

Tradition & Discovery

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The Polanyi Society

Preface

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With this issue comes the annual subscription renewal flyer. I encourage you, while you are thinking about it, to pull out the inserted colored page and put it—with your check or credit card information—back in the mail to me. Given the general spectrum of professional societies, the Polanyi Society is certainly on the low budget end. We soon shall need modestly to increase our \$20/year subscription rate, but we are presently able to meet our basic expenses which are primarily printing and ever-rising mailing costs. At the annual meeting upcoming in November, there will be discussion of a plan to have the Polanyi Society incorporate and apply for official non-profit status. This status would allow the Polanyi Society to accept tax deductible contributions which could help address future increases in expenses.

The schedule for the annual meeting to be held November 20 and 21, 1998 in the Walt Disney World Dolphin Hotel in Orlando, Florida is included on page 5. One session will feature a paper by Paul Craig Roberts, who was one of Polanyi's last students working in the social sciences. Roberts has held important U.S. government positions and has been a prolific writer; his paper will focus upon Polanyi's economic thought. The second session will feature a panel, Dale Cannon, Diane Yeager and David Rutledge, seasoned teachers addressing the theme "Teaching Polanyi and Polyanian Teaching." You can get the prepared papers sometime in early November by downloading them from the Polanyi Society WWW site (<http://www.mwsc.edu/~polanyi>).

Please also particularly note the information in "News and Notes" on *Polanyiana*, the journal of the Michael Polanyi Liberal Philosophical Association in Hungary. Inquiries imply that there is some confusion about *Polanyiana*. English issues are on WWW; contribution to support the continued publication of *Polanyiana* are welcomed by the MPLPA.

The articles in the current issue are an interesting set. There are two different review articles on recent Polanyi-related publications as well as responses to the reviews by the authors. Walter Gulick looks carefully at Stephen Turner's exploration of the social dimensions of practices in Turner's 1994 book, *The Social Theory of Practices: Tradition, Tacit Knowledge and Presuppositions*. Stephen Turner's thorough response to Gulick helps the reader see what is at stake in the discussion, to wit, some genuinely different construals

continued on p. 3

Tradition and Discovery is indexed selectively in *The Philosopher's Index* and *Religion One: Periodicals*. Book reviews are indexed in *Index to Book Reviews in Religion*.

NEWS AND NOTES

of the notion of practice and equally different takes on Polanyi. Both derive insights from Polanyi but believe it is important to revise Polanyian claims in light of recent new insights; these writers tend to emphasize different aspects of the Polanyian heritage. Jeff Pflug's review article treats Stephania Jha's 1995 Harvard dissertation titled Michael Polanyi's Integrative Philosophy; both Pflug and Jha comment on Polanyi's late notion of from-at knowing, a notion that Marjorie Grene criticized. Jha's response also outlines her effort to provide, in her dissertation, important elements of what she dubs an "intellectual biography" of Polanyi.

Last but not least in this issue is Philip Mirowski's "Economics, Science and Knowledge: Polanyi vs. Hayek," a fascinating article which documents and explores the relationship between Friedrich Hayek and Polanyi's philosophical and economic ideas. Mirowski is a scholar interested the connection between economics and philosophy of science; he has followed the personal links between Hayek and Polanyi and tied this historical to the publications of these figures. He cogently argues these thinkers have some sharp differences about government policy and that this grows out of their rather different ideas about science and epistemology.

Phil Mullins

An *Appraisal*-sponsored Polanyi Conference at Sheffield, UK occurred in April of 1998; revised versions of the papers will be published in the next 2 issues of *Appraisal*. *Appraisal* will sponsor a conference on Friday, April 9th and Saturday April 10th, 1999, at the University of Surrey, Guildford. Fee, with room, breakfast and lunch on Saturday, plus papers in advance, will be about L40 Sterling. Papers are invited, especially proposals primarily interested in the further application of Polanyi's ideas. Not all papers need be directly related to Polanyi. Please send proposals and enquiries to R.T. Allen, 20 Ulverscroft Rd., Loughborough, Leich. LE113PU, England or e-mail (Richard_Allen_21@compuserve.com).

Richard Allen has forwarded the following list of paper topics from the *Appraisal*-sponsored Sheffield UK conference held in April of 1998: Marty Moleski, "Polanyi and Fuzzy Logic"; Norman Wetherick, "Polanyi and Psychology"; James Lund, "Philosophy and Paideia: Their History and Relation to Pragmatism"; Julian Ward, Polanyi's Epistemology and Kant's"; Paul Dean, "Polanyi and the Teaching of Literature"; Norman Sheppard, "Polanyi and "Philosophy of Science"; Percy Hammond, "Technical Knowledge"; Robin Hodgkin, "What Is A Tool?" Revised versions of all papers will appear in the next two issues of *Appraisal* (Vol. 2, No's 2 &3).

Walter Mead's article entitled "William H. Poteat's Anthropology: Mindbody in the World," appeared in September in the 1998 volume of *The Political Science Reviewer: An Annual Review Of Scholarship*, XXVII: 455-533. Copies are available from 14 South Bryn Mawr Avenue, PA 19010-3275 (\$12). Portions of Parts I and II of this essay appeared in *TAD* 21:1(1994-95): 33-44.

Update on *Polanyiana*

Professor Éva Gábor, one of the editors-in-chief of our sister journal *Polanyiana* which is published by the Michael Polanyi Liberal Philosophical Association (MPLPA) in Budapest, recently visited the United States and met with several members of the Polanyi Society. The following information about *Polanyiana* is based upon conversations with Gábor.

Since 1991, MPLPA has published both English and Hungarian numbers of *Polanyiana*. The most recent issue in English is Volume 6, Number 2, 1997, which came out in the late Spring of 1998. The publication of *Polanyiana* has for several years been supported by a grant from the Soros Foundation. Unfortunately, this grant will very soon be expended and MPLPA will not be able to get additional support from the Soros Foundation.

The cost of mailing copies of *Polanyiana* from Budapest to many distant areas of the world is extremely high. MPLPA has therefore begun a program of putting issues of *Polanyiana* on the World Wide Web. Complete articles identical to those in the printed issues can be downloaded and printed locally by anyone wishing to read them. You can find the *Polanyiana* home page at either of two sites:

- (1) <http://www.kfki.hu/chemonet/polanyi>
- (2) <http://www.ch.bme.hu/chemonet/polanyi/>.

On the home page, there is a link to the electronic archive of four earlier *Polanyiana* issues, two in English and two in Hungarian. You may reach the archive directly at <http://www.kfki.hu/chemonet/polanyi/archive.html>.

Also on the home page is a link to the most recent issue available on the WWW; you can reach this issue directly at <http://www.kfki.hu/chemonet/polanyi/9702/contents.html>. The Table of Contents for the most recent *Polanyiana* (Volume 6, Number 2) follows:

To the Hungarian reader

ELŐSZÓ

Hírek és közlemények

Phil Mullins: “Polanyi’s Participative Realism”

Tibor Frank: “George Pólya And The Heuristic Tradition”

Gábor Palló: “The Role Of Morality: A Comparison Of Michael Polanyi And Imre Lakatos”

Judit Szapor: “Laura Polanyi 1882-1957: Narratives Of A Life”

Robin A. Hodgkin: “Making Space For Meaning”

István Hargittai: “John C. Polanyi-Interview”

You can also find a list of materials in *Polanyiana* through the Polanyi Society home page (<http://www.mwsc.edu/~polanyi/>) under Other Journals with Special Interest in the Thought of Michael Polanyi. Links are provided there to *Polanyiana* internet sites.

At this stage, the continued publication of *Polanyiana* is in some jeopardy. Alternative, cheaper means of publication are being investigated. Anyone interested in making a donation to the MPLPA may do so by sending a check or a credit card number (with name and expiration date) to Phil Mullins (Polanyi Society, MWSC, St. Joseph, MO 64507; fax 816-271-4386; e-mail: mullins@griffon.mwsc.edu). Please clearly indicate that your check or credit card debit is for the MPLPA. If you consolidate Polanyi Society membership renewal with a donation for MPLPA, you must provide explicit instructions about the division of funds. Since there are problems with currency exchange, please do not send checks directly to the MPLPA. I will aggregate all donations to MPLPA and do a single bank transfer late in the Fall. Given the difficulties in currency exchange and credit card policies, the suggested minimum donation is \$25.

To contact Professor Éva Gábor directly, use any of the following: (1) mail: Michael Polanyi Liberal Philosophical Association, 1111 Budapest, Műgyetem rkp. 3. K. I. 59, Hungary; (2) phone: (36 1) 463 1181; (3) fax: (36 1) 463 1042; (4) e-mail: GABORE@TTTK2.TTTK.BME.HU.

Phil Mullins

Schedule for November 1998 Polanyi Society Meetings

The Polanyi Society will have two meetings on November 20 and 21, 1998 at its gathering held in conjunction with the 1998 annual meeting of the American Academy of Religion/Society for Biblical Literature. Meetings will be in the rooms listed below in the Walt Disney World Dolphin Hotel in Orlando, Florida. You do not have to register for the AAR/SBL meetings to attend Polanyi Society meetings.

The papers to be discussed are listed below; as in past years, the focus of each session will be on discussion. Participants are requested to read the paper before the session. By early November, papers can be printed from the Polanyi Society WWW site (<http://www.mwsc.edu/~polanyi/>); if you have difficulty accessing the WWW version, you may write or e-mail Phil Mullins (CSTH, MWSC, St. Joseph, MO 64507 or mullins@griffon.mwsc.edu) to request a copy by US mail for a nominal fee.

Special Session With Paul Craig Roberts 9-11 p.m. November 20, 1998 Dolphin--Europe 3

Phil Rolnick, Greensboro College, Presiding

Paul Craig Roberts, “The Economic Thought Of Michael Polanyi”

“Teaching Polanyi and Polanyian Teaching” 9-11:30 a.m. November 21, 1998 Dolphin--Europe 4

Ron Hall, Francis Marion University, Presiding

Panelists:

Dale Cannon, Western Oregon University

“A Polanyian Approach to Conceiving and Teaching Introduction to Philosophy”

Diane Yeager, Georgetown University

“Reclaiming ‘Science as a Vocation’: Learning as Self-Destruction, Teaching as Self-Restraint”

David Rutledge, Furman University

“‘Beyond Logic and Beneath Will’ — Teaching in a Polanyian Spirit”

11:15 a.m. Business Meeting

Marty Moleski, Presiding

Prolegomena to a Polanyian Theory of Practice A Critique of Stephen Turner's Account

Walter B. Gulick

Stephen Turner, *The Social Theory of Practices: Tradition, Tacit Knowledge, and Presuppositions*. Chicago: University of Chicago Press, 1994. Pp. x + 145. ISBN 0-226-81737-7 (cloth), 81738-5 (paper).

ABSTRACT Key Words: Michael Polanyi, practices, traditions, social theory, tacit knowledge, sociological explanation, habits, social causes, presuppositions

Stephen Turner explores the social dimensions of practices, probing to see if the notion of a shared practice can be understood as a cause or mechanism whereby knowledge arises and is used. When he concludes that practices are not some mysterious collective object but are best explained as individual habits, he thereby rejects an attenuated notion of practice and replaces it with a needlessly atomistic notion in which habits carry the full burden of explanation. Turner makes use of aspects of Polanyi's thought, but this article suggests ways in which a fuller appropriation of Polanyian insights can salvage a social, telic notion of practices that illuminates human behavior.

The complete title of Stephen Turner's provocative book indicates accurately what the book is about. Practices, as Turner views them, encompass both social phenomena -- traditions -- and individual embodied states of mind -- tacit knowledge and presuppositions. *The Social Theory of Practices* attempts to ferret out the mechanisms by which knowledge utilizing practices are produced, shared and passed on -- are, in fact, social. Turner's overriding concern is to clarify the notion of practices so that its explanatory value in social science is illuminated.

The terrain Turner explores is broad indeed. "Practices" and its cognates have been used as explanatory terms in fields as diverse as philosophy, anthropology, history, psychology, and jurisprudence. In recent decades the concept of practices has become a leading candidate to fill the explanatory and justificatory vacuum created by the demise of most foundational theories in philosophy. In line with this philosophical development, Turner prefaces his first chapter with a citation from Wittgenstein and a paraphrase of Heidegger's thought. These citations illustrate how "practices" and allied social concepts have central prominence in the thought of the twentieth century's two most influential philosophers. Wittgenstein claims that socially mediated ideas provide the context within which serious thought occurs: "It is the inherited background against which I distinguish between true and false" (*On Certainty*, 15e). According to Hubert Dreyfus, "Heidegger argues that . . . even when people act deliberately, and so have beliefs, plans, follow rules, etc., their minds cannot be directed toward something except on a background of shared social practices" (*Tines Literary Supplement*, July 12, 1991, 25).

Granted that many disciplines utilize the term "practices," what does Turner mean by this notion? He recognizes two distinct meanings. One is the "telic notion of practice" (8); the other is a causal notion.

He explains the telic notion through a definition given by Kant: it is "an activity seeking a goal 'which is conceived as a result of following certain general principles of procedure'" (8, quoting from Kant's "This

May Be True in Theory but Does not Apply to Practice”). Telic definitions have their origin in Aristotle, and currently MacIntyre and Borgmann are examples of thinkers using this notion. It is not difficult to identify examples of telic practices. These would qualify: playing a game of chess, hosting an annual New Year’s Eve party, baptizing a baby according to the way it is done in some denomination, sitting in on a jam session, going fly fishing.

Turner, alas, does not investigate telic practices. Rather, in the first five of his six chapters he examines practices insofar as they might function as foundational (causal) explanations in social science. He inquires as to whether practices can explain and justify claims to knowledge and truth.

When he explores practices in this causal sense, he finds usages that tend to cluster around two basic modes: “those that are based on the model of hidden premisses of deductive theories, ‘shared presuppositions’, and those that refer to embodied knowledge, such as skills, ingrained cultural or moral dispositions, or linguistic competencies” (3).

The second mode of causal practices, embodied knowledge, is influenced by Polanyi’s notion of tacit knowledge. In part, Polanyi’s notion is attractive because it “trades on the interdependence of skill and presupposition that is part of the scientist’s way” (3). Unfortunately, there are many Polanyian insights that Turner ignores to the detriment of his book’s cogency, insights which would have helped Turner detour around the rough road that faces anyone who attempts to envision practice as a collective, causal sort of explanation of human behavior. To understand how Polanyian conceptuality might be helpful, let us first see how Turner carries out his investigation.

When Turner sets aside the telic notion of practice to concentrate upon the extent to which practices might contribute to causal comprehension, he sets himself upon a course which inevitably leads to disappointment. Seduced by the allure of sociology in the grand tradition of Durkheim, he falls prey to an objectivist model of social science. Actually, Turner’s set of presuppositions about what makes for good sociological explanation seems even more closely modeled on the physical sciences than the notions of Durkheim. Ironically enough, Turner’s dissatisfaction with the notion of “practices” is generated by the sociological *practice* he applies to the notion. Here is my reading of five presuppositions governing the sociological practice he embraces in the first five chapters of his book, presuppositions which inevitably lead to dissatisfaction with any viable notion of practice.

First, Turner assumes that the proper goal of sociological theorizing is to take note of the empirical evidence of human life patterns in their social and individual forms and explain the regular patterns as well as account for the anomalies. With this basic assumption, I have no quarrels whatsoever, so long as explanation and accounting are carried out in manners appropriate to the evidence being studied.

Second, Turner assumes that the only really reliable explanations are those discovered in the empirical evidence. Explanations which are in some way constructed are tainted with subjectivity. Behind this assumption is a belief that what is discovered is objective and therefore reliable, whereas what is postulated or regulative is unreliable. This belief about the nature of truth is evident in the following passage:

Aren’t facts about practice just ‘made’ facts as well? If so, isn’t the story that social constructionists tell itself [sic] just another story about the facts being ‘made’ by scientists,

law courts and the like -- and not the 'true' story? (38)

To assume that there is one true account of practice is, of course, highly contentious, and the way he relies on empirical evidence places Turner--against his own inclinations, I believe--in the camp of behaviorists. Understandably, Turner wishes to avoid relativism if possible, but to resort to a behavioristic model of truth is to narrow seriously the notion of evidence to measurable public evidence and the notion of explanation to a form of nominalism.

Third, Turner assumes that causal social arguments require that selfsame causal objects be shown to act in the same way over time and across cultures. The requirement that a practice must be consistently the same, a requirement apparently borrowed from the structure of law in physics, haunts Turner's analysis in the first five chapters.

Fourth, Turner assumes that causes must be conceived as objects which impinge on other objects. This assumption, of course, is but an implication of the second and third assumptions just noted. Still the idea that objectivity requires objects is pervasive in what he writes and is worthy of being regarded as one of a cluster of logically related assumptions, what might be termed his "objectivity requirement." How can he connect his objectivity requirement to a causal notion of practices which is grounded in such apparently nonobjective phenomena as either "hidden *premisses*" or types of embodied *knowledge* (my emphases)?

Fifth, Turner assumes, reluctantly, that if an explanation cannot be based directly on empirical evidence, then the inference to a hidden causal object is required. When the causal processes are not independently observable, one is required to accept inference to the best explanation (18), an inherently weaker theory than a behaviorist theory.

The problem with the assumptions Turner accepts is that they are inappropriately modeled after the physical sciences. Obviously, human intentions and actions are responsive to a vast number of influences, including those lumped together under the elusive notion of freedom as well as those which are observable. The notion of lawlike (causal) behavior is in itself appropriate to borrow from the natural sciences (including biology) so long as its explanatory limits are observed.

To the extent that a causal explanation of practices is warranted, Turner's usage of the notion of cause is flawed. His usage seems more patterned upon mechanics rather than upon quantum mechanics. But all the elements in a mechanical system are in principle observable. This is not the case with the causes of human behavior (assuming it is even appropriate to speak of causes of human behavior). Any theory of human behavior which settles just for some simple causal influences, whether it be genetic inheritance, environmental influence or free decision making, is doomed to failure. The complexity of influences suggests human causality be at best considered as analogous to quantum mechanical explanation. Heisenberg's uncertainty principle does not abandon the principle of causality; rather it recognizes formulaically the limits of human knowledge and does not attempt greater objective knowledge than is possible in the microworld. The paths (positions and velocities) of photons must be understood in terms of probabilities. Similarly, even if Turner developed a precise, empirically grounded definition of practices, the incredible number of variables affecting human behavior would render causal explanation and accurate prediction impossible. The most that might be expected would be a probabilistic analysis.

But, understandably enough, Turner is never able to offer a precise, empirically grounded definition of practices. The four middle chapters of the book show how sociology shaped by the five assumptions listed above cannot produce a satisfactory notion of practice. The questions Turner raises about any putative concepts of practice are many. “How does one distinguish first, or human, nature and second nature, or practice?” (21) How can one confidently select among the various historical accounts of how we acquire patterns of behavior so impervious to change that they may be identified as practices (27)? How does one know that practices are not “as if” explanations rather than causal explanations (43)? Where is the location of practices: in some collective object? in individual habits? in some form that is both private and public (50)? Are practices best understood as commonly possessed tacit objects, rules that govern behavior, or merely personal habits (63-77)? If practices refer to basically stable patterns of behavior, how is one to understand how practices change so as not to undermine their essential stability (83-92)? Can one meaningfully distinguish between such cognate terms as practice, form of life, tradition, custom, mores, habitus, paradigm, etc. (93)?

In following the assumptions of objective sociology, Turner conceives practices as hidden convictions or habits shared by a group. Here he follows a trail blazed by Freud rather than by Heisenberg. The group practices that Turner discusses seem ontologically congruent with the individual unconscious that Freud discusses. Both are nonempirical constructs postulated to account for observed phenomena. Turner is scrupulous in tracking the problems such fundamentally metaphysical notions (or at best regulative ideas) raise for science. He is well aware that these notions cannot in principle be falsified. His conclusion is, therefore, not surprising:

I think the case for practices, or practice, understood as a hidden collective object, is faced with such serious difficulties with respect to the means of the transmission and acquisition of these objects that it cannot be accepted, and that appeals to ‘practice’ used in this sense, either in philosophy or social theory, are therefore appeals to nothing. (100)

Turner consequently reaches the end of his protracted road toward the destination of objective sociology without having been able to collect any cargo of causal insights. Indeed, the results of his journey read more like an exercise in the deconstruction of “practices” (although Turner’s sympathies are certainly not with postmodernism) than like the constructive clarification of this term. Moreover, the road he traveled proves to be full of detours and bogs, and the destination he sought seems nebulous and lost in airy abstraction. The concept of practice, whether it is conceived cognitively, as a kind of presupposition, or causally, as a kind of mental trace which disposes thought or action in a certain way, is epistemically elusive. Practices are not directly accessible, and the means of accessing them indirectly are fraught with difficulties, of which underdetermination is the most obvious and pervasive. (43)

In his last chapter Turner settles for an instrumental, individually grounded notion of practice rather than a causal, shared notion.

We cannot do anything to get behind the notion of practice, either in a causal or a justificatory way, because practices are not objects, but are rather explanatory constructions that solve specific problems of comparison and unmet expectations.... We cannot identify practices as such. We cannot even separate the ‘social’ part of practice from the natural part. (123)

His tone throughout the chapter makes it evident that this modest sort of conclusion comes as a disappointment to Turner. One cannot identify practices in such a way as to defeat relativism, justify claims, or perform other such heroic deeds for our culture as Turner originally hoped. But is the problem with the notion of practice? Isn't the problem rather with the inflated dreams of social science modeled on physical science? What was most surprising to me as I read through *The Social Theory of Practices* was that Stephen Turner, obviously a well read and thoughtful practitioner (that term!) of social theory, a man who acknowledges his appreciation for the works of Gadamer, MacIntyre, and Rorty in addition to Polanyi, would be transfixed by these dreams.

* * *

How might Polanyian conceptuality have been helpful to Turner? First, a person influenced by Polanyi's epistemology would not be likely to be lured into the objectivist assumptions that marked Turner's project. Since Polanyi demonstrated the illusory aspect of objectivist ideals in the physical sciences, how much less likely it is that objectivist ideals could be sustained in the social sciences! Turner's work, perhaps inadvertently, functions as an object lesson to anyone who would embark on an objectivist definition and description of practices.

Second, a Polanyian perspective would find much of interest in the telic notion of practice that Turner initially discarded. For Polanyi, all acts of knowing have some degree of personal purpose — commitment — embedded within them. While Turner does not explicitly state that the modest notion of practices he discusses in his last chapter represents a return to the telic notion he earlier dismissed, in fact that seems to be the direction toward which his thought is tending.

Unfortunately, he never quite shakes free of his objectivist leanings. Instead, Turner concludes that practices "are individual formations of habit that are the condition for the performances and emulations that make up life" (123). There is a problematic nostalgia for causality (objectivity) that clings to this reductionistic formulation. Practices are not helpfully seen as collections of habits which are *the condition for* (awfully close to "cause") performances and emulations. Performances and emulations are of many sorts and may be produced for many reasons. Practices are best regarded as one type of performance or emulation, namely that type carried out according to standards of excellence set forth in traditions of interpretation. Both the performance and the commitment to communal standards of excellence are required if one is to talk meaningfully of a practice. A Polanyian would reject Turner's concluding reduction of practices to individual habits. In place of such arbitrary atomism, a Polanyian would insist that practices exhibit social structure and telic qualities.

Third, a Polanyian analysis of human behavior would resist temptations to speak of causes. "A complete causal interpretation of man and human affairs disintegrates all rational grounds on which man can hold convictions and act on these convictions. It leaves you with a picture of human affairs construed in terms of appetites, checked only by fear" (*Logic of Liberty*, 28). Polanyi notes that all living beings have active centers in which multiple influences are coordinated, and in humans these centers are developed to the extent that genuine autonomous behavior emerges (see *PK*, 402-495). Autonomous behavior takes account of background influences but is not simply the product of their vectorial impact. Practices are ways humans insulate themselves from random environmental influences; through practices people organize, coordinate and focus behavior so as to increase the scope and weight of action.

This implies, fourth, that the chief significance of practices is not to be found, as Turner assumes, in their explanatory power, but rather it rests in their ability to illuminate meaning producing social and personal acts. MacIntyre and Borgmann, rather than Polanyi, have most tellingly seized upon this characteristic of practice and developed its implications. MacIntyre defines a practice as “any coherent and complex form of socially established cooperative human activity through which goods internal to that form of activity are realised in the course of trying to achieve those standards of excellence which are appropriate to, and partially definitive of, that form of activity” (*After Virtue*, 175). A person engaging in a practice is *guided* by its standards rather than being *caused* to perform in some manner.

Albert Borgmann inquires after those practices which especially orient and enrich life. He calls them focal practices. By searching out the focal practices that call and claim us, we can guard against the frittering away of time and energy that occurs when we assume “that the shaping of our lives can be left to a series of individual decisions. . . [W]ithout a practice an engaging action or event can momentarily light up our life, but it cannot order and orient it focally” (*Technology and the Character of Contemporary Life*, 206-207). Focal practices typically connect persons with those things in the world that evoke effort and care because they have weight and depth. To speak less metaphorically, focal practices include such activities as backpacking in the wilderness, participating regularly in a book discussion group, or cooking gourmet meals. Borgmann’s attention to the life enhancing qualities of practices is most effectively carried out in the language not of the social scientist, but in terms of what he calls deictic discourse, language of testimony and appeal.

Fifth, one of the means of transmission of practices is through deictic discourse. But people find practices attractive for a variety of reasons. In his “special moments” section (63-67), Turner explains some of the mechanisms by which practices are passed on. People learn the rules (67-76) of a practice in a variety of ways, often indwelling them so they are carried out automatically (habitually). In asking his readers to choose between understanding practices in terms of “rules, social minds *or* habits” (76, my emphasis), Turner seems unnecessarily to pose disjunctive alternatives. A Polyanian approach would be to affirm the usefulness of understanding practices in terms of each alternative at some partial level of analysis. Practitioners die, but as long as a practice continues to elicit interest, new practitioners will identify with its goals and take responsibility for ensuring that its standards are maintained.

Polanyi’s thought can be mined to provide examples of such processes. For instance, *Science, Faith and Society* explains how the practice of science is grounded in a community of inquirers, passionately seeking intellectual satisfaction and committed to common standards of research and review. Typically, persons engaged in a practice have a convivial relationship to one another. Indeed, such essential Polyanian terms as connoisseurship, commitment, conviviality and meaning serve well to describe the dynamics of engaging in practices. These terms of social engagement seem to me more useful in comprehending practices than the notion Turner emphasizes from Polanyi, tacit knowing. Much that is central to a practice is explicitly known.

In conclusion, Stephen Turner’s book provides much material of interest to anyone pursuing the topic of practices. The outcome of his labor would be more satisfying had he incorporated into his thought the sensitivities of Michael Polanyi.

Polanyian in Spirit: A Reply to Gulick

Stephen Turner

ABSTRACT: Key Words: Michael Polanyi, practices, traditions, social theory, tacit knowledge, sociological explanation, habits, social causes, presuppositions

Walter Gulick criticizes The Social Theory of Practices for its non-Polanyian views of the problem of the objective character of tacit knowledge, its insistence that there should be plausible causal mechanisms that correspond to claims about tacit knowledge and its "social" transmission, and its denial of the social, telic character of practices. In this reply it is asserted that the demand for causally plausible mechanisms is not scientific or for that matter non-Polanyian, that the book has a view of objectivity that parallels Polanyi's own, and that the idea of telic practices is subject to the same problems over mechanism as non-telic ones, with the additional problem that telic concepts need supra-individual feedback mechanisms, of which no plausible examples exist. In each case, the non-social or personal explanations of the phenomenon of "practice" are better than the "social" ones. The discussion concludes by posing the challenge of connectionism to Polanyi, as well as the opportunity it presents.

I thank Walter Gulick for the civility of his review essay, and for recognizing that *The Social Theory of Practices*¹ is very closely related to Polanyi's concerns. As will become clear, I think the criticisms he makes are largely off-target. But others have read, or misread, the book in a similar way, so it is important to clarify the issues he raises. The key issues are the role of teleology in practices, the problem of objectivity, the kinds of explanation appropriate to human things, and the problem of the social nature of practices. A secondary issue, but an important one, is the question of the way in which Polanyi's views bear on these issues.

On some of these issues, such as objectivity, Gulick attributes views to me that I sought to avoid, because I do not share them, and which in any case the argument of the book undermines. On others, I think his interpretation of the argument of the book is wrong. I also think his reading of Polanyi is misleading in some respects, notably through a neglect of what Polanyi quite insistently called the "personal." But I also think that Polanyi needs to be revised, or will ultimately need to be revised, especially in light of some issues that are important for cognitive science. All these issues are important for the community of those who share Polanyian concerns.

Practices as an Object of Knowledge and as Causal

Gulick thinks that my complaints about concept of practices are epistemic, that is to say that they somehow amount to saying that because a thing is difficult to know objectively it therefore does not exist. Although the book does on occasion make the uncontroversial point that practices are not directly accessible, the argument which Gulick attributes to me does not appear in the book. The focus of the book was relentlessly on one topic, mechanisms and the explanatory uses of the concept of practice. The argument was very straightforwardly against the usual explanatory uses of the concept of practices understood as something with causal powers but shared and therefore identical between individuals.

Let me briefly recapitulate the argument. To say that people “share” presuppositions or practices means that they have the *same* presuppositions or practices. The usual argument for this is transcendental: people do something, such as communicate; they could not communicate unless they shared the same framework; therefore they share the same framework. This argument mimics a standard strategy used by Polanyi and many others to argue that explicit rules are never sufficient and need to be supplemented by something tacit. But the argument that something extra (and tacit) is needed to explain, for example, communication or scientific discovery, *is not the same* as the argument for a shared framework or for the possession of the same practices. The argument for “sharing” or sameness requires us to believe that there is some mechanism by which the same rules, presuppositions, or practices get into the heads of different people. But if we consider the various possible strategies for solving this problem of transmission, we soon see that it is insurmountable. The claim that the *same* practices, presuppositions, and the like get into the heads of many people requires a means of transmission that is little short of magical.

The details of this argument are too complex to repeat here, but the point may be seen in a simple consideration. Ordinary communication is difficult, even if we use the full range of available explicit language, as anyone who has tried to write an instruction manual knows. One version of the tacit knowledge hypothesis, which I attack, makes the following astounding assumption: that people can (and routinely do) obtain perfect reproductions of the tacit possessions of others. In other words, people “share” extremely complex common frameworks. moreover, somehow they acquire these framework through means that are radically less error-prone than ordinary explicit communication is. The means in question must be much more effective than ordinary “training,” which is, of course, imperfect. My questions related to these hypothesized means, and I concluded, that they are phantasms: that acquiring the tacit possessions that people need is an imperfect training-like process that could not guarantee that people would “share” anything tacit, but could only, like training at its most successful, assure that people had habituated capacities to perform. Training of this sort only affects external similarities of performance: it tells us nothing about sameness of tacit possessions. Learning “from experience” is likely to produce an even greater diversity of habits than formal training.

The “habituation” alternative to “sharing,” once we look carefully, seems to accord better with what we know about the causal processes that actually operate in the world and with the known facts that practice theories purport to explain. This alternative account of what is going on when people learn to communicate, make scientific discoveries, and so forth, will be more plausible as an explanation because it does not appeal to any quasi-magical processes of transmission. Individual habituation (with the term being broadly construed to include all acquired learning that is tacit), I argued, does explain the same things, and we can even make some sense of such mysterious things as our common feelings by reference to the role of rituals and performances in inducing habits. This approach inverts the usual explanation of a tradition, for example, by saying not that its rituals are performed because people share a common framework, but that instead that rituals are behavioral technologies that produce a certain uniformity of habits--but a uniformity that is literally superficial, a matter of external similarity, with internal or personal consequences that vary from individual to individual. Prayer, for example, has effects on those who pray. But the effects vary from person to person.

Gulick thinks that this emphasis on cause and mechanism must represent some sort of crypto-scientism, behaviorism, or positivism. He quotes Polanyi, who makes a comment against what appear to be Hobbesian reductions of autonomous human activity to causal processes. But this line of attack is misdirected, because the argument outlined above says nothing reductive about anything *but* the “social” aspect of the notion of practices. Nothing about human agency, commitment, or autonomy is denied by the argument. Indeed, far

from undermining autonomy, it supports it by undermining the idea that people are trapped in a framework which is mysteriously reproduced within them, a notion that is a commonplace of postmodernism.

Objectivity, Reflexivity and Causality

The postmodernists, and the best of the social constructionists, such as Steve Woolgar (see his comments on the “methodological horrors” of representation in constructionism in *The Very Idea*²), have made an important point about the logic of social constructionism, and the reflexivity problem it produces. This is the problem of reflexivity: if the concepts of science are rooted in representational practices, what is the status of representations of representational practices? Are they “real” or are they also rooted in representational practices? It holds equally for the position Gulick takes, and may be seen very simply in the following question about tradition. Is the concept of tradition simply a narrative device that is found within a particular tradition, a fiction, so to speak, employed within a tradition? Woolgar, for one, accepts that the logic of social constructionist accounts of practices applies to social constructionism itself, and that these accounts are, so to speak, fictive.

The problem for every “non-causal” model of practices that I know of is that they cannot escape the “methodological horrors” that arise from the problem of reflexivity. The passage Gulick quotes from my book, which asks rhetorically whether “facts about practices [are] just ‘made’ facts” expresses this reflexive difficulty. As he admits, the basis for his own view of the subject is writers other than Polanyi, such as MacIntyre and Borgmann. The reflexive problem clearly does arise in their cases. Polanyi, in contrast, was concerned to root his account in the results of psychology, and not only accepted, unlike the postmodernists, that his views should be consistent with science, he actively concerned himself with empirical psychology³ a point I will make again below.

Any attempt to claim that practices are somehow not subject to considerations of causality and substance, including his own, are subject to this problem in one form or another, and in each case that I know of it leads to relativism, self-contradictory evasions, or dubious metaphysics, such as the idea that there is a fundamental metaphysical quality called “normativity.” Is saying this “scientism”? I don’t think so. The issue is not my invention, nor is it taken seriously only by “scientific” types. I can cite in my support the fact that social constructionists and others have wrestled with and drawn large implications from these problems of reflexivity. Is talking about cause by itself enough to convict one of scientism and slavery to the model of physics (or worse, to the wrong model of physics) as Gulick suggests? I do not think so. I made no commitments to a particular model of causality. Unless I am greatly mistaken, the notion of causal law never appears in the book: the philosophically different and more neutral terms “mechanism” and “process” are used throughout. My objections to the mechanisms of transmission postulated by various social theorists were not, as Gulick suggests, objections to postulation as such. After all, the mechanism of individual habituation I find more plausible is postulated or hypothesized as well. My argument is just that it is a strikingly more plausible hypothesis.

Frameworks and the Personal

Gulick says that my non-social account of tacit knowledge is not Polanyian. This is not an objection that admits of a simple answer. It is literally true. But what I say could be taken either as a critique of Polanyi

or as a clarification of some issues which are not entirely clear in Polanyi and which have become more important as a result of other writings on tradition, paradigms, and so on. I would also claim that in a very important sense my line of argument is spiritually closer to Polanyi than to many of these other writings, for reasons that need a certain amount of background to understand, and which relate to the notion of “personal,” which Gulick is right to raise.

Polanyi routinely objected to such things as theories of meaning that “depersonalized” meaning. At the same time, he was concerned that asserting the personal character of meaning would expose it to being reduced to the status of dogmatic subjectivity.”⁴ He could have said that meaning is secured from this fate by virtue of the fact that people share tacit frameworks of meaning. But Polanyi emphasized the personal, and said “repeatedly that we must reaccredit our own judgement as the paramount arbiter of all our intellectual performances” and that “I shall yet try to elaborate the structure of this ultimate self-reliance, to which this entire book shall bear witness.” He summarizes his own views by saying that “into every act of knowing there enters a tacit and passionate contribution of the person knowing what is being known, and that this coefficient is no mere imperfection, but a necessary component of all knowledge.”⁵

In retrospect, the important difference between Polanyi and writers like Kuhn is this emphasis on ultimate self-reliance, on the personal. Kuhn believed in shared frameworks, saw individuals as trapped in their frameworks, and was concerned with the problem of the circularity of justifying frameworks through evidence arising within frameworks. Kuhn had, as many interpreters suggest, a *social* theory of knowledge. It was precisely this social use of the notion of tacit knowledge and presuppositions that I attacked in *The Social Theory of Practices*. As the title of his great book makes clear, Polanyi saw *personal* knowledge as primary.

Polanyi, of course, said that he was “a person intellectually fashioned by a particular idiom, acquired through my affiliation to a civilization that prevailed in the places where I had grown up,” and that “all my amendments...will remain embedded in the system of my previous beliefs.”⁶ He did not think that this was the end of the matter, and that people are condemned to a socially determined relativism as a result. This was my conclusion as well: one can admit to the importance of what Polanyi called being “fashioned by a particular idiom” without falling into the error of thinking that this means that one is condemned to one’s intellectual framework as an iron cage in which one is totally dependent and unfree.

My way of thinking about what we can know and how we are dependent on frameworks is summed up in the slogan I used at the end of the book, which revised Stanley Cavell’s famous saying “We learn language and the world together,” by which he meant that the processes of learning the one were inseparable from the processes of learning the other. I said that we should add to this that “Not only do we learn language and the world together, at the same time as we learn them we acquire habits that enable us to be more or less proficient in using both language and the world.”⁷ By this I meant that the processes of learning “objective” or public things were inseparable from tacit processes of habituation. My point was that the feedback mechanisms of experience that produce habituation are personal, or individual, but at the same time bound up with learning an idiom and experiencing the world. This is closely *parallel* to the position taken by Polanyi between objectivity and subjectivity.⁸

Suppose, as I suggest, that our experiences are partly the result of habituation that is necessarily personal, as well as of idioms that are in some sense impersonal or public. This is an anti-objectivist supposition. For me, objectivity, in science, the law, or elsewhere, cannot be merely a matter of my individually applying

a public framework to the world, because the tacit element, which I take to be a matter of habituation, is personal, not objective. So, for me, objectivity is a public achievement or claim. To say that some fact is objective is to say that others will accept it, under the right conditions. I explain all this in part to explain why the term objective and its cognates do not appear in the book, and why I am not, as Gulick says, an objectivist.

“Telic” Practices: Personal vs. Social

Gulick notes that the book itself begins by admitting a *lacuna* in the argument. The book does not attempt to deal with what it calls telic or purposive notions of practice, such as the one which appeared in Kant’s essay on the saying that may be true in a theory but it is not in practice. There Kant argued that there was something left over when one attempted to reduce certain *purposive* activities to theory or to rules. Medicine is the primary example of this. The examples Gulick gives, of playing a game of chess, hosting a New Year’s party, going fly fishing, and so forth fit Kant’s model nicely: there is something left over in each of these cases that cannot be reduced to rules or to what can be said explicitly, that is to say to “theory.” In these cases, the skilled practitioner knows more than he or she can say.

I did not discuss these cases for a simple reason. I agree with the idea that there is something tacit “left over” in these cases. The question that motivated the book was the question of what the tacit stuff is, and particularly to one special aspect of the question: whether the tacit stuff could be said to be “shared” and in this sense “social.” The problems with the “yes” answer to this question arise whether one’s conception of practices is telic or not. So by declining to pursue the issue of telic practices I did not mean to say that the argument was irrelevant to them. I simply did not wish to burden an already complex presentation with responses to positions that I thought no one now held.

In any case, it doesn’t matter. The argument applies to telic practices as well. So the *lacuna* is less of an issue than it might appear, and indeed is not really a *lacuna* after all in the sense that Gulick thinks it might be, i.e. a loophole through which one might sneak the “social” notion of tacit knowledge I criticize. Understanding why this is so, however, is a little difficult, so I will have to subject you, gentle reader, to a digression on the problem of purpose.

The issue of telic practices is confusing because two quite different claims have often been run together. The first is the claim that the tacit knowledge in question is “social” rather than personal; the second is the claim that the purposes of the activity itself are tacit. Gulick’s examples are of cases where the purposes *themselves* do not appear to be tacit: the point of chess is winning, and there is nothing tacit about that; of fly fishing to catch fish, and so on. But not all of his examples need be interpreted in this way. Perhaps one might say that a great party-throwing host could tell whether a party was good without being able to explain what it was that made it good, or seek to throw a good party without being able to articulate in any depth what “good party” meant, and, better yet, strived to produce good parties.

In this last case, the purpose is tacit, and there is also tacit stuff, such as a sense of propriety, that the host needs to possess in order to achieve the purpose. There seem to be no cases in which the purposes are tacit but the rules themselves are all explicit. It is logically possible to claim, for example, that the law is nothing more than a body of explicit rules, but that its purposes are hidden and understandable only by reference to purposes that are inarticulable. This, however, is a very odd argument, for an important reason that I will discuss shortly.

In any case, it is an argument that runs in the opposite direction from Polanyi, who would say that the law is a body of explicit rules that can be used only on the basis of a vast body of tacit knowledge.

My view, and Polanyi's, would be that every case of tacit purposes is also a case in which something tacit needs to be possessed in order to achieve the purposes. The reason I did not devote a separate discussion to the telic case should now be clear: if the argument applies to the tacit things that need to be possessed to achieve the purposes, it applies to telic practices of which they are supposedly an element. The difficulty the book is concerned with is one that sets in when it is claimed that the goals are not personal but social. In the "personal" case, there is some point to saying that the host is guided in his party-producing activity by goals that in the nature of the case cannot be articulated--after all, none of us imagine that the success of a party can be reduced to elements or explicit rules, such as "properly heat the cocktail hot dogs," which, added together, would always and necessarily produce something that would count as a great party. To say that a collectivity is guided by inarticulable goals is a different matter.

Theories that purport to identify shared but tacit purposes that have actual causal effects on the world that inhere in an activity but in some mysterious way transcend the various purposes of the practitioners have a long history in social theory. They even exist today. Pierre Bourdieu claims to find hidden purposes such as the accumulation of cultural capital lurking behind the practices of various dominant social groups. There is, however, an objection, nicely formulated by Jon Elster, which I consider to be fatal to such theories. Elster says, essentially, that to have a notion of purpose it is a minimal requirement that some sort of feedback mechanism exist by which the activity in question can be modified as a result of changes in circumstances in accordance with the purposes.⁹

Without a feedback mechanism, Elster suggests, there is nothing to the notion of purpose, at least in the sense of a purpose that has effects on, and guides, behavior. If I have a thermostat on my air conditioner, it might be said that the thermostat has the purpose of regulating temperature and acts to switch the power on and off in accordance with this purpose. But thermostats have feedback mechanisms. They are connected to thermometers. The thermometers feed back to the switching mechanism when the temperature reaches a particular point in a range. Arguably, of course, such mechanisms can be characterized entirely in causal terms. But having a feedback mechanism is, so to speak, an essential condition for asserting that something has an inherent purpose or a purpose of the sort that practices are supposed to have, i.e. one that guides what people do.

Now we can see what is so odd about saying the law is fully explicit but has tacit purposes: if we mean that the law has purposes in the sense that it is guided by purposes, there must be a feedback mechanism. But there is no hidden feedback loop through which the tacit purposes of the law operate to produce the explicitly stated law. Legislators write the laws and vote on them, all explicitly. They may have tacit purposes, of course. But the explicit law is a fixed document, and thus like an unguided missile. It cannot be "guided" by tacit mechanisms, because its course is fixed, so to speak, at the point it becomes explicit. This is not say that the judges who apply and interpret the laws do not have tacit purposes, and "guide" the law in the light of these purposes. It is rather to assign them personal causal responsibility for this guiding, rather than to assign it to some unknown and mysterious regulative mechanism by which the course of legal interpretation is automatically righted.

The issue of tacit purposes may be put in Polanyian terms, and in terms of Polanyi's concerns: scientific discovery is goal-oriented or telic, but not in the sense that many skilled activities are. A golf champion

may have inarticulable skills, but the goal of getting the ball in the hole in the smallest number of strokes is not inarticulable. The goals of scientific discovery, in contrast, are not always articulable in advance in this way, and this fact greatly impressed Polanyi, who sought to understand how great scientists proceeded in ways that were apparently intuitive, but who wanted also to do better than to rely on notions like intuition. His alternative idea was that great scientists in some sense knew in advance what they were looking for, but could not articulate it until they found it.

In *Science, Faith and Society*¹⁰ Polanyi notes that astronomical observers making physical records of observations with telescopes correct their observations by a mathematical factor that represents their reaction time in marking the positions. This is called their personal coefficient. Different observers have *different* personal coefficients. This is why they are personal, and irreducibly so. I have no objection to this personal notion of tacit possession And I accept that individuals seek and are guided by goals that they cannot articulate. But I would like to know how this process works, and to find this to be a credible explanation of anything I would have to believe that there is a process or mechanism by which it might work.

Here is a suggestion about how it might. Consider the key question for Polanyi: are great scientists especially good at discerning the significant patterns in what they observe? Do they do so in particular cases of discovery because their awareness has been prefocused or refocused, again in ways that they cannot articulate, in such a way that the patterns that are significant are more obvious and recognizable to them than to other scientists? Polanyi would have given a “yes” to both questions. Recent work on connectionist ideas about the mind lend some additional plausibility to these ideas, in the sense that they point to “tacit” but entirely casual processes through which people learn and change their expectations without being aware of this happening. The case of the great scientist who anticipates but cannot articulate the discovery he or she is about to make seems to me to fit very nicely with the idea that individuals have trajectories of connectionist learning that differ and that enable them to do and learn differently, including making different discoveries. I think it is reasonable to say that there is a kind of feedback mechanism operating when people learn and change their expectations without being able to explicitly articulate these changes. I call this habituation, and give the example of a person who gets better at reading certain kinds of difficult philosophical texts.

One might say that there is some sort of feedback mechanism that operates in more or less the same way with groups. Musicians may develop a sense of one another that enables them to play together collectively and through a feeling for the quality of the performance regulate their behavior in accordance with the collective outcome, for example the recorded sounds that they can listen to after a session. But while the performance is a collective product, the feedback mechanism is individual: each member of the group hears for himself or herself and adjusts accordingly. Mutual adjustment, getting comfortable with one another, and so forth are not themselves collective acts, but rather processes of interaction and mutual taking-account-of by individuals using individual feedback mechanisms. If it is a matter of individuals adjusting to one another in the course of collective activity, I would claim, it is a matter of habituation, and personal.

Gulick wants to go beyond this. He suggests that “a Polanyian would insist that practices exhibit social structure and telic qualities” and that to reduce practices to individual habits is “arbitrary atomism.” My response to this is simple: to say that practices have telic qualities, as distinct from saying that individual practitioners have purposes, means that there are feedback mechanisms by which shared or social practices are guided by shared or social purposes, tacit or otherwise. If the only plausible mechanisms are individual ones, then the skills they produce through the continuous process of feedback will necessarily be personal, not

“social” in the sense of being “shared,” simply because people will get personalized feedback leading to personalized habits. Polanyi is, I think, of two minds on this: he saw the purposes of the legal or scientific tradition as in some sense inherent and discoverable, but also believed that they rested ultimately on personal acts of commitment. I accept the latter, but in the book argue that the tacit part of traditions can only be carried by and subsist in individual habits, by which I mean to include habitual dispositions.

Conclusion: What it Takes to be a Polanyian Today

Where does this discussion leave us with Polanyi? The legacy of Polanyi surely was meant as something to be continually rethought, and to be made consistent with new kinds of considerations, arguments, and scientific findings, even if that meant revising it. One central idea in Polanyi is “the well-known fact that the aim of a skillful performance is achieved by the *observance of a set of rules* which are not known as such to the person following them.”¹¹ I have altered the emphasis on this quotation to bring out the thesis that is now controversial: that skilled practice is a matter of the observance of a set of rules. I will merely cite two texts by John Searle, which express a widely but not universally accepted idea: that the notion of deep unconscious rule-following is incoherent, because rules are not self-interpreting, and the notion that there are in the mind rules to interpret rules and rules to interpret the rules that interpret rules *ad infinitum* is computationally impossible for physical brains.¹² The challenge for Polanyians is to deal with these arguments, even if it requires them to revise Polanyi. In my view, the arguments can be strengthened by these revisions, and that Polanyians have nothing to fear from science. In a sense, this kind of friendly revision was the aim of *The Social Theory of Practices*, a book rooted, as Gulick rightly observes, in a deep appreciation of Polanyi.

Notes

¹ Stephen P. Turner, *The Social Theory of Practices* (Chicago: The University of Chicago University Press, 1994).

² Steve Woolgar, *Science: The Very Idea* (London and New York: Tavistock Publications, 1988), 30-38.

³ I note that he was very concerned to find support for his suggestions about discovery in the experimental study of psychology. The late Donald Campbell told me that he had many conversations on these and other psychological issues with Polanyi in the early fifties, and the imprint of these concerns with causal psychological processes is evident in *Personal Knowledge* and elsewhere. I think it is wholly to Polanyi’s credit that he sought to make his own insights fit with the available evidence about the causal nature of the processes in question, as I have also attempted to do. Times change, and there are different ways of making sense of the problem today that enable us to point to different interpretations of the processes.

⁴ Michael Polanyi, *Personal Knowledge: Towards a Post-Critical Philosophy* (Chicago: The University of Chicago Press, 1962), 253.

⁵ *Ibid.*, 312.

⁶ *Ibid.*, 252.

⁷ Turner, *Practices*, 121.

⁸ “Parallel,” but not “the same as.” Polanyi solved the problem of our personal stake in frameworks by means of the notion of personal commitment to a framework. This notion I would question. How can one commit oneself to something tacit, and how can commitment be separated from the pre-existing possession of a framework? To use Polanyi’s language quoted here, if one is formed by an idiom, aren’t one’s commitments inseparable from the idiom? Don’t we acquire and make commitments as we acquire mastery of an idiom, in a process that cannot be meaningfully separated? Do we learn the language of love one day and “commit” to it the next? Or are we formed by this language and able to make the commitments we make because we have been formed by it, but at the same time able to learn because we are committed? I think commitment and learning are inseparable, and that “commitment” is a misleading concept, especially when used in connection with “framework.” Thus in my view there is a tension within Polanyi, which Gulick and I resolve differently. When Polanyi says “that we must reaccredit” our judgment I agree--but reaccrediting is a continuous process, coterminous with mastering and acting, and not a matter of a separable “commitment” to a framework.

⁹ Jon Elster, *Sour Grapes: Studies in the Subversion of Rationality* (Cambridge, London and New York: Cambridge University Press, 1983), 105-7.

¹⁰ Michael Polanyi, *Science, Faith, and Society* (Chicago: The University of Chicago Press, 1964).

¹¹ Polanyi, *Personal Knowledge*, 49.

¹² John R. Searle, *The Rediscovery of the Mind* (Cambridge, MA: The MIT Press, 1992), especially chapters 7 and 9, and John R. Searle, *The Construction of Social Reality* (New York: The Free Press, 1995), chapter 6.

Polanyi Society Membership

Tradition and Discovery is distributed to members of the Polanyi Society. This periodical supercedes a newsletter and earlier mini-journal published (with some gaps) by the Polanyi Society since the mid seventies. The Polanyi Society has members in thirteen different countries though most live in North America and the United Kingdom. The Society includes those formerly affiliated with the Polanyi group centered in the United Kingdom which published *Convivium: The United Kingdom Review of Post-critical Thought*. There are normally three issues of *TAD* each year.

Annual membership in the Polanyi Society is \$20 (\$10 for students). The membership cycle follows the academic year; subscriptions are due September 1 to Phil Mullins, Humanities, Missouri Western State College, St. Joseph, MO 64507 (fax: 816-271-5987, e-mail: mullins@griffon.mwsc.edu) Please make checks payable to the Polanyi Society. Dues can be paid by credit card by providing the card holder's name as it appears on the card, the card number and expiration date. Changes of address and inquiries should be sent to Mullins. New members should provide the following subscription information: complete mailing address, telephone (work and home), e-mail address and/or fax number. Institutional members should identify a department to contact for billing. The Polanyi Society attempts to maintain a data base identifying persons interested in or working with Polanyi's philosophical writing. New members can contribute to this effort by writing a short description of their particular interests in Polanyi's work and any publications and /or theses/dissertations related to Polanyi's thought. Please provide complete bibliographic information. Those renewing membership are invited to include information on recent work.

Stephania Jha's Integrative Interpretation of Polanyi

J. S. Pflug

ABSTRACT Key words: Michael Polanyi, integrative philosophy, tacit knowing, mind and body.

This review essay discusses Stephanian Jha's account of Polanyi's thought in her dissertation, Michael Polanyi's Integrative Philosophy (Harvard University, Gutman Education Library: Thesis J47, 1995); I criticize her understanding and use of Polanyi's notion of "from-at" integrations.

In her doctoral thesis, "Michael Polanyi's Integrative Philosophy" (Harvard University, Gutman Education Library: Thesis J47, 1995), Stefania Jha seeks to provide us with a more precise understanding of Polanyi's theory of tacit knowing. According to Jha, this theory grounds all explicit knowing in an active process of integration—the personal, tacit event of organizing clues into a meaningful whole in one's quest for truth, guided by an aesthetically sensitive vision of reality. She defends, expands and evaluates this interpretation while drawing extensively from Polanyi's entire corpus, including his personal letters, making her work an excellent introduction to Polanyi's thought. However, her understanding of Polanyi's work reveals no new treasures which previous interpretations have left veiled. Although Jha believes she is picking up "where Polanyi left off" (p.200) when she stresses Polanyi's distinction between *from-to* and *from-at* knowledge, her description of this distinction is quite ambiguous if not confused, as I shall detail.

Polanyi emphasized the role of belief in his earlier work, *Personal Knowledge*, but Jha notices that, in later works, he relied more upon the role of "integration" in personal action and thereby better depicted his concepts of knowing and truth. Jha characterizes Polanyi's theory of truth as an "open" version of coherence theory, "a blend of coherence-correspondence theories, with 'coherence theory' most strongly emphasized" (p.140). This accurately captures Polanyi's insistence that, although "truth lies in the achievement of contact with reality" (p.140), reality is known only through the selective and heuristic functions of personal knowledge; that is to say, it is only through a scientist's impassioned unity of purpose that he will discern "gestalten that indicate a true coherence in nature" (*KB*, 138). From this, Jha correctly concludes that the scientist's interaction with reality will always have an indeterminate character. As she says, "Integration is anchored in and evoked by the significance of the focus of attention . . . it opens meaning up to richer possibility" (p.152-153).

Jha's integrative interpretation reiterates what others have found to be most significant in Polanyi's theory of personal knowledge. First, he offered an account of scientific explanation (a form of "insight") that is grounded in tacit knowing. Because we can never specify the grounds on which we believe something to be true with complete certainty, scientific explanation "is a reinterpretation, a reconception, illuminated by previously unexplained connections and offering new principles" (p.158). Second, he hoped his work would overcome the prevailing deterministic, mechanistic worldview, and its insatiable appetite for reductionist explanations. In its place, he transformed his theory of stratified, hierarchical meaning into an ontological theory. As sounds are integrated into words and words into phrases, so too there are aspects of lower levels of reality which emerge to form more complex levels; furthermore, "each level is subject to dual control—its own laws and the laws of the level above it" (p.100).

Therefore, higher levels cannot be fully explained by lower levels, making reductionism impossible. Finally, with his emphasis on the inextricable personal element in all knowledge, he meant to bring scientific knowing closer to the humanities; more specifically, he aimed at “treating science as continuous with other areas of knowing in culture” (p.190).

From the perspective of the “standard view of science,” Jha recognizes that Polanyi’s theory of explanation leaves an unsatisfactory account of scientific truth, because Polanyi’s data are not independent of our conceptions. If facts can only be tacitly integrated into a theory as we strive to prove or confirm that theory, then all facts are theory-laden, depending on our conceptions and assertions. Jha admits that she finds this is very problematic, for Polanyi “seems to blur the distinction between a claim of contact with reality and actual contact with reality, and between the commitment to truth and truth itself” (p.108).

Ultimately, Jha contends that the only way for Polanyi’s theory of tacit knowledge to gain recognition within the scientific community is for someone to show how the “standard view” is untenable as a complete account of scientific knowledge. In Jha’s words, “if we can grant Polanyi that tacit knowing underpins explicit knowing, and both play a role in scientific knowing . . . [this] would allow Polanyi to claim that his theory of tacit knowing subsumes theories of explicit knowing, i.e., explicit knowing is a special case of tacit knowing” (pp.190-191). Therefore, Jha attempts to show the merits of Polanyi’s personal knowledge over that standard view of science by applying his epistemology and ontology to medicine as a way of uniting the humanist “soft” approach (which treats the patient as a person) and the scientific “hard” approach (which treats the patient as a mere bodily mechanism).

Ontologically, Polanyi’s philosophy offers, Jha suggests, an anti-mechanistic alternative which demands that the physician approach his patient with empathy, for his patient is not a mere mechanism, but a person. Using Polanyi’s notion of stratified, hierarchical (emergent) entities, Jha argues that a person (i.e., a mind) cannot be reduced to a mere collection of parts. Instead, a mind is a “closed causal loop,” an anticipatory system functionally dependent on an environment. The mind must be treated as a whole that operates in a non-linear fashion, and therefore “the ‘mind-brain problem’ falls irretrievably outside of science” (p.196). Hence, when a physician seeks to diagnose a patient, he or she will feel obligated to go beyond the mere instrumental advantages of asking the patient questions about her physical condition to an ontological attitude of treating the patient as a person.

Jha argues that Polanyi’s epistemology best describes the way physicians settle on diagnostic hypotheses as they talk with a patient about his or her condition. Because the mind is the meaning of the body, the physician could have no better place to begin in assessing the condition of the body. Jha identifies the *from-to* conception of knowing as this revelatory encounter between the physician and his patient; in fact, she says the physician needs to employ this way of knowing “as the foundation of the clinical relationship” (p.201).

While the tacit, personal *from-to* form of knowing is fundamental in giving global insight (i.e., a vision of coherence) to the physician, he still needs to treat the patient’s body as a mechanism in his effort to validate his diagnostic hypothesis. Therefore, according to Jha, “in the stage where the scientific problem-solving is predominant, the ‘from-at’ mode of knowing is in operation and the ‘from-to’ mode is latent, the perception of the patient as the phenomenological-existential body is in subsidiary awareness” (p.203). Thus it is the physician’s responsibility to learn how gracefully to “oscillate” between personal

integration (i.e., *from-to* knowing) and empirical analysis (i.e., *from-at* knowing), so that the effectiveness of his work is enhanced.

In conjunction with this rather curious notion of “oscillation,” Jha criticizes some of Marjorie Grene’s discussion of Polanyian points. Jha contends that Grene inappropriately discusses the *from-to* structure of knowing as a ubiquitous “doctrine” rather than a flexible principle; Grene does not regard the *from-to* structure as an optional working hypothesis. Jha is correct that, for Grene (and I would say Polanyi, too), all awareness moves from subsidiaries to the focal object of attention. As Grene puts it, there is no structure of knowing other than *from-to*: “No cognitive performance ever has any other structure” (“Tacit Knowing: Grounds for a Revolution in Philosophy,” *Journal of the British Society for Phenomenology* 8:3 (Oct. 1977), p.169). But this conviction, argues Jha, makes Grene unable to see how the difference between Polanyi’s *from-to* and *from-at* knowing may contribute to Polanyi’s epistemology.

Jha believes that Polanyi speaks of *from-at* knowing as having a fundamentally different structure of attention than *from-to* knowing; in *from-at* knowing, there is a “shifting [of] one’s attention from the direction on which the subsidiaries bear and focusing instead on the subsidiaries themselves” (“Logic and Psychology,” *American Psychologist* 23.1-6 (1968), p.39). Jha proposes a way of distinguishing these two forms of knowing: while *from-to* knowing deals with phenomenal awareness (i.e., wholes), *from-at* knowing deals with information (i.e., particulars). Using this distinction, Jha argues that *from-at* knowing is made manifest when a physician gives focal significance to a former subsidiary (e.g., the perception of the patient as a mechanical body) of an irreducible meaning (i.e., the perception of the patient as a mind) which was grasped by the physician’s *from-to* knowing. Yet she also states that the former meaning of *from-to* knowing (i.e., the mind) now becomes subsidiary. In this rather convoluted scheme, Jha does not convincingly show how *from-at* knowing does *not* move from subsidiaries to a focal object of attention. Far from being opposed to Grene, it seems that she, too, upholds the *from-to* structure of all knowing as a “doctrine.”

As far as I tell, it seems Jha’s chief motive for distinguishing these two forms of knowing is to provide a way in which we can give special significance, in the case of *from-at* knowing, to the subsidiary perception of an irreducible meaning (i.e., “the perception of the patient as the phenomenological-existential body”). If this is an accurate portrayal of Jha’s position, she must demonstrate how a knower could give focal significance to his or her object of attention (i.e., the patient’s body), while also giving another form of attention (focal?) to the latent, subsidiary meaning of the person. What exactly does it mean to say that *from-to* knowing, including its inherent focal awareness, can become consciously latent? Polanyi’s position is that we cannot give focal significance to two different objects at the same time. Jha seems to be proposing an expansion of Polanyi’s account.

We are left with the question of what Polanyi meant by distinguishing *from-at* and *from-to* knowing. Did he really intend to create a substantial difference between two forms of knowing, or merely to stress the contrast in the functions of the mind’s two forms of awareness (i.e., subsidiary and focal)? Although Polanyi can be easily misread on this issue, I believe the latter is the correct interpretation. As he says, “Tacit knowing is a *from-to* knowing. Sometimes it will also be called a *from-at* knowing, but this variation will be only a matter of convenience” (“Logic and Psychology”, p.29).

In “Life’s Irreducible Structure”(*KB*, 225-239), Polanyi emphasizes that there is a qualitative

difference between the reality of the brain and the reality of the mind by demonstrating how a subject viewing a cat and a neurologist viewing this subject's brain are related to the subject's brain in different ways. Both the subject viewing the cat and the neurologist viewing the subject's brain have *from-to* knowledge, but the neurologist cannot be said to fully participate in the focal meaning of his or her subject's object of attention (i.e., the cat); however, this is only because the neurologist's attention is focused on the subject's brain, and not the cat.

While the subject is subsidiarily relying on his brain to integrate (i.e., interiorize) the meaning of the sense data concerned with the cat and his body's responses, the neurologist does not—in fact, cannot—rely on the subject's brain in this same way; that is to say, the neurologist can neither participate in nor know of the subject's *from*-knowledge. Instead, the neurologist has a focal relation to the subject's brain and subsidiarily relies on his own brain (among other things) to interiorize the mechanics of how the subject's brain neurologically processes the information about the cat to help support, for example, his theory of how perception is the result of neural gestalts. From the perspective of the subject viewing the cat, the neurologist is temporarily “alienated” from integrating the perception of the cat, because his attention is focused on the subject's brain. Here, *from-at* knowing is the term Polanyi uses to refer to the neurologist's alienation, for he cannot participate in knowing the subject's mind; rather, he can only participate in his own understanding of the subject's neurological processes.

Jha could better achieve her hope of grounding all explicit knowing in tacit knowing by demonstrating how the humanist “soft” approach and the scientific “hard” approach do *not* have to be *united*, for these two approaches are always already united. Beyond this, she could cite case studies showing how those physicians who temporarily bracket their abundant technical skills and methodological assumptions in order to communicate authentically with patients actually give more accurate diagnoses. For example, when a friend of mine once complained of respiratory problems, no appetite, and abnormal fatigue, a physician followed these clues no farther than assigning them to a mere cold. However, a month later after an x-ray, another doctor discovered a tumor the size of a volley-ball in his (now collapsed) left lung.

I believe Jha was on the right track in stressing the revelatory encounter between the physician and his patient as the foundation of the clinical relationship, but her attempt to make the epistemological structure of this encounter utterly unique from the physician's investigation of the patient's body is unnecessary. Also, I would argue that Polanyi's theory of tacit knowing can best serve the physician by helping him understand the importance of subsidiarily relying on the art of communication, so he may learn to trust the authority of the patient's experience, and then integrate it into his diagnosis. Polanyi argued, “A true communication will take place if, and only if, . . . assumptions of [mutual] authority and trust are in fact justified” (*PK*, p.206). Rather than solely relying on the often cold, shallow and general techniques one learns in medical school, a physician must develop a virtue of personal concern as he strives to interiorize the patient's experience as a—or in some cases *the*—key subsidiary clue to interpreting the patient's sickness or injury. In other words, as opposed to the physician imposing his conceptual framework onto the patient's symptoms—and as a consequence, neither questioning his own assumptions nor entering into an interpretative relationship with his patient—the physician should “participate feelingly” (*KB*, p.149) by communicating authentically with his patient.

Notes on Contributors

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Stephen Turner is Graduate Research Professor of Philosophy at the University of South Florida, Tampa. He has written extensively on the philosophical aspects of social theory, and has dealt with specifically Polanyian issues in a variety of places, including his book with Regis Factor, *Max Weber and the Dispute Over Reason and Value* (Routledge, 1984), which has a chapter on the intellectual context of the revival of the concept of tradition in the wartime and postwar period, especially in Britain. He has also worked extensively on the problem of the governance of science, and is presently engaged in a major project on the constitution of science which is designed to update the analyses made by Polanyi in such texts as *The Logic of Liberty*.

Walter Gulick teaches at Montana State University-Billings where he also now serves as Director of International Education and Honors Programs following a stint as interim Provost. He was a Fulbright Scholar at the Technical University of Budapest in the Spring term of 1993 where he helped put together an early English issue of *Polanyiana*. Gulick serves as the *TAD* Book Review editor; he has written a number of reviews and articles which have been in *TAD* over the years.

Polanyi's Integrative Philosophy and My New Interpretation: A Response to Pflug's Review.

S.R. Jha

ABSTRACT Key Words: from-to awareness, from-at awareness, scientific truth, philosophic roots, intellectual biography.

In this response to Jeff Pflug's review of my dissertation Michael Polanyi's Integrative Philosophy, I note that Pflug focused on my discussion of possible extension of Polanyi's epistemology; he has also taken my statements on scientific truth out of context. In addition, he ignored the four major elements of the dissertation, thereby not giving the reader a "map" to the meaning and the rationale of the work – an intellectual biography of Polanyi.

I am glad J.S. Pflug found the dissertation to be a good introduction to the study of Polanyi. I am also gratified that he found my exploration of Polanyi's speculations on *from-at* knowing interesting, though he disagrees with me.

He is in good company. Marjorie Grene, who kindly commented on this dissertation some years ago also disagrees with me on this point, as well as on my label "doctrine" applied to her comment in her famous 1977 essay "Tacit Knowing, Grounds for a Revolution in Philosophy" in the *Journal of the British Society for Phenomenology* (8, October, 1977). In this essay and in her comments to me she held that Polanyi committed an error and obscured his revolutionary idea of *from-to* structure of all knowing by introducing a distinction between *from-to* and *from-at* awareness. All knowing has a *from-to* structure. In the dissertation (pp. 49-51) I explored the implications of a late development in Polanyi's thought, i.e., that he wanted to make the distinction between *from-to* and *from-at* awareness for the purpose of pointing out the difference between the way of knowing another mind and the way of knowing another body. This move was not for the purpose of reviving Cartesian dualism, as Grene feared, although Polanyi's use of the term "dualism" is fluid and somewhat misleading. The context of his speculation offers some clarification. Polanyi speculated that the mind and the body are known by differently directed attention. (see his Logic and Psychology and The Tacit Dimension). This is the crux of the claim and the point I explored. Grene objected to Polanyi's *from-to from-at* distinction, "For the theory of mind mediated by the doctrine of tacit knowing is a theory of mind as fundamentally and irrevocably incarnate." (Grene, p. 169) My label "doctrine" referred to this statement, in which I interpreted "doctrine" in the strong sense as an unchangeable rule. Grene apparently intended "doctrine" in the sense of "theme", the weaker sense of the term. This was not clear to me.

My point in exploring Polanyi's speculation on *from-at* knowing was to see how a "loose end" could be extended and elaborated. If Polanyi's theory of tacit knowing is partially open, analogously to good scientific theories, then it should have "intimations of fruitfulness" in the future. That is, his theory also should have a scope still left indeterminate. My exploration of *from-at* knowing was an exploration of the implications of Polanyi's epistemology to test its fruitfulness. Although the dead cannot defend themselves, I think that Polanyi who was trained in the sciences would agree with me. About the somewhat strong label "doctrine," I belatedly

apologize to Marjorie Grene; it should be mentioned, that the discussion of these implications of *from-at* is but a few pages in the dissertation.

Pflug's descriptions of some traditional points of Polanyian philosophy in the dissertation are correct. However, I have not "recognized that Polanyi's theory of explanation leaves an unsatisfactory account of scientific truth, because data are not independent of our conceptions"(20) nor did I "admit that (I) find this is very problematic, for Polanyi" (as Pflug quotes from my dissertation) "seems to blur the distinction between a claim of contact with reality and actual contact with reality, and between the commitment to truth and truth itself" (20).

These "admissions" are taken out of context. The first point, that data are theory laden, is a commonplace now, though it may have been an "unsatisfactory account" in the eyes of the opponents of Polanyi and Hanson at the time of the Polanyi-Grunbaum debate. This point is clear in the dissertation, as the long chapter devoted to Polanyi's epistemology shows. The dissertation quotation in the second point, needs to be put in context of my discussion and broader evaluation of Polanyi's philosophy, where I show Polanyi's notion of the two poles of knowing: the personal pole (claim of contact with reality, claim of truth) and the external pole (contact with reality, truth itself--Jha, p.167). Pflug has not paid attention to my gradual development of the argument. The opinions presented in the quotations from sources and ideas referred to in the process of building of the argument have been attributed to me as my assessment of Polanyi's whole philosophy. This is unfortunate.

It is also unfortunate, that the four obvious elements of this intellectual biography-- the roots and development of his thought--have not been pointed out to guide the potential reader. They are these:

- (1) The detailed discussion of Polanyi's philosophical roots in Kant and in Kant's "children"-- from Gestalt psychology through phenomenology and Dilthey's existentialism, to Peirce's logic.
- (2) The laying out of the conceptual model to show how these roots are transmuted into Polanyi's *from-to* schema of tacit knowing. I published this model in "A New Interpretation of Michael Polanyi's Theory of Tacit Knowing: Integrative Philosophy with 'Intellectual Passions'" in *Studies in the History and Philosophy of Science*, (28:4 [1997]: 611-631).
- (3) The analysis and demonstration of the structure of Polanyi's epistemology as can be seen in mathematics, physics and biology. This analysis is the groundwork for the argument for the nature and justification of personal knowledge culminating in the Polanyi - Grunbaum debate, that is the debate between the "personal knowledge view of science" and the "standard view of science." It provides the intellectual backbone for Polanyi's innovation.
- (4) The biography, which uses untapped archival materials in Hungarian between Polanyi and his colleagues of his formative years as a chemist, and correspondence with family members, as well as early essays unpublished at the time of dissertation writing. These archival materials enrich the background, enhance the nuances and point to the origin of Polanyi's concerns with civic freedom grounded in moral obligation, and with freedom of thought. These newly introduced materials provide a balance to those interpretations, which remake Polanyi in an image approximating a conservative British Episcopalian.

I believe, all four of these aspects are “new treasures,” especially as elements of a portrait of “man in thought,” even though Pflug did not find them so.

It is true that W.T. Scott has written of Polanyi’s philosophy as a Gestalt Philosophy (Polanyi did not agree with this assessment (Jha, p. 33)) and Marjorie Grene’s assessment of Polanyi has a distinctly phenomenological flavor. However, most commentaries did not (at the time of writing) go beyond these two layers; neither Dilthey’s existentialism nor Peirce’s logic has been explored in connection with Polanyi in the manner the dissertation has analyzed these sources, showing the layering and development of Polanyi’s epistemology. My documented roots of Polanyi’s philosophy in Kant especially with respect to the crucial dynamic of Intellectual Passions, the fulcrum of “personal” in personal knowledge, has not been previously analyzed thoroughly, to the best of my knowledge.

That there is an aesthetic element in Polanyi’s conception of scientific creativity is obvious, but the philosophical roots of this element have not been so. The same can be said of the notion of “universal intent.”

Overall, the review of the dissertation is of its highly selected parts, missing (and not indicating) the organic structure of the work. This has the effect of missing the meaning and rationale of the work. The meaning is that Polanyi owes more to Kant than he tells, and the rationale is an accessible conceptual model of Polanyi’s epistemology. In spite of this, I appreciate Pflug’s recommendation of the work as a good introduction to Polanyi.

Economics, Science, and Knowledge: Polanyi vs. Hayek

Philip Mirowski

ABSTRACT Key Words: F.Hayek; M.Polanyi; J.D.Bernal; economics; social studies of science; philosophy of science; epistemology

The relationship between Friedrich Hayek and Michael Polanyi is documented and explored with respect to philosophy and economics. Their respective positions on epistemology and science are shown to fundamentally govern their differences with regard to the efficacy of government policy with regard to the economy.

I. Philosophy of Science as Economics

It frequently happens lately that when I first encounter strangers who may have some familiarity with my work, they ask me: Aren't you really a philosopher of science, or do you consider yourself an economist? Leaving aside my own ambivalence about the state of the economics profession in the United States, I find myself increasingly responding that I see very little to distinguish the concerns of the two disciplines at the close of the twentieth century, and that a competent understanding of the one often requires a proficient understanding of the other. Indeed, it is bland and complacent ignorance of the history and practices of the sciences that permit economists to make such outrageous statements about the capacities of agents to accumulate and process knowledge, just as it is the disdain for and ignorance of the history and practices of economists which leads philosophers to make such unwarranted statements about the "marketplace of ideas." But the situation is not uniformly one of mutual incomprehension and reciprocal ignorance, however much one can find instances of both. Some philosophers of science are coming to acknowledge that there just might be a pervasive political economy of epistemology, while some historians are beginning to uncover the numerous links between politics and beliefs about how science does (and sometimes doesn't) work.¹

Another reason that the situation is not uniformly bleak is that there have been many figures in history who have straddled the divide between economics and the philosophy of science; it is only our own historical amnesia and narrow academic disciplinarity that prevents us from recognizing this fact. A few names that spring to mind in this regard are David Hume, William Whewell, John Stuart Mill, Auguste Comte, William Stanley Jevons, Charles Saunders Peirce, John Maynard Keynes, Nicholas Georgescu-Roegen--the list could go on and on. But in this essay I want to focus on the really remarkable concentration of figures who came out of Central Europe at the beginning of this century and whose work was indelibly marked by the political events of the time. A wider purview would need to encompass the political economy of the Vienna Circle², the Wittgenstein/Sraffa connection, the impact of such thinkers as Karl Menger, Jr. and Oskar Morgenstern, and the motivations of figures such as Karl Popper and Paul Feyerabend.³ However, in the interests of tractability, but also timeliness, I will confine myself to only two such figures: the Nobel prize winner Friedrich von Hayek, and the physical chemist-turned-philosopher Michael Polanyi.

Hayek economists have heard of, but Michael Polanyi is probably a different matter, although they

often have a glancing familiarity with Michael's brother Karl Polanyi, who wrote *The Great Transformation*. Michael Polanyi was born in 1891 in Budapest, and died in Northampton England in 1976; and thus he was a near contemporary of Hayek, born in 1899 in Vienna and living until 1992. However, his metric of nearness to Hayek can be defined in more than simple chronological terms. They both made the long trek from their initial disciplines to philosophy in amazingly parallel trajectories. The break with their initial identities was made roughly simultaneously, in the 1930s, and for roughly the same reason, namely, opposition to developments they saw as exemplified by the regime in the Soviet Union. Both were deeply disturbed by intellectual trends in Britain at the time, where they were both in residence—Hayek at the London School of Economics, Polanyi at the University of Manchester. Both wrote books on macroeconomics; indeed, in some quarters the roster of Polanyi's disciplinary credentials reads: physical chemist/economist/philosopher.⁴ But more importantly, they knew each other personally and were intimately acquainted with each other's work, and for a very good reason: both were essentially working on the same problems from the mid-1930s until the ends of their respective careers. Although neither much acknowledged this in print, it was the subject of a brief section of an interview with Hayek in 1978:

Buchanan: Let me ask you about your relationship, or did you know or how close were you, to Michael Polanyi? Did you know him very well?

Hayek: Yes, he was for a few years my colleague on the Committee on Social Thought (at the University of Chicago), and there was an interesting relationship for a period of ten years when we happened to move from the same problem to the same problem. Our answers were not the same, but for this period we were always just thinking about the same problems. We had very interesting discussions with each other, and I liked him personally very much. I think, again, he is a somewhat neglected figure, much more—well, I think he suffered from the usual thing; if you leave your proper subject, other people regard you as an amateur in what you are talking about. But he was in fact very competent. I would almost say he's the only non-economist that I know who wrote a good book on economics.⁵

Two quick parenthetical comments help provide necessary background here: First, it may sound as though the decade to which Hayek is referring occurs late in life, during his tenure at the University of Chicago, but this would be misleading. Examination of the Papers of Michael Polanyi at the University of Chicago suggests rather that the decade of closest contact was the mid-30s to mid-40s, a fact that takes on extra significance when one realizes this is the decade of Hayek's "Transformation", as Bruce Caldwell calls it.⁶ Secondly, Polanyi wrote two books on economics; so Hayek is implicitly rejecting the second of them with his back-handed compliment, as I later will explain.

There is an interesting story of neglected intellectual histories and tangled cross currents of influence here, one that fascinates me, but I will not digress upon the knotted narrative, except insofar as it bears upon my purpose, which is to discuss the importance of the philosophy of science for an understanding of the nature and significance of the treatment of knowledge in economics. Hayek's primary warning against the pretensions of socialist planning was that human knowledge is intensely personal and irretrievably distributed throughout the population in such a manner that it would be impossible to collate, assimilate and act upon it within the ambit of any collective entity which aspired to better or even match the co-ordination capacities of markets. This is the message which is developed from his article "Economics and Knowledge" of 1937 through his well-known article, "The Use of Knowledge in Society" of 1945; it also animates the series of articles on "scientism" which

were later collected together as *The Counter-Revolution of Science*.⁷ It is important to note that this is uniquely a claim about epistemology, individual and social; and further that it was couched in an explicit discussion of the nature and character of scientific knowledge. Hence, in this sphere, philosophy of science and political economy became fused together into a single set of propositions.

In these reflections, I want to demonstrate that Michael Polanyi shared these very same concerns with Hayek in exactly the same time period; and furthermore, held discussions with Hayek while Hayek was formulating his own positions. What is fascinating for me is that Polanyi ultimately arrived at different answers, as Hayek acknowledges: different answers concerning the institutional character of science, different perspectives on the personal character of knowledge, and different prognoses concerning the future of political economy. I personally think Polanyi's answers were richer and better supported with subsidiary arguments than Hayek's, though that is certainly open to dispute. However, prosecuting the comparison will raise the issue of why Hayek has been lionized while Polanyi has largely been forgotten—except, of course, by a few philosophers.

II. How Michael Polanyi Became an Economist

Some biographical information on Polanyi is in order, if only to situate the events I shall cover in context, and to make up for the lack of any published biography. Michael Polanyi was the son of a Jewish civil engineer in Budapest who lost his fortune in 1899, but whose family maintained contact with a wide artistic and intellectual circle. He became a medical doctor in 1913 and served as a medical officer in the AustroHungarian army during World War I, also taking a Ph. D. in chemistry at the University of Budapest in 1917. He moved to Germany with the Hungarian uprising in 1919, and attained a position at the Kaiser Wilhelm Institute in the fall of 1920. He lived in Berlin from 1920 until 1933, becoming well-known as an expert on the adsorption of gases and crystallography, and developing a circle of friends which included Alfred Einstein, Eugene Wigner, Leo Szilard, John von Neumann and Max Born, among other illustrious scientists.⁸ 'With the rise of the National Socialists to power in 1933, Polanyi accepted a chair in physical chemistry at the University of Manchester, which he held until 1948, exchanging it for a chair in Social Studies at Manchester. This was the period of maximum overlap with Hayek, who held the Tooke Professorship in political economy at the London School of Economics from 1932 to 1949. After that they drifted apart, with Hayek accepting a position at the University of Chicago in 1950, while Polanyi stayed at Manchester until accepting a research fellowship at Merton College Oxford in 1958.⁹

It was only after the move to Manchester that Polanyi became actively involved in economics, with all his publications in that area falling within the period 1935-1946; this in itself goes a long way towards explaining the close contact with Hayek. Why did he relinquish a stellar career in physical chemistry in exchange for a tenuous perch in a subject in which he had no standing or credibility? The answers range from the prosaic to the profound. The first is simply that the move from Berlin to Manchester made him profoundly unhappy, and as his friend Wigner writes, "I doubt he was ever again as happy as he had been in Berlin."¹⁰ It does not appear that he ever felt as at ease in the community of British chemists as he had in Berlin. The second reason was one shared by a whole raft of trained physical scientists who moved over into economics in the 1930s: they were driven to distraction by the economic and social upheavals of the Great Depression, and felt that their scientific training might allow them to make a special contribution to solving those problems. The third reason was more specific to Polanyi: he made a trip to the Soviet Union in April 1935

at the invitation of some scientific confreres, and he was appalled at what he saw there. As he tells us in his book *The Tacit Dimension*, he was shocked to the core by Bukharin telling him that in a socialist regime there would no longer be anything called “pure science.” This galvanized him to quickly pen his first book outside of physical chemistry, *USSR Economics*, in 1936; it was the first serious critique of Soviet economic statistics published in the West. The fourth and final reason for his turn to economics was the set of developments in British science in the 1930s variously known as the “Social Relations of Science” movement, the Association of Scientific Workers, or “Bernalism.”

The events of the science planning movement in Britain have yet to receive comprehensive study.¹¹ For our own purposes it is enough to suggest that the British science planning movement of the 1930s and 40s was as important as the rise of Keynesian economics, the “socialist calculation controversy,” or the growth of the Communist Party in provoking what we now think of in retrospect as the Hayek Critique of socialist planning.¹²

This has been obscured by Hayek’s subsequent references to Karl Popper as his staunch philosophical beacon, which have only served to muddy the waters. During the important gestation period for Hayek’s Transformation, he was in close contact with Polanyi about refutation of the movement.¹³ I quote from his letter to Polanyi dated 1 July 1941: I attach very great importance to these pseudo-scientific arguments on social organization being effectively met and I am getting more and more alarmed by the effect of the propaganda of the Haldanes, Hogbens, Needhams, etc. I don’t know whether you’ve seen the latest instance, C.H. Waddington’s *Pelican on the Scientific Attitude*. I think this last specimen is really quite contemptible...” [MPP:4:5].

Polanyi himself had numerous motivations to be one of the first to jump into the fray with the “Bernalists”: Bernal was one of the other premier crystallographers in Britain, but his collegial relations with Polanyi were not all that close; Bernal’s 1939 *Social Functions of Science* had become a best-seller; Polanyi was revulsed by his Communist sympathies and praise of the Soviet Union, and distressed at the increasing evidence of Bernal’s influence in journals such as *Nature*, in the British Association and in the highest levels of government.¹⁴

Thus Michael Polanyi progressively opted for social theory in lieu of physical chemistry. In the decade 1935-45, this assumed three alternate manifestations: (1) some early essays on the social structure of science, to be described shortly; (2) empirical work describing economic conditions in the Soviet Union; and (3) a project for cinematic treatment of economic theories for the purposes of popular education. The last may seem incongruous, but was of major importance for Polanyi, since it was intended to counter what he considered to be economic fallacies spreading throughout the citizenry, which would undermine the future of democracy. Ultimately he managed to get two films produced. The first, the 1938 film entitled “The Workings of Money”, already revealed some nascent Keynesian leanings.¹⁵ The initial concentration upon the topic of money was no accident, however, given that Polanyi initially shared Hayek’s conviction that monetary disturbance was the primary cause of business fluctuations. Consequently, in the early 1940s Polanyi was a rare bird, indeed: a respected natural scientist who voiced adamant hostility to Communism and unremitting criticism of the Soviet Union, adamantly rejected all talk of planning of science or of the market, and yet stood relatively isolated as a strong supporter of Keynesian macroeconomics.

So why did Hayek persist in regarding Polanyi as an ally in the 1940s? The short answer is that their

commonalities overrode their differences. Their critique of the Soviet Union was essentially the same, even though Hayek did not actually engage in any empirical work on the topic. They both held liberty as the primary political virtue, to be defended above all others. There was also a practical consideration: up until *The Road to Serfdom*, it was Polanyi, and not Hayek, who was the more visible and publicly effective spokesman against the Left in Britain. It was Polanyi, for instance, that the BBC recruited to debate the Marxist Julian Huxley on a series of radio programs. But, most importantly, Hayek had just embarked upon his own crusade against Bernalism and the science planning movement with his first installment of “The Counter-revolution of Science” in the London School of Economics house journal *Economica* in February 1941. Hayek’s counterblast to “scientism” never quite managed to make it out of the realm of French 19th century texts, so it was not so very clear to many readers precisely whom the modern targets of his wrath might be; but this was made much more transparent in some less accessible texts as his review of Polanyi’s 1940 collection of essays: “The analysis of Professor Bernal’s book in the essay on the ‘Rights and Duties of Science’ is perhaps the most illuminating discussion yet attempted of the psychological propensities which so frequently turn the man of science into an ardent advocate of central planning, and of the inconsistencies which this attitude involves.”¹⁶ Indeed, it was Polanyi and not Hayek who was situated at the axle of a vast wheel of controversy over political economy and science in Britain in the 1940s.¹⁷ The spokes radiating outward from Polanyi led to the most amazing collection of natural scientists (such as Max Born) and literary figures (such as Arthur Koestler); but his ability to maintain intellectual engagement with such diverse economic thinkers from Hayek to J.R. Hicks to Karl Mannheim to his own brother Karl was nothing short of miraculous. In another context, his friend Wigner called him an “artist of encouragement,” and that skill is revealed in the quality and candidness of expression he evoked from his correspondents.¹⁸

Quite early in this process, before Hayek had demonstrated any palpable interest in epistemology, we find Polanyi already foreshadowing his later positions on tacit knowledge in a note in *Philosophy of Science* in 1936.¹⁹ He wrote there, “if at any time chemists would have been so ill-advised as to let themselves be frightened by physicists into abandoning all vague methods, and to restrict themselves to the field where exact laws (or what are supposed to be such by physicists) pertain, the development of chemistry would at that moment have stopped dead...” He then suggested the description of chemical substances had much more in common with “the art of commanding human behavior.”

Nevertheless, Polanyi distinctly began to intellectually diverge from Hayek by the late 40s, if not before, around the same time that he essentially left economics behind to become a full-time philosopher. By most accounts his most significant books appeared after this period: *The Logic of Liberty* (1951), his magnum opus *Personal Knowledge* (1958), and *The Tacit Dimension* (1966). It is especially in these latter works that he carried on a lonely crusade against the logical empiricism and positivism which dominated the philosophy of science in that era; yet for him it was not simply an academic diversion, but part and parcel of a crusade to diagnose the modern malaise wherein science and morality were regarded as being at odds, and intellectual freedom seemed to lack all rational justification. Towards the end of his life, he felt that he had not been all that successful in his campaign.²⁰ Perhaps this was in part because he had earned the reputation of an incurable moralist and inveterate sermonizer by the 1960s; unlike Keynes, he was not a Cassandra that people suffered gladly. Another possible explanation is that he tended to get bracketed together with Thomas Kuhn in the 1960s and 70s as another author who had uncovered the repressed irrationalist component of science. Neither author would have agreed with that interpretation, but full explication of the issues involved would carry us too far afield from our present task. Instead, we shall outline how Hayek and Polanyi, starting from positions relatively close to one another, came to espouse epistemologies so diametrically opposed that it should make us re-

evaluate our own grasp of the case for freedom in the academy and in the marketplace.

III. Hayek vs. Polanyi on the Nature of Knowledge

Throughout most of the 20th century, questions of economic planning have been intimately bound up with conceptions of what knowledge is or could be, and disputes over how it may or may not come to be known. Michael Polanyi felt the weight of these questions in the 1940s, and fairly quickly came to reject Hayek's epistemological stance as inadequate to the task at hand. He never opted to discuss Hayek's approach in his major books, but we can reconstruct his objections from reviews and correspondence. To begin, he felt that the wistful nostalgia for Burke and Acton would not suffice to provide foundations for modern philosophy or politics. "But is it certain that our disorders can be clearly defined in the words of an age so remote in its unsophisticated integrity? The attempt may entangle us in contradictions."²¹ Far from the standard scientific fascination with the shock of the new, Polanyi's objection here was rather that Burke and Acton praised tradition, but what they had had in mind surely could no longer be commensurate with what "tradition" would mean in the 1940s; and furthermore, Hayek absolved himself from much that his own contemporaries would regard as stabilizing traditions—for instance, specific religions, or Cartesian abstraction. The problem was that the content of "tradition" would appear as idiosyncratic and arbitrary as tyranny itself if it were not unpacked in rational discourse. This did not imply for Polanyi that all tradition must be reduced to rational stipulation; the one commitment he did persist in sharing with Hayek was acknowledgement of the inarticulate component of practice and a disdain for what Hayek called "constructivism." What Polanyi held against Hayek was that he was apparently not willing to describe the interplay between the inarticulate and rationalizable aspects of practice, be they in the marketplace or elsewhere, and therefore, he had effectively reneged on the promise to theorize the role of knowledge in economics.

As one might expect, Hayek's crusade against "scientism" also made Polanyi nervous. Hayek's grasp on the natural sciences was tenuous, which created some problems, but what bothered Polanyi more was the tendency to tar all scientists and engineers with the same brush, accusing them of narrow technical training and a predilection for mechanical rationalist prediction and control; anything that didn't fit their models wasn't worth knowing. Polanyi, as we have already indicated, had personally known a number of these scientists, including many who were professed socialists, and he could not bring himself to write them off in quite so imperious a manner. The solution was diagnosis and treatment, as was made clear in his review of Hayek's *Counter-revolution of Science*:

And yet one is tempted after all to caution Hayek the fighter in the name of Hayek the political thinker. In the other half of his book, where he examines the true scope of science in human affairs, he writes: 'The most dangerous stage in the growth of civilization may well be that in which man ... refuses to accept or submit to anything that he does not rationally understand' and 'This may well prove a hurdle which man will repeatedly reach only to be thrown back into barbarism.' If this be true then modern 'scientism' is merely a waywardness, due to a deeper and indeed total instability of reason at its present level of consciousness. It may appear then also that only by curing this basic disorder can we hope to prevail against the variety of delusions that have arisen and must continue to arise from it.²²

The immediate need was to find out what, if anything, had gone wrong with modern science such that it induced

reasonable people to propose infringements upon liberty and the quality of life, and not to berate the scientists for their hubris. This is in fact the task to which Polanyi devoted the remainder of his career.

It would be amiss not to note that Polanyi had also come to distrust Hayek's economic theories. As he wrote, "He addressed an age obsessed by the fear of mass unemployment while turning an indifferent eye on this problem. This surely was a mistake."²³ In a roundabout way, this too was linked to Polanyi's conception of science. He believed that openness of information was central to the success of science, and that openness was imperative in the political sphere as well. He felt that people must be made aware of what was being expected of them in their roles as economic actors, and that the opacity of Hayek's theories was itself not conducive to this public function. Rightly or wrongly, he thought that, "Keynesian theory is really quite simple—perhaps difficult to grasp at first, but once understood quite easy to handle. . . . It is a . . . veritable egg of Columbus."²⁴

Therefore, Michael Polanyi essentially exited economics after 1947 in order to construct an epistemology which was suited to both 20th century science and 20th century market economies. To do justice to the products of his quest, and especially to his rather untidy text *Personal Knowledge*, is beyond my capabilities in this venue. Part of the problem is due to the fact that the more Polanyi sought to elevate science as the paradigm human accomplishment, the more he fearlessly uncovered unsavoury aspects of the actual process of scientific research, upsetting comfortable notions like precision measurement, falsification, freedom from external authority, objectivity, open-mindedness, and the like. Rather than track down each and every observation on the history of science or the distinct positions to which they gave rise, I shall here only provide a brief survey of the main points of his epistemological system, chosen with an eye towards comparison with the work of the later Hayek.

Methodological subjectivism was central to the way Michael Polanyi approached most topics, and therefore it is no surprise he wanted his epistemology to be rooted in individual cognition. Yet unlike so many other philosophers and social theorists who then interpret this to mean that mind must be reduced to the physiological functions of the brain, as a way station to final reduction to physics, Polanyi posited a hierarchy of levels of phenomena, where mind could not be reduced to brain. He had recourse to Gestalt psychology to try and lend this notion some legitimacy; and later even tried to add his own theory of the inarticulate control of the body as the paradigm of tacit knowledae. Since he believed that everyday modes of knowing were in principle no different from their scientific counterparts, this was intended as a general theory of the inarticulate component of knowledge.

His chosen psychology led directly to his prescription of uninhibited liberty of thought, expression and economic activity, unlike utilitarian psychology, which Polanyi believed actually served to encourage totalitarian tendencies. Whereas the utilitarian treats the individual as the unmoved first mover in a game where desires are fixed and modalities of gratifications are transparent, Polanyi plumped for a situation where goals were surrounded by a penumbra of indeterminacy and most individuals could not articulate how they attained them in many instances; as he never tired of insisting, "we know more than we can say." Since this was true in science, the idea that scientific research could be directed into uniformly utilitarian paths was a travesty for Polanyi; and of course, the idea that economic activity could be planned was equally an anathema. Liberty was thus a necessary prerequisite for progress in science and in the economy.

Still, no one could be expected to acquiesce in this position unless they understood, “How can the combination of fragments of knowledge existing in different minds bring about results which, if they were brought about deliberately, would require a knowledge on the part of a directing mind which no single person could possess?”²⁵ Was it quantitative measurement, or the reduction of facts to impersonal observation language, or any other positivist conception of a “scientific method”? No, said Polanyi; none of these attempts to obliterate subjective differences between scientists could do the job. As one might expect, he attempted to found his case upon subjective commitment: “Unfettered intuitive speculation would lead to extravagant wishful conclusions; while rigorous fulfilment of any set of critical rules would completely paralyze discovery. The conflict can only be resolved through ... his scientific conscience ... the tone of personal responsibility in which the scientist declares his ultimate aims... full initiation into the premises of science can be gained only by the few who possess the gifts for becoming independent scientists, and they usually achieve it only through close personal association with the intimate views and practice of a distinguished master.”²⁶ Thus there was no mystery about the means by which the knowledge was transferred, though it might be difficult to render its content more fully explicit. Tradition was the counterweight to subjective freedom in science.

Polanyi found that throughout later life that he often would be saying things about the processes of science which would provoke cries of outrage from those who regarded it as the summit of all human rationality, so he was forced to repeat that he also thought it was the paradigm of human achievement and remarkably effective in getting at the truth. One way he chose to put this in his 1962 paper on “The Republic of Science” was to compare the self-organization of science to the self-coordination of a market.²⁷ In retrospect, we can see that he had implicitly been doing something like this since the 1940s, but when he at last made it explicit, it was misconstrued by all and sundry as conforming to some neoclassical model, which it clearly did not. This should have been apparent from his discussion of the subordination of one scientist’s standing to the opinions of others, even though they could not ever hope to be fully cognizant of all the specifics of the individual’s research. This voluntary allegiance to authority was also supposed to extend to the layperson, who should freely acknowledge the superiority of the expert in this vast web of self-organized networks. This, then, was another stick to use to beat the Bernalists, since the prognosis was that the public should pretty much just let the scientists do what they wanted, and simultaneously defer to their superiority due to tacit knowledge whenever the polity came upon a question bearing upon their expertise. For Polanyi, the choice was stark: give the scientists free rein, or else relinquish all hope of growth of knowledge.²⁸

It is instructive to compare Polanyi’s philosophy of science to Hayek’s later development of his theory of the self-organization of complex orders. Hayek decided that he, too, must found his subjectivism upon some sort of psychological principles; but he set out in 1946 to construct his own system out of mid-19th century associationist psychology, and the result was published as *The Sensory Order* in 1952. Based upon some superseded neural theories from the turn of the century, it attempted to portray the central nervous system as an apparatus of multiple classifications processing a stream of sensory input which are not themselves stored anywhere in the brain. For a subjectivist, physical stimuli need never directly map into fixed impressions, so,

What psychology has to explain is ... something which we experience whenever we learn anything about the external world ... and which yet has no place in our scientific picture of the external world and is in no way explained by sciences dealing with the external world: Qualities. Whenever we study qualitative difference between experiences we are studying mental and not physical events, and much that we believe to know about the external world is, in fact, knowledge about ourselves.²⁹

But instead of the coordination of mental stimuli serving as a metaphor for the coordination taking place in the market, the reverse was true here, as Hayek himself later admitted, indicating that the point of departure was his model of the Austrian period of production in his *Pure Theory of Capital*: “I liked to compare this flow of ‘representative’ neural impulses, largely reflecting the structure of the world in which the central nervous system lives, to a stock of capital being nourished by inputs and giving a stream of outputs.”³⁰ It is not clear that much more is going on here than an *a priori* belief in the efficacy of the market is being projected upon the neural cortex in the guise of a metaphor, only then to be reflected back as an “explanation” of the efficacy of the market. Polanyi generally did not succumb to such circular arguments.

Of course, Hayek wished to draw a conclusion similar to Polanyi’s to the effect that no one was capable of knowing enough of either the facts on the ground (since the mind did not deal in Machian “raw feels”) or the rules of tacit inference in order to adequately plan the coordination. But again, in contrast to Polanyi who structured the argument along a telos, Hayek argued in a functionalist circle, the very thing Polanyi thought was the path of least resistance down the slippery slope to serfdom: “Like scientific theories, [rules of conduct] are preserved by proving themselves useful, but, in contrast to scientific theories, by a proof which no one needs to know, because the proof manifests itself in the resilience and progressive expansion of the order of society which makes it possible.”³¹ This divergence from Polanyi actually induced Hayek to back away from methodological individualism, to depend ever more heavily upon biological metaphors which were imperfectly understood, and to backpedal on his condemnation of scientism— all subjects of extensive commentary in the secondary literature on Hayek.³²

The divergence from Polanyi could not be clearer when we come to the politics. Hayek’s move from the individual to the meta-level of social organism is at least in part due to the fact that very little can be promised to the individual economic agent in his system: she can’t know the real meaning of price signals, she can’t count on the market rewarding economic effort along any conventional criteria of justice, she can’t pretend to comprehend the telos of the system as a whole since it can’t be known, and she certainly shouldn’t place any credence in the pronouncements of experts. As Jeremy Shearmur has put it, Hayek “would seem to be a consequentialist whose subjective views and ideas about the philosophy of the social sciences imply that one cannot make out a consequentialist case for his own ideals.”³³ This, of course, is why Hayekians are so suspicious of actual existing democracies and wish to restrict sufferage along age, property, and other criteria. This flies in the face of much of Polanyi’s efforts to buttress the role of experts, render the theory of the economy available to the populace through films, and have individuals subjectively acknowledge their allegiance to a system which they can see the point of, even if they don’t fully understand where it is headed.

I have attempted in these reflection to argue that doctrines which pass as political economy are frequently thinly disguised *Methodenstreit* over images of science and what it is we are capable of knowing. Quoting Polanyi now, “the main influence of science on modern man has not been, as it is often supposed, through the advancement of technology; it has come, rather, through the imaginative effects of science on our world view,”³⁴ So perhaps I can sum up the tensions between Polanyi and Hayek as a contest of genres, a battle for the soul of Romanticism. Polanyi, as usual, saw the connection:

The romantic movement of the 19th century mitigated the dilemma [of the divergence between appearance and reality] by claiming that the content of art is predominantly subjective, personal. Thus it does not imitate. It merely expresses our subjectively personal

feelings. But the progressive sharpening of skeptical thought, leading to the wholesale questioning of traditional values, including the value of the individual person, espoused by the romantic movement, was presently to make any emphatic statements of man's deeper feelings sound trivial ...³⁵

Hayek was a romantic writer, which is why he appeals so very much to our *fin de siècle* sensibilities after languishing so long amongst a small coterie of Austrian economists and conservative politicians. His entire oeuvre can be compared to a *roman à clef* which looks very much like Mary Shelley's *Frankenstein*. There is a mad scientist, and a monster, and a "constructivist" project which is bound to fail because no one can fully encompass the unintended consequences of trespassing where angels fear to tread. It all is set in a castle somewhere in Eastern Europe, though the hero is British. The moral of the story is that there is knowledge which is intrinsically forbidden fruit; there are things which are better left unknown. The whole thing turns Gothic when we realize that there is plenty of room here for any number of sequels, all with roughly the same plot.

Michael Polanyi spent his entire life arguing that Romantic narratives like this are a symptom of a basic fallacy in how we think about science and the place of the subjective individual in the modern world. I sometimes get the feeling Polanyi wanted to counter Romanticism with something like Milton's *Paradise Lost*, jazzed up for modern tastes, if not *Areopagitica*. I cannot assess the odds on such a revision of the canon — I can't predict how it would sell in the marketplace.

Endnotes

¹One recent acknowledgement of the political significance of trends in the philosophy of science is Joseph Agassi, "Contemporary Philosophy of Science as Thinly Masked Antidemocratic Apologetics," in K. Gavroglu et al, eds. *Physics, Philosophy and the Scientific Community*, Boston: Kluwer, 1995, pp. 153-169. The political economy of recent philosophers of science is described in Wade Hands, "Blurred Boundaries" "*Studies in the History and Philosophy of Science*," 1994, (25): 751-772. An example of what is being done with the history of earlier incidents in the philosophy of science is Steve Fuller, "Being There With Thomas Kuhn" *History and Theory*," 1992,(31) :241-275.

² Rob Leonard, "Ethics and the Excluded Middle", *ISIS*, 1998 (89):1-26, and Peter Galison, "Aufbau/Bauhaus," *Critical Inquiry*, 1990, (16):709-752. The locus classicus of this work is, of course, Stephen Toulmin and Alan Janik, *Wittgenstein's Vienna*, New York: Simon & Schuster, 1973.

³ But see Paul Feyerabend, *Killing Time*, Chicago: University of Chicago Press, 1995.

⁴ This is how he is described in Harry Prosch, *Michael Polanyi: A Critical Exposition*, Albany: SUNY Press, 1986. This book contains a comprehensive bibliography, revealing two books (1936,1945) and numerous articles devoted solely to economics, including articles published in *The Manchester School* and the *Review of Economic Studies*.

⁵ Unpublished interview of James Buchanan with Friedrich von Hayek, "F.A.Hayek: Nobel Prize-winning Economist." Transcript of an oral history interview conducted in 1978 under the auspices of the Oral History Program, University Library, UCLA. Copyright 1983, Regents of the University of California. Text courtesy

of Bruce Caldwell.

⁶ Bruce Caldwell, "Hayek's Transformation, 11 *History of Political Economy*, Winter 1988, (20) -.513-54-1. See also Nicolai Juul Foss, "More on Hayek's Transformation," *History of Political Economy*, Summer 1995, (27) :345-364, which in my opinion strains too mightily to situate the transformation in 1933 by doggedly keeping the focus upon the narrowly defined business cycle theory. I would especially like to thank Bruce Caldwell for his extensive help with matters germane to the original October, 1995 talk (to a group of economists) in Kracow, Poland, that was the basis for this essay; all remaining errors and disagreements are my responsibility alone, however.

⁷ The aforementioned articles are reprinted in Hayek, *Individualism and Economic Order*, Chicago: Gateway, 1972; see also Hayek, *Counter-Revolution of Science*, Indianapolis: Liberty Press, 1979 [1952].

⁸ See, for instance, William Lanouette, *Genius in the Shadows*, New York: Scribners, 1992; where on p.76 there is some evidence of Polanyi's interest in economics before he moved to Manchester.

⁹ Hayek's memory with regard to the 1950s quoted in the previous section was a little faulty in this regard. Michael Polanyi was offered a position on the Committee on Social Thought at the University of Chicago in 1951, but was blocked from accepting it by the US State Department on the grounds that he had belonged to a "subversive organization" in his youth. On this incident, see material in box 46, folder 5, in The Papers of Michael Polanyi held by the Department of Special Collections of the University of Chicago Library. All quotations from the Papers of Michael Polanyi are used with permission of the University of Chicago Library; all subsequent references to these papers will assume the format [MPP:box:folder].

¹⁰ *The Recollections of Eugene Wigner*, as told to Andrew Szanton, New York: Plenum, 1992, p.157 On his early hesitation in leaving Berlin, see Lanouette, op. cit., pp.111,119. In this he appears to have differed from Hayek, who upon emigration to Britain seems to have become more British than the British. On this, see the numerous comments in *Hayek on Hayek*, ed. Stephen Kresge and Leif Wenar, University of Chicago Press, 1994.

¹¹ There are two book length treatments, the first and most well-known being Gary Werskey's *The Visible College*, London: Allen Lane, 1978; but it is hampered by its hagiographic approach to the figures of J.D.Bernal and Joseph Needham, its strident Marxist tone, and the lack of provision of deeper context surrounding the whole problem of the relationship of the state to science in Britain. A brief treatment from the other end of the political spectrum is Neal Ward, *Communism and the British Intellectuals*, New York: Columbia University Press, 1959, esp pp. 131-141; but this provides very little in the way of background at all. The other book-length treatment is William McGucken, *Scientists, Society and the State*, Columbus: Ohio State University Press, 1984. Chapter Nine is concerned with the Society for Freedom in Science, which Michael Polanyi helped form in 1940. This latter book is overly concerned with the mechanics of the various British organizations and their response to the "Social Relations of Science" movement in the 30s and 40s; and lacks the fire and motivation of the other texts. For some primary sources, see [MPP:15:1-2]. Some of the broader issues of Britain's perceived backwardness in science relative to the German context can be found in Tom Wilkie, *British Science and Politics since 1945*, Oxford: Blackwells, 1991.

¹² This statement requires much more historical specification than we can devote to it here. What I intend to refer

to, crudely, is the “Transformation” of the Hayek critique from the more recognizably “economic” version in *Collectivist Economic Planning* to that found beginning with the essay “Economics and Knowledge” and elaborated upon throughout the rest of his life. In this I can only concur with a statement in a letter from Bruce Caldwell, dated 27 June 1995:

“What increasingly worried Hayek in the late 1930s was the excitement for all sorts of planning among the non-economist members of the intelligentsia of Britain, the pre-war Laski being a notorious example, but more particularly the natural scientists whose enthusiasm got popular attention and who were accorded such respect in this heyday of the positivist age. Rather than explaining his critique of socialism, I think that responding to the ‘Social Relations of Science’ movement helps explain Hayek’s move away from economics and towards (1) his 1938 “Freedom and the Economic System” ... and (2) his critique of scientism...”

The sources of what later became known as Hayek’s critique of socialism have yet to be adequately explored. While the standard account, certainly encouraged by Hayek himself, is to trace its genealogy from the Austrian school of economics through Mises, there lingers the problem that many of the early progenitors, such as Menger and Wieser, were not all that hostile to statism, and did not share Hayek’s later positions with respect to knowledge. Other more likely sources of the epistemological critique of planning might be found in such collateral figures in Vienna such as Oskar Morgenstern and Hans Mayer, or indeed in Polanyi himself. What would seem to emerge from such a reevaluation is a distinctive Austrian approach to epistemology (and especially the question of knowledge and uncertainty) rather than a distinctive Austrian economics.

¹³ See, for instance, the correspondence between Hayek and Polanyi concerning the attempt to start a “Liberal Journal,” “mainly, but by no means exclusively concerned with the problem of economic and social policy and the general questions of individual and particularly intellectual freedom” (Hayek to Polanyi, 2/4/40, [MPP:4:31]. The journal apparently was never published due to a dearth of financial supporters.

¹⁴ On this see Werskey, op. cit, chap. 8; Wood, op cit., pp. 134-5; McGucken, op.cit. A phenomenon which requires further research is the role of the “Tots and Quots” club, which brought together many of the main figures of the science planning movement and provided a connection to the Keynesians through the membership of Roy Harrod. Harrod was one of the very few to review Polanyi’s 1945 book *Full Employment and FreeTrade*.

¹⁵ The other, entitled “Employment and Money”, was even more explicitly Keynesian. The second film was funded by the Rockefeller Foundation, and was screened throughout Britain and the US. Jacob Marschak even requested to use it in his course on “Monetary Policy” at the New School! [MP:4:51.] Nevertheless, neither film was very popular with its target audience, the lay public. In the opinion of one educator, it could not be viewed with benefit in the absence of extensive lectures and preparation. Cf. H. Shearman to Polanyi, 4/5/45 [MPP:4:12]

¹⁶ F.A. von Hayek, Review of *The Contempt of Freedom* by M. Polanyi and *A Critique of Russian Statistics* by Colin Clark. *Economica*, May 1941, NS 8, p.212. The original articles which eventually became the bulk of *The Counter-revolution of Science* were published in the same journal in Feb. 1941 NS 8, pp.9-36; May 1941, pp.119-150; Aug. 1941, pp.281-320; Aug. 1942, NS 9, pp. 267- 291; Feb. 1943, NS 10, pp.34-63; Feb. 1944, NS 11, pp.27-39.

¹⁷ Of course, there were some good reasons for Hayek maintaining a lower profile: “he was forced, for example, to always refer to Germany as the paradigm case of totalitarianism and to mute or veil his opinions about the Soviet ally; also, he was from the German-speaking world, so was going to be viewed by at least some as himself a bit suspect.” (Bruce Caldwell, letter to Philip Mirowski, 6/27/1995).

¹⁸ One letter will have to suffice here to buttress these claims. This is from a letter from J.R.Hicks to Polanyi dated Nov. 18, 1945: “You have certainly done a good work in helping to provide the framework for a new Liberalism. I don’t know that I shall end up as 100% Liberal as you are, though I am more on that side than the other. But I agree with Harrod in having sympathy for your vision than with the ancien regime of Hayek and Fisher.”[MPP:4:13].

¹⁹ Michael Polanyi, “The Value of the Inexact,” *Philosophy of Science*, 1936, (3):233-234.

²⁰ This is the interpretation of Harry Prosch, op. cit., chap. 20, and Eugene Wigner, op cit, p.315. The extent to which this was a function of his perception of the political scene in the late 60s is not at all clear from the sources, nor from his last work, jointly authored with the philosopher Harry Prosch, *Meaning*, University of Chicago Press, 1975.

²¹ Michael Polanyi, Review of F.A.Hayek’s *Individualism and Economic Order*, *Economica*, Aug. 1949, p.267.

²² M. Polanyi, Review of Counter-revolution of Science by F.A. Hayek. *Manchester Guardian*, Jan. 2, 1953, D.3.

²³ Polanyi review of Hayek, *Individualism*, op cit., p.267.

²⁴ Michael Polanyi, *Full Employment and Free Trade*, 2nd ed.,Cambridge: Cambridge University Press, 1948; pp. xv-xvi.

²⁵ The quote is not Polanyi’s, but rather the “central question of all the social sciences” from Hayek’s 1937 “Economics and Knowledge”, op cit., p.54. I switch personas in midstream to make the point that it was not a question that had occurred to people in the social sciences before it had occurred to philosophers of science; and this has some bearing on the “Transformation” of the Austrian program.

²⁶ Michael Polanyi, *Science, Faith and Society*, London: Oxford University Press, 1946, p.27, 29. It has been common to read such passages as references to theological guarantees, but the Jewish Polanyi never committed to any particular religious faith.

²⁷ See “The Republic of Science:Its Political and Economic Theory” reprinted in Michael Polanyi, *Knowing and Being*, ed. Marjorie Grene, Chicago: University of Chicago Press, 1969. On this, see Philip Mirowski “On Playing the Economics Trump Card in the Philosophy of Science” PSA 97, *Philosophy of Science* supplement to V.64:127-138.

²⁸ I do not want to evaluate the pros and cons of Polanyi’s solution to the problem of order in science in this paper; nor do I wish to unambiguously endorse his insistent theme that scientists can be trusted to do whatever they want in the name of research. Much more work would be needed to situate Polanyi himself in an era in

which the tabletop science of his youth was largely giving way to “big science”, and the extent to which his exit from physical chemistry had something to do with his own disillusionment with this trend. And it is noteworthy that while he often would excoriate the Soviets for the Lysenko affair, he never, to my knowledge, commented upon the various medical “experiments” during the Nazi era, or the development of atomic weapons in the US, even though some of his Hungarian friends such as Szilard and von Neumann played major roles at Los Alamos. These latter phenomena were relatively free choices by scientists involved that caused the participants anguish upon scales previously undreamt of in the West.

²⁹ F.A. Hayek, *The Sensory Order*, Chicago: University of Chicago, 1952, pp.6-7. It is easy to see in this passage another version of the aversion to objectivist and behaviorist aspects of scientism which was a major theme of Hayek’s in the 1940s.

³⁰ F.A. Hayek, “The Sensory Order after 25 Years,” in W. Weimer & D. Palermo, eds., *Cognition and the Symbolic Process*, vol 2, Hillsdale: Erlbaum, 1982, p.291. Those familiar with modern cognitive science, from Dennett’s parables about brains in vats to John Holland’s classifier systems, should experience a frisson of recognition here. On a separate note, Walter Weimer recognizes the parallels between Polanyi and Hayek: cf, pp.245-247 in the same volume.

³¹ F.A. Hayek, “The Errors of Constructivism” in *New Studies in Philosophy, Politics, Economics and the History of Ideas*. London: Routledge, 1978, p.10. That Polanyi would define this sort of argument as “scientism” can be observed from, eg. , *Personal Knowledge*, Chicago: University of Chicago Press, 1958, pp.141 et seq.

³² See, for instance, Victor Vanberg, “Spontaneous Market Order and Social Rules,” *Economics and Philosophy*, 1986, (2) :75-100; Geoffrey Hodgson, *Economics and Evolution*, Ann. Arbor: University of Michigan Press, 1993, chaps. 11 & 12.

³³ Jeremy Shearmur, “Hayek and the Case for Markets” in Jack Birner and Rudy van Zijp, eds, *Hayek, Coordination and Evolution*, London: Routledge, 1994, p.196.

³⁴ Polanyi & Prosch, *Meaning*, op cit., p.104.

³⁵ Ibid, p.110.

Electronic Discussion Group

The Polanyi Society supports an electronic discussion group exploring implications of the thought of Michael Polanyi. For those with access to the INTERNET, send a message to “owner-polanyi@sbu.edu” to join the list or to request further information. Communications about the electronic discussion group may also be directed to John V. Apczynski, Department of Theology, St. Bonaventure University, St. Bonaventure, NY

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. Use MLA or APA style. 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.

Manuscripts should include the author's name on a separate page since submissions normally will be sent out for blind review. In addition to the typescript of a manuscript to be reviewed, authors are expected to provide an electronic copy (on either a disk or via e-mail) of accepted articles; it is helpful if original submissions are accompanied by an electronic copy. For disks, ASCII text as well as most popular IBM and MAC word processors are acceptable. Be sure that electronic materials include all relevant information which may help converting files. Persons with questions or problems associated with producing an electronic copy of manuscripts should phone or write Phil Mullins (816-271-4386). Insofar as possible, *TAD* is willing to work with authors who have special problems producing electronic materials.

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WWW Polanyi Resources

The Polanyi Society has a World Wide Web site at <http://www.mwsc.edu/~polanyi/>. In addition to information about Polanyi Society membership and meetings, the site contains the following : (1) the history of Polanyi Society publications, including a listing of issues by date and volume with a table of contents for recent issues of *Tradition and Discovery*; (2) a comprehensive listing of *Tradition and Discovery* authors, reviews and reviewers; (3) information on locating early publications; (4) information on *Appraisal* and *Polanyiana*, two sister journals with special interest in Polanyi's thought; (5) the "Guide to the Papers of Michael Polanyi" which provides an orientation to archival material housed in the Department of Special Collections of the University of Chicago Library; (6) photographs of Michael Polanyi.

