Tradition & Discovery

The Polanyi Society Periodical

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Preface

This issue begins with a very thoughtful memorial note on Edward Shils, a figure who was both Michael Polanyi's friend and colleague. Stephen Turner does a nice job of showing how Polanyi influenced Shils' social theory as well as how Shils influenced Polanyi's ideas. Following is one of Edward Shils' final essays which was substantially completed at the time of his death in January 1995. "On the Tradition of the Intellectuals: Authority and Antinomianism According to Michael Polanyi" grew out of the wonderfully witty and illuminating material Shils prepared for his address at the Polanyi centennial at Kent State in April 1991. It is only through the efforts of Richard H. Schmitt and Christine C. Schnusenberg, two longtime Shils associates, that *TAD* has been able to see to conclusion Professor Shils' desire to publish his reflections. The care and commitment shown in preparing the final essay by these two reflects not only their own integrity but that of Edward Shils.

There is also in this issue both a short review as well as a longer review essay on Sheldon Richmond's recent book *Aesthetic Criteria: Gombrich and Philosophies of Science of Popper and Polanyi.* Barbara Baumgarten's brief reflections are complemented by Andy Sanders' point by point examination to which Richmond kindly agreed briefly to respond. Additionally, there are reviews of a 1993 collection of essays by William Poteat and Joan Crewdson's new book on Polanyi and Christian doctrine.

You will find on page 4, the annual call for papers for the Polanyi Society meeting to be held next Fall. Proposals need to be received by David Rutledge by the middle of March.

Please be aware that this issue is the second in the current subscription cycle. Dues are collected each Fall at the beginning of the academic year. If you have neglected to pay your dues, please do so immediately. Remember that several business procedures are new this subscription cycle: We are diligently attempting to shift all subscriptions, including those outside the United States, to the cycle identified above. All dues are now mailed to me (Phil Mullins) rather than Richard Gelwick, as they have been in the past. Also you now may charge membership dues with a credit card if you provide the card number, expiration date and name.

Phil Mullins

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*.

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NEWS AND NOTES

We are working on publishing, in a form as inexpensive as possible, the proceeding of the Kent State Centennial Conference. A January deadline has been set for submitting copies of presented papers, and we have attempted to contact all persons who did presentations. We will not have all of the papers for several reasons. Some persons, such as John Polanyi, wanted presentations to remain informal and unpublished. Others did not submit copies of their papers. Some we were never able to reach to solicit papers. Nevertheless, we do have most of the major addresses and papers. Presently, it looks like it may take two or three volumes to hold them. These materials look as if they will be useful for future scholarship on Polanyi. When these proceedings are available, Polanyi Society members will be notified.

If you have not already, you are invited to join the electronic discussion list on Polanyi which John Apczynski moderates. As a recent convert to e-mail communication, I want to add my encouragement to earlier invitations from Apczynski and Phil Mullins. It is invigorating and stimulating to be able to sit down on any day and have conversations about Polanyian matters with scholars across the world. In the past month, I noticed correspondence from scholars in Austria, New Zealand and Australia as well as the United Kingdom, the Netherlands and the United States. See the section of this issue titled "Information on Electronic Discussion Group") for directions on how to subscribe.

From the database Expanded Academic Index (1/16/96), the following article is relevant to Polanyi:

"Postmodern Ethics: Richard Rorty and Michael Polanyi". John Rothfork. *Southern Humanities Review* (Winter 1995), vol. 29, no. 1, pp. 15-34. *Abstract*: The issue with postmodernism is whether postmodernism can maintain a positive ethics with the scathing action of its methods which destroys epistemological assertions about truth and metaphysical assertions about substantive concepts such as God or Reason. Richard Rorty was always clear about the inevitable outcome of social and moral thought. Michael Polanyi and his Aristotelian orientation have been incorporated to support the moral and political responsibility of Rorty's stand.

R. T. Allen is launching a new journal *Appraisal* early in 1996 and its first issue will include three articles as well as reviews focused on Polanyi and Polanyi influenced scholarship. Although not devoted exclusively to Polanyi, *Appraisal* plans to reprint some material from *TAD* and *Polanyiana*. Allen writes that a new book that he has edited, *Society, Economics, Philosophy: Essays by Michael Polanyi*, is scheduled for publication by Transaction Publishers (Rutgers) in Spring 1996. The volume will include 25 of Polanyi's articles not previously incorporated into his books, including two articles that are English translations by Endre Nagy of essays originally published in Hungarian. Allen has recently written a short entry on Polanyi for the new Routledge *Encyclopaedia of Philosophy* and reports that **Norman Wetherick** has written a similar entry for Routledge's *Biographical Dictionary of Psychology*.

Richard Gelwick

The Polanyi Society
Call for Papers

The Polanyi Society will convene again during the annual meeting of the American Academy of Religion/ Society of Biblical Literature in New Orleans, LA, November 23-26, 1996. Scholars interested in Michael Polanyi's work and its implications are encouraged to submit one-page paper proposals for the meeting to David Rutledge by March 15, 1996. Though all submissions are welcome, we are particularly interested in receiving proposals on the following topics: "Teaching Polanyi and Polanyian Teaching," and "Tacit Knowing and Psychology." As in past sessions, we anticipate the AAR/SBL will allow the Polanyi Society one program session on Saturday, November 23, in which two papers will be discussed. Proposals for shorter papers and/or an additional program session will, however, be considered.

Proposals should be sent to:

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We have enjoyed excellent sessions in recent years, and want to thank Polanyi Society members for their participation. We look forward to receiving your proposals for New Orleans!

Edward Shils
1910-1995

ABSTRACT Key Words: Edward Shils, Michael Polanyi, tradition
Michael Polanyi and Edward Shils shared a great many views, and in their long mutual relationship influenced one another. This memorial note examines the relationship and some of the respects in which Shils presented a Polanyian social theory organized around the notion of tradition.

Edward Shils, Professor of Sociology and Social Thought at the University of Chicago and longtime fellow of Peterhouse, Cambridge died in January of 1995 at the age of 84. Shils was one of the grand intellectual figures of his generation, and he had close personal relations with several of the other grand intellectual figures of his generation, including Michael Polanyi. In what follows, I will concentrate on his relation to Polanyi, which in many ways was formative for his sociological thought. Indeed there is a strong sense in which Shils' mature thought represents the extension of the central Polanyian themes of the role of faith at the basis of intellectual traditions, the importance of the direct personal initiation of an individual into a tradition, and the idea that "tradition" was a term wrongly appropriated by the Enlightenment to describe superstition and irrationality. Shils also devoted much of his time in his later years to the editing of the journal *Minerva*, a journal devoted to matters of policy with respect to learning, science, and higher education. *Minerva* focused on precisely those themes that had been formative for Polanyi himself in the crucible of the struggle against the social relations of science movement in the thirties and forties. Shils' involvement with these issues and with the Polanyian theme of tradition dates from this period as well.

The thought of Polanyi on science itself was deeply engraved in the whole of the massive editorial effort which Shils gave to *Minerva*. *Minerva*, founded in 1964, soon became a journal read and admired by a small but influential number of prominent academic and scientific administrators; several such figures were frequently represented on its pages, as were scholars in many areas related to the problem of the history and significance of the present organization of intellectual life, such as historians of philanthropic efforts in support of science and scholarship. Typical themes from Polanyi's writings in opposition to Bernal and the social relations of science movement, such as the distinction between science and technology, were frequent topics in *Minerva*. Polanyi wrote an essay in the first issue of *Minerva*, which in many respects set out the principal themes of the journal over the next twenty years. The essay was one of Polanyi's best: "The Republic of Science." It synthesized and updated his writings on science and its governance and, in several ways, strikingly anticipated later work in "science of technology studies" particularly with respect to the networks which check and sustain scientists' beliefs in their own results.

Shils shared these ideas and concerns. But I think that there is a much deeper and more encompassing relationship or engagement with Polanyian themes to be found in Shils, and in what follows I will briefly examine its character, and its historical origins. The atmosphere among intellectuals and especially scientists in the thirties and forties has been written about endlessly. The era was dominated by the highly visible fact of the long economic depression of the thirties which gave new life to the hope of a rational regulation of economic life and encouraged the idea that communism represented the best chance for bringing this about. "Progressive" forces were identified with the Soviet Union and its policies, and the status and merits of the Soviet model, which focused on heavy industry, were widely discussed. J. D. Bernal, the product of a famous British scientific family and member of the intellectual

establishment, led a scientists' movement to create a socially responsible science, organized collectively to serve collective purposes. Only a transformation of science, Bernal argued, could overcome the irrationalities of the present form of organization of society and only a transformed society could fully utilize the benefits that properly organized and productively oriented science could produce. The term of art which was used in progressive circles to describe the rational organization of society was "planning." Bernal advocated the "planning" of science and the planning of society. Polanyi, of course, opposed both, and his turn toward philosophy arose from this opposition.

The notion of planning has not yet found its historian. It was a notion which found favor (both as a way of describing the political tendencies of the day as well as a way of proceding politically among a wide variety of political viewpoints). Many American observers, for example, saw the rise of Nazism, Fascism, Bolshevism, and European Labor Socialism as embodiments of the political idea of planning, and indeed the Nazis, Fascists, and Bolshevists all engaged ostentatiously in planning and for large projects of various kinds. For some socialists, such as Hendrik de Man, planning became a term which defined the movements themselves, and de Man accordingly changed his allegiances from socialism to fascism, which he reasoned would be more able to initiate planning.

This movement produced its own countermovement. The war against Hitler produced a great deal of soul searching about what precisely differentiated the enemies of Hitler from Hitler, a problem given a certain specificity by the Hitler-Stalin pact of 1939, which suggested that the difference between Bolshevism and Nazism was primarily tactical and their relation contingent. A great outpouring of writing took place on the common heritage of democracy, drawn in part on a long quiescent body of 19th century ideas about the ancient Germanic roots of Anglo-Saxon freedom and constitutionalism. T.S. Eliot sought to reconsider the Christian roots of culture at the same period, and his writings influenced Shils as well.

Sociologists contributed something to this literature, which itself reflected sociological themes of an earlier generation. But during the late forties there was a change in sociology, much of which was the result of a high level of optimism that the field was on the verge of becoming a true science. Shils had a hand in this effort, and indeed for many academic sociologists he is best known for the brief period of his collaboration with the Harvard sociologist Talcott Parsons. But Shils was soon disenchanted with the quest for a scientific sociology. And because he was more attuned to the problem of tradition than his contemporaries, it fell to Shils to say something of importance about it. What he said drew on many sources.

Shils became involved with the British war effort and with the interviewing of German soldiers who had been captured, and through this route arrived at conclusions that pointed toward some important insights into the way in which traditions and central values figure in social life. He came to understand that what sociologists call the primary group, the people with whom one has face to face relationships, was the basis of the solidarity of the fighting group. The mutual loyalty of the members of the unit, rather than devotion to Nazi idealogy, was the basis of the effectiveness of the unit. Nevertheless, Nazi ideology had a role. Some member of the primary group was, typically, oriented to the ideology and provided the link of solidarity and commitments which connected devotion to the regime to the devotion of members of the fighting group ([1948] 1975, pp.351-352). Shils came to see that this model applied to tradition generally. Some people are, as he put it, oriented to the center, that is to say, oriented to the core embodiments of a tradition, and these people constitute, so to speak, the spine of society to which others are connected through their face to face relations as ribs are connected, that is to say the spine supports the body with the help of the ribs just as tradition holds together society but largely through the mediation of face to face relationships of loyalty and the like and personal bonds

rather than directly through the influence of the tradition on each individual.

Shils greatly elaborated this insight about the center of traditions, and similar ideas were developed by such contemporaries as Robert Redfield, anthropologist at the University of Chicago, in this own studies of Mexican villages. But although Shils thought systematically about these topics, he did not write systematically about them, or at least did not write a systematic treatise in which the relation between all of his central ideas were precisely defined. Shils became fascinated with the many ways in which the center of the tradition and indeed various special traditions and local traditions of society were presented and experienced symbolically. In one of his most famous essays, "The Meaning of the Coronation," Shils and Michael Young examined the ceremony of the coronation of Queen Elizabeth and the complex ways in which the English responded to it. His point was that the symbolic representation of the central political and religious institutions of the society as represented in the coronation ritual itself evokes a response even from those who oppose these institutions which choose the remarkable extent to which they recognize and respect this centrality ([1956] 1975, pp. 135-152). Shils saw also that the antinomianism and antiestablishment attitudes of intellectuals derived from an even more complex relation to central institutions: they treated the ideals of the center, to which they were especially attuned, as standards against which they could hold the actual conduct of the central institutions and their representatives, and found them wanting (1972, especially pp. 3-22).

For an audience familiar with Polanyi, I need hardly point out the connections with Polanyi's own thought. The image Polanyi had of science as a tradition parallel to the Church, defined by a distinctive kind of common faith, is like Shils image of society as a whole, society oriented to a common spiritual center. And Shils, like Polanyi, had a great respect not only for religion but for the traditions of the special professions, such as the law. He certainly took from Polanyi the emphasis on the personal transmission of these traditions, and the idea that it was especially difficult to establish a living tradition. The problems of creating a scientific tradition in the countries of the third world, for example, signalled to Shils something basic about the character of traditions in general, as well as the dependence of science on its "traditional" and personal character.

If we were to go through Shils' various concerns one by one, we would find Polanyian connections, strong or weak, in almost every one of them. It is clear that Shils' relationship to Polanyi was intensely instructive to Shils and amounted in a way to a kind of dialectical partnership that stimulated Shils' thought and Polanyi's as well. Polanyi's relationship to social scientists and psychologists is as yet unchronicled, but the relationships were often quite formative for those who came into contact with him. Donald Campbell, for example, has told me of his own discussions with Polanyi in the fifties and the value they had in his own development -- but one would be hard pressed to simply catalog "influences" of Polanyi on Campbell. I think it is significant that Polanyi stimulated the people he came into personal contact with in ways that cannot be reduced to a model of "influence." The ways in which Polanyi himself drew from these relationships is difficult to chart, but it clear that the range of Polanyi's later thought, and its quality, depended on these personal relationships and the discussions that he participated in. In *Personal Knowledge*, I think, there is evidence of Polanyi's absorption of these concerns, especially in the chapters on skills (1958:53-65) and even more so in the chapter on conviviality (1958:203-245).

Shils, I think, reflected on Polanyi and Polanyi's experiences long after Polanyi's death, and they had an important role in his own thought. Polanyi represented for him a kind of model of the great scientists. I knew Shils in the last 14 years of his life, and I am struck in retrospect by the number of times he quoted Polanyi or relayed information about Polanyi in the course of making points about matters that had nothing to do directly with Polanyi. At one point,

for example, we were discussing the history of sociology, a subject we both shared an intense but somewhat ironic interest in, as well as the history of the University of Chicago. He often quoted Polanyi as saying that science has an apostolic succession, and he used this to make a point about the university and its great sociologists as well. Robert E. Park was his own teacher, or rather the great figure in the Sociology Department whose aura and style had suffused it and created the Chicago School of Sociology, which was beginning to break up when Shils came to Chicago and Park retired. Park, Shils thought, was one of those rare individuals that make a university great by their presence. Shils believed that even at a great institution like the University of Chicago there were only four or five such individuals at any time who were truly great and who sustained the greatness of the university.

Shils believed that Polanyi had this kind of greatness as well, so it is perhaps no surprise that Shils was so receptive to the kind of dialogue or dialectical relationship with Polanyi that I have hinted at. What Shils in the end made of this relationship will perhaps be seen in his large project to be posthumously published, *The Movements of Knowledge*. I would expect to find Polanyian themes throughout, but his own themes developed and were transformed through contact with a new range of topics and new ideas from various sources, not least from Shils himself.

Shils observed, at the Kent State Conference, that Polanyi had been a brilliant eulogist, and commented that it was hardly worth dying nowadays, without Polanyi to give one's eulogy. In Shils' last years, he wrote a number of biographical essays on scholars he had known and outlived. These essays were in a sense Shils' own work as a eulogist. The titles of the essays are often somewhat deceptive. Stories about one scholar often appear in articles about another scholar. Polanyi figures in several of these, in some cases in large ways, as in Shils' paper on Robert Hutchins, in which he relates his efforts to bring Polanyi to the University of Chicago and the Committee on Social Thought (1991, 191-192). In other essays, Polanyi makes a cameo appearance, as in Shils' posthumously published recollection of Karl Mannheim:

> In the early evening of January 7, 1947, I was in a taxi accompanying Michael Polanyi to King's Cross Station for his return journey to Manchester, where he was still a professor of physical chemistry. Polanyi was a little older than Mannheim, but they were both members of the Galileo Society. They had known each other in Budapest. Although he was working in the Kaiser Wilhelm Institute for the study of man-made fibers, Polanyi was an intellectual highbrow, especially interested in social affairs. He was a liberal and a severe critic of social planning. He had very little sympathy with Mannheim--to the point of indifference--while Mannheim was very sensitive to "Michi's" lack of sympathy. When I said to Polanyi, as we were nearing the station, that Karl Mannheim had died, Polanyi made no reply at all. He was probably thinking one of his philosophical conundrums and there was no clear category in his mind for Mannheim. (1995, 234)

I think this also conveys something about Polanyi's ability to make hard intellectual judgments and live with them. Shils had this ability as well. Like Polanyi, Shils was sensitive to the element of the sacred in human activities, and respected it when it was genuinely present. But Shils was entirely unsentimental about individuals, and did not hide his judgements of them. Shils not infrequently expressed these judgements in a kind of sardonic humor that was never quite cynical, but was sometimes quite, to use a Victorian word, shocking, both in its directness and tone. Yet Shils was also an astonishingly helpful person, who went out of his way for others whose motives and efforts he respected, and especially for students. He would say "pull your socks up, Mr. So and So," but he would also spend precious time in helping Mr. So and So to produce something constructive and valuable, and he had a sharp eye for the valuable. Much of his editorial work at *Minerva* consisted in the transformation of poorly developed texts into useful

contributions -- a process in which the author sometimes became a bemused bystander.

Shils was a great scholar. His scholarship was deeply influenced by the times in which he lived. He was an active participant for the causes that Polanyi was also an active participant in, and shared Polanyi's sympathies, for example for religion. Time will tell what survives these concerns and what reflected narrow circumstances that have changed. The generation to which Shils belonged was Polanyi's generation. Shils had an affinity for older men and women and made discerning judgements about them. But he also respected their achievements. His preservation of his reflections on them was a great service and an act of Polanyian respect for intellectual greatness. When Shils said at Kent State that there was no one any more who could give a good eulogy, as Polanyi had done, it was a comment that reflected the passing of this generation. We are still far too close in time to this generation to sit in judgement of them, as I have suggested. But we are not so close as to be unable to select from the vast outpouring of academic writings of this century the bright lights whose memory we need to preserve and whose thought we need to continue to reflect on. Shils, like Polanyi, belongs in this category.

Stephen Turner

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On the Tradition of Intellectuals: Authority and Antinomianism According To Michael Polanyi

Edward Shils

Editor's Note: This essay was originally drafted by Edward Shils in preparation forhis address at the celebration of the Polanyi centennial at Kent State,which he delivered in April 1991-- in a manner more extemporaneous and anecdotal than the written text, as was his custom on such occasions. At the time of his death, he had reviewed this draft in preparation for publication here; we have made only minor corrections to the text that follows and have included several passages at the end which had been removed from the final version.It is chiefly through the diligent efforts of Richard H. Schmitt and Christine C. Schnusenberg that *TAD* has been able to see to conclusion Professor Shils' desire to publish his reflections.

ABSTRACT Keywords: tradition, authority, intellectuals, tacit knowledge,scientific training, scientific community, history of science,sociology of science, politics and science, Michael Polanyi, Karl Popper.

Michael Polanyi made an original contribution in his reflections on tradition within the scientific community. Starting with his Riddell Lectures (Science, Faith and Society), he considered the role of authority and the transmission of tacit knowledge within the scientific community, an analysis that can be extended to other, often contrasting, realms of intellectual life.

I

I think that it is worthwhile on this occasion for the reassessment of Michael Polanyi's achievements to recall his reflections on the traditions of intellectuals. Tradition and traditions have had a hard time of it ever since the seventeenth century, but the traditions of intellectuals have, for better or for worse, thrived. Neither Bacon nor Descartes had much patience with knowledge which is acquired through tradition. The conviction that progress was a moral imperative made what was received from the past into a burden; the sooner it was discarded the better. More narrowly, the data obtained by direct observation of present events and the rational analysis of these observations meant that nothing was to be taken for granted except the primacy of direct observation (or observation through reliably uniform instruments), and rational analysis.

In the social sciences, economic theory had no place for traditions in its account of human action. Sociology, likewise, seeing societies moving irresistibly from *Gemeinschaft* to *Gesellschaft*, likewise provided little place for it. Political science, despite its motley composition, did not think that traditional belief or attachment to old practices had any role to play in the conflicts and compromises of "interest groups" in their struggle for power or in the calculus of "rational choice." Anthropology might have appeared to be the one branch of social science which would be compelled to deal with traditions and traditional practices, i.e., long recurrent and persisting beliefs and practices and a positive attachment to things inherited from the past and an appreciation of persons who lived and events which happened

in the past. But no, it has not been that way even in a branch of study devoting itself to what have been called traditional societies.

These academic attitudes towards tradition are only special manifestations of a more widely espoused refusal to acknowledge the existence, endurance and value of tradition.

Very recently, we have seen this war against tradition conducted through the rationalistic contention that "tradition" has been "invented." It is part of the fashionable derogation, which attempts to discredit cultural and intellectual achievements of long standing and high merit. This, among historians who because they deal with persons and events more concretely and without the interposition of theories and doctrines could be expected to show a little more understanding of human life than social scientists who deal in abstractions or who deal with events under very general rubrics.

In the humanistic disciplines, where the objects of study have been works long carried and received as valued traditions, a passionate denial of the value of the canon of highly esteemed works has become widespread. The canon is viewed, as it is in the notion of the "invention of tradition" as a contrivance with the intention of domination and suppression. "Deconstruction", the "new historicism", the "sociological interpretation" are all intended to erode the preeminent standing of highly appreciated works which have long been accepted as the best of the Western intellectual tradition. In the humanistic disciplines, we see an active attack against tradition, not a mere indifference towards it.

Nor does "neo-conservatism," the intellectual movement which sets its face against the political radicalism of recent years in the "humanities," accord much more value to tradition or attribute more important functions to it. It too tends to be very rationalistic.

Michael Polanyi was one of the first writers of the twentieth century to call attention to the traditional element in the natural sciences. Polanyi put forward his views on tradition in scientific activity in the Riddell lectures which he delivered at the University of Durham in 1946.[1] A few years later than Polanyi, Karl Popper published his essay "Towards a Rational Theory of Tradition".[2]

II

In Polanyi's view even in the natural sciences with their powers of experimentation and control, precise and reliable observation and the possibilities of mathematical formulation and deduction, there remains a zone or margin of discretion, where intellectual judgments are made but without the guidance of precise rules and explicitly stipulated procedures. He said that scientific propositions could not be derived exclusively from observation by adherence to explicit rules. In 1946 he wrote: "There is a residue of personal judgment required in deciding -- as the scientist eventually must -- what weight to attach to any particular set of evidence in regard to the validity of a particular proposition." (*SFS*, 31).

(Later in his life, Polanyi referred to this zone of discretion as the realm of tacit knowledge. Tacit knowledge he defined as knowledge which we possess but cannot articulate.)

Of course, Polanyi did not deny the need for meticulous accuracy. "Care and honest self-criticism" -- being

"conscientious" -- are among the most stringent requirements of scientific work. They are among "the first things that a student is taught on being apprenticed to science" (*SFS*, 39). But they are only the beginning. They are conditions for entry into the outer periphery of the scientific community. This kind of "routine conscientiousness" (*SFS*, 40) is indispensable to scientific work but if it is not complemented by "real scientific conscience," it will come to nothing (*SFS*, 40). It is the latter kind of conscience which is pertinent to judging "how far other people's data can be relied upon and avoiding at the same time the dangers of either too little or too much caution" (*SFS*, 40).

Polanyi in one place speaks of the mind of the scientist as the "theater of three modes of action." "Unfettered intuitive speculation could lead to extravagant wishful conclusions; while rigorous fulfilment of any set of critical rules would completely paralyse discovery" (*SFS*, 41). It is the "scientific conscience" which transcends both "creative impulses" and "critical caution." This is the "moral element in the foundation of science" (*SFS*, 41).

In his later writings about "tacit" knowledge, he went further and asserted that we cannot assert all that we know explicitly but that we know it nonetheless. This added to the penumbra of discretion lying between observation and rules on the one side and the theory on the other, another penumbra which lies outside theory but which is necessary to it.

These two penumbra of knowledge cannot be rigorously controlled. Yet, they are not arbitrary. Without intellectual discipline which is provided by the authority of outstanding scientists, by consensus within the scientific community and by the scientific conscience of the individual scientist, the results of scientific activity would be chaotic. They acquire the rudiments of that discipline through their submission as young students to the authority of those persons who embody the intellectual traditions of their subject or who express those traditions in their actions. Later, they become less submissive to the authority of individuals but they remain positively responsive to the scientific beliefs of other scientists conceived as a collectivity or community.

"A master's daily labours will reveal these to the intelligent student and impart to him also some of the master's personal intuitions by which his work is guided. The way he chooses problems, selects a technique, reacts to new clues and to unforeseen difficulties, discusses other scientists' work and keeps speculating all the time about a hundred possibilities which are never to materialize, may transmit a reflection at least of his essential visions. . . ."[3] (*SFS*, 43-44). "Thus his mind will become assimilated to the premisses of science. The scientific intuition of reality henceforth shapes his perception" (*SFS*, 44). The consensus of the scientific community is the consensus of those who have learned to respect the authority first of their teachers and then of their traditions.

Intellectual traditions, general and particular in the reference, are both explicit and implicit. The explicit ones are the propositions which make up the bodies of scientific and scholarly knowledge at any point in time; they are the propositions ranging from specifically descriptive to general and explanatory which have, at best, been supported and have not been refuted by methodical research and analysis. There is widespread -- although not always complete --agreement about them among those who work in the field and they are usually regarded as given in the work which goes on in the field. None of these substantive propositions is immune or exempt from being criticized or rejected but the criticism or rejection accepts the correctness -- at least for the time being -- of most of the other propositions specifically descriptive and generally explanatory which make up the existing body of knowledge in that field and which are the objects of the consensus of that particular sector or sub-community of the larger scientific community.

III

For any particular individual member of this sub-community, these propositions are the point of departure for subsequent work in the field. They are added to, revised, corrected and forgotten but none of these is done to all the propositions all the time. They are the tradition of the subject.

Popper in his essay on a rational theory of tradition also has much to say about scientific tradition. He makes an eloquent and forceful argument which is all the more striking because he himself for the early part of his intellectual life lived in an intellectual community which had little sympathy with any idea of the importance of tradition. Popper asserts about tradition that it is a precondition of the growth of scientific knowledge, which grows not by direct observation but by the juxtaposition of observations and received theories, i.e., tradition. Without such traditions, a science would have to begin afresh in every generation or indeed with every individual scientist.

Polanyi would surely have agreed with Popper on this particular point although as far as I know he never expressed himself about this. (Popper and Polanyi avoided any reference to each other. As far as I recall, whenever I mentioned the one to the other, I was answered with silence.)

Polanyi went further than Popper in the analysis of the tradition of science. With all due respect to Popper, Polanyi's analysis was deeper. Beyond his statements about the existing state of theoretical knowledge as the tradition to which every scientist has to respond, Popper touched only briefly on the scientific sensitivity to problems and possibilities in a short passage on the difficulties to establishing a scientific tradition in a society in which there has been none previously. Here Popper showed that he knew that there is more to the scientific tradition than the givenness of theories at any particular point in time. He did not however go any further. This Polanyi did.

He wrote: "...the process of learning must rely in the main on the acceptance of authority. Where necessary, this authority must be reinforced by discipline" (*SFS*, 46). But it is not authority as such which legitimates itself; it is rather the obvious devotion of the person in a position of authority to scientific truth. The claim to the right to exert authority and the readiness to acknowledge it rests on the fact that "masters and pupils...possess in general sufficiently sincere attachment to science and a sufficiently authentic vision of it to find therein a common ground for agreement" (*SFS*, 46).

There are occasions on each side where this condition is not met, when students are recalcitrant and where teachers are dogmatic and prejudiced. But these instances are relatively infrequent and they do not occur at the centers of scientific creation.

It is obvious that tradition is more than the simple factual existence of propositions which a newcomer must acquire as his own point of departure. Tradition has a normative content. There is a norm implicit in the exemplary action of persons in positions of authority; it must be heeded and observed on grounds other than prudence or inevitability. The persons to whom authoritative statements are addressed must accept them because they are rendered -- at least for the time being -- valid to him through the fact of their enunciation by the authoritative person and not solely on the basis of their rational or empirical persuasiveness.

Polanyi regarded this exercise of authority as a transient condition in the relationship between any particular

teacher and his particular pupils. In the course of time, the authority of the teacher yields to the evidenced validity of the proposition which he asserts. "The authority to which the student of science submits tends to eliminate its own functions by establishing direct contact between the student and the reality of nature. As he approaches maturity the student will rely for his beliefs less and less on authority and more and more on his own judgment. His own intuition and conscience will take over responsibility in the measure in which authority is eclipsed" (*SFS*, 45). His conscience is reinforced by the presence, proximate or remote, of other scientists whose own scientific attitudes, expressed in publications and conversation, heighten the scientific collective self-consciousness. The teacher might continue to enjoy a distinctive centrality because he enunciates more interesting and more plausible or more propositions with better evidence than other persons in the scientific community. His authority therefore becomes more specialized and less compelling than it was in his formally pedagogical role. His authority now resides in his indicating subject-matter, problems, hypotheses, techniques in a manner which, without being compulsory, is worthy of serious attention. The scientist who is regarded as outstanding remains an authority; he remains a center of a community which studies particular subjects. His authority is, however, more suggestive than it is imperative.

IV

Tradition has frequently been used to refer to the unformulated, the vague, the unspoken, the unwritten, which is accepted on grounds of the authority of those who declare it. This seems to be remote from rational thought. It seems to be antithetical to the rational grounds for the acceptance of a proposition or of a pattern of thought. Yet reasons might still be good reasons even if they are not clearly formulated. Vagueness might be a mist of unclarity around an image, or an insight, or a proposition which, when it is clarified by subsequent analysis, turns out to be an important image or an important insight. This would indicate that what is regarded as true was present first approximated through vague, unclear images, insights or propositions.

This general statement applies to the guiding rules or norms or procedures by which observations are turned into theories without the person who makes that operation knowing just how he has done it. But not to know a theory in a formulation which is explicit and precise does not mean that it is not known in a less explicit, rigorous and precise formulation. A person knows how to construct a theory from data but this does not mean that he knows the theory so well that he can make that procedure of construction clear to others.

These kinds of "prepositional propositions" are part of what a newcomer to a given field of learning acquires from those who are already practitioners of that field (i.e., who are already members of the particular scientific community or scientific sub-community) which the scientist or any other intellectual creator "follows" when he is creating an intellectual work.

Polanyi has emphasized the role of these two kinds of penumbra knowledge in substance; i.e., in propositions which constitute the substance or content of a field or sub-field of science. I would like to extend the account of their function to the skill or art of the choice of problems for one's own research, of the perception of the relative importance of particular or general problems for a whole field (or sub-field) to the assessment of individual scientists (or literary men or whatever other kind of intellectual).

The knacks of perceiving important problems, of perceiving clues which can lead to important interpretations, of imagining the potentialities and limitations of an existing theory are properties of individual minds; they are acquired

as penumbra traditions from the scientifications and bearing of teachers and eminent scientists with whom the younger scientist is in intensive and prolonged contact.

It is not possible to "score" intellectual works numerically with any pretense to validity. "Citation" indices are useful only *post facto*; they can only support assessments made in other terms or by other means but they cannot themselves constitute such assessments. The assessment of the intellectual importance of a work or of the intellectual merit of a particular individual creator of works requires skills much like the skills which are operative in constructing a theory from observations, or in the application of an inarticulated or "tacit" proposition to the assessment of a theory or of a body of data.

Polanyi has repeatedly emphasized that the basis of scientific work (or scholarly work of any sort) is the agreement that there is reality outside oneself and that it is the same reality for others as it is for oneself. A further basis is the belief that this reality can be known, that it is worth knowing and that there are rules of discipline for knowing it although these rules are not sufficient for knowing it. Yet what must be added to those rules are modes of judgment which are not arbitrary. These modes of judgment have constancy between individuals and within individuals. The latter rules must also be learned by combinations of the experience of investigation and the experience of close intellectual association with a person who has mastered them and who applies them in a way which can be apprehended, even though neither the person who performs them nor the person who sees them can state exactly and concretely -- or abstractly -- what he is doing or what he is seeing.

These phenomena of the penumbra might become traditions. They too can become part of what is given, just as the explicit theories can be traditions embodied in written or printed form; they are just beyond or prior to writing and print. They are nonetheless acquired just as written or printed propositions can be acquired.

V

The scientific community is a community because it is approximately consensual about the kinds of things which have been referred to in the foregoing pages. The consensus need not be -- in fact, it could not be -- perfect; not all scientists agree on everything in their field. Yet, there could be no body of scientific knowledge and there could be no intellectual growth of such a body of scientific knowledge, nor could there be any territorial expansion of that body of knowledge if there were no consensus and hence no community of the individuals participating in that consensus.

When we speak of a body of scientific knowledge, we speak of a body of scientific propositions. These are propositions which have been established and confirmed by investigation (research) and assessment. That confirmation of a single proposition and the acceptance as confirmed of a body of scientific knowledge is consensual means that a plurality of individuals are participants in the consensus. It is this consensus of pluralities of scientists, together with their interaction as scientists, that justifies our speaking of a scientific community or scientific communities.

The consensus must exist not only with respect to substantive propositions. It must also exist with respect to penumbra knowledge and to penumbra criteria of judgment. But the consensus is not required beyond the World 3^4 of substantive propositions, penumbra knowledge and substantive and penumbra criteria of judgment.

Polanyi wrote in 1946: "The consensus prevailing in modern science is certainly remarkable. ... each scientist follows his own personal judgment for believing any particular claim of science and each is responsible for finding a problem and pursuing it in his own way; and ... each again verifies and propounds his own results according to his personal judgment. .... And yet ... we see scientists continuing to agree on most points of science" (*SFS*, 50).

The consensus of scientists is about substantive propositions in the first place. It is also about methods and theories. It extends beyond these rationally declarable things into two directions. One of these directions is towards and into the stratum of penumbra knowledge; the other is towards the institutions in which scientific work is performed and towards the scientific community in its wider reaches. (The scientific community embraces formally established institutions and informal patterns of relationships -- within the scientists' own (national) society and with scientists of other societies. The scientific community is a community because it has consensus about the interestingness of a set of common objects and objectives, because it has common rules governing not only the conduct of research -- "the scientific method" and the ethics of research -- but also governing the assessment of particular pieces of research and of whole fields of research and of the individuals who have done the research.) Polanyi wrote: "... no one can become a scientist unless he presumes that the scientific doctrine and method are fundamentally sound and that their ultimate premisses can be unquestioningly accepted." (*SFS*, 45).[5]

The membership of an individual in the scientific community is no formally inscribed or recorded condition. It exists when an individual who does scientific research has assimilated and is guided by its rules, affirms the values entailed in its objectives, and conducts himself towards his colleagues within the lines implied by those rules and objectives.

VI

The substantive propositions, ranging from the most explicit to the most general or abstract and from the most explicit to the most tacit, form at any particular moment a tradition for scientists working in a particular field. It is a tradition in the factual sense of an inevitable point of departure. The unarticulated or tacit "propositions" are not written down and by their nature are not explicitly stated. They are also traditions in the sense of being vague and unformalized. They must be transmitted largely orally and even then, in implicit rather than explicit form, although they can also be transmitted in the setting of the formation and exposition of more formalized propositions.

Authority plays a part in the transmission and acceptance of traditions. Does it also play a part in the traditions of the scientific community? It does but it is the authority, not of particular individuals in particular roles but of the scientific community as a whole and of the individuals who are regarded as capable of representing the scientific consensus. There are authorities whose judgments are regarded as worthy of acceptance with the understanding that they are subject to examination, revision and correction or outright rejection -- but never in their entirety at any moment of time. In any case, the existence of any confrontation of the tradition is ineluctable.

Of course, it is conceivable that each generation would or could develop penumbra kinds of knowledge from its own experience of seeking knowledge about reality. If it did so in any given generation, it is unlikely that it would fail to transmit it together with the scientific knowledge that had accumulated. Indeed penumbra knowledge is always transmitted in connection with the search for and the transmission of knowledge about reality. Just as the knowledge so gained becomes at least, for a time, tradition, so too the penumbra knowledge formed and practiced alongside it also becomes tradition.

It may be said that penumbra knowledge is more traditional, i.e., it lasts longer, as tradition, than does substantive knowledge. Penumbra knowledge acquires increments and some of this may pass into substantive knowledge and hence is rendered transient. Nevertheless, penumbra knowledge which is about the acquisition of substantive knowledge remains valid for a longer time than does substantive knowledge, which is revised, corrected and supplanted over shorter spans of time than are the arts or skills and "tacit propositions" of penumbra knowledge.

VII

Membership in the scientific community, accorded on the basis of the possession of a qualifying degree or of publications in accredited organs or of appointment as a teacher or research worker or high scientific administrator, constitutes grounds for trust. The belief that another person is a scientist, i.e., a member of the informally constituted scientific community, accredits him as trustworthy. The scientific community is constituted in part by the mutual confidence of scientists in the probity of their partners. There might be discrimination in the relative assessment of the scientific capacities of members but there is seldom doubt regarding their probity. This confidence is in part a product of the assurance that critical scrutiny is never, or at least very seldom, relaxed and that even if the moral disposition of those members were not sufficiently strong, they would be counselled by prudence to conform with the expectations of honesty. It is usually anticipated that detection of the failure to meet such expectations would result in severe sanctions. Even the suspicion of such failure to conform with the standard of probity damages the reputation of a scientist.

There are no officially and formally codified rules in the scientific community; scientists have no "code of ethics" like some other professions. The falsification of observations, claims to have made observations which were never made, and plagiary of the results of other scientists are among the few activities which are unusually denounced but until recently prohibitions have not been formally promulgated. Yet their infringement leads to sanctions. The sanctions include dismissal from appointments, removal, permanently or for a specified period, from lists of persons eligible for the financial support of their research, etc. Most of the obligations entailed in membership of the scientific community are positive; but they too are not clearly promulgated.

The fundamental belief on which the consensus of the scientific community is focused is the obligation of the scientist to discover the truth about the stratum or sector of reality which falls within the boundaries of their discipline or subject. It is probable that most living scientists do not like to use the term "truth" or to speak of "the truth" except in the context of very particular propositions. They regard the term "the truth" as embarrassing or even repugnant. They object to the use of the term "the truth" except on ceremonial occasions or when discussion becomes rhetorical. It sounds too metaphysical or idealistic for them. Yet it is it the postulate of their activity. By far the majority of scientists, both outstanding and obscure, act as if they have an obligation to seek the truth and to criticize error. They are indeed usually very scrupulous to adhere to this obligation.

VIII

The foregoing presents, with some reformulations, Polanyi's account of the scientific community and the obligations of its members. It is perhaps a little too idealistic. It perhaps makes scientists out to be somewhat more virtuous than they are in fact. There are more strains in the scientific community than Polanyi indicates. Teachers and supervisors of research and research students do not always conform with the pattern which Polanyi set forth.

He overstates the degree of autonomy of scientists in the choice of their problems of research, given the financial dependence of scientific research on governmental and private institutions who grant support in anticipation of practically usable results. The great increase in the scale of research projects also entails a diminution of the autonomy of individual scientists, particularly younger ones and those who are not the most highly reputed. It has nothing to say about the probably still very slight but nevertheless increasing frequency of ease of fraud in research.

Even if Polanyi's account of the scientific community has to be corrected somewhat, it still remains an account of a section of the intellectual stratum -- nationally and internationally -- which is very devoted to the transcendental ideal of truth and orderly, peaceful and rational in the conduct of activities which presuppose an irreducible consensus among the disagreement and criticism.

What however about other features of the conduct of scientists? What about the other sectors of the intellectual stratum? What about the other sectors of the academic profession? Does Polanyi's analysis portray the situation of these other sectors? Does it even portray the activities of scientists outside of science?

In the academic parts of the intellectual stratum, outside the natural sciences, there are certain fields in which there is a some approximation to the situation obtaining in the field under Polanyi's scrutiny. In the past, the classical and philological disciplines were very rigorous. They were for the most part concentrated on observations. When the collation, emendation and annotation of ancient texts was the main task of classical philosophy, a very strict discipline was observed. Unlike the natural sciences, they did not seek to construct theories. Any generalization which sought to place the particular data in the setting of a wider or more fundamental proposition was not only scrupulously examined; it was, in fact, frequently treated contemptuously, as if it were illegitimate. In the social sciences, the situation of respect for the rules varied greatly; consensus within the disciplines was less comprehensive and less compelling.

There may be several causes or reasons adduced for such variations in the adherence to rules. The degree of direct observability of the phenomena studied might be one variable; the more directly or the more mechanically observable by the investigator, the more applicable are strict rules of observation. Social scientists do not see directly the objects of their investigation; they do not see political parties or national states or social classes; they have to construct their objects of study from fragments. The degree of personal contact between pupil and master in the conduct of observations might be another. Closely related to these two is the degree of precision which is attainable in the definitions of terms and in the formulation of concepts.

In the disciplines in which observations can be made more directly, in which the pupil stands closer to the master and in which terms, operations and propositions can be stated more precisely, the scientific conscience of the individual benefits from the imposition of the resulting discipline. The conscience of each individual scientist working in a given field is supported by his awareness that his colleagues are under the same discipline; he sees that attitude manifested in their conversation and in their work. All this strengthens the consensus of the communities of natural scientists.

The difference between the degree of consensus in the natural scientific stratum and the degrees of consensus in the other parts of the intellectual stratum may, in part, be accounted for by the relatively high degree of self-containment of the scientific community (and sub-communities) *vis-à-vis* the other spheres of society. Of course, it would be an exaggeration to suggest that the scientific stratum and community are entirely self-contained. After all,

scientists must be maintained as physiological organisms and insofar as they do not maintain themselves from the sale of the knowledge which they inherit and discover, they must be supported from revenues accruing to them from their own private property or from patronage by rulers, ecclesiastical authorities and persons or institutions with revenues coming to them through the inheritance of property or from their own economic activities. (Alternatively, they could maintain themselves from the sale of their services as teachers to the families of their pupils. This they have seldom done; higher educational institutions have only to a very small degree been supported by tuition fees alone.)

Michael Polanyi's portrayal of the scientific community refers primarily to the one and a half centuries between the Napoleonic regime in France and self-reforming Prussia at one end and the Second World War at the other end. These fifteen decades were not only the great decades of pure science; they were also the decades of the rise of academic science to dominance in the scientific community. This was a period of a marked increase in size and density of the population of academic institutions and of their expansion over a larger territory and a great multiplication of the institutions through which it was practiced. Scientific activities and scientists became more independent of ecclesiastical authority and religious doctrine; they also became more dependent of direct patronage by the earthly powers of state and economy.

Although there were numerous points at which the scientific community touched on the economy -- in seeking financial support from it and in the discovery of knowledge which was of practical interest to business enterprisers and their agents, the connections were not as frequent or as dense as they have since become. The same was true of the relations between scientific activities and the state in its civilian and military aspects. Despite these connections with state and economy, these connections could be reasonably regarded by scientists as marginal to the main objective of scientific activity which was the scientific discovery of scientific truth. Within those conditions at the margin of the scientific community, Polanyi's account is largely correct. Qualifications need to be introduced here and there, but as regards the scientific activities as such, the scientific community is intact and must continue to be so as long as scientific knowledge, in which truth is the desideratum, continues to be sought.

Nevertheless, qualifications must be introduced into Polanyi's account. For one thing -- and very important it is -- scientific activity has become much more dependent on separately budgeted financial support. Its main support is no longer provided from the budget of the university which covered both teaching and research without distinction. It has become more dependent on specialized institutions, governmental and private, which now supply the greatly increased funds for scientific research. This has been accompanied by a complicated distribution of the power of decision as to what research should be done. Although much of the power of decision as to what research should be done and how it should be done still rests with scientists, that power is now shared with non-scientists in governmental and private institutions, private foundations, business enterprises, etc.

Certain scientific developments which were less well advanced and less prominent at the time when Polanyi wrote about these things have brought scientific knowledge and practical economic activities closer together in content and in the lapse of time between discovery and application. This has made the commercial interest in scientific research more specific; it has also made academic scientists in these fields more interested in the pecuniary benefits to themselves of particular fields of research. Hence, the criteria of choice of subjects of research have become less exclusively scientific. It must be pointed out however that these observations refer only to very limited fields of research.

Finally, the autonomy and separation of the scientific community from its surrounding society has been

diminished from within the scientific community by the heightened interest of scientists in politics. This was not very prominent in Polanyi's mind in the 1930s. He was a severe critic of the movement in the small but influential circle of British scientists to further "the planning of science." Indeed, I think that it was his disapproval of the effort in the 1930s to introduce political and other external criteria into the allocation of funds and the choice of problems for scientific research that precipitated the elaboration of his ideas about the autonomy of the scientific community. (Before the Second World War, Polanyi was a severe critic of the claims made on behalf of economic planning in the Soviet Union.[6] He must have been critical of communism since 1919 but he was not active politically.) His desire was to protect the scientific community from the intrusion of politics into its government. That is also the reason why he founded, with John Baker, The Society for the Protection of Freedom in Science. He took a serious interest in the suppressions of Mendelian genetics in the Soviet Union and it was this interest which led him to found the Committee on Science and Freedom in 1953 (with Alexander Weinberg and with the support of the Congress for Cultural Freedom).

Polanyi was never really interested in extending the political influence of scientists. The "atomic scientists' movement" in the United States and its even smaller British companion never aroused Polanyi's interest in the efforts of scientists to influence legislative decisions. In fact, he was utterly uninterested in it, even though some of his friends such as Leo Szilard and Eugene Wigner were much interested in it. As far as I know, he was not attracted by the "Pugwash movement." His political interests as a liberal did not go further in his thought than to set forth arguments for the freedom of scientific investigation and to elaborate the institutional arrangement with support of the freedom of science.

I think that he would have been very distressed by the strong political declarations made by American scientists; but, by the second half of the 1960s, his intellectual attention had moved far away from his earlier preoccupation with the freedom of science and with the traditions of the scientific community which sustains that freedom.

Had he taken that matter in hand again, it would have been incumbent on him to include the politicization of academic scientists. My own view is that he would not have had to change his account of the internal structure of the scientific community but he would certainly have had to broaden the range of his analysis to the external connections of the scientific community and consider the extent to which these were affecting the autonomy of the scientific community, the freedom of scientific work and the course of scientific development. He would also have had to consider the consequences for the internal structure of the scientific community of the much increased engagement of scientists in politics. In consequence of this interest in politics, the scientific community was renouncing one of the conditions which had kept it so concentrated on the pursuit of scientific truth. As long as scientists addressed themselves to an audience of other scientists and were contented to do so, they were kept under the discipline of their shared love of truth. They could also express themselves with confidence that they would understand each other.

IX

Thus far I have confined my account of Polanyi's writings on intellectuals to one particular sector of the intellectual stratum. Polanyi himself did not confine himself within those boundaries. From about the middle of the 1950s, he began to write about intellectuals of other kinds, literary intellectuals, publicistic intellectuals, intellectuals who wrote on philosophical matters and on society. In an essay in *Encounter*, in his Eddington lecture *Beyond Nihilism* and in chapters of *Personal Knowledge*, he dealt with intellectuals who had yielded to the totalitarian temptation. This new interest overlapped with his older interest in scientific intellectuals but he never entered into this new subject in

the fundamental way in which he analyzed the scientific sector of the intellectual stratum.

Polanyi was a cultivated man and in Manchester, London and Oxford, he moved in an intellectually mixed company of scientists, philosophers, literary critics, educated businessmen, leader writers and editors of the *Manchester Guardian*, *Encounter*, etc. Yet, until the mid-1950s, perhaps until the Hungarian uprising in the autumn of 1956, he was rather insensate to the political overtones of the intellectual society around him. He had no awareness of the political dispositions of the intellectuals of his own time. He did, it was true, oppose those who wished to bring political objectives into scientific activity but he did not seem to be aware that this movement of the Stalinist and fellow-travelling British scientists of the 1930s and 1940s had any wider roots or affinities.

In January 1947, I went to Manchester on his invitation to address the Manchester Literary and Philosophical Society. It was a small meeting, held in a private house of modest dimensions. I think that the quarters of the Society on John Dalton Street had been damaged by a German air war raid during the war; perhaps the Society was faltering towards its end. The meeting was attended by little more than ten persons. I spoke on "Intellectuals in Politics" and I stated what has become a commonplace since then, and which was perhaps not all that original, even then. This was before *L'Opium des intellectuels* which gave the imprimatur of a famous scholar to the ideas I expressed. It was before Lionel Trilling coined the term "adversary culture." What I said was centered mainly on that common feature of the culture of intellectuals in the West since romanticism and the Enlightenment, namely, their obsession with the deficiencies of their own societies, their vehement criticism of the rulers of their own society, their hatred of the bourgeoisie, etc. and since the middle of the nineteenth century, contempt for the liberal democratic order. All these things are perfectly obvious now.

They were not obvious to Michael Polanyi at that time. Polanyi disagreed completely with what I said. Perhaps he was ashamed of his rustic protégé? In his charming, soft-voiced way, he disrupted the meeting. He objected to everything I said. He gave no counter arguments. He repeated over and over again that I was wrong. Since I was his guest and he was my senior by several decades, I persisted by citing numerous individual instances from Rousseau and Voltaire to Sartre and his friend Koestler with many references to nineteenth century French and English novelists, and let it go at that. It was clear to me that Polanyi, with all his very intimate knowledge of scientists and his exceptional sensitivity and imagination about them, had none of these qualities when it came to thinking about intellectuals other than scientists. I think that he failed to see that scientists were intellectuals in a special situation. Nevertheless, he did towards the end of the ensuing decade begin to face the fact that there was a widespread outlook among non-scientific intellectuals which was not what he found among scientists within the scientific community. He became aware that many intellectuals did not participate in the consensus of natural scientist-intellectuals and in the moral trust which he thought was integral to scientific work. (He did not say that many of these intellectuals were also scientists, a significant omission.)

His Eddington lecture on "nihilism", delivered in Cambridge in the late 1950s, took up belatedly the view which he had unqualifiedly rejected when I set it forth in Manchester in January 1947. Of course, he tried to put it more philosophically than I had. The question is: was it adequate to account for the tidal wave of hostility to modern Western liberal democratic society? I think that it was not.

He described the outlook of a large block of these intellectuals as nihilistic. I think that this was inadequate. The intellectuals who became communists and fellow-travellers after the Russian Revolution of October 1917 were not

nihilists in the sense of having no beliefs or of believing that nothing was of any value. They were nihilistic in the sense that Bazarov was a nihilist; they wanted to make a clean sweep of all existing institutions and beliefs and wished to replace them by a rational society based on scientific knowledge.

After the Hungarian insurrection of 1956, which affected him deeply, he tried to understand what had led some of his contemporary intellectuals to espouse the Marxist-Leninist (and Stalinist) view of the world. I think that he did not make great progress in this effort. The mixture of scientism, the appreciation of violence, the tradition of hedonistic humanitarianism was not quite within his field of comprehension. He was unable to grasp that there were intellectuals who hated their own society.

The willing submission of scientists to the fundamental and unspoken values accepted throughout most of the scientific community about which he wrote with such deep insight was so different from the fundamental rejection of their own society by many intellectuals, including scientists, that he could not quite accommodate it.

But the tradition of scientific rationality is not the only tradition of intellectuals. Some of the traditions of intellectuals present and sustain the very antitheses of scientific rationality. For example, romanticism is no less important in modern societies and cultures than the tradition of scientific rationality. There are many variants of the romantic tradition; these run into other traditions. For example, the tradition of moral purity, the eschatological or chiliastic tradition, and the traditions of rejection of the world have all remained very much alive among intellectuals in the Western world.

The intellectual supporters of revolution were very far from nihilists. They believed a great deal. They believed that the kingdom of God on earth was possible -- except that they did not believe that God existed. They believed that human beings could and must be commanded and cajoled into conformity with a rationally conceived scheme for the ordering of society. They believed that an elite with a thoroughly scientific outlook could discover the right pattern for the ordering of society and that this right pattern would be ethically just, rational and efficient. They believed that there was nothing ethically problematic about coercion in the service of these ethically right ends.

Polanyi was right when he asserted that they had excessively high moral demands. This seems to me to be much closer to the truth than his statement about nihilism as a result of moral inversion.

There were other things in which they believed. They -- not all but many -- believed in the ethical value of violence; they saw not just that it was useful removing their enemies and intimidating them but because it had a transfiguring effect. They seemed to believe that human beings were ethically perverse and that violence could purge them of that perversity. They praised violence as an intrinsically purifying condition and they praised it as a means of overcoming the natural deficiencies of human beings. They thought that violent destruction was the prelude to a state of ethical perfection, a means of purification.

Polanyi glimpsed but did not follow-up his glimpse of the fundamental position of the anti-bourgeois attitude of many quite eminent literary intellectuals. He did see the important part played by bohemianism as the institutional counterpart of the scientific community. Although he interested himself in aesthetics in the last years of his life, he failed to see that the doctrine of “art for art’s sake” was a counterpart of the scientist’s love of truth for its own sake.

Although in his analysis of science he made much of the discipline imposed by an audience of the equally qualified, he did not see that the audiences of most other intellectuals were of the laity. The lay audience could always be accused of being insufficiently appreciative of literary or artistic achievement while such an accusation could scarcely be made in the scientific community. He also failed to see that literary and artistic intellectuals were enterprisers attempting to sell their products in a market as a means of obtaining a livelihood; this stood in contrast with scientists who did not sell their products to laymen and who did not depend on such sales for their livelihoods. These differences account for the existence of Grub Street and the bohemian mode of life and outlook among literary artistic and publicistic intellectuals in the eighteenth and nineteenth centuries, especially in the latter. It was from the culture of bohemia that the hostility of intellectuals to bourgeois emerged. There were many other factors such as an increased social sensitivity and the correspondingly increased publicity about the outermost peripheries of society.

X

I do not think that any other writer on the tradition of scientific knowledge and of scientific activity has ever penetrated so deeply and intimately into that subject as did Michael Polanyi. He was living in a society which did not understand much about tradition and his achievement is very noteworthy; it is indeed unique in his discussion of the various ways in which traditions operate in science. But there his penetration ended. He failed to extend his analysis of intellectual traditions, intellectual communities, intellectual disciplines beyond the practitioners of science.

He was steadfastly courageous in opposing the politicization of science, i.e., the bringing of political ends into the scientific community but he did not grasp the historical rootedness of the efforts to infringe on the autonomy of science.

It might sound odd that I should accuse Michael Polanyi of being oblivious to the dangers of scientism. He was unaware of the traditions of scientism. I do not think that Polanyi was guilty of scientistic arguments -- as Frank Knight very tenaciously insisted.[7]

I think rather that he was precluded from seeing the dangers of scientism because he thought that all factual or empirical statements about society or human actions were necessarily normative or moral statements. If one believes that empirical statements are also moral statements,[8] then there is nothing formally wrong with the scientistic view that scientific knowledge contains moral imperatives. This is what the adherents of "scientism" believe. I think that Michael Polanyi was insufficiently aware of this affinity between his own views and those who thought that knowledge of the laws of history and society confers knowledge of the right order of society and the ethically justifiable path to the attainment of that order.

Michael Polanyi's discussion of the character of modern literary and publicistic intellectuals is of such limited value compared with his understanding of scientific intellectuals that it calls for an explanation. The simplest explanation is that he spent nearly his whole life until his last years among famous scientists and felt very much at home with them. He had very strong sympathy with them. He started from the other end in dealing with literary and publicistic intellectuals. He had no intimate knowledge of literary and publicistic intellectuals comparable to what he knew of scientists.

Perhaps when he was younger he was so habituated to association with persons who were of socialistic

inclinations that he thought that was what most intellectuals were. I doubt whether he was ever a socialist. He was I am sure never a communist or a sympathizer with the short-lived Communist dictatorship in Hungary. Yet, when he went to the Soviet Union in about 1928, he did not become an anti-Communist; he became that much later.

It is true that he was a liberal in economics. Throughout his entire career at Manchester and for much of the tenure of his fellowship at Merton College, Oxford, he was closely associated with Professor John Jewkes who was a stout-hearted proponent of the institution of the market as a fundamental and necessary feature of an economic system. He was certainly very critical of the pretensions of economic planning. But until the Hungarian insurrection of the autumn of 1956, he did not turn his mind to the task of understanding why collectivistic beliefs were so widespread among intellectuals.

XI

Let me conclude by saying that in his analysis of scientific traditions and the scientific community to the constitution of which they are so essential, Michael Polanyi made an enormous contribution to our understanding of the nature of the community of scientists and of how that community makes possible the growth of scientific knowledge. In doing so, he has also provided a pattern of analysis which can be extended with equal fruitfulness to the understanding of other intellectual traditions and other intellectual communities. The process of extension of his theoretical scheme to subject-matters different from that to which he himself applied will illustrate the process of the growth of knowledge which opens up to his successors.

Other Fragments

Editor's Note: The end of the existing typescript contains several passages which were largely replaced with text above that develops the theme in a different manner. Perhaps something of Professor Shils' method can be perceived in the comparison with these rejected passages.

A

Although, in the last decades of his life, Michael Polanyi stirred in the direction of the Christian understanding of divinity, it was in fact a variant of his earlier analysis as the scientist's striving to disclose a hitherto unknown reality in the existence of which he is confident. The traditions of the modern Western intellectual lie for the most part outside the traditions of scientific rationality and the tradition of the approximation to the unknown divinity. (There is of course an affinity between the tradition of scientific rationality and the tradition of scientism but Polanyi rejected out of hand the concrete political results of adherence to the tradition of scientism.)

Polanyi refused to have any connection with many of the traditions with which intellectuals outside the scientific community are sympathetic just as he would not accept the political version of scientism -- e.g., planning -- which some of the scientists accepted.

B

I have undertaken a very difficult task here. It is to account for the absence of certain ideas from the thought of a person who developed so richly closely related ideas. I do not wish to make a sociological or psychological

interpretation of Michael Polanyi's inadequate perception or understanding of the hostility in a great variety of forms of so many Western intellectuals to their own society. This is a very complex and differentiated matter and it would be wrong for me to suggest that I have adequately described and analyzed the phenomenon. I did however know Michael Polanyi, for a long time discussed many matters with him and have heard him speak freely among friends. I can say that this hostility came into the forefront of his attention only relatively late in his intellectual career.

The question why was it so marginal to him? One answer is that he was interested primarily in discerning and promulgating the constitutive and preconditions of creative scientific activity. An affirmative attitude towards the discovery of truth, the critical acceptance of tradition and of the institutions through which the traditions of science are maintained and improved interested him above all, once he ceased doing scientific research himself. Although towards the end of his life he interested himself in aesthetics, it was less out of an interest in understanding works of art than it was of extending his epistemological theories to artistic creativity.

The fact is that the hostility of modern Western intellectuals to their own society, although it is a very tenacious and widely accepted tradition is of secondary importance in intellectual creation. It is true that it enters into the substance of novels and poems and of works in social science but it is really irrelevant to their intellectual and aesthetic value. That hostility is a foible as far as serious intellectual activity, like the natural sciences; it is not of significance in the production or assessment of scientific works. It is of interest to a sociologist attempting to understand the attitudes and role of intellectuals in modern societies but it is of little significance in dealing with the topic which interested Polanyi above all others.

When the matter became urgent to him after 1956, he could not really master it. The strength of his writing about scientists is that he had spent most of his life among and had an intimate understanding of them. He did not have similar relationships to literary, publicistic and social scientific intellectuals, and he was not greatly interested in their works. He did not have enough familiarity with them to do what he had done for scientists and scientific works.

C

What Michael Polanyi did for the community of natural scientists is indeed susceptible to extension to other kinds of intellectual activity.

Note: There now appears a two-sided task: one is to delineate precisely the way in which the authority of a scientist works through the precipitation of penumbra tradition; two is to delineate the extension of the authority of a scientist beyond his strictly scientific role as a working scientists -- but as a center of institution counsel and governmental advice.

The scientific community and each sub-community within it is hierarchical or in other words attentive to a center. There are official institutions and whole institutions in the scientific community which are central not so much because of their power to allocate funds, to make appointments and to accept works for publication but because of the achievements of their incumbents. "A scientist is granted exceptional influence by the fact that his opinion is valued and asked for" (*SFS*, 48). This is the way in which the scientific community forms around its centers. The centers are exemplary in a very general way; they are consultative and advisory. Their imperative powers are generally very limited.

D

This order, a product of consensus about the value of scientific knowledge and of the ways of its attainment is only a part of the very diverse stratum of intellectual activities in any large society. Scientific activities are only a part of the stratum of intellectual activities and the natural scientific activities are only a part of the stratum of scientific activities.

Such consensus as exists within the vaguely effective boundaries of the natural sciences does not obtain in much of the social sciences. It does not obtain either in the sector of those intellectual activities having to do with the production and assessment of literary works of the various genres nor in the production and assessment of artistic works or of that field of intellectual activity which consists of observations and assessments of events in the political or public sphere -- overlapping with the social sciences.

In all these fields we find effective traditions -- there would be very little accomplishment in any of these fields without the availability and observance of traditions.

Endnotes

[1]*Science, Faith and Society*. (Riddell Memorial Lectures. Eighteenth Series). University of Durham. (London: Oxford University Press, 1946). All citations use pagination in the more commonly found 1964 University of Chicago Press reprint.

[2]Delivered at a Conference on the Rationality of Press Associations in Oxford on 26 July 1948 and published in *The Rationalist Annual* (London: The Rationalist Press, 1949). Reprinted in *Conjectures and Refutations: The Growth of Scientific Knowledge* (New York: Basic Books, 1962), pp. 120-135.

[3]Polanyi interjects parenthetically: "Perhaps a modern version of the Apostolic Succession."

[4]Editor's Note: a reference to Karl Popper's world of statements in themselves in contrast to the world of physical objects (World 1) and the world of subjective experiences (World 2). See Karl Popper, *Unended Quest: An Intellectural Autobiography*, revised edition (LaSalle, IL: Open Court, 1982), pp. 180-87.

[5]Polanyi here quotes the Church Fathers: "*fides quaerens intellectum*" (*SFS*, 45) -- "to believe in order to know."

[6] See *USSR Economics* (Manchester: Manchester University Press, 1935) and *The Contempt of Freedom* (London: Watts, 1940).

[7] See *Ethics* about 1950. Editor's Note: Probably "Virtue and Knowledge: The View of Professor Polanyi" *Ethics* LIX: 4 (July 1949), 274-284, a review of SFS (1946) and *The Foundations of Academic Freedom*, Occasional Pamphlets of the Society for Freedom in *Science*, No. 6 (Oxford, 1947).

[8] Leo Strauss also seems to have believed this.

Polanyi, Popper and Methodology: A Reply to S. Richmond

Andy F. Sanders

1. Introduction

It is sometimes said that Polanyi's ideas have been summarized, analyzed, explicated, clarified and reconstructed often enough and that we should rather use his theories as a rich source of inspiration and a common platform to work on than go on repeating them. However, when accounts of his work appear that contradict the standard interpretation, one is forced to go back to the old texts in order to check the merits of these accounts. A good example is Sheldon Richmond's recent comparison of the theories of Polanyi, Popper and the historian of art E.H. Gombrich.[1] Apart from a rather surprising interpretation of Polanyi's philosophy of science, Richmond also presents a Popperian critique of it. My aim in this paper is to discuss critically his interpretation of Polanyi's epistemology and to show that his criticism fails.

One of Richmond's central questions is "whether there can be a Popperian reply to Polanyi's critique of methodology''(113). This is interesting because we already have a Popperian reply to Polanyi, namely, Popper's own *Logic of Scientific Discovery*. In the preface to the first English edition of 1959, Popper indicates that the book is an attempt to get rid of pseudo-psychological and ''subjective'' methodologies and that it is meant to

> save the sciences ... from an obscurantist faith in the expert's special skill, and in his personal knowledge and authority; a faith that so well fits our 'post-rationalist' and 'post-critical' age, proudly dedicated to the destruction of the tradition of rational philosophy, and of rational thought itself.[2]

Apart from this anti-Polanyian *cri de coeur* and a few scattered allusions in footnotes, it is hard to find any direct exchange of views, critical or otherwise, between what Joseph Agassi has called "the two outstanding philosophers of the mid-century," Popper and Polanyi.

What we do have, however, and what I take to be the most extensive and explicit Popperian criticism of Polanyi, is Alan Musgrave's doctoral dissertation *Impersonal Knowledge: A Criticism of Subjectivism in Epistemology* (University of London 1969).[3] Although Richmond does not seem to have considered secondary sources such as this, his own search for a Popperian reply to Polanyi's "critique of methodology" seems interesting in its own right. Especially since he maintains boldly that Popper's methodology as a system of conventional rules for discussing theories not only contradicts, but in fact refutes Polanyi's critique. This claim is crucially important for Richmond's whole endeavor because, as he himself points out (113), his overall position rests on a double analogy: (a) if Polanyi's critique of methodology parallels (in part) E.H. Gombrich's critique of aesthetics, and (b) if Popper's theory of methodology refutes Polanyi's critique, then (c) Gombrich's Polanyianism is mistaken also. But

of course if (b) is wrong, Richmond's position collapses and (c) fails, at least as far as the analogies go. My concern is neither with the analogies between Polanyi and Gombrich nor with Richmond's critique of the latter, but rather with (b), the viability of Richmond's "Popperian reply" to Polanyi's critique of methodology.

In order to establish whether Polanyi's critique of methodology is worthy of a more elaborate examination, Richmond raises the preliminary question of its consistency: (a) "Can Polanyi's theory of personal knowledge be true of itself?" and (b) "Is Polanyi consistent on the issue of the role of disagreement in science and philosophy?" (114). These are important questions so let us have a look at how they are dealt with.

2. Is Polanyi's Epistemology Consistent?

Can Polanyi's theory of personal knowledge be true of itself? According to Richmond, Polanyi has a theory "that no scientist can articulate his know-how without inhibiting his research."[4] He then goes on to tell us that if Polanyi's theory of personal knowledge is true of itself,

> then we contradict Polanyi's theory that no scientist can articulate his know-how without inhibiting his research. If ... Polanyi's theory cannot be true of itself, then Polanyi's theory is a counter-example to itself. (115)

Unfortunately, this seems seriously confused. Certainly, the piano player who is focussing her attention on the movement of her hands and fingers instead of on the music, will seriously inhibit her performance during a recital. But while practising she will be shifting her attention constantly and consciously between the particulars (touch, finger setting etc.) in order to improve her skill. Or, to mention a few other Polanyian examples, "[m]otion studies, which tend to paralyze a skill, will improve it when followed by practice" (*TD* 19) or "[t]he formal rules of prosody may deepen pour understanding of so delicate a thing as a poem" (*TD* 20). And in the case of research, discovery or coming to know in general proceeds by an alternating "see-saw" analysis and integration (cf. *KB* 129).

There is no need for further examples. The first part of a Polanyian rebuttal of Richmond's claim is simply that tacit awareness of clues and focal attention are mutually exclusive only at a particular point in time. It is possible, and sometimes even necessary, to shift one's attention to subsidiarily functioning particulars (though it may be difficult to retrieve them) within the problem situation. The second part, of course, is that explicit knowledge of rules cannot replace the activity or practice in which these rules are embedded. Knowing the rules of tennis by heart does not make one a good player. Thus whatever rules or procedures Richmond's scientists may come up with, these rules can never wholly replace the tacit operations involved in the practice of problem solving.

As regards the role of rules in scientific practice, I think it quite clear that Polanyi is neither saying there are no rules, nor that "methodology is worthless" —as Richmond wants us to have it — but that any rules that can be laid down for the guidance of research can be but "vague maxims" (cf. *PK* 125).[5] I think Polanyi's critique of "methodology" is summed up nicely in the following statement:

> It is only when we are confronted with a live scientific issue, that the ambiguity of the formal processes and of the various attenuated criteria of scientific truth becomes apparent, and leaves us without effective guidance (*PK* 150).

Note, however, that no effective guidance is not the same as no guidance at all. Richmond is totally misconstruing Polanyi's position by saddling it with the false dilemma between "either you have an exhaustively specifiable set of rules of method or you have no rules at all." Hence presumably his identification of the "no rules at all" option with tacit knowing: "[r]ules of method cannot guide science, because scientists depend on inarticulate sources of knowledge for progress" (114).

Pace Richmond, there simply is no Polanyian "theory" that scientists cannot specify their know-how without inhibiting their research. Consequently, there is no "serious flaw in Polanyi's theories" either.

Is Polanyi consistent on the issue of disagreement? In conducting his second consistency test, Richmond maintains that according to Polanyi

> ... disagreement is a block to scientific progress; only when scientists agree is there progress in science ... [o]nly when scientists agree on the fundamental views of nature, can they apply their views and so progress on the understanding of nature. (116)

To repeat, no evidence is produced to show that Polanyi really held this view, either explicitly or by implication. In the latter case, we would expect some textual evidence, but Richmond only comes up with the well-known passage (*PK* 151) where Polanyi, preparing the way for Kuhn, talks about *temporary* incommensurability of competing theories and the role of persuasion in debates between their proponents. Reliance on research traditions involves agreement on certain fundamentals and is necessary for any field of inquiry. But consensus need not, and indeed should not be total. Without some measure of dissent from scientific consensus no discovery, and without discovery no progress at all. Dissent already starts, for instance, in the first step to discovery which, according to Polanyi, is to find a good new problem, one "that no one has yet sighted." or to find a new way of solving a known problem (cf. *KB* 202; *TD* 22). In general, Polanyi is in fact warning that the dangers of scientific consensus "are an unceasing menace to scientific progress" (*KB* 94).

I conclude that Polanyi's position on the matter of scientific controversy does not warrant Richmond's claim that he "disallows disagreements in science" (116). Hence, the question concerning the consistency of Polanyi's theory of personal knowledge of which Richmond makes so much, turns out to be a non-issue. Let us now have a closer look at Richmond's Popperian reply to Polanyi's historical, psychological and logical criticism of methodology.

3. Historical Criticism: History of Science Versus Rational Reconstruction

According to Richmond, the theory of personal knowledge does not specify the procedures of discovery and testing and therefore it cannot have normative implications and it cannot be used as a guide by scientists.

To a certain extent, Richmond is right. But, of course, Polanyi's epistemology is not meant to specify rules but rather to give an account of the general conditions under which discovery and progress are reasonably possible at all. Whether this is a weakness of Polanyi's philosophy of science wholly depends on whether, and in what sense, philosophy of science should be conceived of as the watchdog of science ("methodology") or not.

As a true Popperian, Richmond sees its task as normative and so he construes the controversy between Polanyi and Popper as a dilemma between the history of science and methodology (cf. p.146). In contrast to Popper, Polanyi is said to advocate the view that the history of science should dictate the philosophy of science (117).

What to think of this? To begin with, it should be pointed out that Polanyi's philosophy of science is but part of his overall epistemological program, the critique of objectivism and the development of a viable alternative. Given his aims and his new conception of knowing, it seems quite natural for him to concentrate on and emphasize the *informal* aspects of scientific discovery. It seems odd to expect him to engage in what his logical positivist and critical rationalist opponents were already so busy doing. For instance, discussing the question of how the rationality of scientific progress could be safeguarded, inductively or deductively, and specifying explicit, preferably formalized, rules or procedures. However, this is asking too much charity of a true Popperian, so I will not pursue this issue further.

I think it is a caricature of Polanyi's position to say that he advocates the primacy of the history of science. As we have seen, Polanyi's point is not so much historical but rather meta-methodological: live scientific issues of real importance cannot be solved by applying algorithms. I think there is not a shred of evidence to suppose that Polanyi believes that selection and testing of hypotheses, though ultimately personal acts, are not also subject to rules. Nowhere is Polanyi even suggesting that methodologically "anything goes" or that the maxims of scientific procedure are unimportant. Scientists are guided by these maxims but since maxims are not algorithms, success cannot be guaranteed.

Next, it seems to me that the controversy between Popper and Polanyi is not at all about the alleged primacy of the history of science. For Polanyi, who in this respect comes close to the position of the later Wittgenstein, practice is primary. Just as Popperians use examples from the history of science in order to find support for their theories, Polanyi uses historical examples in order to support his attack on method fetishism by showing that many rules proposed by the "philosophers." if in fact applied to the letter, would have inhibited progress (at least in the natural sciences). In addition, his examples show that scientific standards or rules, instead of being a priori or universal, change over time:

> To every change in scientific value, from Kepler to Laplace to Einstein, there has corresponded a change in scientific method, which can be formulated in changing maxims of procedure (*PK* 170).

At this stage it might be interesting to point out very briefly some differences (and similarities) between the Popperian and the Polanyian approach to the history of science as can be found in their treatment of the Copernican revolution. Let us start with the Popperians.

4. Intermezzo: Popperian and Polanyian History of Science

I. Lakatos's and E. Zahar's beautiful account of the Copernican revolution is an attempt to reconstruct that episode such that it can be shown that science progresses in a rational way.[6] That is, they try to show that there are, with hindsight, good objective reasons to adopt Copernicus's heliocentric assumption at the time of its proposal — irrespective of the psychological or historical question whether or not Copernicans like Kepler and Galileo actually believed in, or were aware of, these reasons.[7]

Indeed, from Popper to Lakatos (and to Richmond, I should add), the Popperians did their utmost to keep philosophy of science within the bounds of the (allegedly) deductive context of criticism. Only the objective "third world" of logical contents of thought and language matters and so true epistemology is "epistemology without a knowing subject." Where this leaves the Popperian admonition to adopt the critical attitude remains a mystery. From a Polanyian point of view the question here is: who is to adopt that attitude if we are supposed to proceed on the assumption that there is nobody to adopt it? Or, what "objective" account can be given of the Popperian doctrine of fallibilism that human beings are prone to error if the possibility cannot be excluded that errors may be due to the incorrect use (by human subjects) of reliable methods?[8] Finally, it seems that (sophisticated) Popperian reconstruction of the history of science (as in the case of the Copernican revolution) allows for any measure of irrationality and dogmatism on the part of individual scientists (such as the Copernicans). Since the context of discovery is a matter of psychology (or sociology) only, it is irrelevant what scientists actually believe.

Polanyi's interests are markedly different. As Richmond recognizes, he naturalizes epistemology by bringing the heuristic and "psychological" dimensions of knowledge acquisition within its scope and thus within that of applied epistemology (philosophy of science). In doing so he was (with Quine) one of the first to jettison the Kantian distinction between the contexts of discovery (fact) and justification (norm). Unlike Popper, part of Polanyi's aim is to explain how coming to know is reasonably possible and (partly) for that reason he proposed innovative conceptions like, for instance, tacit knowing, intuitive foreknowledge, intellectual beauty and personal judgement.

All this can be traced in Polanyi's account of the Copernican revolution.[9] Again very briefly, for him the problem is how Copernicus's system could vastly exceed Ptolemy's in its anticipations though both had about the same explicit (empirical) content.[10] His solution is to a considerable extent identical to that of Lakatos, but it *also* attempts to specify the grounds on which "Copernicus fastened his hopes during thirty years of travail and ... he and his followers claimed, against bitter opposition, that the heliocentric system was real."[11] In addition, Polanyi correlates the logical, the psychological and the aesthetical realm by his suggestion that, ultimately, the heuristic superiority of the Copernican system lies in its appearance, that is, in its aesthetic qualities like harmony, depth and coherence. According to Polanyi, these aesthetic criteria in science are marks of intuitive truthlikeness and intimations of reality.[12] It goes without saying that on his account the appreciation of these qualities is a matter of fallible tacit knowing, not of mechanically applying a set of rules.

5. Psychological Criticism: Commitment versus Universal Criticism

According to Richmond, Polanyi holds that two requirements of the methodological approach, namely the critical attitude towards theories and the universality of discussion, are psychologically impossible to meet (cf.p.113). A critical attitude towards a theory involves appreciation. In order to appreciate a theory, however, one has to interiorize a-critically the tacit framework in which it is embedded. Thus, according to Polanyi, "one can only appreciate a theory if one is a-critical of it" and therefore critical methodology is impossible (119). Worse even, the tacit frameworks in question are conditioned by local culture. Hence "people from different cultures with different implicit premises cannot have discussions" (113).

Richmond's reply to his own construal runs as follows. Popperian methodology only proposes conventions for the procedure of discussing theories "as impersonal trials seeking truth" (119). It does not describe actual attitudes of scientists but it can be used to change them. For example, Richmond suggests that we could agree to ignore our tacit presuppositions towards theories and proceed to discuss them as impersonal trials. Similarly, scientists from different cultures can agree "to ignore their tacit views and ... to apply the agreed-upon rules to their disagreements" (*ibid*.).

My rejoinder can be brief: Richmond has failed to grasp the meaning of "a-critical" as an important characteristic of tacit knowing (cf., e.g., *PK* 264). The mental act of integration which constitutes tacit knowing (like personal judgement, choice and decision) can as such, that is, *at the time of its making*, be rash, premature or unwise but not critical.[13] It would be a mistake, however, to think that this means that an a-critical mental act of integration could not be performed in order to criticize something else, or could not itself be criticized afterwards. Similarly, tacit frameworks are interiorized in a largely a-critical way and function at subsidiary levels, but it does not follow that one cannot be critical of particular elements of the theory one is working on or even of the particulars of the framework in question (insofar as they are specifiable).

Finally, we have Richmond's claim that Polanyi's theory of knowledge implies the impossibility of cross-cultural discussion. If true, this would imply that Polanyi advocates total incommensurability of interpretative schemes and thus a radical relativism of truth (and that would make his position utterly inconsistent). But Richmond does not present a shred of evidence for his claim, which I find totally incomprehensible.

6. Logical Criticism

On Richmond's account, Polanyi's logical criticism of methodology is based on Plato's Meno paradox. We are told not only that Polanyi adopts Plato's solution, but also that

> [g]iven Meno's dilemma that methodology either finds and so is unnecessary, or that methodology cannot find and so is useless, the consequence is that methodology cannot guarantee certainty (120).

Again we are forced into a dilemma: either give up certainty (Popper's choice) or give up methodology (Polanyi's choice). If certainty goes, we accept that no finality can be reached. Methodologies are just conventional guidelines for improving our trials by asking how we can more quickly expose error.

Again, the tendency to simplify matters leads to distortion and confusion. First of all, Polanyi's solution to the Meno paradox can hardly be classified as "Platonic." Tacit knowing is not necessarily a kind of retrieval from memory, nor as is suggested elsewhere some "inner, hidden source of knowledge" (110) but rather a capacity of cognizing agents for solving problems. More importantly, it is totally incomprehensible to me how the theory of tacit knowing could be taken as an attempt to safeguard certainty. This is supposed to be Polanyi's position:

> Polanyi's idea is correct that science cannot guarantee truth. However, he is too utopian in expecting that science should guarantee truth (120).

But what does this mean? Unfortunately, no explanation is forthcoming. If we are supposed to come up with bold conjectures, I would say that, yes, according to Polanyi, science is our best bet if we want to have reliable knowledge. Precisely for that reason, science itself (as an institution) should be safeguarded. If this is utopian, Popper and the overwhelming majority of philosophers of science, not to mention scientists themselves, are equally utopian. As Richmond seems to acknowledge, Polanyi is convinced that certainty cannot be had. But he does seem to overlook that Polanyi, like Popper, is not only an anti-foundationalist but, as I have tried to show elsewhere, an impeccable fallibilist also.[14] This brings me to a final point, Richmond's own Popperianism.

7. How "Popperian" is Richmond's Reply?

Not only Richmond's rendering of Polanyi is highly problematic, his summary of the Popperian view of methodology as mere "conventional rules for evaluating theories" (117) is questionable too.

To begin with, the rules of logic which play a crucial part within the Popperian system are not conventional at all. On the contrary, they are, at least according to the Popperians themselves, about as near to "absolutes" as one can get. Surely, there is nothing particularly "conventional" to *modus tollens*, the logical backbone of early (naive) falsificationism.[15]

Next, Popper's rejection of induction and his ban on *ad hoc* strategies to protect theories from falsification are just too central to his program to have them treated as mere conventions or topics for discussion. Similarly, the decision to jettison the non-deductive heuristic ("irrational") context of discovery from philosophy of science and to consider only its ("objective") products, the ready-made theories, worthy of philosophical attention can hardly be seen as a convention, let alone as a proposal for discussion.

This brings me to a final point. According to Richmond, when there is a clash between scientific practice and philosophical rule Popper holds that "scientists and philosophers should *discuss* not dictate" (118). But this is puzzling because what now remains of the normative role of philosophy of science? As Richmond acknowledges, the whole point of normative Popperian philosophy is to legislate: "Philosophers of science ... ask what *should* scientists do?." I do not think that the ambiguity can be solved by suggesting that Popper thinks "there should be a dialogue between philosophers of science and scientists" (*ibid*.). Of course this would nicely legitimize the role of the philosopher of science, but apart from the fact that to prescribe dialogue remains a prescription all the same, it also seems watering down the normative aims of Popper's epistemology.

To sum up, whatever the merits of Richmond's other interests, his account of the similarities between Gombrich and Popper and Gombrich and Polanyi, his Popperian reply to Polanyi fails. Not only are Polanyi's ideas frequently misconstrued, but his own position is in several respects less Popperian than he is suggesting.

ENDNOTES

[1] Sheldon Richmond, *Aesthetic Criteria: Gombrich and the Philosophies of Science of Popper and Polanyi*, Series in the Philosophy of Karl R. Popper and Critical Rationalism (ed.) K. Salamun, Vol. VI, Amsterdam/Atlanta, GA: Rodolphi, 1994.

[2]K. R. Popper, *The Logic of Scientific Discovery*, London: Hutchinson, 1959, 23.

[3]As the title suggests, Polanyi is considered a paradigm example of a so-called "subjectivist" epistemology. Musgrave's thesis was written under Popper's supervision whereas various versions were commented upon by J.W.N. Watkins and I. Lakatos. J. Agassi might be an exception but it is questionable whether he should be taken as a Popperian (cf. his "Genius in Science." *Philosophy of the Social Sciences* 5 (1975), 145-161) . Musgrave's criticism of Polanyi is extensively discussed in my *Michael Polanyi's Post-Critical Epistemology*, Amsterdam 1988, 159-226.

[4]Richmond, *Aesthetic Criteria*, 115. The textual evidence for this claim are *PK* 56 and *The Study of Man* (*SM*) 31 with examples of skills (piano and tennis playing) and the thesis that subsidiary and focal awareness are mutually exclusive.

[5]Cf. also *PK* 311, where Polanyi rejects "the vain pursuit" of a *formalized* scientific method and advises us to abandon the search for *strict* criteria and *strict* procedures: "The scientist's procedure is of course methodical. But his methods are but the maxims of an art which he applies in his own original way to the problem of his own choice." Quite rightly, the Popperian Lakatos interpreted Polanyi as advocating a "case law approach." by contrast to Popper's "statute law" approach. In my view Lakatos even comes close to becoming a Polanyian when he points out that the scientific standards, as applied by the scientific elite in particular cases, have constituted the main yardstick of the philosopher's *universal* laws. Cf. Lakatos, "History of Science", *The Methodology of Scientific Research Programmes*, Cambridge 1978, esp. pp.136-138. Interestingly, though, Lakatos makes two exceptions: when a research tradition degenerates or when a new bad one is founded. As examples of the latter he mentiones "some of the main schools of modern sociology, psychology and social psychology" (Lakatos, *ibid*., 137). If Lakatos is right, this might explain Richmond's concern for an autonomous and legislative role of philosophers of science as a methodology. It may well be that certain fields of inquiry are in need of (better) rules, whereas the natural sciences can proceed on their own steam. In that case Polanyi, at least insofar as philosophy of science proper is concerned, does not distinguish sufficiently between the state of the art in the various sciences. But neither does Richmond.

[6]Cf. Lakatos (and E. Zahar), "Why did Copernicus's Research Programme Supersede Ptolemy's?", *The Methodology of Scientific Research Programmes*, p.168-192.

[7]Briefly, the objective reasons are in conjunction: Copernicus's theory was superior to Ptolemy's because: it predicted a wider range of phenomena, it was corroborated by novel facts (though only much later) and it showed more heuristic unity. Lakatos and Zahar, *ibid*., p.189.

[8]For an interesting criticism of Popper's attempt to eliminate the knowing subject, cf. Susan Haack, "Epistemology With a Knowing Subject", *Review of Metaphysics* 33(1979), 309-335; see also my *Michael Polanyi's Post-Critical Epistemology*, p.218ff.

[9]Cf. M. Polanyi, "Science and Reality", *British Journal for the Philosophy of Science* 18 (1967), 177-196. For a more elaborate comparison of the Lakatos-Zahar and the Polanyian account than I can undertake here, see my *Michael Polanyi's Post-Critical Epistemology*, p. 138-145.

[10]Cf. Polanyi, "Science and Reality", 189. Interestingly, his answer is in part similar to that of Lakatos and Zahar: the Copernican system was superior because it explained the main features of Ptolemy's system, it predicted the relative orbital radii of the planets and it had greater heuristic power.

[11]Polanyi, *ibid*., 185; the objective ground was Copernicus's theory which explains planetary loopings and predicts a plausible sequence of orbital radii.

[12]As far as I can see, Richmond does not go into the issue of the important role of *aesthetic* criteria like, e.g., "intellectual beauty." within Polanyi's epistemology.

[13]Cf. also *PK* 314 where it is pointed out that mental acts cannot be corrected "at the moment of acting."

[14]See my *Michael Polanyi's Post-Critical Philosophy*, p.22,192f.,210; see also *PK* 271, 314f., 404.

[15]Since Richmond (cf. p.9, 25f.) keeps his Popperianism safely confined to Popper's position of the *Logic of Scientific Discovery* (1959), he does not have to address the awkward problem that naive falsificationism (a theory is refuted when it is contradicted by observation statements) is untenable, as later developments in the philosophy of science (including critical rationalism) bear out.

Meta-Aesthetics and Meta-Methodology: A Response to Andy Sanders Review Essay

Sheldon Richmond

I had a strange dream the other night. Two elderly gentlemen were sitting on a park bench engaged in a philosophical discussion. I came on the scene long after the discussion between the two had started. I started taking notes, but the notes were incomplete. Suddenly the two men departed from the scene and I had nothing to go on but my partial rendering of a discussion that I had come upon long after it had started. So, I was faced with the decision whether to discard my notes altogether or reconstruct the debate. On the one side, if I were to discard my notes, I would lose for myself what I thought would be an enlightening and intellectually crucial discussion. On the other side, if I were to reconstruct their discussion from my incomplete notes, I would create a somewhat fictional and false account of their discussion. I chose the latter course of action: perhaps my misinterpretation might shed some understanding on a rather complex but fundamental problem.

The problem I believe I heard the two talking about was a modernized version of Plato's Meno problem. The modernized version concerned the role of method in scientific discovery. One of the elderly gentlemen apparently claimed that scientists use what he called tacit knowledge in the process of scientific discovery. In a sentence, tacit knowledge is the hidden background for what we know. I know there is a lot more to tacit knowledge than I have said, and I doubt I have said what can be said in a sentence. The other elderly gentleman put forward what seemed to me to be a contrary view: scientific discovery involved the use of the method of conjecture and refutation. I listened intently to their discussion and took many notes; and, many times I attempted to interrupt and ask questions. But they did not pay any attention to my interruptions and continued their discussion without stop. I wish I had had a video camera, or, at least a tape recorder. If only I could have slowed them down and if only they would have attended to my one simple question: Gentlemen, do scientists learn from having critical discussions with one another?

My dream took a strange twist at this point. The two gentlemen reappeared in the head of a third elderly gentleman and I somehow could hear them talking about my question in the head of this third man. But I was also talking to this third man about the nature of aesthetics and the role of method in art. The voices of the two other men somehow came out through the mouth of this third man. The third man said, aesthetics is impossible because the appreciation of artistic value can only be learned through personal contact with experts or connoisseurs. Then, the third man said in another voice that artists improve their work through the method of trial and error. I said to the third man that he seemed to be contradicting himself: artistic discovery occurs according to a method; but, the appreciation of the products of this method occurs with a judgment that one gains through apprenticeship to connoisseurs. Can we have both in art: method and personal judgment?

The third man suddenly jumped up and said that he must run. I tried to delay him and while grabbing on to his coat, he turned into a fourth person. This fourth person was somewhat younger, and a bit brash and what some would call rude.

He turned to me and said, you don't know what you are talking about. I meekly replied yes. I am no artist and I am no scientist, so I don't know art and I don't know science. He said, if you don't know them, don't talk about them. I then asked, you mean I should be silent about that which I don't know. No, you must talk, but talk about the talk about art and science. I was puzzled. He explained to me what the strategy he called meta-aesthetics and meta-methodology would involve. His explanation went something like this: Aesthetics asks, what are the criteria for artistic judgment? Meta-aesthetics asks, can we critically discuss the alternative answers to the question of what the aesthetic criteria are? Methodology asks, what methods do scientists use? Meta-methodology asks, can we critically discuss the answers to the question of what methods scientists use? He then asked me: you do see the parallel don't you? Before I could respond, he threw several hundred books and several thousand articles at me.

The books and articles kept falling out of the sky. Before I could finish writing a note, a new book appeared. But then the third man returned and remarked in a mocking tone: Ha—philosophers don't read. When they do read, they misread. I wasn't sure which of the original two elderly gentlemen were speaking. Or was that his own genuine voice? Do any of us have a genuine voice? Or, do we all speak the distorted voices of other voices? These questions tormented me. Was my dream turning into a nightmare? What started out as me listening to a wonderful discussion between two wise philosophers turned into me hearing distorted echoes of conversations by poor mimics. I wondered whether I could retrieve the original conversation by the original thinkers themselves.

The fourth person reappeared and said in his brusque, no nonsense manner. Wake up Sheldon, the two old men you talk about didn't talk together at all. You waste your time in this silly dream of a conversation between the two who turned their backs to each other.

I woke up. I still wonder whether those who think that science grows by the use of method can talk to those who think that science grows through the use of tacit knowledge; and, I still wonder whether those who think that artistic appreciation involves the use of aesthetic criteria can talk to those who think that artistic appreciation is a matter of connoisseurship.

Here is a suggestion: May we drop the question, "who are the genuine interpreters, scholars, and heirs of Popper and Polanyi?", and just talk the talk—i.e., how does science grow? how do artists create? how do scientists and artists learn from each other? how do people learn? when do we know that we have learned? where, with whom, when, and how can we talk so that we can learn from each other? In other words, my suggestion is: Let's each speak with our own voices and find for ourselves where the conversations lead.

Reviews

Sheldon Richmond, *Aesthetic Criteria: Gombrich and Philosophies of Science of Popper and Polanyi*. Atlanta, GA: Rodolphi, 1994. pp. 152. $29.70. ISBN: 90-5183-618-X.

Art historian Sir Ernst Gombrich was an ardent follower of the thought of Sir Karl Popper. Or was he? Richmond's thesis is that in matters of aesthetics, Gombrich breaks away from Popper and instead adopts ideas from Michael Polanyi.

Richmond describes Polanyi's criticism of methodology and Gombrich's criticism of aesthetics to demonstrate the analogies of their critiques. He applies a Popperian reply to both Polanyi and Gombrich to show the disanalogous thought between Popper and Gombrich and to refute Gombrich: a Popperian aesthetics is not only possible but worthwhile.

Richmond's thesis rests in Polanyi's rejection of methodology and Gombrich's implicitly devaluing of aesthetics. Since Gombrich does not explicitly critique aesthetics, Richmond generates his critique by first stating Polanyi's critique of methodology and then substituting terms such as “aesthetics,” and “work of art,” for “methodology,” and “scientific theory.” The problem is that Richmond does not understand Polanyi.

Richmond takes a wrongful leap from Polanyi's stating that no explicit rules can exist to guarantee discovery to stating that no rules exist to aid in discovery. Richmond misses the crucial feature of tacit knowing: many parts are involved in the discovery of a whole. Polanyi does not dismiss rules and methodologies; he clearly states that more is involved than rules in the discovery process. Rules are important. But they are guides that contribute jointly with other guides to discovery, such as tradition, perception, facts, intuition and imagination. Because Richmond bases his thesis on Polanyi's rejection of methodologies, his whole argument collapses.

Richmond crowns his argument with Polanyi's call for an unconditional adherence to an idea with the result that Polanyi does not allow error to act as an instrument for learning. This is analogous to Gombrich who says “it is impossible to learn from our mistakes in aesthetics” (54).

What I find refreshing in Polanyi's epistemology is that we can make mistakes and learn from them. Our adherence to an idea is not unconditional, but always open to correction and enlargement. Polanyi does stress that we need to be impassioned by an idea; we trust in it, or we could not progress. But our passions and beliefs are not closed. A fundamentalist approach also hinders growth in knowledge. We proceed to learn, backed by a belief in something which may be incorrect. Polanyi frequently reminds us that as knowers we are fallible. Our fallibility is not a hindrance but an opportunity to break out into something new.

Richmond's thesis is an interesting idea. However his belief that Polanyi's theories are inconsistent and irrational is wrong. I encourage this author to re-read Polanyi with an open mind. He has tried to ally Gombrich with Popper in regards to the history of art and Polanyi in regards to aesthetics. To join in one thinker (Gombrich) two disparate thinkers (Popper and Polanyi) is a fascinating and creative undertaking. Perhaps another attempt would bear some fruit.

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The Primacy of Persons and the Language of Culture: Essays by William H. Poteat. Edited with an Introduction by James M. Nickell and James W. Stines. Columbia: University of MO Press, 1993. pp. 343. $49.95.

I was a graduate student at Duke University when I first read "Persons and Places." It was circulated by Bill Poteat to his students before publication and the occasion is memorable because it was a particularly existential one for me. Earlier, I had read Poteat's "Myths, Stories, History, Eschatology, and Action" in a seminar and had appreciated its insight, particularly its elucidation of the logic of narrative, but "Persons and Places" was special to me. I was overwhelmed by it, or in Tillichian terms, I was "grasped." The essay was "about" the concept of "person" and how it was inextricably connected to "place." Furthermore, it showed that modern sensibility, replacing "place" with "space," denatured "persons" into objective "selves," abstracting us from any fundamental placement in the world or before God. It was a powerful essay, well-written and argued with unique insight, in that sense, an ordinary Bill Poteat product. But it was more than that to me. As I read the essay, I sensed (tacitly?) in my person what Poteat was writing "about." The essay performed a change in my perceiving, not just my perception. It was the first time I "caught on," in some intellectual/existential way, to what Bill Poteat was trying to "do" with me. After that experience, I began to seek out and read all his essays, published in a number of journals and covering a variety of topics.

I relate that experience because after I read *The Primacy of Persons and the Language of Culture*, I have had trouble knowing exactly how to respond to it. This collection of essays by William H. Poteat, written over a period of three decades is, by any academic standard, an impressive work. The essays are organized in four categories to permit a variety of readings other than merely a chronological one. The introductory essay by James W. Stines and James M. Nickell is, in itself, a wonderful entree into the life's work of Bill Poteat and deserves careful reading. This collection of essays is an important contribution to anyone who is interested in exploring the dynamics of our modern conceptual framework. It seems to me to be a required resource for any student of Western culture, philosophy of religion or philosophical anthropology. Even theologians, properly situated, would do well to deal with these essays, especially the explicitly theological ones.

But what to make of them as a whole, a corpus, a volume, is another matter. Stines and Nickell take up this very issue in the opening paragraph of their introduction by stating that the collection "should be understood as in some sense a kind of prolegomenon for anyone wishing to study his (Poteat's) more recent work" (1). They are referring specifically here to three books by Poteat _ *Polanyian Meditations*; *A Philosophical Daybook;* and *Recovering the Ground*, all lengthy pieces of philosophical reflection written since 1985. It is interesting, however, that they are guarded in this suggestion, for they see that clearly these essays are not an ordinary introduction to Poteat's work, nor essential to reading his later work. Rather, they go on to add their belief that "this collection, part of which is previously unpublished, is valuable in its own right" (1)

It is valuable, they go on to argue, because, "the malady of the modern world is a Cartesian malady"(9) and "What is needed...is the exposure of Cartesianism in its tacit as well as its explicit dimensions in such a way that it can be recognized for what it is. It is one of the great merits of these essays that they undertake this task, revealing in an unavoidable and comprehensible fashion the hidden Cartesian commitments and their implications which lie buried in some of the most seemingly innocuous positions" (10).

Here it seems to me, the editors have put their finger on the importance of these essays and why, upon re-reading "Persons and Places" I was transported back to Duke graduate studies. For these essays reveal a great deal of what is required to critique the Cartesianism of modern thought (or post-modern thought, for that matter). Our involvement in objective thought forms, abstractions from our lived experiences, and impersonal claims which

systematically erode our ability to accredit our own involvement in the world so constituted by our life, is so persuasive, so unnoticed, that no merely straightforward "argument" about these issues will do. The remarkable thing about these essays, and why they are so valuable as a corpus, is that they display a person *in the act of recovering himself* from an involvement in a Cartesian world which pervades and distorts our thought about ourselves, our culture, our language and (especially?) God.

In short, *The Primacy of Persons and the Language of Culture* is important precisely because it is, at one level, uneven and tentative. In that regard, it is not an introduction to Bill Poteat's thought. But, at least for me, it is an introduction to an intellectual vocation, in part by example (Poteat's life work), in part by the thoughtful, hard-nosed reflection found in the essays themselves. A chronological presentation of these essays would not do both, as Stines and Nickell sensed. But neither was systematic handling possible. That is why "Persons and Places" was exactly the "correct" or "fitting" essay to open this work. It does for the book what it did for me some twenty-eight years ago. It re-focuses reflection by inviting the reader to re-imagine oneself in the act of reflection. It places one as the tacit prerequisite for thinking. For only then can one begin to reflect adequately on the work required to appreciate one's relation to world, persons, God, culture, language. The task, as Poteat knew, is a life's work; it is also the work of and in community. Anyone reading these essays, especially if proper attention is paid to what "Persons and Places" does to the reader, will be initiated into some sort of citizenship in that community.

James Stines and James Nickell have made a unique contribution to intellectual life with this book.. I recommend it strongly, not for historical purposes, but for active use.

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Joan Crewdson, *Christian Doctrine in the Light of Michael Polanyi's Theory of Personal Knowledge (A Personalist Theology)* . New York: Edwin Mellen Press, 1994. pp. xi +445. $109.95/£59.95. ISBN 0-7734-9150-3.

Joan Crewdson has been working on her major study of Polanyi's relevance to Christian doctrine for at least fifteen years. The result is a thorough investigation of the fundamentals of trinitarian and incarnational Christianity from a perspective steeped in Polanyi's work. Her analysis is powerful, her own faith very clear, and the centrality of Polanyi's thinking for her interpretation of Christianity inescapable. Any reader concerned to explore the intricacies of the trinitarian faith will find much here to become engrossed in, and there are places where the depth of scholarship will reward careful and repeated study.

What others less prepared to take a very orthodox trinitarian and Christological line will make of it will depend very much on what one looks for and expects. It is not a *critical* study: it has little to say of Polanyi that is not respectful, and much to say that is devotional; there is no sustained attempt to address the kinds of questions that will be raised by those doubtful of trinitarian and incarnational issues, and to some extent the book needs the variety that such discussions would have provided. At times I felt overly bound up with a description of a certain highly orthodox position when something more radical and challenging might have brought new perspectives and freshness to a text that in its four hundred pages makes heavy demands on the reader's willingness to explore every nook and cranny of an idea. If it has a serious fault, it is that it tends to state and restate what the orthodox position is while scarcely seeming to acknowledge that there are alternatives. For Crewdson, there is a Christian view, a trinitarian analysis, a Christological perspective, where for others each of these may prove more multi-faceted and problematic.

The difficulty for the reviewer is knowing whether to address it as scholarship or as meditation (even mysticism). It is certainly both, but it has some serious deficiencies (not least its apparent inability to view Polanyi as a

human being with faults, whose philosophy, far-ranging and profound as it was, did not anticipate everything that came after it other than by dint of a pregnant vagueness and openness -- Polanyi can be to philosophy what Nostradamus is to fortune-telling). It always worries me that Polanyi seems to incite this one-sided adulation in his admirers. Until he is deemed worthy of and able to withstand a far more critical appraisal, it is difficult to see how he can enter the ranks of the genuinely epoch-making philosophers of our time.

As homage and meditation, Joan Crewdson's book scores much more strongly. There is a reverential quality about it, a *personal* quality, that stretches beyond anything general and merely scholarly and must be understood in terms of specific human devotion. Polanyi, for Crewdson, was undoubtedly the greatest of all thinkers. It is not for any reviewer to deny such a discipleship; only to point it out. I would have failed in my duty if someone were to part with $100 expecting this to be more than a spiritual homily or as open-ended as Polanyi's own thought. Joan Crewdson knows where she is going, knows with whom she is prepared to make the journey, and explores every step of the way with selfless thoroughness. There is a little too much "I believe +" and "I am arguing that +", but there are also some wonderful poetic phrases and some passages of extended meditation which invite reverence and awe, not criticism.

On the other hand, there are few open valences here, few unopened doors, few hanging or pregnant possibilities. At times the exploration is as exhausting as it is exhaustive, simply because there is such a concern to make things clear, to communicate the depth of the love and power of God that Joan Crewdson perceives in her own life, that we are left with almost no room to be ourselves and other. At times -- too many times -- I felt almost churlishly rebellious, wanting to say "yes, but +" far too often, but checking myself because that is not why we are here. This is less a study than an adulation; less and analysis than an assertion; less a question than an answer. As spiritual homily it is little short of profound: few prepared to explore these many pages will fail to be moved; some, like me, will wonder whether they ought in all propriety to have been invited to enter these inner sanctums of the human mind at all. The place whereon thou standest (and wherein thou readest) is holy ground. Walk (and read) respectfully.

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Notes on Contributors

Stephen Turner is a philosopher who teaches at the University of South Florida; he was both a student of Edward Shils' work and a friend during the last fourteen years of Shills' life. Turner's recent book, *The Social Theory of Practices: Tradition, Tacit Knowledge, and Presuppositions*, will soon be reviewed in *TAD*.

Edward Shils was a friend and colleague of Michael Polanyi. He was Professor of Sociology and Social Thought at the University of Chicago and fellow of Peterhouse, Cambridge. Shils was a special speaker at the April 1991 Kent State centennial conference on the life and work of Polanyi; the article in this issue, based upon his Kent State address, was one of Shils' last essays before his death in January 1995.

Andy Sanders is a member of the Theology Faculty at the University of Groningen in The Netherlands. He is author of *Michael Polanyi's Post-Critical Epistemology*.

Sheldon Richmond is author of *Aesthetic Criteria: Gombrich and Philosophies of Science of Popper and Polanyi.*

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 14778-0012 PHONE: (716) 375-2298 FAX: (716) 375-2389.

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 two or three issues of *TAD* each year.

The regular annual membership rate for the Polanyi Society is $20; the student rate is $12. The membership cycle follows the academic year; subscriptions are due September 1 to Phil Mullins, Humanities, Missouri Western State College, St. Joseph, MO 64507,. Please make checks payable to the Polanyi Society. Dues can be paid by credit card by providing the following information: subscriber's name as it appears on the card, the card name , and the card number and expiration date. Changes of address and inquiries should be mailed, faxed or e-mailed to Mullins (e-mail: mullins@griffon.mwsc.edu ; fax: USA 816-271-4574).

New members must provide the following subscription information: complete mailing address, telephone (work and home), institutional relationship, and e-mail address and/or fax number (if available). 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.

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 doublespaced 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 5.25" or 3.5" disk) of accepted articles; it is helpful if original submissions are accompanied by a disk. ASCII text as well as most popular IBM word processors are acceptable; MAC text can usually be translated to ASCII. Be sure that disks include all relevant information which may help converting files to Word Perfect or ASCII. 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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