Economics, Science, and Knowledge: Polanyi vs. Hayek

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ABSTRACT Key Words: F.Hayek; M.Polanyi; J.D.Bernal; economics; social studies of science;philosophy of science; epistemology
The relationship between Friedrich Hayek and Michael Polanyi is documented and explored with respect to philosophy and economics. Their respective positions on epistemology and science are shown to fundamentally govern their differences with regard to the efficacy of government policy with regard to the economy.

I. Philosophy of Science as Economics

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

Another reason that the situation is not uniformly bleak is that there have been many figures in history who have straddled the divide between economics and the philosophy of science; it is only our own historical amnesia and narrow academic disciplinarity that prevents us from recognizing this fact. A few names that spring to mind in this regard are David Hume, William Whewell, John Stuart Mill, Auguste Comte, William Stanley Jevons, Charles Saunders Peirce, John Maynard Keynes, Nicholas Georgescu-Roegen--the list could go on and on. But in this essay I want to focus on the really remarkable concentration of figures who came out of Central Europe at the beginning of this century and whose work was indelibly marked by the political events of the time. A wider purview would need to encompass the political economy of the Vienna Circle[2], the Wittgenstein/Sraffa connection, the impact of such thinkers as Karl Menger, Jr. and Oskar Morgenstern, and the motivations of figures such as Karl Popper and Paul Feyerabend.[3] However, in the interests of tractability, but also timeliness, I will confine myself to only two such figures: the Nobel prize winner Friedrich von Hayek, and the physical chemist-turned-philosopher Michael Polanyi.

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

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

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

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

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

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

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

II. How Michael Polanyi Became an Economist

Some biographical information on Polanyi is in order, if only to situate the events I shall cover in context, and to make up for the lack of any published biography. Michael Polanyi was the son of a Jewish civil engineer in Budapest who lost his fortune in 1899, but whose family maintained contact with a wide artistic and intellectual circle. He became a medical doctor in 1913 and served as a medical officer in the AustroHungarian army during World War I, also taking a Ph. D. in chemistry at the University of Budapest in 1917. He moved to Germany with the Hungarian uprising in 1919, and attained a position at the Kaiser Wilhelm Institute in the fall of 1920. He lived in Berlin from 1920 until 1933, becoming well-known as an expert on the adsorption of gases and crystallography, and developing a circle of friends which included Alfred Einstein, Eugene Wigner, Leo Szilard, John von Neumann and Max Born, among other illustrious scientists.[8] 'With the rise of the National Socialists to power in 1933, Polanyi accepted a chair in physical chemistry at the University of Manchester, which he held until 1948, exchanging it for a chair in Social Studies at Manchester. This was the period of maximum overlap with Hayek, who held the Tooke Professorship in political economy at the London School of Economics from 1932 to 1949. After that they drifted apart, with Hayek accepting a position at the University of Chicago in 1950, while Polanyi stayed at Manchester until accepting a research fellowship at Merton College Oxford in 1958.[9]

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

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

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

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

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

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

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

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

Quite early in this process, before Hayek had demonstrated any palpable interest in epistemology, we find Polanyi already foreshadowing his later positions on tacit knowledge in a note in *Philosophy of Science* in 1936.[19] He wrote there, "if at any time chemists would have been so ill-advised as to let themselves be frightened by physicists into abandoning all vague methods, and to restrict themselves to the field where exact laws (or what are supposed to be such by physicists) pertain, the development of chemistry would at that moment have stopped dead..." He then suggested the description of chemical substances had much more in common with "the art of commanding human behavior."

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

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

III. Hayek vs. Polanyi on the Nature of Knowledge

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

Endnotes

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

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

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

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

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

of Bruce Caldwell.

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

[28] 1 do not want to evaluate the pros and cons of Polanyi's solution to the problem of order in science in this paper; nor do I wish to unambiguously endorse his insistent theme that scientists can be trusted to do whatever they want in the name of research. Much more work would be needed to situate Polanyi himself in an era in

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

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

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

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

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

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

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

[35] Ibid, p.110.

Electronic Discussion Group

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

Submissions for Publication

Articles, meeting notices and notes likely to be of interest to persons interested in the thought of Michael Polanyi are welcomed. Review suggestions and book reviews should be sent to Walter Gulick (see addresses listed below). Manuscripts, notices and notes should be sent to Phil Mullins. Manuscripts should be double-spaced type with notes at the end; writers are encouraged to employ simple citations within the text when possible. Use MLA or APA style. Abbreviate frequently cited book titles, particularly books by Polanyi (e.g., *Personal Knowledge* becomes *PK*). Shorter articles (10-15 pages) are preferred, although longer manuscripts (20-24 pages) will be considered.

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

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WWW Polanyi Resources

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