is ironic because Polanyi argues that what is missing in evolutionary theory is an *active* principle that is necessary to explain the emergence of novel centers in the course of geological history. "Random impacts can *release* the functions of an ordering principle and suitable physico-chemical conditions can *sustain* its continued operation; but the *action* which *generates* the embodiment of a novel ordering principle always lies in this principle itself" (PK 401). But in explaining the ordering principle as the "*potentiality* of a stable open system" Polanyi both introduces a finalistic character to evolution and offers what seems to be a passive outcome rather than an active force as his explanatory tool.² When an active principle is called for, generally it is assumed that it would be a kind of force, which is what Bergson postulated in his version of vitalism. Now I do not think Polanyi is some kind of vitalist relying on a mysterious active substance a la Bergson, as I have made clear on other occasions.³ I referred to the vitalistic aspect of his thought because *Polanyi describes himself* (in the passage I [and Paksi] quoted from PK 390) *as a vitalist* insofar as he thinks a principle beyond the laws of physics and chemistry is what is necessary to explain evolution. My point is neo-Darwinian thought provides all the principled richness necessary to account for evolutionary emergence and does not deserve censure on this point.

To be sure, the potentiality Polanyi postulates can be seen as a kind of lure comparable to Aristotle's Unmoved Mover, but that still seems different than the actively generating principle he calls for. It would also add a metaphysical element to Darwinian science. Polanyi's proposal is ironic in that his major advance in epistemology/ontology beyond Gestalt theory was to argue for an active quest for contacting and understanding reality rather than settle for the thrust towards equilibrium characteristic of Gestalt theory. Yet the ordering principle he calls for prioritizes equilibrium.

Polanyi's criticism of neo-Darwinian thought is unfortunate not only because it is unnecessary, but also because it apparently gives the support of an eminent scientist to those who wish to support non-scientific

accounts of evolutionary theory, or who wish to replace evolutionary theory with a metaphysical or religious alternative. In the North American context a few years ago, this opening led to the creation of a center that argued for intelligent design rather than evolutionary process and legitimated this center with Polanyi's name.⁴

Finally, I would like to return to an assumption Paksi, following Polanyi, makes which is crucial to his point of view but which I think does not stand up to close inspection. He claims that because the evolutionary process as a whole (specifically, natural selection) is seen in neo-Darwinian thought not to be teleological, that therefore no evolutionary advance is possible. All must remain locked in meaningless transition within the realm of physics and chemistry. But it is precisely to challenge such an argument that the notion of emergence comes into play. Out of chemical processes life emerges in gradually more complex steps. The integration of chemical and biological materials and processes into conscious thought is an example of an ongoing process of emergence employed in writing and reading this article. Elsewhere I have described two genuinely and one inappropriately considered types of emergence and shown how they operate differently in three different ontological orders.⁵ One conclusion that may be drawn from these reflections upon emergence is that various types of emergence are immanent within the very structure of reality. Consequently, one task of the scientist and the philosopher of science is to show how such emergence operates within a domain the scientist studies. Neo-Darwinian theory does just that.

It should now be evident that my argument is much more with Michael Polanyi than with Daniel Paksi. Paksi is generally accurate in expounding Polanyi's thought about neo-Darwinism. In the final analysis, I believe, as I hinted at in the article Paksi critiques, that if Polanyi's theories of tacit knowing and emergence replaced the significant/random contrast and the elements of teleology and progressivism in Polanyi's treatment of evolutionary theory, his philosophy would in fact be consistent with the best neo-Darwinian thought.

Endnotes

¹I use the term "goal" here, but obviously this is not a teleological goal that the non-living world strives for. Paksi acknowledges this in his quote from Ayala 42. Individual living beings, however, are repositories of telic drives, as Paksi also acknowledges, for instance when he quotes Polanyi to the effect that "all physiology is teleological" (PK 360). However, just because beings with drives and purposes can emerge from non-teleological sources does not necessarily imply that the whole world process must therefore be teleological, as Polanyi seems to claim. The fact that the world includes the capacity for producing emergent features can be seen as an aspect of its factuality.

²I elaborate further on this point in "On Structured Societies and Morphogenetic Fields: A Response to Joseph Bracken," *Tradition and Discovery* 31:2 (2004-2005), 34.

³See, for instance, my "Response to Clayton: Taxonomy of the Types and Orders of Emergence," *Tradition and Discovery* 29:3 (2002-2003), 35.

⁴On the home page of the Polanyi web site, see the entry entitled "Information on Polanyi and 'Intelligent Design.'" See also the book by Barbara Forrest and Paul R. Gross, *Creationism's Trojan Horse: The Wedge of Intelligent Design* (New York: Oxford University Press, 2004), which was reviewed by Phil Mullins in *Tradition and Discovery* 32:2 (2005-2006), 52-53. An associated web site, www.creationismstrojanhorse.com contains many further resources, including an essay by Richard Gelwick, "Polanyi Scholarship and the Former Baylor Polanyi Center."

⁵"Response to Clayton," 32-47.

Michael Polanyi and Karl Popper: The Fraying of a Long-Standing Acquaintance

Struan Jacobs and Phil Mullins

ABSTRACT Key Words: Michael Polanyi and Karl Popper, critical rationalism and post-critical philosophy, Popper's open society and Polanyi's dynamic orders.

Based upon archival correspondence and their publications, this essay analyzes the interaction of Karl Popper and Michael Polanyi. Popper sent Polanyi for review in 1932 an early draft of The Logic of Discovery. Friedrich Hayek helped both Polanyi and Popper publish some of their writings in the forties. Polanyi renewed his acquaintance with Popper in the late forties when Popper took a position at the London School of Economics and they met to discuss common interests. In the early fifties, as Polanyi prepared and presented his Gifford Lectures and published The Logic of Liberty, Polanyi became increasingly clear and articulate in distinguishing his social philosophy and philosophy of science from Popper's ideas. Polanyi's 1952 paper "The Stability of Belief" forthrightly presented Polanyi's post-critical ideas that Popper overtly rejected in an important letter. After this, they had little to do with each other.

I. Introduction¹

In the books and essays of Michael Polanyi, there are only a handful of references to Karl Popper; likewise, in Popper's books and essays, there are only a few references to Polanyi. Most of the references in each figure's later writings are pointedly critical, although sometimes veiled and cryptic, as the following examples show. There is an unnamed but unmistakable and acerbic shot at Polanyi at the very end of Popper's "Preface to the English Edition, 1958" of *The Logic of Scientific Discovery*, published in 1959, a year after the publication of PK.² Popper says he holds that only a revival of interest in the

riddle of man's knowledge of the world can save the sciences and philosophy from narrow specialization and from an obscurantist faith in the expert's special skill and in his personal knowledge and authority; a faith that so well fits our 'post-rationalist' and 'post-critical' age, proudly dedicated to the destruction of the tradition of rational philosophy, and of rational thought itself (23).³

Six years after Polanyi's death, in Popper's 1982 Introduction to the 1983 re-publication of *Realism and the Aim of Science*, Polanyi is named and used as a bludgeon to club Thomas Kuhn.:

Kuhn's views on this fundamental question [the nature of truth] seem to me affected by relativism; more specifically, by some form of subjectivism and elitism, as proposed for example, by Polanyi. Kuhn seems to me also affected by Polanyi's fideism: the theory that a scientist must have faith in the theory he proposes (while I think that scientists—like Einstein in 1916 or Bohr in 1913—often realize that they are proposing conjectures that will, sooner or later be superseded) (xxxi-xxxii).

Popper's passing references to Polanyi condemn Kuhn by association. The implication is that Kuhn is a relativist. Popper thinks neither Kuhn nor Polanyi has an account of truth that is adequate. Further, Popper suggests Kuhn is tainted by Polanyi's fideism which Popper implies does not affirm that scientific discoverers recognize their views will be superseded.⁴

Polanyi's few published comments about Popper also tend to be cryptic, critical remarks, although somewhat more diplomatically articulated. Still early in his philosophical career, in his Preface to *The Logic of Liberty* (1951), Polanyi challenges but does not directly name Popper's views in *The Open Society and Its Enemies* (1945):

Freedom of the individual to do as he pleases, so long as he respect the other fellow's right to do likewise, plays only a minor part in this theory of freedom. Private individualism is no important pillar of public liberty. A free society is not an Open Society, but one fully dedicated to a distinctive set of beliefs (vii).

In *The Tacit Dimension* (1966), Polanyi notes that idealized notions about science as dispassionate are currently fashionable, and such idealizations deem "the scientist not only indifferent to the outcome of his surmises but actually seeking their refutation" (78). In a footnote, Polanyi identifies Popper as the figure who has forcefully expressed this view of science, and he quotes from *The Logic of Scientific Discovery* (279) to illustrate this view which Polanyi rejects. In his 1972 essay "Genius in Science,"⁵ Polanyi points out that the "temper of the age . . . prefers a tangible explanation to one relying on more personal powers of the mind" (46). This has led to the presumption that scientific discoveries are tentative hypotheses and that "unless a hypothesis produces testable conclusions it should be disregarded as lacking any substantial significance" (46). In a footnote, Polanyi identifies Popper as the source of the "widely influential" idea that Polanyi calls "the principles of 'refutationalism.'"⁶ Polanyi only indirectly addresses these "principles," by turning to the history of science to show that testing discoveries is often unnecessary and may be impracticable.⁷

These examples suggest that palpable tensions developed between Karl Popper and Michael Polanyi. Their accounts of science were quite different and eventually these figures clearly recognized this. This recognition provides some explanation for their few, pointed and abbreviated late comments about each other, but there seems to be more involved than disagreements about philosophical accounts of science. This essay is an historically oriented effort to sketch what is known about the relations between Popper and Michael Polanyi.⁸ We attempt to illumine the context in which Polanyi and Popper's philosophical differences emerged.

Although Polanyi was eleven years older than Popper and had formative experiences as a research scientist rather than a philosopher, Polanyi and Popper came from the same cultural milieu; both were Jewish émigrés⁹ who eventually work in English academic institutions. They in fact knew each other from the thirties and seem to have cooperated and shared some intellectual interests in certain periods, although their relationship became fraught with tensions in the fifties and sixties. There is a small but interesting collection of letters that Popper and Polanyi wrote to each other in the Hoover Institution's Popper Archives. These letters are supplemented by a few additional letters that Michael's brother Karl and his wife Ilona Polanyi wrote to Popper (also in the Popper Archives). There is also a letter to Popper in the Papers of Michael Polanyi and, in the Papers of Edward Shils, there is some correspondence between Polanyi and Shils that mentions Popper, who was a mutual friend. Together this material, when linked to some publications, provides insights about the relations of Michael Polanyi and Popper. Although the material leaves many questions unanswered, it places

in a living context some of the issues concerning not only science but also social philosophy that separated Popper and Michael Polanyi, suggesting how what seems once to have been a somewhat collegial relationship became strained.¹⁰

II. Early Connections

A. Popper's 1932 Letters to Polanyi

Karl Popper and Michael Polanyi apparently first became acquainted in 1932 when Popper, then a secondary school teacher,¹¹ sent Polanyi, a prominent scientist at the Kaiser Wilhelm Institute for Physical Chemistry,¹² a manuscript to review. There are carbons, in the Popper Archives, of two typed letters in German from September and October of 1932 from Popper to Polanyi.¹³ The September 17, 1932 letter makes clear Popper is writing to Michael Polanyi at the suggestion of Karl Polanyi, who Popper knew in Vienna in the twenties and early thirties. Popper participated in informal seminars in Karl and Ilona Polanyi's apartment in this period, according to Felix Schaffer.¹⁴ Karl and Ilona continued to write Popper (see letters discussed below) after they and Popper left Vienna, and Popper notes in his intellectual autobiography, that Karl Polanyi introduced him, in 1926, to Professor Heinrich Gomperz, the second professional philosopher with whom he became acquainted (Autobiography, 57-58) and with whom he discussed "the psychology of knowledge or discovery," although he notes that his primary interest was in "the logic of discovery" (Autobiography, 59).

Popper's 1932 letters to Michael Polanyi were concerned with his manuscript "Die beiden Grundprobleme der Erkenntnistheorie, I" (The Two Fundamental Problems of Theory of Knowledge, I). Polanyi apparently received the manuscript in June but did not promptly review and return it, and Popper's September 17, 1932 letter requested the return of the manuscript:

Prof. Dr. Michael Polanyi
Berlin Zehlendorf
Waltraudstr. 15

Dear Professor!

On the recommendation of your brother Karl, I sent you in June the manuscript of a work ("The two fundamental problems of theory of knowledge", I). I assume that you have not yet found the time to examine the work more closely. Since I now most urgently need the manuscript, I would respectfully ask that it be returned to me as soon as possible. If, by coincidence, you are just now reading it, I am asking you to notify me and only send the manuscript after your completed evaluation.

I beg you to excuse any inconvenience that I have caused with the sending of the manuscript, and now am continuing to cause with my present request for a return of the manuscript.

With highest respect,

Vienna, 17 September 1932
13, Anton Langergasse 46

Exactly what this manuscript mailed to Polanyi was is not altogether clear. In 1979, a Popper book with this German title was published,¹⁵ and an English translation, *The Two Fundamental Problems of the Theory of Knowledge*¹⁶ finally was published in 2009. Book I of this volume is on induction and Book II on falsifiability. The title of the manuscript noted in his September 1932 letter thus suggests that Popper had sent to Polanyi the material on induction if the early manuscript was organized like the later book. In his intellectual autobiography, Popper says that he completed what he at the time regarded as the first volume “very early in 1932,” and that it was “conceived, from the beginning, largely as a critical discussion and as a correction of the doctrines of the Vienna Circle” (Autobiography, 67).¹⁷ But exactly what Polanyi received to review remains ambiguous. Popper seems to have been extensively revising his manuscript in the period in which Polanyi was mailed a copy. Popper apparently first develops his criticisms of induction and his views about deduction and then puts this together with his ideas about demarcation marked by falsifiability. Hacohen contends that it was not until the spring of 1932 that Popper clearly saw falsifiability as an alternative demarcation criterion. Popper re-wrote some sections of his manuscript in the summer of 1932. Hacohen argues that Popper’s earliest discussions of the limitations of induction as a model for science change as “falsification moved from the margins to the center” as a criterion of demarcation (2000, 198). That is, some sections of the manuscript were rewritten as Popper tried to “create a new framework for a book that had been superseded” (2000, 199).

Why was the manuscript sent to Polanyi? Presumably, Karl Polanyi suggested that Popper send it to his brother the prominent scientist for review since it was concerned with science. The follow-up October 18, 1932 letter from Popper implies that Popper’s motives were likely concerned with getting his work published.

Dear Professor,

With many thanks I confirm the arrival of the manuscript. Recently I wrote to Professor Carnap (Prague) (in a matter that does not pertain to my book), and on that occasion I reminded him of the promised intervention with Frank by dropping the remark that you had recommended to me that I should seek assistance from Frank.

— Since you have tried once before, as you write, to read my work, I send you herewith a succinct statement of the basic ideas (2 ¼ pages). This presentation I wanted to publish as a “letter” in “Naturwissenschaften” (Natural Sciences), but it was rejected (with the remark that it does not fit into the framework of the journal).

I won’t take more of your time, sparing you even short letters.

Thanks again.

Yours truly,

Vienna, 18 October 1932

This second letter confirmed that Popper’s manuscript was returned, and noted that Popper had, in a letter, reminded Rudolf Carnap of Carnap’s promise of assistance with Phillip Frank, a physicist who Polanyi likely knew who was an editor, along with Moritz Schlick, of a series of works by members of the Vienna Circle. Popper reminded Carnap of his promise by noting that Polanyi recommended that Popper seek help from

Frank. The fact that Popper confided this implies that Popper and Polanyi had communicated earlier about how to get Popper's manuscript some attention and into the hands of Frank. The brief second paragraph indicates that, since Polanyi had earlier tried to read Popper's work and had apparently found the manuscript too long and dense, Popper was now enclosing a short summary of his basic ideas. He confided that he had hoped to publish this summary as a letter in a scientific journal but it was rejected as unsuitable. Unfortunately, a copy of the 2 ¼ page summary seems not to be in the Popper Archives collection of materials. But if Polanyi did receive such a short summary (which perhaps reflected recent Popper revisions during the 1932 summer), it seems reasonable to assume that Polanyi does know something quite early about Popper's developing ideas, even if he did not read the manuscript carefully.

Not long after Popper's correspondence with Polanyi, Popper did manage to get his manuscript (or at least part of it) on the road to publication. Popper recalled that his book was "read first by Feigl, and then Carnap, Schlick, Frank, Hahn, Neurath, and other members of the Circle" (Autobiography, 67). It was accepted for publication in 1933 by Schlick and Frank in their series, most of whose books were by members of the Vienna Circle. However, Springer, the publisher, required that Popper's work be "radically shortened" (Autobiography, 67). Popper notes that by the time the book was accepted, he had written most of the second volume and thus "little more than an outline of my work" (Autobiography, 67) could be published in the pages allotted by the publisher. He put forth a new manuscript with the agreement of Frank and Schlick, drawing material from both volumes. What Popper finally published in December of 1934, under the title *Logik der Forschung*, was what he termed "extracts from both volumes" (Autobiography, 67; see also Hacoen, 2000, 188). Popper says his uncle Walter Schiff produced the final text by cutting about half of what was available in order to meet the publisher's strict requirements for length. This publication is what launches Popper's career as a philosopher.¹⁸

B. Popper, Polanyi, and F. H. Hayek

As the Nazis' program began to unfold, Michael Polanyi left the prestigious Kaiser Wilhelm Institute for Physical Chemistry in Berlin in 1933 for a faculty appointment at the University of Manchester. Although Polanyi continued to hold this appointment in chemistry and continued to do outstanding research for another fifteen years, his interests became broader and his research and writing was not only in chemistry after moving to Manchester¹⁹ (Scott and Moleski, 120, 139, 152, 154-155; Nye, 2011: 145-181). In fact his interests were already expanding before he left Berlin. On several scientific trips to the Soviet Union, Polanyi took a keen interest in the economic changes emerging there in the twenties and early thirties. Both in Berlin and in Manchester, Polanyi was in conversation with economists. In 1935, he published *USSR Economics—Fundamental Data, System and Spirit*, "which gave one of the earliest accounts . . . of Soviet production and consumption figures, of government regulation, and of the basis of the Communists' appeal to the public" (Scott and Moleski, 160). In 1936, Polanyi carefully studied Keynes' *The General Theory of Employment* and came to think of himself as a qualified Keynesian. He became very interested in economics education in the late thirties and early forties (Scott and Moleski, 165-167). His interest in economics and the Soviet Union was also linked to his interest in the plight of persecuted Soviet scientists.

In 1935-36, Popper, while on a leave of absence from his teaching position in Vienna, made two long visits in England; his *Logik der Forschung* (1934) was well reviewed in England and this brought opportunities to lecture in London and Cambridge (Autobiography, 85-86). There is no evidence that Popper had any contact with Michael Polanyi in these early visits to England. Popper does, however, report that on

one of these visits he read a paper titled “The Poverty of Historicism” in a Hayek seminar at the London School of Economics (Autobiography, 90). Hayek seems to have become an important Popper friend and ally thereafter.²⁰ In March, 1937, Popper accepted a lectureship in Canterbury University College, Christchurch, New Zealand (Autobiography, 87-88). Popper speculates that Hayek may in part have been responsible for his New Zealand appointment (Autobiography, 87). Hacohen suggests that Karl Polanyi helped Popper with this appointment, since Karl Polanyi’s friend John Macmurray chaired the appointment committee; Karl Polanyi tried “to facilitate his [Popper’s] move by providing contacts with previous New Zealanders” (2000, 120).

Popper also acknowledges that Hayek (although Popper notes he did not yet know him well enough to ask), along with his friend Ernst Gombrich, helped find a publisher for his *The Open Society and Its Enemies* (1945),²¹ which he completed in New Zealand in February, 1943 (Autobiography, 95). This book, Popper says, developed out of an elaboration of one section of the expanded poverty of historicism material whose germ was given as a paper in 1936 in Hayek’s seminar. As noted above, the poverty of historicism material itself eventually became three articles published by Hayek in *Economica* in 1944 and 1945, after it was earlier rejected by *Mind* (Autobiography, 90-95). *The Poverty of Historicism* appeared as a book in 1957.²² About both the open society and poverty of historicism material, Popper says, they

were my war effort. I thought that freedom might become a central problem again, especially under the renewed influence of Marxism and the idea of large-scale “planning” (or “dirigisme”); and so these books were meant as a defence of freedom against totalitarian and authoritarian ideas, and as a warning against the dangers of historicist superstitions. Both books, and especially *The Open Society* (no doubt the more important one), may be described as books on the philosophy of politics (Autobiography, 91).

In sum, Popper’s early social and political philosophy is developed in the thirties and forties and is published with the help of Hayek. It turns out, as we describe below, that this political philosophy, which is developed before and during the war, Polanyi comes to think is at odds with his own political philosophy developed in the same period, again with some encouragement from Hayek, and this difference in political philosophy is in fact entangled with the philosophical differences about science that are articulated in the early fifties.

In January 1946, Popper returned to England to a position as Reader in Logic and Scientific Method at the London School of Economics. Hayek played an important role in arranging this appointment and in arranging for Popper to get out of New Zealand just after the war. Popper describes his job offer as coming in a cable (apparently received in late 1945) signed by Hayek who also thanked him for offering the articles recently published in three installments in *Economica* (Autobiography, 96). About Hayek, Popper commented in his autobiographical reflections on this period, “I felt that Hayek had saved my life once more” (Autobiography, 96).²³

In the late thirties, Polanyi, like Popper, became a friend of Friedrich Hayek and they cooperated on several projects thereafter. Polanyi met Hayek in August, 1938 at a Paris conference honoring Walter Lippmann’s recent book *An Inquiry into the Principles of the Good Society*, which criticized collectivism, planning, and totalitarianism.²⁴ Hayek saw Polanyi’s film “An Outline of the Working of Money” at this conference (Scott and Moleski, 167-168). Hayek seems subsequently to have become very interested in Polanyi’s work analyzing the Soviet economy and his outspoken criticism of the British “planned” science movement. As Polanyi’s lectures and publications from the late thirties make very clear, although Polanyi was a new British citizen, he

quickly took on a role as a scientist opposed to the popular movement to centralize science. Hayek reviewed Polanyi's 1940 book *The Contempt of Freedom* in the May, 1941 issue of *Economica* (New Series, 8:30: 211-214). The book is primarily a compilation of Polanyi's writing from the previous five years and it incorporates the material in Polanyi's *USSR Economics—Fundamental Data, System and Spirit* (1935) as well as Polanyi essays, a lecture and review articles resisting Marxist-influenced views of science.

Correspondence between Polanyi and Hayek in 1941 led to the publication in November, 1941 in *Economica* of what is perhaps Polanyi's most important early essay, "The Growth of Thought in Society," articulating his liberal political philosophy. This essay is a lengthy review article attacking a new publication, J. G. Crowther's 1941 book *Social Relations of Science*, promoting "planned" science. The correspondence with Hayek and the references in Polanyi's article to other literature on "planned" science suggests that Polanyi was eager to write more than a simple review of the new Crowther book.²⁵ Polanyi's own ideas about science and society and particularly some of his ideas about freedom are jelling in this period. His essay offers criticisms of "planned" science as well as fascism and communism, but these are presented in the larger context of articulating Polanyi's constructive vision of a liberal society.

Polanyi's philosophical account of science treats science as one of the apparently many "dynamic orders" found in modern liberal society.²⁶ Particularly important in this essay is Polanyi's characterization of liberal society as a domain in which thought grows (hence his title) because society is a complex fabric of many "dynamic orders" or independent, self-governing social networks or communities of practice. Some orders have a more practical orientation and some—the ones that Polanyi seems to regard as especially important—have a more intellectual orientation as does science. In his article, Polanyi focuses on (and compares and contrasts) three such orders: science, the common law and the market,²⁷ which he likely intended as representative. The case of the operation of science Polanyi emphasizes since this is a counter to Crowther's views. Polanyi portrays science as a growing organism of specialized thought which engages many creative persons in its research programs and it cannot be centrally planned. The success of science (as well as other orders) depends upon the "mutual" or "spontaneous" adjustment of individuals in the community; scientists pay attention to ongoing research, adjusting their own inquiries and views, as they serve the transcendent ideals in their community.

In this essay, Polanyi first discusses what he dubs "public liberty" as a necessary component of liberal society. Polanyi's ideas about public liberty seem to grow out of some of his ideas found in essays and lectures written just prior to "The Growth of Thought in Society" (1941). His 1940 lecture "Collectivist Planning," which becomes a chapter in *The Contempt of Freedom* (also SEP, 121-144 [copy cited here]), identifies "two alternative methods of ordering human affairs" (129), "planning" or "comprehensive planning" and "supervision" or "supervisory authority" (129). The latter "is in the first place the method by which the cultivation of things of the mind is regulated" (127). In "Rights and Duties of Science," first published in 1939 and also incorporated in *The Contempt of Freedom* (also SEP, 61-78), Polanyi connects thought and freedom:

Freedom . . . becomes *necessary* because the State cannot maintain and augment the sphere of thought which can only live in pursuit of its own internal necessities, unless it refrains from all attempts to dominate it, and further undertakes to protect all men and women who would devote themselves to the service of thought, from interference by their fellow citizens, private or official—whether prompted by prejudice or guided by enlightened plans" (68).

Polanyi in these essays recognizes the importance of mutual adjustment effected through the free participation of informed scientists in the public conversation about things like new scientific theories. More generally, thought can grow where there are protections encouraging the vigorous public conversations that lead to mutual adjustment. By 1941 in “The Growth of Thought in Society,” he identifies “public liberty” as integral to guarantee such participation in science and other dynamic orders and distinguishes it from “private liberty” or “private freedoms.” Public liberty as implemented in society’s intellectual dynamic orders is not the liberty an individual has to do as he/she pleases; instead public liberty is the protected opportunity for discourse that individuals (who are serving the ideals embedded in a community) have to promote the ongoing work within a community like science. An individual scientist serves truth by articulating new scientific ideas even if these are in tension with prevailing scientific opinion; he/she is obligated to share new research as well as criticize the views of others as part of the ongoing public conversation in the scientific community, which is the vehicle through which science as an intellectual order progresses (or “mutually adjusts”) and scientific thought in society grows. Public liberty is thus an essential element in dynamic orders like science through which thought in society grows.²⁸

To summarize the several threads in this section’s discussion, Friedrich Hayek was an important friend who supported the intellectual development of both Popper and Polanyi in the thirties and forties. As Popper’s views are beginning to take shape, Hayek has a hand in seeing that some early Popper writing best described as Popper’s wartime-generated political philosophy is published. This material, which became three *Economica* essays published by Hayek in 1944 and 1945 and eventually the book *The Poverty of Historicism* (1957), reaches back to a paper read to friends in Brussels in 1935 (Autobiography, 90) and then again in 1936 in Popper’s visit to England to a Hayek seminar. Hayek helps Popper publish in 1945 *The Open Society and Its Enemies* which grew out of Popper’s work on the poverty of historicism material.²⁹ Hayek also was an important figure involved in bringing Popper as a faculty member to the London School of Economics in 1946.

Hayek meets Polanyi in the late thirties and they become allies opposing the “planned” science movement and criticizing Marxist-influenced economic ideas and experiments. Hayek reviews Polanyi’s 1940 book *The Contempt of Freedom*³⁰ and publishes an important early Polanyi essay which articulates Polanyi’s vision of liberal society, buttressed in what Polanyi terms “public liberty.” The essay outlines Polanyi’s early understanding of science (a view that Polanyi develops further in the mid and late forties and fifties) as one important “dynamic order” in a modern liberal society. That is, his early philosophy of science is tightly woven with his broader political philosophy. Friedrich Hayek later saw that both Popper and Polanyi were included in his Mont Pelerin Society which was concerned with preserving liberal society and which met periodically for several years beginning in 1947.³¹

III. Interactions in Popper’s Early Years at the London School of Economics

A. Early UK Contact and Collaboration

Most of the Polanyi-Popper letters in the Popper Archives were written in the late forties and in the early fifties. Four letters are actually from Karl or Ilona Polanyi who Popper seems to have known well in Vienna in the twenties and early thirties; after he returned to London in 1946, he received these four letters from his friends who provided personal news, asked small favors³² and exchanged pleasantries. One letter implies that Karl Polanyi regarded Michael Polanyi as one of Popper’s contemporary intellectual friends. Karl Polanyi asked (in an undated letter whose contents make clear it was written in the fall of 1947), “Have

you eventually bridged the gap separating you from Hayek in economic policy? Tell me about this, also about Shills (sic), my brothers (sic) work, and especially about your own.”

Before Popper arrived in London in January 1946 to begin his new position at the London School of Economics,³³ Polanyi had invited him to visit Manchester to make a late spring presentation for The Manchester Literary and Philosophical Society. Polanyi's December 10, 1945 letter to Popper invites him “to give us an address on the subject of your recent book” which was *The Open Society and Its Enemies* published in mid November 1945. He adds that “your work has aroused considerable interest in our Society and many of us would be happy to be granted an opportunity to listen to you and to discuss your ideas with you.” A January 26, 1946 follow-up letter from Walter James, secretary for the Social Philosophy Section of The Manchester Literary and Philosophical Society, explained that Polanyi was away from the University, and that James had been requested to discuss arrangements for Popper to give a talk at Manchester in late June, apparently on the topic the “open society.” The letter from James indicates that Popper has already affirmatively responded to Polanyi's invitation to give a talk: Popper agreed to come to Manchester, but advised Polanyi that he “may not be able to prepare a paper for publication in time.” James suggested that if Popper cannot “prepare a finished paper, we shall be happy to have you talk to us on some matter related to your conception of the ‘open society.’” Popper made the projected trip to Manchester, and Polanyi wrote a short letter to him on June 24, 1946, thanking him “very much for your visit to Manchester and for the most interesting lecture which you gave.”³⁴

Other letters from the late forties make clear that Polanyi and Popper were interested in each other's work. These letters seem cordial and generally amiable. Polanyi wrote to Popper on January 29, 1948 thanking him for having sent two papers but acknowledging that he had not yet read them. He reported he had heard “enthusiastic comments” about one paper from John Jewkes, a colleague and friend who was an economist. The other paper, “Logic without pre-suppositions”³⁵ Polanyi proclaimed to be one “which I happen to need very much” but he had apparently mislaid the paper so he requested that Popper save another copy for him, adding he was “undeserving of this added kindness but I assure you at least, that your paper will be quite vital to my work.” Polanyi apparently recognized this might appear an odd claim to make, given that he had not read the paper, and added, “but it is not unreasonable according to my view of science.” Polanyi apparently was referring to his view that scientists rely on assumptions and what he later identified as tacit elements. Polanyi remarked that he appreciated that Popper had “called . . . [him] by the name my oldest friends use, because through Karl [Polanyi] we are really very old friends, and this was revived in me by talking about you to Ilona [Polanyi]. You have been a good friend to her too.” Polanyi thus invoked their shared history. He commented at the end of his letter that he had tried unsuccessfully to phone Popper in London and added that he might be coming to the London School of Economics to deliver a paper in March, “but it is not yet certain. Anyhow, I really must try to meet you soon.”

Correspondence suggests Polanyi and Popper did meet for discussions on occasion in the final months of 1949 and early 1950 (prior to trips both took to the US) as they seriously pursued some topics of common interest. A letter from Polanyi dated October 11, 1949 tried to set up a meeting with Popper later in the month. Polanyi noted that he received a reprint from Popper and commented that he had “gone through your ‘Logik der Forschung’ some time ago and shall be interested to see what principles you consider to be most central to its argument.” Polanyi indicated that he was sending an outline for a paper to Popper to review; it is apparently this outline that he hoped to discuss with Popper in the upcoming meeting.³⁶ The meeting took place and was one Polanyi found very fruitful.³⁷ A November 26, 1949 letter from Polanyi to Popper proclaims “I am most anxious to see you again and talk to you about a number of things that occurred to me since our last meeting.”

Polanyi also asks “whether your paper mentioned over the telephone is available. I should like very much to read it.” A December 6, 1949 letter from Polanyi to Popper concludes “I should very much like to know what your movements will be between now and the middle of March, for I should like to go to see you in London for a couple of hours or so, if you cannot come to address us here.” This late 1949 correspondence makes quite clear that Polanyi sought intellectual contact with Popper and valued his input.³⁸

Writing to Popper on January 12, 1950, Polanyi indicated that he would go to London to meet with Popper on January 17 if January 26 or 27 proved impossible dates. Polanyi promised to mail to Popper the “ms which includes the Polycentricity discussion as soon as I get it back from a colleague who is reading it.” He ends by noting that “I greatly look forward to seeing your paper on the problem of mind and machine which has occupied me so long.”³⁹ Polanyi’s reference in his January 12, 1950 letter to a manuscript dealing with polycentricity is to his “Economic and Intellectual Liberties,” an essay published later in 1950 in a German journal.⁴⁰ “Manageability of Social Tasks,” is a slightly modified version of this essay⁴¹ which was published the following year as the concluding chapter of *The Logic of Liberty*.

Polanyi later notes, in a June 7, 1951 letter to Popper, that he is sending to Popper in a separate mailing a copy of his new book. *The Logic of Liberty* had apparently very recently been published when Polanyi wrote to Popper on June 7, 1951. A review appeared in May, 29, 1951 in *The Manchester Guardian* and another review appeared in the issue of *The Times Literary Supplement* (an organ Popper was more likely to see) dated June 8, 1951 (359)—the day after Polanyi wrote Popper—and in fact this *TLS* issue may already have been available at the time that Polanyi wrote.

Polanyi’s June 7, 1951 letter also remarks that he was “very grateful to you [Popper] for your help in revising the last essay. I have been able to incorporate most of your suggestions in the proofs. Without your stimulus this essay would never have been written.” “The Manageability of Social Tasks” (as well as its predecessor “Economic and Intellectual Liberties” which apparently appeared late in 1950) incorporate most of Polanyi’s 1941 essay “The Growth of Thought in Society” and material from Polanyi’s 1944 publication on patent reform (“Patent Reform,” *Review of Economic Studies*, XI, Summer 1944, 61-76).⁴²

B. Differences Over “Liberalism”

The most interesting element in Polanyi’s June 7, 1951 letter to Popper comes later in the letter: Polanyi avers, “I think we do not agree on the formulation of liberalism, but that only proves how inadequate all formulations are in these matters as our views are fundamentally similar.” This is a declaration which manages both to challenge and conciliate. The challenge identifies that Polanyi and Popper do not conceive liberalism in the same way. But in the same breath Polanyi acknowledges the inadequacy of all efforts to formulate the nature of liberalism and then acknowledges a basic kinship between his own and Popper’s political philosophies. Polanyi’s statement seems primarily to have been intended as a diplomatic gesture which might prepare Popper for what he would soon read in the Preface of *The Logic of Liberty* being mailed separately to him. One section of the Preface seems to be an overt rejection of Popperian terminology and elements of the perspective elaborated in *The Open Society and Its Enemies*: “Private individualism is no important pillar of public liberty. A free society is not an Open Society, but one fully dedicated to a distinctive set of beliefs” (LL, vi). Polanyi does not name Popper here, but he does capitalize “open society.” He clearly aims to re-define “a free society” in terms of what he calls “a distinctive set of beliefs” rather than what he associates (and thinks

his readers associate) with “an Open Society.”

Some of the discussion in *The Logic of Liberty*—especially that in the last chapter which Popper has presumably already read in the virtually identical “Economic and Intellectual Liberties”—helps clarify (in terms of his ideas about “public liberty” as opposed to “private freedoms” as integral to spontaneous orders of liberal society) what Polanyi means when he emphasizes (in his Preface) “a distinctive set of beliefs” which is, for Polanyi, bound up with “public liberty.” There are here in this last chapter of LL also hints about what Polanyi found missing in Popper’s discussion of an “open society” and what Polanyi thinks is wrong with Popper’s conception of liberalism. Polanyi says, for example, “A free society is characterized by the range of public liberties through which individualism performs a social function, and not by the scope of socially ineffective personal liberties” (LL 158; “Economic and Intellectual Liberties,” 415). “Public liberty” is a means to produce a social function and should not be conflated with “private liberty.” Later Polanyi complains about just this conflation which he likely thought Popper’s social philosophy was guilty of:

The liberal conception of society which attributes a decisive part to the operation of individual freedom in the public life of nations, must recognize that this entails a distinction between two aspects of freedom: public and private. Both deserve protection; but it is damaging to the first that it should be demanded and its justification sought—as often happens—on the grounds of the second” (LL 158-159; “Economic and Intellectual Liberties” 415).

As noted above, in his 1941 essay “The Growth of Thought in Society” (most of which is recycled in LL), Polanyi first discussed “public liberty.” Polanyi champions the type of liberty embodied in the second nature beliefs and practices of agents engaged in particular communities or circles such as science and those embodying common law practices. Such agents are embedded in particular “dynamic orders” of liberal society. Society is a network of such orders, although such orders are not all identical and the non-intellectual economic order in particular differs from intellectual orders like science which rely on professional opinion. Those who participate in particular orders serve certain ideals and values revered in that particular order. Agents within a particular order are independent but have common mores and transcendent standards (i.e., have concrete shared beliefs and habits which they put into action) which give the order both coherence and dynamism which comes through the ongoing interaction of independent agents (i.e., through mutual adjustment or spontaneous adjustment). Polanyi seems to envision a public conversation as an ongoing feature of an intellectual dynamic order like science (just as interaction is ongoing in the competitive economic order) and each inquiring scientist has a right and a responsibility to participate in the conversation. That participation is protected by certain guarantees for free speech. Agents of a particular order thus share what Polanyi calls in his Preface to *The Logic of Liberty* “a distinctive set of beliefs” which he links to his vision of a free society. Agents have, or their actions embody, “public freedoms” or “public liberties.” Government in liberal society provides a certain amount of general but indirect support for orders like science though institutions like universities which have certain traditions. Government as well as practices within science and other dynamic orders preserve “public liberty.” Each order is self-governing or self-maintaining through professional opinion and mutual adjustment, and each produces certain intrinsically valuable social goods which support the well-being of larger society.

Polanyi likely sensed profound differences in his account of liberal society, which relies on “dynamic orders” and “public liberty,” and the account of Popper, which projected a major shift in history from the “closed” to the “open society.” Jarvie’s discussion of Popper’s social philosophy is helpful for illuminating this difference.⁴³ Jarvie argues that Popper comes up “with an original conception of the social using the polar

concepts of the open and the closed societies, and of abstract and concrete social relations” (26). Certainly, it is easy enough to recognize these motifs used architectonically in *The Open Society and Its Enemies*. On the opening page of his Introduction, Popper speaks of “the transition from the tribal or ‘closed society’, with its submission to magical forces, to the ‘open society’ which sets free the critical powers of man” (1966, 1). Later, in his chapter titled “The Open Society,” Popper says “in what follows, the magical or tribal or collectivist society will also be called the *closed society*, and the society in which individuals are confronted with personal decisions, the *open society*” (1966, 173). Still later, Popper positively discusses how modern society has lost “organic character” (174) and moved toward being a more “abstract society” (1966, 174) and this is linked to “a new individualism” involving “personal relationships of a new kind” (1966, 175) which “can be freely entered into” (1966, 175). Popper’s “open society” is connected with rational, critical thought, and the “closed society” is exemplified by modern totalitarianism⁴⁴ and is at least indirectly linked with what Popper calls “historicism,” which, as Jarvie notes, is “the idea that there are inexorable laws of history” (91). Just as Popper’s “philosophy of science had rejected induction as the characteristic method of science” so also “his philosophy of social science rejected historicism as the characteristic method of social science” (93). As Jarvie makes clear, Popper’s polarities are ideal types and certainly Popper did not think his positive ideal of an “open society” was in more than its infancy (143).

It seems very likely, however, that Polanyi found Popper’s general way of framing the discussion of political philosophy and Western historical development in terms of movement between his polarities to be problematic. By 1941, he is already trying to work out his own ideas about “dynamic orders” and “public liberties” as the key to modern liberal society. He likely saw Popper’s social philosophy, despite its cautionary ambience, as close to the standard Enlightenment narrative. By the time (i.e., the late forties and early fifties) that Polanyi is putting together his Gifford Lectures and writing the Preface of *The Logic of Liberty* (and his essay “The Stability of Beliefs” discussed below), he is working to pull together both an alternative political philosophy and an account of science that recognizes the importance of belief.⁴⁵

C. A Close Reading of the Preface to *The Logic of Liberty*

As we have suggested, Polanyi tightly links public liberty and his conception of liberalism (i.e., what he calls a “free society”) in his Preface to *The Logic of Liberty*, hinting that differences with Popper turn on this linkage. Polanyi’s full Preface deserves a more careful consideration since it raises the larger question as to how much kinship—despite his June 7, 1951 letter’s claim—Polanyi did in fact, think he had with Popper. His Preface reads more like a mild mannered manifesto outlining how a whole set of new philosophical ideas—ideas that Popper would not accept—belong together and need to be articulated. The Preface is a prolegomena of sorts for the philosophical perspective that Polanyi is beginning to articulate in his First Series Gifford Lectures which began May 7, 1951.

Polanyi introduces his Preface with a quotation from Kant which points out that only after wrestling unsystematically with elements that seem to be deeply embedded in mind do we become able to see the elements clearly and reasonably as they fit together. He then points out that over the eight years in which the essays in his book were written, he has been trying “to clarify the position of liberty” (LL, vi). He notes that he has considered recasting his earlier efforts at clarification in a more comprehensive system but he acknowledges “this seemed premature.” He further explains himself with the claim that a better account of liberty is impossible “without establishing first a better foundation than we possess to-day (sic) for the holding of our beliefs” (LL, vi). Polanyi suggests that he, nevertheless, hopes this collection of essays “may supply

some elements of a future coherent doctrine since it expresses throughout a consistent line of thought” (LL, vi). This consistent line of thought he describes in terms of how his essays “take more seriously here than was done in the past the fiduciary presuppositions of science; that is the fact that our discovery and acceptance of scientific knowledge is a commitment to certain beliefs which we hold, but which others may refuse to share” (LL, vi). He then notes that “freedom in science” is “the Natural Law of a community committed to certain beliefs.” Polanyi links “freedom of thought” and respect for thought and ongoing inquiry, but he points out that freedom is not an end in itself but a prerequisite for the ongoing work of scientific inquiry and, more generally, a commitment to “cultivate the things of the mind”:

On these lines, freedom of thought is justified in general to the extent to which we believe in the power of thought and recognize our obligation to cultivate the things of the mind. Once committed to such beliefs and obligations we must uphold freedom, but in doing so freedom is not our primary consideration (LL, vi).

Polanyi’s next paragraph shifts from discussing freedom in science to what he dubs “economic liberty” (found in the market, a competitive and non-intellectual dynamic order). He contends this type of liberty is “a social technique suitable, and indeed indispensable, for the administration of a particular productive technique” [LL, vii]. “Economic liberty” thus is a means through which “individualism performs a social function” (LL 158) insofar as it is integral to the particular productive technology now employed, Polanyi acknowledges the present deep commitment to a market economic system with its particular set of economic liberties enjoyed by producers and consumers, but he points out that “other alternatives may one day present themselves with strong claim in their favour” (LL, vii). Thus “economic liberties,” like “freedom in science,” are not valuable in themselves but insofar as they serve ends in a particular domain or order of society and, more generally, the “cultivation [of] the things of the mind.”

Near the end of his Preface, Polanyi says somewhat more directly what his previous comments have hinted at:

There is a link between my insistence on acknowledging the fiduciary foundations of science and thought in general, and my rejection of the individualistic formula of liberty. This formula could be upheld only in the innocence of eighteenth-century rationalism, with its ingenuous self-evidences and unshakable scientific truths. Modern liberty, which has to stand up to a total critique of its fiduciary foundations, will have to be conceived in more positive terms. Its claims must be closely circumscribed and at the same time sharpened for a defence against new opponents, incomparably more formidable than those against which liberty achieved its first victories in the gentler centuries of modern Europe. (LL, vii).

In sum, Polanyi implies in his Preface that philosophical views of liberty must be wary about conceiving liberty too individualistically and too independently of the operation of particular dynamic orders such as science and the market economy where certain kinds of liberty serve a very important but subsidiary role. It seems very likely that Polanyi holds Popper’s “open society” does conceive liberty too individualistically and too independently of particular dynamic orders such as science, common law and the market economy. The conception of modern liberty in “more positive terms” (i.e., terms that can “stand up to a total critique of its fiduciary foundations”) that Polanyi here mentions is Polanyi’s “public liberty.” Polanyi’s Preface suggests that he thinks it is necessary to connect the project of sorting out the nature of liberty and the larger project of

understanding the fiduciary foundations of science and of thought in general.⁴⁶ This is his emerging broader agenda as a philosopher, namely to show how belief (what he calls here “the fiduciary foundations”) is the foundation of knowledge and to show how this is the case even in science. This larger project Polanyi later sometimes identifies as “post-critical philosophy.”⁴⁷

Work on this larger project was formally initiated in his Gifford Lectures whose First Series lectures began May 7, 1951, a month before his June 7 letter to Popper suggesting that his conception of liberalism differed from that of Popper. Although Polanyi’s critical-yet-conciliatory comments about “liberalism” in his June 7 letter seem to try diplomatically to prepare Popper for his more forthright remark about the “open society” in his new book’s Preface, it is not clear that Polanyi deeply believed, as he put it in his June 7 letter, that “our views are fundamentally similar.” The claims—visible in the larger context of Polanyi’s Preface—hint at the program of his Gifford Lectures. Mark Mitchell recently noted that “Polanyi should be understood as a political philosopher who rightly grasped that liberty depends on resources beyond politics.”⁴⁸ The Preface to *The Logic of Liberty* is essentially an announcement that this is the case. Polanyi’s Preface, and some of the material in the book itself, seem quite different than ideas developed in both Popper’s social and political philosophy and his philosophy of science and Polanyi likely recognized this at the time he wrote his Preface and his letter.

Despite his veiled criticism (i.e., a criticism with no citation of Popper’s book) in the Preface to *The Logic of Liberty* and his June 7, 1951 letter’s comment about liberalism, Polanyi continued to be collegial with Popper⁴⁹ and Popper responded in the same vein. Popper’s October 30th 1951 letter enthused that he would “love to see you when you can come to London and as much as you like.” Popper also invited Polanyi to attend his seminar that term: “Do you think you could possibly attend my Seminar either for reading a Paper there or for discussion? It is on Thursdays from 2 to 4 p.m. The topic of the Seminar at present is a very wide and ambitious one—on ‘the principles of a Good Society.’”

IV. Popper’s 1952 Letter Concerning “The Stability of Beliefs”

A. The Context of Popper’s 1952 Letter

Another particularly interesting letter in the Hoover Archives correspondence between Popper and the Polanyi is an undated one in Popper’s hand that he very likely wrote in the summer of 1952. It is a letter directly challenging the argument that Popper understood Polanyi to have made in his paper “The Stability of Beliefs,” which Polanyi delivered on June 9, 1952 in London before the Philosophy of Science group which Popper chaired.⁵⁰ Polanyi’s paper soon appeared in the November 1952 issue of *The British Journal for the Philosophy of Science*, which was under Popper’s editorship. This 1952 letter, it seems reasonably clear, marks a turning point in the interaction between Polanyi and Popper.

Polanyi’s comment (in his June 7, 1951 letter) about liberalism, as well as his new book’s Preface with both its apparent criticism of Popper’s open society and his broader suggestion that social philosophy and the understanding of science must be recast in a fiduciary context, offer largely subterranean hints about Polanyi’s sense that he does not agree with some things Popper affirmed. Popper’s summer 1952 letter to Polanyi is an overt announcement that Popper does not accept the account of things that Polanyi presented before the philosophy of science group. Given the context and claims of Polanyi’s paper, it certainly seems

that Polanyi used this London presentation to make more clear and more public his emerging philosophical perspective which he now clearly recognized differed markedly from Popper's ideas. Popper's forthright letter of response takes the wraps off this difference in perspectives since it boldly challenges Polanyi's views.

The 1952 archival letter from Popper may be missing one element. It has numbered points: points 1 and 2 are on the first page of the letter; points 3 and 4 are on the second page, points 6 and 7 are on a separate sheet and the last sheet has a closing sentence. There is no point 5 but there very well could have been in the original letter on another separate sheet but a check with the Hoover Institution Popper Archives has not turned up a misfiled page containing a point 5. Even absent a point 5, Popper's criticisms are clear. Before presenting Popper's letter of response, we analyze the argument in "The Stability of Beliefs" in some detail since it is more tightly woven than it initially appears. As a prelude to analyzing this argument, we describe the specific context in which this particular Polanyi paper and published essay should be situated.

B. The Context of "The Stability of Beliefs"

The published article is representative of the epistemological views and the account of science that Polanyi was pulling together in the late forties and early fifties as he worked on his 1951 and 1952 Gifford Lectures. That is, "The Stability of Beliefs" fits into the general context of this fertile period when Polanyi is reconsidering the importance of belief. There is clear thematic kinship between "The Stability of Beliefs" and several other Polanyi publications from this period. The discussion above of Polanyi's Preface to *The Logic of Liberty* (which was written close to the time of the 1951 publication of this collection of essays) has outlined how Polanyi aspired to link his account of liberty with a broader understanding of the fiduciary foundations of science and of thought in general. Another Polanyi article titled "Scientific Beliefs"⁵¹ that was published in 1950 has close affinities with "The Stability of Belief." In "Scientific Beliefs," Polanyi argues that "any rigorously cognitive conception of science . . . requires to be supplemented by fiducial elements" (26). This is an essay, like "The Stability of Beliefs," in which Polanyi cites and builds upon ideas found in Levy-Bruhl, and he draws upon Evans-Pritchard's work on witchcraft and magic of the Zande.

The more immediate context in which "The Stability of Beliefs" should be viewed is Polanyi's Gifford Lectures. The First Series was given in May and June of 1951, at the University of Aberdeen, about a year prior to Polanyi's presentation of "The Stability of Beliefs" in London in June 1952. The Second Series was delivered in November and December of 1952 about the time Polanyi's paper was published. Polanyi's Gifford Lectures argued against what he considered to be the disproportionate role that has been given to doubt and criticism in modern thought, and—as a constructive alternative—Polanyi set forth the contours of a new philosophical perspective emphasizing belief, faith, skills and commitment. Polanyi's First Series first lecture (May 7, 1951) proclaimed at the beginning, "philosophy must voice today our decisive beliefs."⁵² The fourth lecture, "The Fiduciary Mode," looked at the pervasiveness of belief. "The Self-Destruction of Objectivism" (lecture five) outlined how, since the Enlightenment, "radical skepticism grew from the doubt that had cleared the ground for the progress of science," undermining the traditional virtues and liberal society itself and preparing the way for the political upheavals and nihilism in Europe since the nineteenth century (Scott and Moleski, 218). The sixth lecture argued that "the dangers of a frankly fiduciary philosophy cannot be avoided," and Polanyi called for "the rehabilitation of overt belief." In the seventh lecture, "The Doubting of Explicit Beliefs," Polanyi focused on how the modern mind sharply distinguishes between belief and doubt and attacks belief "by pitting against it the method of doubt, in the expectation that this will leave behind a residue of true knowledge." In fact, as Polanyi put matters in the précis of the following lecture, "The Doubting

of Implicit Beliefs,” in doubting both explicit and implicit beliefs, there is “no reduction in the volume of beliefs, but ... an acceptance of new beliefs in place of those previously held.”⁵³

The précis of “The Doubting of Implicit Beliefs” in the Syllabus of the First Series, identifies Zande belief as an example of implicit belief and describes how Zande belief remains stable when challenged. The précis outlines, and the Duke version of the lecture delivers, what is, compared to “The Stability of Beliefs,” a longer, more general discussion of implicit beliefs and topics such as the adaptation of frameworks and the circularity of frameworks. Nevertheless, it is quite clear that Polanyi’s later 1952 paper and published essay “The Stability of Belief” draws directly on much of this Gifford lecture delivered May 30, 1951.⁵⁴

C. The Argument of “The Stability of Beliefs”

“The Stability of Beliefs” opens by asserting that all our beliefs are held in one of two ways, either “explicitly as articles of faith” or “implicitly by reliance on a particular conceptual framework by which all experience is interpreted” (217).⁵⁵ Polanyi contends that “the principle of doubt” has become a presupposition of modern culture where it is taken to be an antidote to dogmatism. In the modern period, “the continued application of doubt seems to have converted all explicit forms of faith into implicit beliefs, ensconced in our conceptual framework, where they elude the edge of our skepticism” (217). Holding beliefs dogmatically as explicit “articles of faith” (217) has come to be seen as irrational. The modern mind has worked to eliminate all “open affirmations of faith” (217) as uncritical. Polanyi suggests the modern application of doubt is like penicillin too frequently used which helps produce resistant strains. The application of doubt has converted forms of faith into implicit beliefs whose power we don’t appreciate. It is how implicit belief works forming a very stable (“doubt-proof”) systemic perspective such as that found in Marxism, psychoanalysis, science and the Zande magical and ritualized worldview that is the general topic of Polanyi’s essay.

Polanyi did not elaborate here what he understood the principle of doubt to stand for in terms of modern epistemology or philosophy of science, nor does he name its supporters. However, Polanyi does carefully set forth his own account of the role of doubt in science. He argues scientists are cautious, but caution is not peculiar to science: “The practice of every art must be restrained by its own form of caution” (227). He qualifies this generalization by saying “caution is commendable in science, but only in so far as it does not hamper the boldness on which all progress in science depends” (227). Discovery—which for Polanyi is the heart of science⁵⁶—requires boldness and there is no rule in research for deciding what is “truly bold” and what is “merely reckless” (227). There is no procedure or rule that will serve to distinguish doubt that “will curb recklessness and will qualify as a true caution, and doubt which cripples boldness and will stand condemned as unimaginativeness or dogmatism” (227). Polanyi thus points out that “caution” is a notion that has built into it already the idea of reasonable doubt: it “acknowledges our appreciation of a successful operation of doubt, without telling us how to achieve such success . . . ‘Caution’ is a form of approval, masquerading as a rule of procedure” (225).

Polanyi briefly summarizes a case from the history of science in which a Swedish professor refused to accept Arrhenius’ later celebrated theory of electrolyte dissociation; the professor tried explicitly and rigorously to apply the principle of doubt and argued he could not accept Arrhenius’ theory because theories were almost certainly eventually to be superseded (228). Rhetorically, this narrative seems in Polanyi’s essay to be something like a *reductio ad absurdum*, ridiculing ideas about exhaustive application of doubt or criticism in scientific practice.⁵⁷ Polanyi also discusses anomalies and observations held at one time to be important

scientific facts but which are discredited and disappear without having been “disproved or indeed newly tested” (229)—the conceptual framework of science changed so that such facts no longer were credible. He points to the fate of research on intensive drying (to stop chemical reactions and reduce evaporation) in the history of chemistry to illustrate his claim (229-230). What facts are relevant and of interest to a scientist, or any other person, depend on the particular framework in which the person dwells. Polanyi considered this to be as true for those who accept the modern scientific account (or a particular theoretical orientation within the larger naturalistic scientific outlook) as for those who believe in Zande witchcraft, psychoanalysis or Marxism: “The process of selecting facts for our attention is the same in science as among Azande, but I believe that science is more often right in its application of it”(230).

The perspective articulated in “The Stability of Beliefs” is linked to a particular theory of language. The theory proposes that each language reflects a worldview which seems to be what Polanyi dubs the “modes of interpretation” inherent in a “conceptual framework” (220).⁵⁸ To use a language confidently “expresses belief in a conceptual framework” (220). The language forms “an idiom of belief” (220) which offers those who use it a particular way of interpreting experience in the world. Polanyi cites Lévy-Brühl as the figure who worked out this account and notes that Evans-Pritchard had elaborated on the idea in his 1937 study, *Witchcraft, Oracles and Magic among the Azande* (220).

Polanyi’s 1952 essay, of course, provocatively used this exotic anthropology to raise and respond to fundamental questions about belief as well as the operation of doubt and reasoning in science.⁵⁹ Polanyi reports that Evans-Pritchard was struck by the conviction with which the Azande held their beliefs “against evidence which to the European seems flagrantly to refute them” (220). He quoted Evans-Pritchard’s view that the Azande “reason excellently ... in the idiom of their beliefs, but they cannot reason outside, or against, their beliefs because they have no other idiom in which to express their thoughts”(221). Beliefs like those held by doctrinaire Marxists or Freudians, scientists and the Azande are, Polanyi suggested, “doubt-proof” since they have “adhesive power” as “interpretative frameworks” (218) They are *acritically* held faith-beliefs.⁶⁰ For its adherents, a framework of belief underlies, interprets, and is confirmed by experience, ruling out the possibility of adherents criticizing (testing) it by experience.⁶¹

Polanyi aimed in his essay to “illustrate the elementary principles by which a conceptual framework retains its hold on the mind of a person believing it” (218). His strategy for showing the dynamics of all conceptual frameworks “examine[d] the same or similar mental operations in the one case from the outside critically, and in the other from the inside, uncritically” (218). In regard to the practice of witchcraft and magic, Evans-Pritchard (a scientifically trained anthropologist with an outside view) could not convince the Azande to experiment with the administration of special ritually-gathered poison so that they could recognize that it is merely the quantity of poison that determines whether the fowl consuming it died or recovered (a matter of great significance to the Azande in their decision-making). Instead, the Azande believed (an inside view), matters depended on whether magical powers were properly introduced into the substance (*benge*) by oracles and magicians. Thus the Azande as a people “hold distinctive systems of beliefs by practicing peculiar modes of interpretation which are inherent in their conceptual framework and are reflected in their language” (220). Nevertheless, Polanyi affirmed that he held as his own framework a modern scientific worldview and thus did not believe that the magical orientation of the Azande is true (nor did he believe that Marxist or Freudian perspectives are true). He emphasized (222) how the Zande conceptual framework (and any conceptual framework, science included), works in a particular human communal setting to thwart efforts to demonstrate that it is false.

Polanyi discussed the operational powers that make conceptual frameworks very stable in terms of several elements. First, any system of implicit beliefs is embodied in a language which meets objections one by one; the system has “circularity” (222). Other unchallenged elements of a worldview continue in use and undermine the credibility of any new “fact” or “experience”: “so long as each objection is defeated in its turn, its effect is to strengthen the fundamental convictions against which it was raised” (222). Second, Polanyi argued that each “idiom of belief” has a “self-expanding capacity”: “all major interpretative frameworks have an epicyclical structure which supplies a reserve of subsidiary explanations for difficult situations” (224). Indeed, Polanyi contended that every well developed interpretative framework has the capacity “to supply secondary elaborations to its beliefs which will cover almost any conceivable eventuality, however embarrassing this may appear at first sight” (224). As an example of the use of epicyclical explanation, Polanyi claimed that (228) scientists often dismiss “contradictions to current scientific conceptions . . . by calling them ‘anomalies’”. This is among the most handy assumptions in the epicyclical reserve that is available for the adaptation of any theory, in the face of adverse evidence” (228). The third power enabling each idiom of belief to maintain its stability, Polanyi called “the principle of suppressed nucleation” (225). This principle complements circularity and the powers of an epicyclical reserve by suppressing and preventing “the germination of any alternative concept on the basis of any single new piece of evidence” (225). That is, adverse evidence must accumulate if it is eventually to become a credible perspective, but suppressed nucleation limits the development of concepts to cover instances that might accumulate and become recognized as significant.

Polanyi suggested that Western scientists reject most Zande beliefs “by discarding mystical conceptions and replacing them by a naturalistic explanation,” but he straightforwardly questioned whether this rejection “is the outcome of any general principle of doubt” (225). He argued that if a principle of doubt existed, it should be “possible to detect it in the first place within science which the adherents of the principle of doubt regard as the best example for the operations of this principle” (225). Polanyi then turned to a discussion of advance in science, suggesting first that advance is “the *assimilation* of fresh topics within its existing system and . . . the *adaptation* of its existing system to the nature of fresh topics; the first is a conservative act, the second a process of reform” (226). However, later discussion makes clear that Polanyi is not simply arguing that science consists of assimilative processes that conserve and adaptive processes by which the framework of science is reformed. Instead what he emphasizes is that the significant expansion of an existing framework of science (assimilation) is dependent on scientific discovery, the central feature of science: “The power to expand hitherto accepted beliefs far beyond the scope of hitherto explored implications is an eminent force of discovery” (226). In his discussion, Polanyi gives a series of historical examples of such extraordinary assimilation and concludes “the assimilative power of an existing scientific framework thus appears no less creative and offers no less scope for the application of scientific genius, than its capacity to sprout into new and entirely unexpected forms” (227). This conclusion leads to the further question as to “what room does such a picture leave for the operation of a principle of doubt?” (227), and this is a question that Polanyi only addresses in a way so as to undermine views attributing special importance to doubt in science: as we have outlined above, he simply points out that every art is restrained by caution and that in science there is no rule for distinguishing proper caution and caution that hampers boldness in research.

Polanyi’s argument in “The Stability of Beliefs” is, in sum, one that makes a careful case for the pervasiveness of belief and the resiliency of all systems of belief, science included. The argument undermines the claim that doubt plays a special role in science; Polanyi emphasizes the importance of discovery in science rather than doubt. At least by implication, Polanyi undermines any claims that might be put forward about the importance of a program aimed at falsification of scientific theories. Although Polanyi’s discussion focuses

on the rather exotic case of the Zande, his consistent comments on the analogous case of modern science and his effort to muster concrete examples from the history of science is an important part of his case. Popper's letter suggests that he recognized that his own and Polanyi's account of science were worlds apart, although Polanyi never used certain terms like "criticism," "falsifiability" and "falsification" that were primary in Popper's lexicon.

D. Popper's Response

Dear Michael,

If you are interested in what I now think concerning the matter we discussed in the very interesting meeting when you addressed The Philosophy of Science Group, here it is.

(1) The parallelism between Zandi [sic] religion and modern science is admittedly far reaching and interesting.

(2) There are structural differences. These can be explained away, of course, by epicyclical arguments. (That epicyclical arguments are always possible as has been pointed out by me in my *Logik der Forschung* I call them there: "Konventionalistische Wendung" [conventionalist strategies⁶²]).

(3) Indeed, we need a faith – "faith in reason", I called it in the "Open Society". But this faith consists, fundamentally, in the realisation of (2), i.e. of the existence of structural differences between Zandi and us; and therefore in the abstention from applying epicyclical method used to explain away these differences.

(4) If, seduced by the obvious possibility of explaining away these differences, the structural identity of Zandi and our science is asserted, then faith in reason is abandoned. This leads to relativism, or skepticism, or mysticism.

(6) The common basis of the relativistic or sceptic or mystic position is always the same. It is disappointment with a rationalism from which more was demanded than it can give, viz. certainty or demonstration where we have to be content without these.

(7) For example, we can never/usually not be certain that a certain argument is not used epicyclically and cannot demonstrate that it is not so used. But why should we?

I suppose you will consider all this useless, and not to the point. However here it is. We all enjoyed your paper very much and we should love to publish it in the Journal as quickly as possible.

Yours ever,

K.

Although Popper's letter begins by acknowledging "far reaching and interesting" parallels between Zande belief and science, he strongly affirms that there are "structural differences." He acknowledges the way in which epicyclical arguments can be used to undermine claims about "structural differences," suggesting that he has himself already pointed out in his 1934 book how epicyclical arguments are always a possible option. But he insists that "we need 'faith in reason,'" invoking the terminology used in *The Open Society and Its Enemies*,⁶³ and what this amounts to is accepting the "structural differences" between Zande religion and modern science and rejecting what he calls the "epicyclical method" which might be used to explain away these differences.⁶⁴

Popper's stress upon the "structural difference" of science apparently alludes to his falsifiability criterion of demarcation between statements of science and those of non-scientific systems (e.g., Zande witchcraft), and to the critical method which he believed is characteristic of science. According to Popper's falsifiability criterion, there are two possible reasons why the statements of Zande witchcraft could be unfalsifiable: first, they could be inherently unfalsifiable (unfalsifiable in principle) because the statements have no empirical reference; second, the Zande might express statements that are structurally falsifiable (falsifiable in principle), but which are rendered unfalsifiable in practice by resorting to something like *ad hoc* hypotheses to explain away empirical contradictions (falsifying observations). Popper's comment about refraining from applying epicyclical method to explain away differences between science and Zande belief may be meant to suggest that he believes any unfalsifiability of Zande belief is practical and attitudinal.⁶⁵ That is, Zande adoption of practical methods that will save empirically contradicted statements from falsification by adding hypotheses (and thus serves to explain putative falsifications away) is "structurally different" than the approach of science where this Popper holds is unacceptable. Popper's response is very concisely formulated—so much so that his argument is not altogether clear,⁶⁶ although it is clear enough that he is rejecting Polanyi's case and affirming that science accepts certain "methodological rules" (to use Jarvie's phrase) including falsification. Ultimately, as his final sentences in the letter suggests, he does not think his case will be convincing for Polanyi.

Popper's last three points in the letter suggest that to be "seduced by the obvious possibility of explaining away these differences"—to assert too much parallelism or similarity (as he thinks Polanyi does) rather than structural difference—is to abandon "faith in reason" and to turn to "relativism or skepticism, or mysticism." All of these options, Popper abhors and characterizes as grounded in "disappointment with rationalism from which more was demanded than it can give, viz. certainty or demonstration where we have to be content without these." Popper's last numbered point is an assertion phrased as a question: since we cannot be certain—nor can we demonstrate—that an argument is not "used epicyclically," we should not focus on (or be preoccupied with) this.

Popper was passionately committed to his own philosophy of science, and intolerant of its critics and doubters. Even Popper's closest friend, Ernst Gombrich, emphasized this, explaining to Bryan Magee that Popper

seemed unable to accept the continued existence of different points of view, but went on and on and *on* about them with a kind of unforgiveness until the dissenter, so to speak, put his signature to a confession that he was wrong and Popper was right. In practice this meant he was trying to subjugate people.⁶⁷

Given his tenacious disposition, Popper's remark in his letter that he "enjoyed" Polanyi's "paper very much" seems overly diplomatic. This likely was not the impression Polanyi received at the London presentation. John Watkins, in the only brief report on the session that we have found, suggests that "Polanyi was gravely offended by the treatment that Popper, as chairman, meted out to him when he read a paper (on "The Stability of Beliefs", 6 March 1952 [sic.]) to the Philosophy of Science Group" (668). Popper apparently recognized that his philosophical perspective was being fundamentally challenged by what he had heard in "The Stability of Beliefs." Popper championed the "principle of doubt" in a falsificationist version; he viewed criticism as the method of rational cognitive advance in science and in general. He likely believed that Polanyi had effectively dismissed all this as a caricature of science and epistemology. At the beginning of his essay, Polanyi admitted his case was "a conscious affront on my part to the critical tradition of modern thought and is bound to shock some readers" (218). Popper seems to have been among those shocked. All of the editions of *The Open Society*

and *Its Enemies* make clear Popper's aversion to what he takes to be cognitive relativism (2002, 679 n. 23); he had explained why a rejection of his critical rationalism would likely encourage violence, and undermine rationality and humanitarian values (2002, 496ff). Popper likely saw "The Stability of Beliefs" as a harbinger of just this. Polanyi, on the other hand, in his own view was developing a constructive philosophy, affirming fiduciary foundations of knowledge and common life as an alternative to the critical tradition of philosophy, which he blamed for having eroded the traditional values of science and Western culture and what he a little later forcefully identified as bringing "moral inversion."⁶⁸ Popper was correct that his letter had no discernible effect on Polanyi's thinking.

If Popper's views were, at least by implication, indicted by Polanyi's paper, the question arises as to why Polanyi did not straightforwardly name and critically discuss Popper's philosophy of science in "The Stability of Beliefs." We speculate that Polanyi probably had no wish dramatically to upend the relationship that he had with Popper and some of their common friends, including Karl and Ilona Polanyi; Polanyi may have considered his twenty year personal link to Popper complicated by the fact that members of their respective families had been killed by the Nazis. It seems likely that Polanyi would have known, from previous contact with Popper, what Popper's students testify to (discussed above), namely that Popper tenaciously resisted granting any credibility to views that differed from his own. If Watkins' report is accurate, this was the case in Polanyi's paper's discussion. Finally, as we have above suggested and our discussion of the Preface to *The Logic of Liberty* and the Gifford Lecture material implies, Polanyi seems to have been in this period preoccupied with working out his constructive alternative vision to the critical tradition (i.e., his "fiduciary philosophy" with its "post-critical perspective") rather than laboring his criticisms of the mainstream in an extended philosophical debate with Popper. The publication of *Personal Knowledge, Towards a Post-Critical Philosophy* in 1958 finally works out in some detail Polanyi's alternative to the critical tradition. The material in "The Stability of Beliefs" has been integrated into the broader discussion of *Personal Knowledge*.⁶⁹ What his *magnum opus* presaged to Popper was suggested in the bitter quotations from the English edition of *The Logic of Scientific Discovery* (1959) and *Realism and the Aim of Science* (1983) quoted in the opening section above. Apparently Thomas Kuhn and Paul Feyerabend did take a close look at Polanyi's *magnum opus*, and perhaps at "The Stability of Beliefs."⁷⁰ Their writing about a decade after *Personal Knowledge* brought an open revolution in the understanding of science.

V. Popper and Polanyi After 1952

Although there are letters in the Popper Archives that were written after 1952, most are concerned with practical projects (e.g., getting funding for a journal) or health.⁷¹ It appears that for about the last twenty years of Polanyi's life—if not all the way back to his 1952 presentation in London—Polanyi's relationship with Popper was strained. One Polanyi letter to Popper dated May 4, 1965 comes close to addressing directly the sharp differences in Popper's and Polanyi's views that both figures recognized. Polanyi says,

It seems possible that in the next few years we may become involved in controversy. This might indeed be the best way to clarify the relation between our views and give the public a better chance to form their own views of the whole area we jointly cover. Later decades may also profit from it.

I hope that we would both enjoy airing our differences, if it did come to it and for my part, I feel sure also that I would learn from it.⁷²

What little controversy there was in the last years of Polanyi and Popper's lives was largely underground. Polanyi seems to have regarded himself as a figure who had not received his due while Popper, as well as Thomas Kuhn, enjoyed undeserved glowing reputations. In August 1970, in a letter to Donald Campbell (professor of psychology at Northwestern University), Polanyi confided that:

My claims ... 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.⁷³

Despite the fact that Polanyi never published much suggesting what he thought about Popper (or Kuhn), he did write a December 16, 1971 letter to the then President of The Royal Society, Professor Alan Hodgkin (joint winner of the Nobel Prize for Medicine in 1963 with J. C. Eccles and Andrew Huxley), advising he was "strongly opposed to the election of Karl Popper to a Fellowship of the Royal Society."⁷⁴

Endnotes

¹Archival material quoted in this essay is with permission of the Karl Popper Library Klagenfurt, and the University of Chicago Library (Special Collections Research Center, Chicago, IL 60637 USA), where the Papers of Michael Polanyi and the Papers of Edward Shils are held. We appreciate the assistance of staff of the Hoover Institution Archives, in particular Carol Ledenham, who searched in archival material for what we believed to be lost elements of correspondence. Dr Manfred Lube, University of Klagenfurt, helpfully answered our queries. Staff in our respective university libraries went to great lengths to help us find historical materials used here. Walter Gulick (Montana State University Billings) and Martin Moleski, S.J. (Canisius College), along with Rafe Champion (independent scholar), provided us with substantive information and advice. Dr. Evelyn McBride, a native Austrian, refined our German to English translations of Popper's early letters to Polanyi. Dr. John Preston (University of Reading) provided a copy of his *Appraisal* article (1997, supplement) on Feyerabend and Polanyi. Professor Alan Musgrave (University of Otago) provided a copy of a relevant part of his PhD thesis. Dr. Peter Vickers (University of Leeds) examined archival material of the British Society for the Philosophy of Science to verify the date on which Polanyi read his paper, "The Stability of Beliefs," to the London Philosophy of Science group (P. Mullins, e-mail communication, February 12, 2010). Finally, we thank the two reviewers, who provided important suggestions improving the final draft of this essay.

²Andy Sanders (*Michael Polanyi's Post-Critical Epistemology* [Amsterdam: Rodopi, 1988], 185) some years ago pointed out this shot at Polanyi as part of his careful and thorough analysis of and response to the criticisms of Polanyi by Popperians (159-225).

³One of our reviewers suggested Popper perhaps took Polanyi's subtitle ("Towards Post-Critical Philosophy") for *Personal Knowledge* very personally as a public rejection of his critical rationalism. Perhaps

this is the case, but, as we argue below, Popper, by 1958, likely was already very aware of important differences with Polanyi about “critical rationalism.” Popper’s comment seems to imply that he knows “post-critical” is a term applied to an “age,” which is the way Polanyi used the term in LL (109). More generally, “post-critical philosophy” is concerned with Polanyi’s affirmation of belief.

⁴Popper is perhaps on target in tagging Polanyi a fideist of sorts, but he seems altogether to have missed Polanyi’s emphasis upon the scientist’s “contact with reality” which for Polanyi means that present formulations will be superceded by further investigation of scientific successors (although Polanyi does not think scientists put forth claims as mere conjectures they will readily give up). It is difficult to read these criticisms of Polanyi as not a little post-mortem score settling. How closely Popper studied Polanyi’s writings is not clear but he apparently has read some things (see further discussion below). Polanyi’s May 4, 1965 letter to Popper (Box 339, Folder 1) in the Hoover Institution Popper Archives says that he was “glad that you have already conducted some seminars on my writings, as this will help me in communicating with your students.” As the discussion below shows, Popper likely began to recognize by the early fifties that Polanyi’s “post-critical” perspective was fundamentally at odds with his views.

⁵*Encounter* 38, 43-50. Quotations use page numbers from *Encounter*. The essay is also available in Richard Allen, ed., *Society, Economics and Philosophy, Selected Papers (of) Michael Polanyi* (New Brunswick and London: Transaction Publishers, 1997): 267-283 (hereafter cited as SEP). Stefania Jha (“The Bid to Transcend Popper and the Lakatos-Polanyi Connection,” *Perspectives on Science*, 14(3): 318-346) contends this essay originated as a lecture Lakatos invited Polanyi to deliver at the London School of Economics after Lakatos succeeded Popper at the LSE and became more openly sympathetic with Polanyi’s views. Jha suggests the lecture “highlighted for his [Polanyi’s] audience the differences between Popper’s position and his own” (335-336).

⁶In note 8 on p. 46, Polanyi cited *Logic der Forschung* (1934) and mentioned the English translation, *The Logic of Scientific Discovery* (1946—Polanyi’s date is apparently wrong since the translation was not published until 1959). He noted that in *Conjectures and Refutations* (1963) Popper had modified in some ways the position of the *Logik der Forschung*, but Polanyi said this “does not substantially affect the principles of ‘refutationalism.’” In the Papers of Michael Polanyi in the Department of Special Collections of the University of Chicago (Box 24, Folder 12), there are some notes and extracts from *The Logic of Scientific Discovery* (1959), from sections of Popper’s 1945 essay “The Poverty of Historicism III” (*Economica* New Series, 12 (46): 69-89), and from his 1949 essay “Towards a Rational Theory of Tradition”(in *The Rationalist Annual 1949*, F. Watts [ed.], vol. 66: 36-56) There is reasonably good evidence that Polanyi eventually knows in some detail Popper’s views, as we show below. As both of the above noted late Polanyi references to Popper suggest, Polanyi is aware that Popper’s views of science have become very popular. As our later discussion makes plain, Polanyi sees Popper’s reputation as undeserved because he believes Popper does not deeply understand scientific practice. There are other critical comments on Popper’s views in other Polanyi publications as well as unpublished Polanyi materials.

⁷As suggested above, Sanders has two carefully argued chapters (159-225) that examine in detail Allan Musgrave’s 1969 Popper-supervised dissertation which sharply attacks Polanyi. Sanders shows many of the charges of Popperians, if not Popper himself, reflect a limited understanding of Polanyi’s philosophical perspective. In fact Polanyi’s account of science is in many ways more nuanced than Popper’s and does share ground with Popper on certain points. Sanders points out that Polanyi often turned to the history of science as the arena of practice to make a case for his views. See also Sanders’ article “Popper, Polanyi and Methodology: A Reply to S. Richmond,” (*TAD* 22:2 (1995-96): 27-35) for further responses to Popperian views. Others have also provided comparisons between Polanyi and Popper’s views on particular topics; see, for example, Struan Jacobs, “Tradition in a Free Society: The Fideism of Michael Polanyi and the Rationalism of Karl Popper,”

TAD 36:2 (2009-2010): 8-25.

⁸In *Karl Popper: The Formative Years, 1902-1945: Politics and Philosophy in Interwar Vienna* (NY: CUP, 2000), Malachi Hacoheh has recognized that there was a connection between Michael Polanyi and Popper but he says only that “In the postwar years, he [Polanyi] and Popper would write each other as if they were best friends, but neither thought much of the other’s philosophy” (215). Hacoheh’s book is hereafter cited by author, year, and page in the text.

⁹Both were non-observant and critical of Zionism. See Hacoheh’s 1999 article, “Dilemmas of Cosmopolitanism: Karl Popper and Central European Culture” (*Journal of Modern History*, 71: 105-149) and Paul Knepper’s 2005 essay, “Michael Polanyi and Jewish Identity” (*Philosophy of the Social Sciences*, 35(3): 263-293) as well as “Polanyi, ‘Jewish Problems’ and Zionism” (TAD 32:1 (2005-2006): 6-19).

¹⁰The authors acknowledge a degree of overlap between the following discussion and their article “Relations between Karl Popper and Michael Polanyi” published in *Studies in History and Philosophy of Science* (42 [2011]: 426-435). Both essays analyze archival correspondence and use it to frame the discussion. The SHPS discussion is briefer and more narrowly focused on issues likely of interest to an audience primarily concerned with the history of the philosophy of science. This essay’s broader discussion presumes an audience with interest in Polanyi’s thought. Section II, B (not part of the SHPS essay) treats the role of Hayek as a bridge figure between Polanyi and Popper. Section III, B and C (not part of the SHPS essay) provide a thorough analysis of the differences in social-political philosophy of Polanyi and Popper, focusing particularly on the way one can see Polanyi’s political philosophy emerging in writing such as his Preface of LL, where he rejects Popper’s “open society.” The differences between Popper’s and Polanyi’s philosophy of science, we emphasize here, are not finally independent of their differences in political philosophy. Although both figures were opponents of totalitarianism and supporters of liberalism and scientific progress, such a generality obscures more than it reveals. Here we also include treatment of some other interesting elements (not a part of the SHPS essay) such as Ian Jarvie’s unorthodox reading of Popper’s thought, which indirectly bears on some of Polanyi’s criticisms.

¹¹See Popper’s autobiography (55), which is included in Book 1 of *The Philosophy of Karl Popper*, Paul Aulur Schlipp (ed.), Library of Living Philosophers, Vol. XIV, Books I and II (LaSalle, IL: Open Court Publishing Co., 1974). The Popper autobiography is hereafter cited in the text as Autobiography with page number. Other citations to material in this volume are in parenthesis by author (unless indicated in the context) by book and page.

¹²See Mary Jo Nye’s discussion (“Historical Sources of Science-as-Practice: Michael Polanyi’s Berlin,” *Historical Studies in the Physical and Biological Sciences* 37:2: 409-434) of Polanyi’s emergence as a world class scientist in his thirteen years in Berlin; she argues that many of his later philosophical ideas about science are rooted in his experience in these years. Nye’s very new book, *Michael Polanyi and His Generation: Origins of the Social Construction of Science* (Chicago: University of Chicago Press, 2011) expands this thesis. There are many very interesting points in this book relevant to matters in this essay (including her discussion of Polanyi and Popper); because the book (cited hereafter as Nye 2011) came out just as the final version of this essay was prepared for publication, there are only a few references.

¹³Most of the letters quoted or summarized in this essay are from Box 339, Folder 1 of the Hoover Institution’s Popper Archive. If the date and author are given in the text, the box and folder of these Hoover Institution materials will not be cited in the notes or the text. Letters or other archival material not from the Hoover Institution collection (but from other archival collections) will be cited individually.

¹⁴See Felix Schaffer, “Vorgartenstrasse 203: Extracts from a Memoir” in the volume *Karl Polanyi in Vienna, The Contemporary Significance of the Great Transformation* (Montreal: Black Rose Books, 2000) edited by Kenneth McRobbie and Karl Polanyi’s daughter, Kari Polanyi Levitt for Schaffer’s discussion of

Karl's salon in which Popper participated in Vienna (328-346; see especially 331 where Popper is listed as a visitor to the salon).

¹⁵Karl Popper, *Die beiden Grundprobleme der Erkenntnistheorie: aufgrund von Ms. Aus d. Jahren 1930-33*, Troels Eggers Hansen (ed), (Tubingen: Mohr, 1979). This version attempts to pull together the various early Popper manuscripts. See the further discussion below.

¹⁶Karl Popper, *The Two Fundamental Problems of the Theory of Knowledge*, Andreas Pickel (trans.); Troels Eggers Hansen (ed.), (New York: Routledge, 2009).

¹⁷Hacohen (2000, 196) suggests that the first draft is written from Oct. 1931 to June, 1932

¹⁸Troels Eggers Hansen, who edited the 1979 German version of *Die beiden Grundproblem der Erkenntnisstheorie*, says that there were only four copies of the original manuscript and they were not identical (i.e., changes made in the master did not always get transferred). Also Popper apparently had various drafts of elements that seem to pre-date the late Spring 1932 "finished" first book. Some early materials have been lost. As noted above, there apparently were revisions in the summer of 1932, and the drafting of what is the second book on demarcation came thereafter. It seems safest to say that what Polanyi likely received and possibly reviewed was a lengthy, late manuscript still being revised of the first book focusing on induction. See Hansen's illuminating discussion of the manuscript in his "Editor's Postscript" in the English translation (485-497).

¹⁹We draw on the Polanyi biography (William Taussig Scott and Martin X. Moleski, S.J, *Michael Polanyi, Scientist and Philosopher* [Oxford: OUP, 2005]) here and later and simply cite it in the text as Scott and Moleski with page numbers in parenthesis. Nye's new book (2011) covers some of the same territory with at times interesting different nuances; we reference relevant chapters.

²⁰See Popper's discussion (Autobiography, 86) as well as our comments below. Popper suggests that it was after the March, 1938 Nazi occupation of Austria that he decided "I could no longer hold back whatever knowledge of political problems that I had acquired since 1919" (Autobiography, 90) and decided to put "The Poverty of Historicism" into publishable form. It was first published in *Economica* (at the time edited by Hayek) in three parts: "The Poverty of Historicism, I, *Economica* New Series, Vol. 11, No. 42 (May 1944) 86-103; "The Poverty of Historicism, II, *Economica* New Series, Vol. 11, No. 43 (August 1944) 119-137; "The Poverty of Historicism, III, *Economica* New Series, Vol. 12, No. 46 (May 1945) 69-89. Correspondence in both the Hayek archives and the Popper archives at the Hoover Institution make it clear that Hayek had a hand in re-shaping (editing and perhaps rewriting sections) and getting published both Popper's poverty of historicism essays and *The Open Society and Its Enemies*. Hayek simultaneously is working hard (often behind the scenes) in this period to get Popper appointed at London School of Economics.

²¹Karl Popper, *The Open Society and Its Enemies* (London: Routledge and Kegan Paul, 2002, 1st edition, 1945). Later editions of Popper's publications often were corrected and revised. References in the text in parenthesis (except where noted by the publication date for the 2002 edition) are to page numbers in the revised 1966 two volume edition published by Princeton University Press.

²²Karl Popper, *The Poverty of Historicism* (London: Routledge and Kegan Paul, 1957).

²³John Watkins, in his 1997 "Biographical Memoir of Karl Raimund Popper" (*Proceedings of the British Academy*, 94, 645-684), provides an extended and amusing account of how Popper came to be appointed, with Hayek's help, to a position at London School of Economics as well as Hayek's role in getting some early Popper writing published (657-660). Popper's sense of gratitude for Hayek's help was certainly due. As noted above, the Hayek-Popper correspondence clearly indicates the way in which Hayek is something of a *deus ex machina*-like presence shaping Popper's early career. Very late in his life, in an appreciative letter to Hayek, Popper suggests that, although Hayek is only three years his senior, he thinks of Hayek as a father figure (Popper to Hayek, April 30, 1984, Box 305, Folder 17 in the Popper Archives).

²⁴Eric Howard's yet unpublished paper "Why Didn't Hayek Finish Reading *Personal Knowledge*? An Investigation Into the Methodological and Philosophical Relationship Between Friedrich Hayek and Michael Polanyi," (presented Nov. 21-23, 2004 at the Southern Economic Association Annual Meeting, New Orleans, and re-presented in capsule as "A Joint Project: The Unique Epistemic Project of Friedrich Hayek and Michael Polanyi" at the "*Personal Knowledge At 50*" Conference at Loyola University, Chicago, on June 14, 2008) quotes a James M. Buchanan interview with Hayek in which Hayek acknowledges that he first met Polanyi at the 1938 Paris conference. Bruce Caldwell notes (*The Collected Works of F. A. Hayek*, vol. x *Socialism and War, Essays, Documents, Reviews* (Chicago: UC Press, 1995) in his Introduction that the conference was "to inquire into the prospects for democratic liberalism" (46).

²⁵See Hayek's letters to Polanyi, 1 May, 1941 (Box 4, Folder 6) and 1 July 1941 (Box 6, Folder 7) in Papers of Michael Polanyi, Department of Special Collections, University of Chicago Library. The earlier letter implies that Hayek asked Polanyi to do a review of Crowther's book which Hayek is reading and that Hayek has requested a review copy for Polanyi. The later letter suggests that if Polanyi thinks the book is "a good opportunity to discuss the general problems involved in his thesis, a review article would be *most* welcome." Hayek speculates about which issue the Polanyi piece can be published in and suggests that the November issue is a good target for a longer review article. This second letter makes it quite clear that Polanyi and Hayek have joined forces to oppose the set of British writers promoting "planned" science.

²⁶Michael Polanyi, "The Growth of Thought in Society," *Economica* 8, (1941): 428-456. See his discussion of "dynamic order," a term he adapts from Wolfgang Kohler (435ff), as well as "spontaneous ordering" (431-433) and "public liberty" (438ff) which we summarize below.

²⁷In "Faith, Tradition and Dynamic Order: Michael Polanyi's Liberal Thought from 1941-1951" (*History of European Ideas* 34 (2008): 120-131), we have carefully laid out the political philosophy (i.e., Polanyi's complex vision of liberalism) that begins to take shape in Polanyi's 1941 essay "The Growth of Thought In Society" but is amplified in *Science, Faith and Society* (1946) and the material that becomes *The Logic of Liberty* (1951) as well as some separately published essays in the forties. When the material of this period is viewed together, one can see the development of Polanyi's political philosophy or what might be called his social vision. We emphasize in the following discussion Polanyi's ideas about "public liberty" which he distinguishes from private freedoms; public liberty is a key element of dynamic orders. Polanyi's constructive political philosophizing is woven with an interesting historical/cultural analysis of the development of European and American societies that comes together in this period. See especially our discussion, 126-131.

²⁸Although he does not use the term "public liberty" in it, Polanyi's essay "Foundations of Academic Freedom," published in several places in 1946 and 1947 and then re-published in revised form as the third chapter of LL (1951), treats ideas about freedom that lead to our conception of "academic freedom." Much of the discussion is akin to discussions of "public liberty" in his 1941 essay and in other sections of LL. Polanyi tries to sort out what seems to be the nature and conditions in science and scholarship in general that make "the co-ordinative principle" (LL, 34) work.

²⁹Ian Jarvie (*The Republic of Science, The Emergence of Popper's Social View of Science 1935-1945* [Amsterdam: Rodopi, 2001]) contends that Popper "strived mightily in the final revisions of the notes to *The Open Society* and "The Poverty" [the poverty of historicism material published in *Economica*] to stress his areas of agreement with Hayek on the nature of the social" (89). In general, Jarvie provides an interesting interpretation of Popper's thought, which he presents as a counter to the mainstream view of Popper. Jarvie's interpretation is worth outlining here because of the way in which it puts Popper's and Polanyi's thought on the same page. Jarvie argues that Popper's early philosophy of science "has a social turn at its centre" and thus "contains a rudimentary sociology of knowledge" (9); Popper's interest in social and political issues predates his interest in the physical sciences (33). The poverty of historicism material and *The Open Society and Its*

Enemies, (i.e., Popper's wartime political philosophy) extend Popper's early "analysis of the social aspects of scientific method" (10). Popper did not, Jarvie ultimately claims, clearly understand that the views developed in his wartime political philosophy would require that he modify his view of science as a model for a rational politics in society. Jarvie argues that the poverty of historicism material is concerned with "methodological similarities and differences between the natural and social sciences" (142). *The Open Society and Its Enemies* is a "companion piece" in that it concerns "the demarcation between pseudo-social science (historicist prophecy) and genuine social science, including social technology" (142). Jarvie thus holds Popper develops a type of sociology of science that is a "sociology of methodological rules" (85): that is, Popper "sociologised" the "problem of demarcation" and offers a "constitutional model" of science as a special institution insofar as it practiced certain "methodological rules" (84). He suggests that "Popper abandoned falsifiability as an intellectual criterion and instead embedded it within a methodology, a set of decisions or choices about how to conduct enquiry articulated as rules" (79). Jarvie's reading of Popper puts Popper closer to Polanyi. In fact, Jarvie argues that Popper and Polanyi are generally aligned in their sociological accounts of science, although there certainly are important differences; he makes an interesting case for this in his concluding chapter (212-231) focused around a comparison of Popper's republic of science and Polanyi's republic of science. Jarvie's account of Polanyi would be much richer and more nuanced if he reviewed some of Polanyi political philosophy written up until the publication of *The Logic of Liberty* (1951). We argue below that Polanyi's liberal political philosophy, emphasizing the growth of thought in science and other dynamic orders rooted in public liberty, is at odds with Popper's "open society." Polanyi becomes clear about this difference at the same time that his criticisms of Popper's falsificationism become sharply focused and the elements of his constructive "fiduciary philosophy" jell. The material in his 1951 LL (particularly the Preface), in his 1951 and 1952 Gifford Lectures and his 1952 paper "The Stability of Belief" (a revised Gifford Lecture delivered to the London Philosophy of Science Group chaired by Popper) as well as the Popper-Polanyi correspondence make this clear. Jarvie apparently does not notice that Popper's "open society" and Polanyi's account of liberal society are at odds.

³⁰Hayek's review appears in May of 1941 and Polanyi's essay is in the Nov. 1941 *Economica*. Hayek's review discusses not only Polanyi's book but Colin Clark's 1939 *A Critique of Russian Statistics*, a book which updated Polanyi *USSR Economics—Fundamental Data, System and Spirit* (1935). The comments on the other essays in *The Contempt of Freedom* are brief and general and point to Polanyi's criticisms of planning, his interest in liberty and his attacks upon what Hayek regards as the treason of the intellectuals (i.e., Bernal and the Webbs), popular Hayek themes. It is possible that "The Growth of Thought in Society," Polanyi's vision of liberal society, was intended to broaden the horizons of some of his allies, like Hayek, as well as his opponents. It is an essay that emphasizes the growth of thought and its rootedness in many dynamic orders that protect "public liberty." It is an essay that articulates Polanyi's very specific ideas about "totalitarianism," whether communist or fascist, as lacking public liberty. Some of his later letters to Hayek make clear that Polanyi regarded Hayek as a very effective trench fighter and an Austrian school economist but not always a thinker whose broader philosophical vision he could agree with. Polanyi often seems to prefer constructive philosophizing in which he articulates his own ideas rather than direct critical engagement with those with whom he sometimes disagrees like Hayek.

³¹See Scott and Moleski's discussion of the Society (203). Some of the Polanyi letters to Popper in the Hoover Archives (e.g., 20 July 1950 and 21 September 1951) refer to Mont Pelerin conferences: the 20 July 1950 Polanyi letter to Popper anticipates meeting Popper at the upcoming conference in the Netherlands; the 21 September 1951 Polanyi letter to Popper laments Popper's absence at a recent conference; Polanyi says Popper's voice was needed in a philosophic discussion of totalitarianism. Some of the correspondence between Polanyi and Hayek in both the Polanyi archives and the Hayek collection in the Hoover Archives indicates that Polanyi at one point considered withdrawing from the Mont Pelerin group as a result of differences with

Hayek and others.

³²For example, Ilona Polanyi's 17 August (year unclear) letter to Karl Popper and his wife asks if she can stay with the Poppers when she is in London for a week, if other arrangements don't materialize. Karl Polanyi's letter to Popper, apparently written on June 14, 1947, discusses his decision to teach at Columbia University rather than Chicago; Ilona Polanyi's August 2, 1949 letter to Popper asks him to intervene on behalf of a LSE student who has been treated unjustly by the Registrar.

³³Popper provides his itinerary in *Autobiography*, 96.

³⁴Watkins confirms that, after *The Open Society And Its Enemies* was published in mid-November of 1945, it was widely discussed even before Popper arrived in January 1946; although Watkins does not mention the trip to Manchester, Popper had several opportunities to make presentations soon after he arrived (Watkins, 660).

³⁵This is likely to be Popper's "Logic without Assumptions," *Proceedings of the Aristotelian Society, New Series, Volume XLVII (1946-47)*, [London: Harrison and Sons, Ltd., 1947]: 251-292. The other paper in question may have been Popper's "New Foundations for Logic," *Mind*, *New Series* 56:223: 193-235.

³⁶This October 11, 1949 letter from Polanyi to Popper is from the Papers of Michael Polanyi, Box 5, Folder 6, in the Department of Special Collections at the University of Chicago.

³⁷A October 13, 1949 postcard confirmed a noon meeting with Popper in his office to be followed by lunch. A November 9, 1949 letter thanked Popper for his help which was apparently concerned with a point in logic: "It all came very much as I was led to expect from what you said. Newman [a colleague - Professor of Pure Mathematics—of Polanyi, at Manchester—see <http://www.turing.org.uk/turing/scrapbook/manmach.html>] and Turing declared that they could construct a machine which would extend indefinitely the production of Godelian sentences." Polanyi indicated that he was now thinking more carefully about the question under consideration and, having written up his views, "will send you a copy in the hope that you might let me have your reaction to it." The letter ended with a comment about a projected trip to London in the middle of the month and the promise of a phone call to Popper's home to set up a common meal.

³⁸The correspondence from the late forties discussed here and that from the early fifties discussed below imply that Polanyi and Popper are somewhat comfortable with each other and are interested in each other's work. In his posthumous *A Fragment of a Sociological Autobiography* (Steven Grosby, ed., New Brunswick and London: Transaction, 2006), Edward Shils reported that he met Polanyi in the autumn of 1946 (78). He found that Polanyi was uninterested in the problem of secrecy (one of Shils' interests which Polanyi declined to discuss with him) and that Polanyi "did not want to hear anything about Karl Mannheim or Karl Popper" (79). Shils reports that, after teaching an LSE seminar with Popper, he suggested to Polanyi that Popper might join them for dinner but Polanyi "was deaf to that. Thereafter I ceased to mention Popper to him. I got on exceptionally well with him" (79). Although Shils does not indicate the date of his seminar, it very likely was in late 1946 or very early in 1947 since Mannheim dies January 9, 1947. Shils' comment implying Polanyi wanted to avoid Popper in this period is puzzling, given the ambience of letters of 1948, 1949 and early 1950 discussed above and below. Before Mannheim's death, Polanyi had been working with Mannheim (the Routledge series editor) on the originally projected version of LL. But there are few records of Polanyi's contact with Mannheim after he signs his book contract in the fall of 1945. See the present authors' detailed discussion of Polanyi's relationship with Mannheim in "Polanyi and Mannheim," *TAD* 32:1 (2005-06): 20-43.

³⁹It is unclear what the referenced Popper paper on mind and machine is, but it seems likely that Polanyi's reference to his own preoccupation with this area is a reference to his participation in an October 1949 Manchester conference entitled "The Mind and the Computing Machine." Polanyi presented a paper "Can Man be Represented by a Machine?" which Scott and Moleski indicate "drew on the ideas of Godel

and Tarski to show that the use of intuition and judgment, which is essential to even the most formal of logical procedures, cannot be represented by any kind of mechanism” (215). In 1951, Polanyi published “The Hypothesis of Cybernetics” (*The British Journal for the Philosophy of Science*, II Feb. 1951: 321-325, in SEP, 309-312) which was related to his 1949 work. Scott and Moleski describe this essay as “part of a discussion on calculators simulating minds... Polanyi pointed out that all our formalized thinking rests on unspecifiable judgments about symbols and operations” (217). Popper chaired the editorial committee of *The British Journal for the Philosophy of Science* at the time the decision was made to publish this paper in the journal. At the beginning of “The Hypothesis of Cybernetics” Polanyi said his “notes formulated some time ago on the question whether machines can be said to think may supplement the discussion of cybernetics conducted in this *Journal*” (SEP, 309) and he listed articles that had appeared in earlier issues of the *Journal*, including an article by Popper in vol. 1.

⁴⁰“Economic and Intellectual Liberties,” *Zeitschrift für die gesamte Staatswissenschaft* v. 106, n. 3: 411-447. If Popper carefully read this essay when he proposed his revisions, he certainly would have understood that Polanyi was developing ideas about the important function of “public liberty” in systems of spontaneous order in society. The content of Polanyi’s later June 7, 1951 letter to Popper (discussed below) indicating that his account of liberalism is different than that of Popper may not have been news to Popper.

⁴¹Notes have been updated to reference materials republished in *The Logic of Liberty*; that is, the journal version cites the original publication information from which much of the material in the book was drawn.

⁴²A copy in Edward Shils’ files of a January 16, 1950 letter from Polanyi’s secretary to Popper indicates “Economics and Intellectual Liberties” is enclosed for Popper. In a January 31, 1950 letter to Shils, Polanyi indicates that Popper has provided minor corrections for “Economics and Intellectual Liberties.” Polanyi advised Shils that he had the only copy and he asked Shils to return the essay so that he could make changes. These letters imply that early in 1950 Polanyi is revising “Economic and Intellectual Liberty” and probably is also in the final stages of preparing the materials to be published in *The Logic of Liberty*. An undated (but almost certainly 1949) Polanyi letter to Shils about the topic of Polanyi’s upcoming spring 1950 lectures at the University of Chicago says “It would be easy to talk on the lines: ‘The Structure of Liberty.’ But my true interest lies in getting my basic position clear which would be hinted in a title like ‘Towards a post-critical age.’” It is of interest that the January 31, 1950 letter to Shils identifies Popper’s revisions as minor. Unless Popper made further suggestions after January 1950 for revisions to “Economic and Intellectual Liberties” (which is almost identical to “The Manageability of Social Tasks”), Polanyi’s later June 7, 1951 letter to Popper seems to exaggerate the importance of Popper’s contribution to revising what became the last chapter of *The Logic of Liberty*. All the correspondence with Shils noted above is in The Edward Shils Archives, Box 4, Michael Polanyi Folder, Department of Special Collections, University of Chicago.

⁴³See note 29 above for the broader outline of Jarvie’s reading of the centrality of Popper’s social philosophy and the tension between this social philosophy and Popper’s philosophy of science.

⁴⁴As noted above, “closed societies” are linked also to societies with magical worldviews. As one of our reviewers pointed out, this may have been something Polanyi found particularly objectionable in Popper’s book. At the least, as the discussion below makes clear, in one of his Gifford lectures and in the revised version of this lecture presented in London in 1952 in the Philosophy of Science group chaired by Popper, Polanyi overtly links the operation and stability of Azande belief (drawing on Evans-Pritchard’s study) and scientific belief.

⁴⁵Popper’s “open society” embraces something closer to what Berlin called a “negative” concept of freedom, focusing on the absence of restraint, whereas Polanyi’s “public” liberty is closer to a “positive” concept of freedom. See Marjorie Grene’s (*A Philosophical Testament* [La Salle, IL: Open Court, 1995])

discussion of Berlin and Polanyi's views where she summarizes her own very Polanyian "positive" concept of freedom as focused on "being a center of actions, being able to act coherently and consistently in accordance with standards that one willingly accepts, that by implication, one has imposed on oneself" (182). See also D. M. Yeager's "Confronting the Minotaur: Moral Inversion and Polanyi's Moral Philosophy" (*TAD* 29:1 [2002-2003]: 22-48), for discussion of Polanyi's emerging cultural critique and its connection with certain ideas about freedom.

⁴⁶The opening 2 ½ pages of Chapter 12 ("Mutual Authority") of Polanyi and Prosch's 1974 *Meaning* (182-184) returns to issues discussed here concerned with Polanyi's version of liberalism and how it differs from Popper's "open society." That is, Popper's liberalism and the "open society" are overtly criticized in ways that fit with what we have outlined above. There is also another reference (*Meaning*, 214) in Chapter 13 ("The Free Society") to the ways in which an "open society" is not to be confused with a free, liberal society in which there is independence of thought in science, art and law. The material at the beginning of Chapter 12 suggests that Prosch understands the difference in political philosophy with Popper since Prosch is clearly recycling material in LL and perhaps other sources; after the opening pages, this chapter draws on TD, 63-79, as Prosch acknowledges (*Meaning*, xiii). For a more general discussion of Prosch's role in pulling together *Meaning*, see Phil Mullins and Marty Moleski, S.J., "Harry Prosch: A Memorial Re-Appraisal of the *Meaning* Controversy" *TAD* 32:2 (2005-2006): 8-24.

⁴⁷See Polanyi's explanation of the subtitle "Towards a Post-Critical Philosophy" of *Personal Knowledge* (PK, 265), as well as Polanyi's comments about the nature of philosophical reflection (PK, 267). It is noteworthy that in *The Logic of Liberty* (1951) Polanyi speaks of "a new intellectual period, which I would call the post-critical age of Western civilization." He notes that in this new age "liberalism . . . is becoming conscious of its fiduciary foundations and is forming an alliance with other beliefs, kindred to its own" (LL, 109).

⁴⁸Mark T. Mitchell, *Michael Polanyi, The Art of Knowing* (Wilmington, Delaware: ISI Books, 2006): xiii.

⁴⁹Polanyi's letter of 21 September 1951 reported to Popper that one of Popper's objections to Einstein's work on the diffusion constant appeared to be warranted according to new research. Polanyi speculated about future trips to London, hoping "to arrange to meet you . . . for friendly discussion of the many problems in which I would value your advice."

⁵⁰Although the published essay indicates the paper was given March 6, 1952, both research for the Polanyi biography (Scott and Moleski, 220) and recent examination of archival material of the British Society for the Philosophy of Science by Peter Vickers (e-mail to Mullins on 2/12/2010) indicate the meeting was postponed until June 9, 1952. Watkins (668) indicates Popper chaired the session.

⁵¹*Ethics*, 61:1 (October, 1950): 27-37.

⁵²The only manuscript of Polanyi's Gifford Lectures is a text that has in some sections been revised, retyped and re-dated with some dates as late as 1954. Polanyi gave this manuscript to Marjorie Grene in May 1957, and it is now part of the Rare Book, Manuscript and Special Collections Library, Perkins Library, Duke University. A copy of the manuscript is available from the Perkins Library as microfilm #222-1-2, including an instructive introduction by Gerald Smith. A "Syllabus" for Polanyi's First Series of Gifford Lectures, including lecture titles, dates and a one-page précis of each lecture, is available in Box 33, Folder 1, Papers of Michael Polanyi, Special Collections Research Center, University of Chicago Library. Quotations in this paragraph and the next, unless otherwise indicated, are from the Syllabus. As we note below, the date on the Duke manuscript is a few days off the date listed in the Syllabus for the eighth lecture. It seems unlikely that the Duke version of the lecture is a version later than the First Series Gifford lecture even if the date does not agree with the Syllabus.

⁵³In *Personal Knowledge* (1958), the book identified as growing out of Polanyi's Gifford Lectures, in the 1964 Preface to the Torchbook edition, Polanyi says (in the same spirit as these suggestions in the Gifford material) that he faced in his book "the task of justifying the holding of unproven traditional beliefs" (ix).

⁵⁴Quotations from and references to the essay use the *British Journal for the Philosophy of Science* (3:11 [November, 1952]: 217-232) copy for citations since it provides pagination. The published essay minus pagination is on the Polanyi Society web site (<http://www.missouriwestern.edu/orgs/polanyi/mp-stability.htm>). Particularly some elements at the beginning and the end of the published essay differ from the Duke version of the eighth lecture of the First Series Gifford Lectures whose typescript, interestingly, dates the lecture as May 23, 1951 rather than the May 30, 1951 date provided by the First Series Syllabus. The longer lecture apparently had to be cut and Polanyi elected to trim discussion of topics like deduction. But the overlap is, nevertheless, great between the Duke version of the lecture and the later published essay. Clearly, Polanyi embellished the later London rendition of the lecture, making the essay somewhat more dramatic by including psychoanalysis and Marxism in the discussion as "conceptual frameworks" (two topics Popper has also criticized) as well as scientific beliefs and Zande beliefs.

⁵⁵Early in his essay, Polanyi makes clear that "I must call science a belief which I share. This accreditive expression can be expanded indefinitely by giving my reasons for believing in science and elaborating the nature of this belief; but it can never be exhaustively justified by statements of fact" (219). This leads to a short discussion of the terminology used in the essay. Clearly, Polanyi used the term "belief" rather than "knowledge" quite deliberately; he says this choice is in order to emphasize "the intention of keeping always open in our minds a broad and patent access to the personal origins of our convictions" (219). He acknowledged that he "must pass over the epistemological problems," which may imply that he recognizes and dissents from the long tradition of thought in Western philosophy of separating "belief" and "knowledge" (219). He suggested his "conceptual reform" will "eliminate the difficulties inherent in the various theories of truth, whether they rest on correspondence, coherence or utility" (219). He ends his short digression on terminology by saying "this general statement of my position may induce readers to bear with this discourse a little longer, as I proceed with it" (219). All of this suggests that Polanyi was quite aware that his paper developed views that philosophers more in the mainstream would find objectionable.

⁵⁶The centrality of discovery is clear from Polanyi's earliest writing (see some of the essays in *The Contempt of Freedom* as well as *Science, Faith and Society* and some of the essays in *The Logic of Liberty*) in which science is portrayed as a growing organism of thought whose growth depends on independent researchers to add to the existing ideas by their innovative new theories. Later discussion in "The Stability of Beliefs" also focuses on the power of discovery in expanding the scientific framework (see below).

⁵⁷Polanyi extends his discussion of Arrhenius' theory of electrolytic dissociation to show how it was quickly accepted and "its further history offers an excellent example for the extraordinary stability of scientific conception in the face of invalidating factual evidence" (228).

⁵⁸Polanyi's terminology is rather loose. He seems to regard a "conceptual framework" as a more or less cognitive scheme (coherent to those who dwell in it) that is rooted in a very broad set of shared beliefs most of which at any given time are implicit and not before the mind's eye of a person. Confident use of language brings with it such an outlook and clearly not all outlooks can be reconciled. In this article, "conceptual framework" is, of course, terminology that Polanyi thinks can be applied to science, psychoanalysis, Marxian views as well as Zande belief and practice. Polanyi later works out his account of subsidiary and focal awareness (he begins work on this in his Gifford Lectures) and tacit and explicit knowledge and the from-to structure of knowing; such ideas give a firmer ground to notions about a "conceptual framework." Some of these refinements grow out of Polanyi re-casting of ideas found in Gestalt accounts of perception. Polanyi does cite anthropological literature in "The Stability of Beliefs" and some other articles from the same period (e.g. "Scientific Beliefs"

cited above) and, since most of “The Stability of Beliefs” is incorporated into *Personal Knowledge* (1958), citations appear here also. But the development of his own Gestalt-related account for the operation of “implicit beliefs” and “conceptual frameworks” leads Polanyi later frequently to point to Gestalt literature rather than primarily anthropology and accounts of language as an important background source of his views. See the discussion in Mullins’ “Michael Polanyi’s Use of Gestalt Psychology” in *Knowing and Being, Perspectives on the Philosophy of Michael Polanyi* (Tihamer Margitay [ed.], Newcastle upon Tyre: Cambridge Scholars Press, 2010), 10-29.

⁵⁹As we have noted, Polanyi uses Evans-Pritchard and the Azande in his First Series Gifford Lectures but also in his 1950 essay “Scientific Beliefs.” It is unclear when Polanyi became interested in Evans-Pritchard’s study but, as we have noted, Popper’s *The Open Society and Its Enemies* picks out tribal societies as typifying closed societies. We have suggested above that it seems likely that Polanyi found misleading the contrast between closed and open societies that Popper uses. Polanyi’s point in “The Stability of Beliefs” is that belief works in much the same way in such a “closed society” like Zande society and in an “open society” such as the modern republic of science.

⁶⁰At some point after the publication of “The Stability of Beliefs,” Polanyi worked out distinctions between “critical,” “uncritical” and “acritical.” What he was focusing on in the 1952 paper is *implicit* belief frameworks which he would later suggest are “tacitly held” and, insofar as they continue to be used or “dwelt in,” are “acritical.” This distinction Polanyi worked out reasonably clearly in *Personal Knowledge* (264ff).

⁶¹Polanyi here qualified this view somewhat when he suggested that adherents may lose faith in their framework, sensing “that its powers were excessive and specious” (218).

⁶²See *Logic of Scientific Discovery*, pp. 82-84 where this is the English translation used for the German in the letter.

⁶³“Faith in reason” is a major motif in Popper’s reading of fifth century Athens in his chapter “The Open Society” in *The Open Society and Its Enemies*. He, for example, proclaims “the new faith in reason, freedom and the brotherhood of all men” as found in Athens as “the only possible faith, of the open society” (1966, 184).

⁶⁴Jarvie also has noted this Popper letter which he links to his claim that Popper’s interest is in the “social practices [of science] and that institutionalizing these in articulated rules facilitated debate” (77). Jarvie affirms that Popper in his letter conceded that a structural difference “could be epicyclically explained away”: “It seems that he is saying the structural difference lies just in the system eschewing such epicyclical and convenient evasions” (77, note 30).

⁶⁵It is of interest that Popper focused only on applying or abstaining from applying the “epicycal method” (i.e., on Polanyi’s second element, the self-expanding capacity of interpretive systems) and that he seems to take this to be the key to affirming or denying “structural differences.” Popper’s letter does not take up Polanyi’s claims (1) that objections (such as those of Evans Pritchard to Zande belief) are met successfully because they can be addressed one by one (i.e., the principle of circularity or the way other elements of a belief system undermine single objections, Polanyi’s first point) or (2) that a “principle of suppressed nucleation” operates in a system of belief to prevent alternative conceptualization and the accumulation of evidence based in such concepts.

⁶⁶Popper’s point, as one of our reviewers suggests, may be a simple affirmation of the idea that his critical rationalism holds there is a special position, one not outside the system of belief being examined, from which reason and belief can be judged. But if this is Popper’s argument, it is an argument that simply ignores the issues Polanyi is trying to raise.

⁶⁷Brian Magee, *Confessions of a Philosopher* (London: Phoenix, 1998): 231. See also Joseph Agassi, *A Philosopher’s Apprentice*, (Amsterdam: Rodopi, 2008), 33 and 287).

⁶⁸See Yeager's discussion (cited above) tracing the development and articulation of this Polanyi theme.

⁶⁹"The Stability of Beliefs" was incorporated by Polanyi in subsections ("Implicit Beliefs," and "Three Aspects of Stability") of chapter 9 of *Personal Knowledge* (286-292), with some elements included in other chapter sections. Polanyi in *Personal Knowledge* criticized falsificationism, but he mentioned Popper by name only once and that was in the context of explaining the difficulties associated with defining mathematics, noting a point made by Popper in an article of 1951 that for each significant mathematical theorem inferred from a set of selected axioms an infinite number of trivial theorems can also be derived (188).

⁷⁰There has been an extensive discussion of (1) how much Kuhn drew, consciously or unconsciously, from Polanyi, and (2) how much philosophical ground Kuhn and Polanyi actually share. The following is a list (not an exhaustive one) of recent literature treating one or both of these issues: Struan Jacobs, "Michael Polanyi and Thomas Kuhn: Priority and Credit," *TAD* 33:2 [2006-2007]: 25-36; Struan Jacobs, "Michael Polanyi, Tacit Cognitive Relativist," *The Heythrop Journal* XLII (2001): 463-479; Struan Jacobs, "Polanyi's Presagement of the Incommensurability Concept," *Studies in History and Philosophy of Science* 33 (2002): 105-120; Maben Poirier, "A Comment on Polanyi and Kuhn," *The Thomist* 53 (1989) 259-279; Struan Jacobs, "Two Sources of Michael Polanyi's Prototypal Notion of Incommensurability: Evans-Prichard on Azande Witchcraft and St. Augustine On Conversion," *History of the Human Sciences* v. 16, n. 2 (2003): 57-76; Maben Poirier, "The Polanyi-Kuhn Issue," *TAD* 33:2 (2006-2007): 55-65; Aaron Milavec, "Public Recognition, Vanity, and the Quest for Truth: Reflection on 'Polanyi vs. Kuhn'" *TAD* 33:2 (2006-2007): 37-48; Martin X. Moleski, S. J., "Polanyi vs. Kuhn: Worldviews Apart," *TAD* 33:2 (2006-2007): 8-24; Richard Henry Schmitt, "Darwin, Kuhn, and Polanyi: A Comment on 'Polanyi vs. Kuhn: Worlds Apart'" *TAD* 33:2 (2006-2007): 49-55. John Preston ("Feyerabend's Polanyian Turns," *Appraisal*. Vol. 1 Supplementary Issue, Part I 1997: S31-36) analyses similarities of Polanyi and Feyerabend and possible debts of Feyerabend to Polanyi. He says that Feyerabend acknowledged that he at some unknown point read and was influenced by "The Stability of Beliefs" (S30). He thinks some elements of Polanyi's case "prefigure not only Feyerabend's famous 'theoretical pluralism,' but also his argument for it" (S31). Preston, however, thinks Feyerabend's early ideas are something of a muddle drawing on several sources; his later thought rejects certain of Popper's ideas, better absorbs certain of Polanyi's claims as well as those of Mach and is more influenced by historical case studies (S32-35). One might also include Imre Lakatos in this group. Stephania Jha's 2006 essay cited above (note 5) treats in some detail links between Polanyi and Lakatos. See also Richard Schmitt's *TAD* review (34:1 [2007-2008]: 51-53 of the Jha essay and the full issue of *Perspectives on Science* devoted to Lakatos' thought.

⁷¹Clearly some cooperation and concern continued between Polanyi and Popper. One of the most interesting small projects was Polanyi's unsuccessful effort to recruit Joseph Agassi, a member of Popper's circle, to come to Manchester in the mid-fifties. In Polanyi's letter to Popper of December 9, 1954, he notes, "I am writing to Agassi to tell him that I am still as keen as ever to proceed with the project of getting him to Manchester next year." Agassi later makes clear that he was a critical admirer of Polanyi's thought; he dedicated his 1981 essay-collection, *Science and Society* (Dordrecht: D. Reidel), "to the memory of Michael Polanyi" (xx). See also Agassi's more recent comments (*A Philosopher's Apprentice* [Amsterdam: Rodopi, 2008], where he declares Polanyi and Popper the "two greatest and clearest philosophers of the mid-twentieth century" (143). He comments later that "the dedication of my *Science and Society* to his [Polanyi's] memory is expiation to some extent," noting that Polanyi "was a model gentleman, a truly open person" who offered him "an assistantship before Popper did" and "wanted me to help him as he put his celebrated *Personal Knowledge* (sic) in its final shape" (179).

⁷²Jha's 2006 article (cited above) treating Lakatos' "Polanyian turn" (342) points out that this Polanyi letter was written after Popper instructed Lakatos to "dis-invite Polanyi" (329) to a 1965 symposium that Lakatos organized to discuss Popper and Kuhn's views. Jha argues Lakatos' attempt to include Polanyi indicated that Lakatos "wanted an open debate between them" (331).

⁷³Moleski kindly drew our attention to this letter (Box 8: Folder 13, Papers of Michael Polanyi, Special Collections Research Center, University of Chicago Library) and advised that Polanyi opposed Popper's election to the Royal Society (see below).

⁷⁴Box 10, Folder 6, Papers of Michael Polanyi, Special Collections Research Center, University of Chicago Library.

REVIEWS

Hubert Dreyfus and Sean Dorrance Kelly, *All Things Shining: Reading Western Classics to Find Meaning in a Secular Age*. New York: Free Press, 2011. Pp 254. ISBN 978-1-4165-9615-8, \$26.00 hb. ISBN 978-1-4165-9616-5, \$15.00 pb.

All Things Shining, a philosophical book written for a popular audience, is an ambitious project which diagnoses a core contemporary problem, uncovers its literary pathology, and writes a curative prescription. The problem is nihilism and “the burden of choice.” Its pathology developed from individualistic tendencies in the writings of Aeschylus, Augustine, Luther, Descartes, Kant, Nietzsche, as well as others. Finally, the prescription is to develop a sense of the sacred in terms of a public mood, especially through the development of skills. Ultimately, whether or not Dreyfus and Kelly’s account is convincing depends upon whether or not the methodology they employ, namely a phenomenological approach to literary analysis, is suited to their goals. Unfortunately, one of the interesting parts of the project, a defense of communitarianism as a counter to liberal individualism, is undermined by their particular use of the phenomenological approach.

The communitarian goal is an implicit one and the evidence for it does not appear until late in the book. In their acknowledgements at the *end* of the narrative, Dreyfus and Kelly point out that the book was the result of encouragement by Sandel and Taylor to address polytheism in a seminar on Taylor’s book, *A Secular Age*. One goal of Taylor’s book is the communitarian hope of bringing a halt to the corrosive effects of liberalism, and Taylor does this by discussing the importance of God in our lives despite Nietzsche’s oft-repeated claim that God is dead. *All Things Shining* has a similar goal but is less enthusiastic about whether monotheistic views can succeed in returning meaning to our lives. This is reflected throughout

Chapter 6 where Dreyfus and Kelly interpret the title character of Melville’s *Moby Dick* as standing in for a monotheism that is empty of meaning. It is also in this chapter that their communitarian solution is suggested in their interpretation of two different scenes in Melville’s novel. In the first, Ishmael, along with others, is working with the spermaceti found in the heads of sperm whales. Their interpretation deserves to be fully quoted:

Ishmael finds himself, along with several other shipmates, assigned to the task of “squeezing these lumps back into fluid.” During this task he sometimes finds himself unwittingly squeezing his co-laborers’ hands. His description of this kind of loving, *communal* experience seems to be the essence of Melville’s understanding of the Christian mood of agape, or Christian love for others (167, emphasis added).

They go on to point out that this secular ritual appropriates the Christian view of the sacred. This scene is then contrasted with that of Pip’s experience of being a castaway. According to Dreyfus and Kelly, once becoming a castaway, “Pip has lost all his connections with other men and the *Pequod*, which is his center. The ship is the stable, human thing that grounds one on the infinite sea. It is the loss of everything connected with this final human thing that Pip finds horrifying” (177). In other words, a complete disconnection from others is the source of a truly horror-filled life. Communitarians such as MacIntyre, Sandel, Taylor and Walzer see liberalism and its commitment to atomistic individuals as leading to loss of connections with others and is one of the core communitarian critiques of liberalism. For Dreyfus and Kelly, however, a shared ritual, like that of squeezing spermaceti, can be the foundation that avoids such a life. But, just as this evidence provides the seeds for

a communitarian reading of *All Things Shining*, these seeds are mixed with individualistic weeds built into their phenomenological method. To see this, however, requires that we look at the organization of the book, how they set up the problem of the contemporary age, and their discussion of the term ‘sacred’.

The circuitous structure of *All Things Shining* somewhat obscures any case it attempts to develop. Consider that an explicit claim of the book is that external constraint is an effective bulwark against nihilism created by individualism. The very nature of the book, however, is an example of how external constraints fail to limit interpretation and loss of value. There is an external constraint on how to describe the organization of the book, namely the number of chapters it has. But the structure of the book allows for multiple descriptions, suggesting that the external constraint has limited effectiveness in restraining individual interpretations. First consider a topical arrangement. Chapter 1 and 2 focus on the joint problems of nihilism and the burden of choice that the contemporary West confronts. Chapter 3 describes a polytheistic alternative to the burden of choice and nihilism found in Homer but not in Classical Greece or after. Chapters 4 and 5 trace the “secret history of the West” in terms of growing individualism and an inward-looking self-conception from Aeschylus to Kant. Chapter 6 attempts to show that Herman Melville, in *Moby Dick*, not only understood and agreed with Dreyfus and Kelly about the development of nihilism, but also tried to suggest a community-based solution. Chapter 7 outlines how *poiesis*, or the craftsman’s skill in bringing out the best in an object, can be the foundation for a meaningful way to confront the West’s current situation. Now consider a methodological arrangement. Chapters 1 and 7 are conceptual analyses while chapter 2-6 are literary analyses. Which description of the book’s organization should be favored? The answer will depend on individual interests. Given this, and the fact that there are more than just these two descriptions of the book’s organization, the ability for external constraints to reign in individualism is called into doubt.

As already mentioned, the problem of living without effective external constraints is the topic of the first two chapters of the book. For brevity, only the first chapter will be discussed. This chapter describes the West’s burden of choice as having too many life alternatives set before us, but no motivation, or reason, to choose between them. Choice requires a reason, but merely picking means pursuing an alternative for no reason whatsoever. The burden of choice that the West has created for itself is a world where we can do nothing but pick alternatives. Furthermore, picking only allows for two forms of motivation: becoming self-confident or becoming enslaved. The person who is self-confident is one who “is committed to bringing the world into line with his version of how it should be” (5). The self-confident person sees external reality as perfectly malleable, as providing no constraints on choice. In the end, reality is just an extension of the self. The enslaved, on the other hand, is “the person who makes no choices about how to act because he is enslaved by obsessions, infatuations, or addictions” (6). For this person, there is no self, merely a set of desires that cannot be resisted. For Dreyfus and Kelly, neither alternative is acceptable. One point of the book is to find another way to deal with the burden, one that is not based in liberal individualism which forms the foundation of the self-confident and enslaved options. The solution will be to find an external source that limits the alternatives and provides reasons for choice. That external source is the authority of the sacred.

Unfortunately, *All Things Shining* does not pay sufficient attention to the notion of sacredness. What do Dreyfus and Kelly mean by “sacred”? In the solution chapters of the book (3, 6, and 7), the “sacred” is described as a mood that is publicly shareable; it involves gratitude and is related to human excellence. Beyond this, it is hard to say what the authors mean by “sacred.” Admittedly, given the fact that Dreyfus and Kelly want to support a polytheistic version of the sacred, it would create some tension for them to provide a definition with individually necessary and jointly sufficient conditions. But this vagueness means that the notion of the sacred cannot perform its key function—it cannot actually provide meaningful

constraints on choice. In fact, it seems as if the meaning of “sacred” is something we merely pick and do not rationally choose.

The closest that Dreyfus and Kelly come to explaining the meaning of “sacred” is in the seventh chapter’s discussion of *poiesis*. This term is related to acts of creation, but not limited to the mere technical act which brings something into existence. Dreyfus and Kelly exploit this notion as related to craft skills which bring out the best in the objects that are at hand. To be a craftsman requires that one see distinctions in the world that those who are not craftsmen cannot see. Thus, a craftsman, as opposed to a mere producer, will recognize that a fact of the matter is out in the world, and will use intelligence and flexibility to bring out that fact in the best way possible. In their example of a craftsman who works with wood, they claim that such a person will recognize qualities in the wood and realize how to create something with the wood that brings out its best qualities. In doing so, the craftsman enters into a relationship with wood that is nurturing and in some sense external to the individual. As they point out, however, this is an essentially phenomenological mode of entering into such relationships. Furthermore, it is one where an *individual* discovers which domains of action—tennis, writing, teaching, woodworking, painting, sculpture, music, etc.—are sacred for him or herself.

Since the meaning of “sacred” is something that is determined by an individual picking amongst possibilities, Dreyfus and Kelly generate an internal tension in *All Things Shining*. The authors try to connect individuals with a community by having moods that are individual in origin become publicly shareable. This, however, gets the explanatory story backwards for a communitarian. A communitarian would begin his or her explanation with the external community as the source of the meaningful and the sacred. But Dreyfus and Kelly begin with individual experiences. Furthermore, there is almost no discussion of the communities in which individuals find themselves. Phenomenology, in the hands of Dreyfus and Kelly, is a method that focuses on individual experiences,

even if the individual experiences occur within a group setting such as a public event. Thus, the sacred and the meaningful that any one person will find will be what emerges from the individual, and not from the community. Such individual-based experiences cannot give rise to the sort of external constraint that Dreyfus and Kelly indicate is needed to avoid the self-confident person’s view that the world is an infinitely malleable entity waiting for a strong will to shape. Despite their efforts, Dreyfus and Kelly have not countered the view that the sacred is something we impose upon our experience.

Despite problems in the argument of *All Things Shining*, there are good things to take away from this book. The desire to provide a foundation, even in outline, for a positive communitarian project is laudable. Individual chapters and their analysis of literature, especially Chapter 6 on Melville, are quite thought-provoking. Finally, the methodological problems in *All Things Shining* do not diminish the potential for the development of the idea that *poiesis* can be a source, once properly understood, of excellence and community, as well as providing a way to avoid nihilism. But this would require beginning with a community of master craftsmen and exploring how that community jointly produces knowledge, understanding, and a sense of the sacred that is transferred to individual apprentices. It does not start with the accidental physical or emotional contact of individuals that can occur when people are spatially and temporally near each other.

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Robert M. Geraci, *Apocalyptic AI: Visions of Heaven in Robotics, Artificial Intelligence, and Virtual Reality*. Oxford: Oxford University Press, 2010. Pp. 233. ISBN: 978-0-19-539302-6. \$27.95 (hb).

Unbeknownst to most of us, a new utopianism is afoot. This most recent edition of utopian ideology arose from the alluring possibilities of capturing the essence of human intelligence in computational

platforms, a view heralded by proponents of Strong AI. Strong artificial intelligence research is based on the assumption that genuine cognition will turn out to be a species of information processing, making the aim of uploading human minds into the supercomputers of the future a reasonable endeavor. The name Robert Geraci gives to this movement is “Apocalyptic AI” (henceforth AAI) because its rhetoric and many of its fundamental beliefs about the future of humanity are unquestionably similar to what are found in the apocalyptic traditions of Judaism and Christianity. In *Apocalyptic AI*, Geraci states that this strange utopian brew shouldn’t be written off as inconsequential fantasies of a few overwrought enthusiasts of ancient arcania or futurist sci-fi geeks because, as he aptly demonstrates, it has already made inroads into our social imaginaries through online game-worlds like *Second Life* which have the apocalyptic agenda written into them, and most significantly, it is already gaining cultural prestige with substantial governmental funding for its research projects which will enable it increasingly to shape our future.

On Geraci’s reading, AAI is, despite its avowed secularism, really a new religious movement that uses the categories of ancient Jewish and Christian apocalyptic theologies for scientific and distinctly secular aims, basing its apocalyptic-like promises not in the interventions of supernatural entities but in the progress of science and technology. Roboticist Hans Moravec (founder of Carnegie Mellon University’s famous Robotics Institute, and presently Chief Scientist at Seegrid Corporation) is the guru of this movement, while world-renowned inventor/entrepreneur Ray Kurzweil (star of the recent documentary *Transcendent Man*) is its chief evangelist. The avowed aim of AAI enthusiasts is to engineer a form of cyber-immortality through which ultimately we, or our near future relatives, will escape the weaknesses and limitations of our flesh and be transformed into software files existing in cyberspace. These secular prophets predict we will upload our intellects into the massive database of a future supercomputer and continue our lives endlessly in virtual bodies and worlds. There will be, however, a cyborgic transition period along the

way to this post-biological virtually immortal form of existence. Before we take up final residence in the unlimited possibilities of cyberspace, we will need to annex our flesh to robotic computational devices and inject nanobots into our bodies to keep us intact long enough, and to make us intelligent enough, to engineer the intelligent machinery of our ultimate redemption from flesh. In this way, AAI, as it were, reconciles religion and science: science and technology bring about the realization of the valuations and aspirations of religion.

Geraci argues quite convincingly that the hopes and promises of key AAI boosters (Hans Moravec, Ray Kurzweil, Kevin Warwick, Hugo de Garis, Marvin Minsky, et. al.) are “almost identical to those of Jewish and Christian apocalyptic traditions” such that should their promises come to pass, “the world will be, once again, a place of magic” (9). Cyberspace will be our home—the heavenly realm of enchantment where our spiritual yearnings for the transcendent will be finally satisfied and our struggles with flesh ultimately concluded. Geraci makes apparent that both ancient apocalypticism and contemporary AAI presume a fundamental cosmic dualism and human alienation from the present physical world, which is viewed as a place of darkness, pollution, and ignorance. Moreover, they both anticipate a future world of redemption that will be infinitely more meaningful than either the present or past, and they expect to be freed from the bondage of the flesh by being resurrected in new angelic (i.e., virtual) bodies. Whereas ancient followers of Christian apocalypticism were hoping for the *parousia* (return) of Christ, proponents of AAI are waiting for “the Singularity”—a kind of secular “Rapture”—to arrive, the point where technological progress “occurs inconceivably fast, leading to a meaningful future that abolishes cosmic dualism and resolves the experience of alienation” (24).

Despite having its roots in ideas and dreams of ancient essentially gnostic worldviews, Geraci convincingly argues that AAI has nonetheless also become a modern research program with a very effective strategy for garnering research funding

as well as an influential shaper of and advocate for online virtual life. Many of the proponents of AAI are motivated by the quest for funding, prestige, and cultural authority. Owing to the evangelical zeal with which AAI is marketed in the public and governmental sectors, AAI advocates have convinced many American politicians that any decreases of funding to robotics and AI research constitutes a “real threat to our country” and its national security (57), a funding tactic reminiscent of the Cold War arms race. Along with this scare tactic, a good part of the success of AAI in acquiring funds and cultural authority is due, says Geraci, to its calling on “religious categories to heighten the allure of their subject matter” (61). After all, if such R & D “can produce cheap, efficient energy, reduce traffic accidents, eliminate earthly pollution, prevent military deaths, care for the elderly, and produce food at almost no cost, then who would resist the moral value of robotics research and who would begrudge our saviors a few extra dollars?” (69).

Geraci shows that sci-fi writings and movies which effectively transpose religious values and goals into techno-scientific narratives have been immensely influential on the techno-culture of the west (56). One of the most important symbols in science fiction is the intelligent machine, a symbol which Geraci suggests is nothing else than deity in a techno-scientific register as engineered matter with capabilities of evoking fear, fascination, and awe. If this is the case, then it explains Marvin Minsky’s quip that compared to sci-fi authors such as Asimov and Pohl, philosophers are just shallow. Stewart Brand goes so far as to claim “Science fiction is the literature at MIT” (52). Geraci reveals how indeed life imitates art in MIT’s robotics and AI community where “researchers try to build the fascinating things described in science fiction” (53). It actually turns out that the idea of mind uploading so central to AAI first showed up in Arthur C. Clarke’s 1953 *The City and the Stars*.

Geraci notes that the programmers and designers of the virtual worlds have written the apocalyptic agenda into fabric of these worlds, and in this way they have normalized the agenda for millions

upon millions of people by virtually relocating the sacred to the digital realm (75-6). AAI devotees of online virtual worlds like *Second Life* view these virtual worlds as theaters that train us for, and as bridges to, the digital paradise just the other side of the Singularity. According to a survey Geraci conducted, nearly 50 percent of the inhabitants of these worlds felt that their virtual friends were as important to them as their offline friends (81), making the prospects of transferring their conscious selves into these virtual worlds alluringly attractive—especially as the online worlds come to contrast more and more invidiously with the offline world.

AAI has made inroads not merely into pop science and culture, but also influences philosophers, lawyers, governments, and theologians in NA and Europe, prompting them to explore larger questions that they would otherwise have likely either ignored completely or excluded from serious consideration. For example, philosophers are now struggling to answer questions about machine consciousness and mind-uploading; serious textbooks on cognitive science for undergrad psychology courses are now dealing with notions that previously were interesting only to sci-fi fans. Questions surrounding the issue of responsibility for intelligent machines’ actions are now discussed by lawyers, decision makers, and political consultants—e.g., is it “the builders, programmers, distributors, users or perhaps even the government agencies that legalized the machines”? (125). “Can intelligent autonomous systems bear Constitutional personhood, legal rights, and responsibilities?” is a question now being probed and debated by legal experts and government agencies largely because AAI zealots like Minsky and Kurzweil have been able to convince such cultural overseers to take the AAI worldview seriously (118). AAI also has sent theologians back to the drawing board to find within their religious hierarchies, doctrines, and practices a place for intelligent machines, machines possessing their own beliefs and desires. Since its beginnings in the 1980s as a largely ignorable counter-cultural movement associated with the Max More’s Extropy Institute, AAI has crept much closer to the 21st century’s cultural center by both garnering for

its agenda massive funding from both private and public research coffers, and by forcing philosophical, legal, and theological discourses to bear the impress of its cultural import. AAI has become tremendously influential today because “it impressively integrates the two most significant areas in modern life: religion and technology” (143) thereby becoming modernity’s only scientifically sanctioned soteriological quest.

This book is a valuable addition to the few books like Noreen Herzfeld’s *In our Image: Artificial Intelligence and the Human Spirit* (Augsburg, 2000) and Anne Foerst’s *God in the Machine: What Robots Teach us about Humanity and God* (Dutton, 2004) that explore the religious dimensions of a fundamentally computational vision of human destiny that is subtly colonizing the dreams and directions of western civilization’s aspirations. Its weakness is that it doesn’t devote much energy or discussion to the obstacles in the way of this secular religion. For instance, Geraci

barely mentions, let alone explores in depth, the bioconservative resistances and challenges to AAI found in the writings of Francis Fukuyama, Leon Kass, Bill McKibben, and George Gilder, who view AAI, in the words of Fukuyama as “the world’s most dangerous idea”. A more pertinent issue for the readers of this journal would be not merely the ideological road blocks to the movement, but the philosophical and conceptual *aporia* that may cripple its technical progress. I have in mind here the question of feasibility regarding the very foundation of its vision: mind-uploading. If Polanyi is correct that human minds bear an ineliminable and non-reducible tacit dimension, then any attempt to “decode” the brain’s patterns into explicit re-codable data structures may quite literally “change the subject” which would certainly be a conceptual obstruction in its flowchart for the future, if not an existential wrench in its machinery for engineering personal immortality.

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