"If I Join Forces With Mr. Kuhn. . . .":

Polanyi and Kuhn as Mutually Supportive and Corrective

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Polanyi lived most of his life as an exile from his native Hungary. When I collaborated with him in Oxford during the summer of 1974, he had lost much of his ability to concentrate and to formulate new patterns of thought. What he did do, however, was to impress upon me again and again the passionate convictions which had marked his own personal calling. During such conversations, I saw Polanyi as a great herald of freedom bent upon exposing the distortions of scientific materialism in Eastern Europe as well as those of logical positivism in the West. Polanyi's mission was to make it possible for free inquiry and the pursuit of truth to again take place in all fields of human endeavor without being dominated by or subordinated to a false and unattainable ideal/ideology of a distorted scientific method.

Had Polanyi lived, he would have rejoiced at the sight of the 1989 revolution in Eastern Europe. Had he been a younger man, he most certainly would have returned to Hungary to help shape the reemerging new order. In effect, however, the spirit and the thought of Polanyi has been returned to his native Hungary. The Michael Polanyi Liberal Philosophical Association has seen to that. Your Centennial Commemorative Conference (24-26 August 1991) and your reception on the occasion of the appearance of the first edition of Polanyiana (02 July 1992) are firm indicators that the exile of Polanyi has ended. I am pleased, therefore, to be invited to contribute, alongside my Hungarian brothers and sisters, to the return of Michael Polanyi to his own people.

Michael Polanyi had already celebrated his seventieth birthday when he first met Thomas Kuhn in July of 1961. Polanyi was asked to respond to a paper by Kuhn who had flown from Berkeley to Oxford to take part in The Symposium on the History of Science. Kuhn entitled his paper, "The Function of Dogma in Scientific Research." Clearly this paper was a first draft of how Kuhn proposed to use his own studies in the history of science and the sociology of knowledge to reframe and to reformulate the static issues of verification and falsification which then dominated the program of logical positivism. Polanyi not only had a chance to hear Kuhn's proposal but to support and to contribute to it. Polanyi's fifteen minute verbal response to Kuhn's paper began with these promising words:

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> The paper from Mr. Thomas Kuhn may arouse opposition from various quarters, but not from me. At the end of it he says that the dependence of research upon a deep commitment to established beliefs receives the very minimum of attention today. I could not agree more; I have tried in vain to call attention to this commitment for many years. I hope that if I join forces with Mr. Kuhn we may both do better (Polanyi, 1963:375).

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

My purpose is to examine how Kuhn and Polanyi might be mutually supportive and corrective so as to join forces in providing a more comprehensive understanding of the progress of science. My presentation will be divided into three parts: (I) The common ground Kuhn shares with Polanyi; (II) Four soft spots in Kuhn and their remedy; (III) Clarifying and upgrading Polanyi appeal to "objective reality."

PART I: THE COMMON GROUND KUHN SHARES WITH POLANYI

When Kuhn first published The Structure of Scientific Revolutions in 1962, he coined the term "paradigm" to refer to the normative theory-informed perceptions and theory-guided programs of research which inform the practice of a particular community of scientists at any given time. In his extensive postscript of 1969, Kuhn emphasized that a paradigm is not so much a theory (as understood in the philosophy of science) but more of that "disciplinary matrix" (Kuhn, 1970:182) imposed upon novices in science which enables them to routinely perceive and judge according to the shared patterns which define the existing scientific community (Kuhn, 1970:176). In their training, for instance, novices reproduce for themselves a classical set of laboratory and pencil and paper problems . . . .

> After he [the student of science] has completed a certain number [of these problems] . . . , he views the situations that confront him as a scientist in the same gestalt as other members of his specialists' group. For him they are no longer the same situations he had encountered when his training began. He has meanwhile assimilated a time-tested and group-licensed way of seeing (Kuhn, 1970:189).

Once the initiation process is completed, Kuhn emphasizes that operative and interpretative patterns are "embodied in the neural apparatus" (Kuhn, 1970:195) which enable the trained scientists to habitually perceive the world differently from the lay person who has received no such initiation:

> Consider the scientist inspecting an ammeter to determine the number against which the needle has settled. His sensation probably is the same as the layman's . . . . But he has seen the meter (again often literally) in the context of the entire circuit . . . . For the layman, on the other hand, the needle's position is not a criterion [i.e. a clue] of anything except itself (Kuhn, 1970:197f).

Kuhn emphasizes that such scientifically informed judgments operate "tacitly" (Kuhn, 1970:196) and "may also be involuntary, a process over which we have no control" (Kuhn, 1970:194). Kuhn acknowledged his indebtedness to Polanyi for his notion of "tacit knowledge" (Kuhn, 1970:191); yet, Kuhn does not always use this term with the same nuance that characterizes Polanyi. None the less, Kuhn firmly takes a stand parallel to Polanyi in affirming that the tacit knowing powers of the trained scientist, informed as they are by his paradigm, operate instinctively and stubbornly. And, since this knowing is locked away within the knowing organism, Kuhn acknowledges that, in the end, "we have no direct access to what it is we know, no rules or generalizations with which to express this [tacit] knowledge" (Kuhn, 1970:196). Kuhn's self-expression here is sometimes awkward and unrefined; yet, the common ground shared with Polanyi is quite evident.

THE PROBLEM POSED BY THE HEURISTIC CIRCULARITY OF COMMITMENTS

Once one allows that tacit powers of knowing operate habitually and stubbornly, one is inevitably faced with the recognition that there is no neutrality in perceiving and analyzing the world. All observation in science is guided and informed by theories and patterns of practice to which the scientist is committed. As a result, from within the community committed to the same paradigm, there exists a functional heuristic circularity. Rational appeals serve to draw attention to what passes for "reasonable" within given circles of commitment. Pragmatic appeals, meanwhile, fail to note that every belief has some degree of workability in the eyes of the believer. Appeals to given authorities disguise the fact that one's prior apprenticeship(s) serve to accredit certain authorities to the exclusion of others. Appeals to the austerity, the virtue, or the passionate sincerity of chosen mentors cannot disguise the fact that systematic errors are compatible with any and all virtues. Even such phrases as "responsible conviction" and "warranted assertability" (Emmet:5) cannot disguise the fact that our particular tacit commitments shape what we habitually perceive as "responsible" and "warranted." In the end, to assert something as "true," as "reliable," as "necessary to take into account" is to be caught red-handed affirming what one has been trained to acknowledge in a commitment situation.

Polanyi's solution to the heuristic circularity of scientific knowing is found principally within his phenomenology of discovery set within a sociological matrix. Kuhn's solution to the same difficulty is found principally within the sociology of discovery set within a historical matrix. What this means in practice will gradually become clear.

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KUHN'S GENERALIZED ANALYSIS OF SCIENTIFIC REVOLUTIONS

For Kuhn, the history of science demonstrates that the growth of knowledge does not take place by virtue of the steady accumulation of facts and theories. From time to time, the process of steady accumulation is disrupted by revolutions within science which result in new paradigms replacing or reinterpreting older paradigms. In so doing, scientific knowledge improves, i.e., the understanding and control which scientist's exert over natural processes advances.

A scientific revolution, according to Kuhn, has three phases. In the initial phase, Kuhn analyzes what he calls "normal science" within the framework of an initiation which trains the participating members to uphold a set of commitments which are functionally circular and provide "considerable resistance to paradigm change" (Kuhn, 1970:64). The accumulation of anomalies (i.e. instances wherein theories fail to account for some of the experimental data) within the conduct of normal science leads to the transitional stage wherein the sense of intellectual crisis prompts some members of the community to shift their energies away from modifying an existing theory so as to explain away anomalies to groping about for a viable alternative outside of the normative system. Kuhn uses the term "paradigm shift" to describe the alteration of commitments and habitual perceptions which the discoverer of a novel theory undergoes as the intellectual dissatisfaction connected with the "crisis" is relaxed. In the final phase, the community is ideologically split by the existence of two incompatible modes of understanding -- both functionally circular and both determined to persuade the other side of their truth. This revolutionary situation only subsides when one side succeeds in converting the other and the return to normal science is again possible.

KUHN'S ANALYSIS OF THE COPERNICAN REVOLUTION

Before going on to identify the soft spots within Kuhn's analysis of scientific revolutions, I want to briefly elaborate upon these three phases using the Copernican revolution. Since both Kuhn and Polanyi have repeatedly used this instance as the principal case study whereby to illustrate and confirm their account of scientific revolutions, the use of this material helps to reveal both their common ground and their differences.

How does Kuhn account for the Copernican revolution? The remote cause, for Kuhn, was the sense of "crisis" which circulated within the community of astronomers as they became increasingly aware of minor discrepancies between prediction and actuality within the Ptolemaic system. The immediate cause, however, was the activity of discontent astronomers who tried "to push the rules of normal science harder than ever to see, in the area of the difficulty, just where and how far they can be made to work" (Kuhn, 1970:87). Relative to the Ptolemaic system, this consisted in attempts to adjust or to add epicycles in order to achieve a better fit between predicted and actual sightings of the planets:

> Given a particular discrepancy, astronomers were invariably able to eliminate it by making some particular adjustment in Ptolemy's system of compounded cycles. But as time went on, a man looking at the net result of the normal research effort of many astronomers could observe that astronomy's complexity was increasing far more rapidly than its accuracy and that a discrepancy corrected in one place was likely to show up in another (Kuhn, 1970:68).

Faced with such a situation, some scientists despaired of ever attaining a perfect fit and decided to tolerate the discrepancies (Kuhn, 1970:81). Others, however, convinced that the key has not yet been found to correctly situate the revolving spheres, redoubled their efforts to find a solution. When success was not forthcoming even after many generations, what had previously been only a vexation turned into a "crisis" (Kuhn, 1970:82). Kuhn identifies the crisis mentality as weakening the hold that the tradition has upon the intuitive powers of the researcher at just the time when he has set his mind to deliberately "magnifying the breakdown" (Kuhn, 1970:87). The scientist in crisis will constantly try to generate speculative theories which, if successful, may disclose the road to a new paradigm" (Kuhn, 1970:87).

Out of this situation, the discovery of Copernicus was born. For Kuhn, "The decision to reject one paradigm is always simultaneously the decision to accept another" (Kuhn, 1970:77). Which other? That which relieves the "crisis" which brought the discontented Copernicus to grope around for an alternative in the first place. As soon as such a viable alternative is worked out by one of its members, the general strain within the community surrounding the old system functions to prompt others to accept the Copernican alternative and simultaneously let go of the Ptolemaic bridge which originally brought them to it. In Kuhn's own words:

> Copernicus' more elaborate proposal was neither simpler nor more accurate than Ptolemy's system. Available observational tests . . . provided no basis for a choice between them. Under those circumstances, one of the factors that led astronomers to Copernicus . . . was the recognized crisis that had been responsible for the crisis in the first place. Ptolemaic astronomy had failed to solve its problems; the time had come to give a competitor a chance (Kuhn, 1970:75f).

A community in crisis, however, does not immediately embrace an alternative system which demonstrates some marginal gains at the price of upsetting long-standing presuppositions regarding the place of the earth in the cosmos. As a consequence, for the time being, there was a split between those who continued to stubbornly believe that some solution would yet be forthcoming within the old system and those who committed themselves to perfect and to prove the worth of the new system. Since each side of the debate embraced incompatible commitments, there was no neutral ground to which either side could appeal by way of convincing those who perceived the reality of things from the other side.

> Like the choice between competing political institutions, that between competing paradigms proves to be a choice between incompatible modes of community life. Because

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> it has that character, the choice is not and cannot be determined merely by the evaluative procedures characteristic of normal science, for these depend in part upon a particular paradigm, and that paradigm is at issue. When paradigms enter, as they must, into a debate about paradigm choice, their role is necessarily circular. Each group uses its own paradigm to argue in that paradigm's defense (Kuhn, 1970:94).

Under such circumstances, Kuhn comes to the conclusion that the grounds whereby each astronomer makes his choice are "ultimately personal and subjective" (Kuhn, 1970:199). Hence, one must wait for the verdict of history to reveal which side of the debate will win out over its contender.

KUHN'S OPPOSITION TO LOGICAL POSITIVISM

Kuhn joins with Polanyi in calling upon the history of science by way of demonstrating that the scientific community does not change its mind on the basis of any simple set of rules governing prediction and experimentation. In Kuhn's own words: "There is no neutral algorithm for theory-choice, no systematic decision procedure which, properly applied, must lead each individual to the same decision" (Kuhn, 1970:200). In practice, this means a repudiation of the norms that were widely accepted by positivist philosophers of science relative to the unique role which experimental verification and/or falsification are repudiated to play within the scientific enterprise.

According to the analysis of Richard Bernstein, Kuhn unwillingness to allow that there are determinate criteria or operative rules for deciding which theory is to be accepted and which rejected has opened him up to virulent attacks by even Feyerabend and Lakatos, the "prodigal sons" of Karl Popper (Bernstein:64). Lakatos, for example, characterizes Kuhn's notion of a paradigm shift as "a mystical conversion which is not and cannot be governed by rules of reason and which falls totally within the realm of the (social) psychology of discovery" (Lakatos:93). While Kuhn does himself sometimes overplay the image of the scientist as undergoing something of a religious conversion, Bernstein correctly notes that the character of such attacks upon Kuhn is an indication of the "Cartesian anxiety" that greets any denial that there are determinate, unambiguous criteria for adjudicating conflicting theories as opening the way to the worst forms of relativism, subjectivism, and irrationalism (Bernstein:60). Anyone who has presented Polanyi's thought will recognize that he also is frequently misunderstood and judged in the same light.

PART II: FOUR SOFT SPOTS IN KUHN AND THEIR REMEDY

From the vantage point of my personal immersion within Polanyi's thought, I would identify the following soft spots within Kuhn's analysis:

1. Kuhn provides no satisfactory explanation as to how or why a crisis situation weakens the hold that the normative paradigm has upon the knowing powers of the inquirer:

> All crises begin with the blurring of a paradigm and the consequent loosening of the rules for normal research (Kuhn, 1970:84).
>
> Crisis simultaneously loosens the stereotypes and provides the incremental data necessary for a fundamental paradigm shift (Kuhn, 1970:89).

It is unfortunate that Kuhn chose the term "crisis" and insisted that it had both a heuristic and social function. According to Kuhn, existing commitments must be "loosened" before researchers begin groping about for a better solution to the anomalies existing within the operative paradigm. Then, when a superior solution does emerge, the social discontent prepares the way for a receptive hearing for the novel (and necessarily disruptive) paradigm. At this point, Kuhn is burdened by the mindset which characterizes the sociology of knowledge as practiced by scholars such as Berger and Luckmann. Any reference to interior states immediately evokes the fear of "subjectivism" and is excluded from the discussion. Yet, if Kuhn would be able to accept Polanyi's tacit dimension as undergirding all knowing during periods of normal science, he might be able to allow that tacit powers are also operative during periods of crisis as well, namely, by way of intimating in advance the possibility of "a better solution." More on this later.

Another problem with Kuhn's appeal to a "crisis situation" is the growing body of evidence that some discoveries take place and go on to gain widespread recognition without following his three phases. Consider the following:

(a) Kuhn himself makes reference to cases wherein a "discovery through accident" opens up a whole new field of inquiry without there being any prior paradigm. Kuhn highlights the case of X-rays (Kuhn, 1970:57).

(b) Despite Kuhn's even-handed analysis of the Copernican revolution in his earlier study (Kuhn, 1957), Kuhn appears to distort the evidence in favor of his analysis by asserting that "the state of Ptolemaic astronomy was a scandal before Copernicus' announcement" (Kuhn, 1970:67). What Kuhn fails to note is that "a thousand difficulties never make a doubt." When I read the historical evidence, it appears to me as though the long-standing limitations of the Ptolemaic system had been accepted as inevitable (cf. Heidelberger:275-277). Hence, far from creating "a crisis," the long-time limitations created a spirit of resignation. But this is precisely where the pioneering genius of Copernicus stands out. Where others were complacent, he was driven by an intellectual dissatisfaction bent upon discovering a better arrangement of things. In his own work, Copernicus provides multiple hints at this dissatisfaction. Consider, for example, the following:

> The fact that the wandering stars are seen to be sometimes nearer the earth and at time further away necessarily argues that the center of the earth is not the center of their circles [of rotation] (Copernicus:515).

Given the absence of any means to measure the distance of planets from the earth, Copernicus here notes that the periodic fluctuations in the light intensity coming from these planets signal that their distance from the observer changes. The Ptolemaic system was designed to calculate where the planets would be observed without giving any attention to how far they were from the observer. Copernicus, dissatisfied with this arrangement, went on to discover a system that allowed for the calculation of not only the correct place but the correct distance of the planets from the observer. This increased intellectual satisfaction necessarily acted as a powerful inducement for its acceptance even among those who were quite complacent relative to the limitations of the Ptolemaic system in this regard.

(c) Other cases can also be provided wherein Kuhn's notion of "crisis" fails to play the role which he wants to insist is there in every instance (Kuhn, 1970:67). For example, David Kitts who has mastered the history and the philosophy of geology contends that continental drift and plate tectonics were introduced into geology without any crisis (Laudan:285-287). John Greene has maintained the same thing relative to the emergence and acceptance of the Darwinian theory of natural selection in biology (Greene:312-317).

In sum, I am inclined to conclude that Kuhn's epistemology forces him to project a crisis at the root of every paradigm change. Polanyi would offer Kuhn his own critique of the heuristic value of doubting (PK:269ff) which might allow Kuhn to speak more of "intellectual dissatisfaction" rather than of "crisis" as having a heuristic role at the origins of new inquiries. Meanwhile, the issue of the acceptance of novel theories clearly sometimes takes place without any preliminary social crisis within the intellectual community. Here, as will be seen shortly, Polanyi provides an alternative. None the less, Kuhn does have the ability to supplement Polanyi in those instances wherein a social crisis does promote widespread research directed toward removing commonly perceived problems within the operative paradigm. The historical studies documenting simultaneous discoveries can best be accounted for in terms of the crisis conditions which Kuhn describes.

2. Kuhn offers no explanation as to how an explorative quest can turn up attractive alternatives without being sent off into a maze of dead ends. At one point, Kuhn suggests something akin to a phenomenology of discovery:

> The new paradigm, or a sufficient hint to permit later articulation, emerges all at once, sometimes in the middle of the night, in the mind of a man deeply immersed in crisis. What the nature of that final stage is -- how an individual invents (or finds he has invented) a new way of giving order to data now all assembled -- must here remain inscrutable and may be permanently so (Kuhn, 1970:89f, 122f).

Here Polanyi's analysis of how deliberate straining, imagination, and guiding intuitions function within problem solving serve to insure us that, even though the creative process cannot be exhaustively delineated, at least it can be perceived as having an internal direction. Polanyi spells this out most forcefully when he describes the heuristic role of the intellectual passions within his larger

phenomenology of discovery (PK:142-159, TD:3-33, Polanyi, 1966). Polanyi's analysis of the problem posed by Plato in The Meno plus his conception of how "indwelling" functions during both normal and crisis science would go a long way toward supplying the ground whereby research is conducted, from beginning to end, in response to deeply held tacit intuitions which evaluate the process along the way. Such informed intuitions are not infallible, as Polanyi notes (PK:143), but without them it becomes impossible to account for originality or for how a pioneer can sustain a quest through years of labor. My suspicion is that Kuhn, not being a practicing scientist, does not know how to properly allow for and appreciate the tacit powers which guides a pioneering investigation. Without them, however, Kuhn's system has no means of discerning how scientists arrive at new propositions save through disenchantment with the status quo.

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

Polanyi himself did use Gestalt experiments in order to illustrate how the bodily integration of clues in one self-satisfying mode positively impedes an alternative. Consider, for instance, this figure of a transparent cube. The heavy dot appears either on the corner facing towards the viewer or on the back side of the cube. In order to perceive the cube in its alternate mode, one must look away for a while or blink one's eyes.

This experiment can be used to illustrate how someone holding the Ptolemaic paradigm is positively impeded from envisioning the Copernican alternative. Unlike the bodily integration of sensory clues which takes place fairly instantly, months of careful study are needed to properly understand and appreciate either the Ptolemaic or Copernican treatises. Moreover, one comes to the study of either with predispositions and commitments which either facilitate or impede this process. In the end, however, once one has mastered the intricacies of both systems, one can intellectually move back and forth between the two intellectual schemas. Unlike the case of the Gestalt cube wherein both integrations hold equal sensory satisfaction, the intellectual alternatives posed by the Ptolemaic and Copernican systems invariably have different degrees of intellectual satisfaction. This explains why someone like Galileo, upon reading Copernicus, came away with a transformed set of commitments. In making up his mind, he was faced with two systems, each with its proper intellectual satisfaction. Scientific judgment has to do with the comparative satisfaction felt by someone who can move back and forth from one system to the other. In the end, however, unlike the case of Gestalt figures, personal commitments follow the greater satisfaction. Hence, in the end, one might expect Galileo to say, "I was once satisfied with the Ptolemaic system. Now, however, that I have understood the superior merit of the Copernican system, I find that my commitments have changed." Kepler, for instance, writes something quite like this:

> The reasoning of the ancients [relative to the order of the planets] is merely probable, but the demonstration of Copernicus, arising from his principles, brings necessity (Copernicus:861).

As for the pioneer himself, Polanyi's analysis of the interplay between intuition and imagination allows one to glimpse how intellectual straining and the sensing of a gradient of gradually deepening coherence guides inquiry from beginning to end (esp. Polanyi, 1966:89-91). When the new discovery finally emerges, the keen satisfaction felt at its discovery is rightly acknowledged as the indicator of the superior merit which greets its contemplation. And, as Polanyi tells us, "this truth-bearing passion is far from infallible" (PK:143) and may be upset by further explorations; yet, there is nothing else which is reliable for advancing knowledge. Furthermore, these judgments are historically conditioned. Copernicus held firmly to the notion that eternal motion required that the planets executed perfect circles at constant velocity. Kepler, persuaded that the path of Mars was somehow determined by some kind of mechanical interaction with the sun, was able to abandon epicycles in favor of some single formula which would specify the whole planetary path both in terms of velocity and shape. This greater intimation of truth did not invalidate the success of Copernicus. Here again, comparative intellectual satisfaction becomes the tacit rule for scientific judgment. Had Kuhn recognized this, he would bring more nuance to his Gestalt model of conversion and allow one to perceive how intellectual visions are never equal. By virtue of personal indwelling within alternative frameworks, one finds oneself committed.

Kuhn suggests that all abandoned theories have to be regarded as "false" because scientists no longer use them to guide their practice of science (Kuhn, 1970:115). Polanyi would agree that "truth" always implies a personal commitment (PK:254-255); however, I do not believe that Polanyi would, of necessity, regard Copernicus as "false" simply because he held on to the premise that eternal motion required perfectly circular orbits. The process of truth-seeking and truth-finding is historically situated such that one must allow that the "truth" of Copernicus was superseded by the "truth" of Kepler, and so on. Even the Ptolemaic system must be granted some degree of truth in so far as it intimated that the movement of the wandering stars could indeed be correctly predicted through mathematical models. While Kuhn does not take the stance of Popper in imagining that there exists some objective truth to which all gradually approximate, he none the less does not seem willing to allow that personal knowing requires historically conditioned affirmations without being relativistic. "No intellect, however critical or original, can operate outside a fiduciary framework" (PK:266).

4. Once Copernicus does come forward with his novel paradigm, it appears to me that Kuhn fails to provide any credible grounds whereby any other astronomer might want to join him. If, as Kuhn suggests, Copernicus has just about the same complexity (in terms of epicycles) as the Ptolemaic system and if Copernicus also fails to overcome the general discrepancies between prediction and observation noted in the Ptolemaic system, then it would appear that the choice of one system over the other is simply a matter of personal taste or, as Kuhn himself suggests, somewhat like committing oneself to one political party over another. In this case, persuasion seems to be reduced to who can manage the media most effectively and gain the support of recognized leaders in the field. According to Berger and Luckmann, alternative

symbolic universes in conflict often resort to raw power when they believe that open persuasion may not suffice (Berger:109). Perhaps it is with this in mind that careful thinkers such as Lakatos accuse Kuhn of suggesting that the social structure of paradigm changes reduces the quest for truth to "a band wagon effect" wherein "scientific revolution is irrational, a matter of mob psychology" (Lakatos, 1968-69:181; cf. response of Kuhn, pp. 259-266).

Ironically, Polanyi too has suffered from misunderstanding and misinterpretation relative to his own understanding of how novel theories are accredited. Hence, by way of resolving this difficulty, I plan to spend the third section sorting out Polanyi's program whereby the community of scientists alter their commitments to a new vision of scientific understanding.

III. CLARIFYING AND UPGRADING POLANYI'S APPEAL TO "OBJECTIVE KNOWLEDGE"

When Polanyi spoke of "objective knowledge" in association with "a theory on which I rely," philosophers of science like Ernest Nagel, Karl Popper, and Thomas Kuhn could hardly object. But Polanyi wanted to go beyond this. In Personal Knowledge, he spoke of the Copernican novelty (a) as having "an inherent quality deserving universal acceptance by rational creatures" (PK:4) and (b) as having "prophetic powers" in the sense of "show[ing] forth its truth through future centuries in ways undreamed of by its authors" (PK:5). Five years later, Polanyi spoke of the decisive importance of reaffirming "belief in the reality of emergent meaning and truth" (SFS:17). As such, Polanyi wished to accredit dedication within an intellectual community as being more than just a self-serving adherence to a shared ideology (as the analysis of Kuhn would sometime seem to imply). To this end, Polanyi prepared The Tacit Dimension as a working statement which draws attention to the discovery process itself as marginally responsive to a reality which is there at hand making its presence felt:

> The pursuit of discovery is conducted from the start in these terms; all the time we are guided by sensing the presence of a hidden reality toward which our clues are pointing; and the discovery which terminates and satisfies this pursuit is still sustained by the same vision. It claims to have made contact with reality: a reality which, being real, may yet reveal itself to future eyes in an indefinite range of unexpected manifestations (TD:24).

INADEQUATE INTERPRETATIONS OF POLANYI'S INTENT

Even though Polanyi used phrases such as "contact with reality," he never wanted to imply that the discoverer has some direct or indirect access to reality as epistemological realists have implied. Nonetheless, one finds interpreters of Polanyi who dress up his post-critical philosophy in the clothes of critical realism. T.F. Torrance, for instance, in his address to the Polanyi Society meeting in 1975, repeatedly committed this mistake. In some unfairness to Torrance, I offer only a single instance of this:

> This [universal intent] means, on the one hand, that the scientist conducts his inquiries in acknowledgement of the universal jurisdiction of reality over him so that his contact with reality necessarily legislates for him how he must think and speak about it, but, on the other hand, it means that the conceptions that he forms and the statements he formulates under the authority of reality he must affirm with a claim for universal recognition from all others . . . (Torrance:26).

R.J. Brownhill effectively interprets Polanyi such that "truth" tends to be equated with the sociological consensus associated with Kuhn (Brownhill:369). When he comes to interpret how new theories are accepted which alter that sociological consensus, he effectively interprets Polanyi to be saying that a "new interpretative framework" recommends itself in so far as "it is instrumental in revealing reality for it leads to new problems and their eventual solution" (Brownhill:370). This notion of "contact with reality" effectively clothes Polanyi's post-critical philosophy in clothes which are better worn by Ernest Nagel, Gilbert Ryle, and Stephen Toulmin.

John Brennan interprets Polanyi with great sensitivity and insight. Yet, even he tends to link "contact with reality" closely with prophetic confirmations and to overdraw the contrast between reality and illusion. In unfairness to his whole argument, I present only a telling conclusion:

> That Copernicus had got hold of an aspect of reality, whereas the Ptolemaic vision was illusory, is evident to us now . . . . [How so?] Ptolemy's system provoked no substantial problems, let alone any important discoveries. Copernicus made contact with reality because the relations which he claimed to have discovered were real relations, as evinced by the indefinite (still unexhausted) number of consequences which have revealed to those who have investigated them (Brennan:149).

Brennan rightly asserts that "Polanyi's theory is, neither in intention nor in fact, a 'fruitfulness' theory" (Brennan:150). Yet, it is difficult to discern, on the basis of Brennan's interpretation of Polanyi, how Copernicus was to accredit his discovery as having made "contact with reality" at the time of his discovery, i.e., before the prophetic consequences of his system had been revealed and tested. Did not the Ptolemaic system also originate out of a passionate quest which, at the moment of its first appearance, was likewise crowned with "a tacit foreknowledge of yet undisclosed things" (TD:23)? If so, it would seem specious to dub one insight as an "illusion" and, in contrast, to dub the Johnny-come-lately as "reality."

POLANYI'S NOTION OF INDETERMINACIES

Perhaps the treatment of Harry Prosch in Michael Polanyi: A Critical Exposition is most honest and satisfactory. Prosch effectively notes that Polanyi offered many coefficients of scientific value and pressed home many insightful critiques of experimental testing; yet, in the end, one is left with only a handful of indeterminacies which plague the very process of verification. The second and fifth

indeterminacies are especially telling:

> (2) The rules for deciding whether a discernable pattern in nature is due to chance or to reality can never be rendered determinate. The decision is made by an act of personal judgment . . . . (5) A fifth indeterminacy is entailed by "the existential choices involved in modifying the grounds of scientific judgment" (Prosch:116f).

Polanyi would seemingly favor Prosch's interpretation. In his 1967 article, "Science and Reality," in which he probes the Copernican discovery in great depth, Polanyi explicitly asks: "But was Copernicus himself, when expressing his belief in the reality of his system, in fact asserting that it had anticipatory powers, which the Ptolemaic system had not?" (Polanyi, 1967:190). This is just the key issue that I suggested above in response to Brennan. Polanyi's response:

> It is not clear how anticipatory powers can be known at all, apart from relying upon them as clues to inquiry. Copernicus obviously did not know that his system represented an aspect of Kepler's laws and of Newton's theory of general gravitation; indeed, being wedded to an explanation of the planetary system in terms of steady circular motions, he would have strictly rejected Kepler's laws and Newton's theory based on these laws (Polanyi, 1967:190).

In the end, therefore, Polanyi's appeal to "contact with reality" appears to have vanished. Ptolemy's Almagest was accepted by 11th century Europeans (after it was discovered among Arab scholars) on the grounds that it opened up a "fruitful solution" to the procession of the equinoxes and the retrograde motions of the planets." A good theory is objective because of its intrinsic rationality; this means that it claims to uncover a rational structure in nature -- to have made contact with reality" (Brennan:149). If this is the case, Ptolemy's system was surely "objective" and could claim to have "made contact with reality." At one point, Polanyi grants this, but then turns around to say that "it becomes legitimate to regard the Copernican system as more objective" (PK:4).

On what grounds can "more objective" be claimed? Is it that Copernicus "relies to a greater measure on theory" (PK:4)? Hardly. Is it that Copernicus demonstrated larger "prophetic powers" (PK:5)? Yes. But isn't it entirely specious to project "intimations of an indeterminate range" upon the winners and to withdraw such claims for the losers? Chesterton said to his contemporaries, "Christianity has not been tried and found wanting; it has never really been tried." Anyone could have made a similar claim for Ptolemy: had it been tried more earnestly, its true worth would have overwhelmed all contenders!

In some ways, Kuhn is more consistent than Polanyi. He would allow that scientists habitually make reference to Copernicus as having made some "fresh contact with reality" or as being "closer" to the truth; yet, in point of fact, every affirmation of "truth" or "reality" is bound up with the tacit theoretical commitments informing the knowing powers of the scientist. In Kuhn's own words: "There is, I think, no

theory-independent way to reconstruct phrases like 'really there'" (Kuhn, 1970:206). In the case of the Copernican revolution, the paradigm of the winner is habitually used as the criterion for judging the losers; hence, Ptolemy will always be instinctively judged as wanting. But had it been otherwise . . . .

BRITISH EMPIRICISM AND POLANYI'S LEGITIMATION OF PROJECTED MEANING

The empirical school of British philosophers took great delight in undermining the reliability of the senses. They did this under the mistaken conviction that science had disclosed the actual nature of reality. According to this norm, the senses all suffer the terrible inadequacy of projecting bodily sensations onto things to which they do not properly apply. The vinegar is not "sour"; the acidic interaction on the surface of the tongue simply registers this "sour sensation." The bottom of the well is not "black"; the absence of reflected light makes any object appear black.

Polanyi allows that all bodily perceptions are projections of interior states but, at the same time, he insists that such projections are spontaneous, necessary and appropriate. Heuristically, such projections are sense-giving and productively guide human interaction within one's environment. Working scientists who project the meanings which they discover while indwelling in their paradigms are similarly functioning spontaneously, necessarily and appropriately. Due to the "semantic aspect" of embodied knowing, the integrated meaning of the clues which originates within the organism appears "out there," i.e., at the focus of one's attention where the clues are originating. Hence, every mental integration, like its perceptual counterpart, discloses what appears to be really "there at hand" quite independent of my knowing of it.

Already in Personal Knowledge, Polanyi had devised the rule that all knowing relies upon the organismic integration of particulars into self-satisfying wholes. Only at the time of preparing The Tacit Dimension, however, did Polanyi fully explicate the repercussions of bodily indwelling. For our purposes here, the "semantic aspect" of this process is most important:

> To see more clearly the separation of a meaning [as the integration of bodily clues] from that which has this meaning, we may take the example of the use of a probe to explore a cavern, or the way a blind man feels his way by tapping with a stick . . . . Anyone using a probe for the first time will feel its impact against his fingers and palm. But as we learn to use a probe, or to use a stick for feeling our way, our awareness of its impact upon our hand is transformed into a sense of its point touching the objects we are exploring. This is how an interpretative effort transposes meaningless feelings into meaningful ones, and places them at some distance from the original feeling . . . (TD:12f).

Polanyi chooses the term "semantic aspect" because he rightly notes that anyone who hears or reads a message rightfully projects the meaning arrived at interiorly onto the source of the clues. The

same thing happens when "sourness" is projected as being in the vinegar or when, through the use of the Copernican paradigm, "movement" was mentally projected onto the earth (even when, sensually and experimentally, for more than three centuries, no such movement was registered).

Bodily indwelling functions analogously to mental indwelling. By virtue of indwelling in our bodies, the clues provided by sensory interaction are integrated and enhanced in order to provide meaningful human perceptions which enable us to function in our environment (e.g., making a cup of coffee or soldering a resistor into an electronic circuit). By virtue of mentally indwelling in our scientific paradigms, the clues provided by our experimental observations are integrated and enhanced in order to provide meaningful scientific understanding which enable us to function in our specialized environment as working scientists (e.g., isolating a malfunction in an electronic circuit or designing an experiment to confirm an expected pattern of proton-proton interaction).

Upon reflection, however, mental indwelling has features which distinguish it from sensory perception. Sensory perception is a first-order integration. Theoretical understanding is a second-order integration which begins with a particular set of observational data obtained through first-order integrations. Such sensory perceptions may be gathered by the unassisted use of the senses or, what is more usually the case, observational data is gathered by scientific instrumentation which extends both the precision and the range of naked bodily perceptions.

THE KINSHIP BETWEEN A SCIENTIST USING A THEORY AND A MOTORIST USING A MAP

When theoretical understanding is recognized as a second-order integration, then a scientist using a theory becomes akin to a motorist using a map. Accordingly, by examining the more familiar case, one can come to understand important elements respecting the reality claims and testing which are appropriate for theories. Typical insights would be as follows:

(a) Maps, like theories, can only be properly understood when they are in use. When a skilled map-reader consults an appropriate map in order to navigate his car to a given address in an unfamiliar part of town, this is akin to a trained scientist making use of specialized theories for the adjustment of an electronic circuit or the design of an experimental apparatus. Both maps and theories are oriented toward human action and, as a result, the correspondence between theoretical anticipations and practical consequences serves to accredit both the map and the map-user at the same time. This is why only trained scientists can properly use and test theories which are designed by other scientists.

(b) Maps, like theories, cannot be directly evaluated on the basis of some "correspondence with or approximation of reality." In the first place, maps are designed by human conventions and rules of logic which are distinctively different from the chemical and engineering principles governing road construction. In the second place, a focal inspection of the "reality" of the map (i.e., lines on a sheet of

paper) is distinctly different from a focal inspection of the "reality" of the road (i.e., a band of asphalt with scattered potholes). Finally, when maps are in use, the lines on the map function subsidiarily as clues which enter into the focal intent (namely, to get to the address in question).

(c) Maps, like theories, all have a limited scope of application. Each serves to integrate certain clues while leaving the driver entirely blind to others. Thus a truck driver hauling an extra-wide or an extra-high load needs a specialized map designed for his specialized need. In parallel fashion, theories of chemical valence are very helpful in predetermining what substances might combine and in what proportions; yet, such theories are quite blind to melting points or to neutron scattering or to such an everyday phenomenon as color. As a result, a scientist has to cultivate the skill of rightly selecting the "map" which fits the phenomenon under investigation.

Map-reading is different from theory-using in a decisively important aspect. Map-readers can always dispense with their maps and drive their cars somewhat successfully using visible landmarks to guide their conduct. Theory-users can never do this. All "observing" in science is "theoretically informed observing." In consequence, every time a technician takes a voltmeter and begins to use it to trouble-shoot an electronic circuit which is malfunctioning, he cannot even begin to meaningfully connect his meter to the circuit unless he has first gained an theoretically informed perception of how the maze of parts before him provides a nexus of functionally interrelated electronic pathways. In this regard, Einstein is quite correct: "Whether you can observe a thing or not depends on the theory which you use. It is the theory which decides what can be observed."

Maps also differ from theories relative to the degree to which they exhibit a "surplus of meaning" (Brennan:151) or "prophetic powers" (PK:5). Maps have a well-defined capacity to lead to exploration and discovery. As an example, consider how a map-reader can easily determine a novel alternative route to his or her destination when a known route is blocked by fire trucks. Theories, in contrast, exhibit a more complex relation to a deeper reality and, as a consequence, their capacity to lead to exploration and discovery is generally much more complex and much more rewarding. Consider, for example, how astronomers were aware of perturbations (i.e., periodic straying) in the orbits of the outer planets for sixty years before Leverrier finally recognized that these minor discrepancies from the predicted Newtonian orbits could be used to predict theoretically the size and orbit of "a hitherto unsighted planet." Using the calculations of Leverrier, Galle in 1846 successfully sighted and tracked the new planet, which came to be called "Neptune." Prior to this, however, "Neptune" had been sighted by numerous observatories, but it had been mistakenly classified as a star. Galle was able to recognize "Neptune" for what it really was only because the prophetic explorations of Leverrier had altered his perceptions of what it was that he was looking for (Polanyi, 1926:8). In the end, therefore, it becomes apparent that map-reading can serve only to approximate the processes involving the use of theories by scientists.

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NEW COMMITMENTS ARE EMBRACED DUE TO THEIR COMPARATIVELY SUPERIOR SATISFACTION

When Copernicus first accredited his solution, it must be granted that this was because the intellectual satisfaction that it afforded far outweighed the satisfaction afforded by his former commitment to the Ptolemaic system. Polanyi asserted this initially in Personal Knowledge, but, in his later writings, he favored such troublesome concepts as "tacit knowledge of yet undiscovered things" (TD:23), "universal intent" (TD:78), and "gradient of understanding" (TD:81) -- notions which I have now come to regard as obscuring what he ought to have developed by way of affirming the comparative intellectual satisfaction which the pioneer uses to accredit his novel insights.

When Copernicus published his theory of the universe, he fully expected that those who shared his "crisis" situation would find the same superior intellectual satisfaction within his system that he himself had found.He had to acknowledge, however, that his system would be considered as "absurd . . . by those who know that the opinion that the Earth rests immovable in the middle of the heavens . . . had been confirmed by the judgment of many ages" (De Revolutionibus, preface). Meanwhile, of course, he believed that those who wanted a superior system for relating not only the position of the planets but for accounting for their distance as well would welcome his proposal in the same way he welcomed it. True, the absence of perceived motion in the earth and the absence of any detected stellar parallax did pose difficulties; yet, the increased intellectual satisfaction served to override these anomalies in favor of, among other things, showing the ordering of the planets and being able to determine their distance from the earth even before there was any precise observational method for making such determinations. In brief, the new theory had clear advantages in terms of its actual or potential scope of application. It would take time and patience to actually realize this advantage; yet, Polanyi is quite correct in noting that, without commitment, no one would even know how and where and why to look for such applications. Commitment, therefore, is prior to usefulness, and commitment is born out of the superior intellectual satisfaction which greets a promising discovery. The increased intellectual satisfaction which a new discovery affords an investigator serves to transfer the interest and commitment from the old to the new system. The new paradigm appears, in the eyes of the pioneer and his colleagues, to hold more promise for unlocking yet other mysteries of what is "there at hand" independent of the knower. This prophetic promise may come to pass or, alternately, the whole new system may prove to be an illusory projection. The human dilemma is that only by yielding to a new commitment, by habitually dwelling within it, can an investigator establish its true worth.

> The anticipation of discovery, like discovery itself, may turn out to be a delusion. But it is futile to seek for strictly impersonal criteria of its validity . . . (TD:25).

CONCLUSION

Epistemologically speaking, one can erode confidence in our senses in the way that the British empiricists did during the last century. In the same way, one can erode trust in theories on the ground that, after all, they are humanly designed instruments which allow us to project the meaning of clues upon an extramental world which can be regarded as quite indifferent to such projected meanings. Nonetheless, despite the anthropomorphic content which is present in every enjoyment of a flower and in every investigation of the planetary orbits, the truth remains that sensory and intellectual perceptions routinely must rely upon the bodily enhancement of clues, the bodily integration of these clues, and the projecting of the consequent meaning-for-us into the locus where the clues originated. As long as we continue to be embodied spirits, we cannot know things as they are for themselves. We know all things as they are for us -- bound up within tacit skills that are historically, culturally, and organicly conditioned. As such, Polanyi and Kuhn are effective guides in allowing us to both accept the heuristic circularity of all knowing at the same time that the phenomenology of discovery is offered as the route to change and, within the human condition, to improve our knowledge. Improved knowledge enhances our human powers to be and to do. But here again, what is worth being and worth doing changes both for individuals and for societies.

This brings me back to my opening comments. Polanyi welcomed Kuhn's proposals made in 1961 at the University of Oxford and suggested "that if I join forces with Mr. Kuhn, we may both do better." My conviction is that Polanyi spoke with prophetic insight on that occasion. What he did not bring to pass, it remains for his disciples to pursue. I welcome those who would join me in this promising enterprise.

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[Note on p.3] For Polanyi, all knowing has a "tacit component" whereby otherwise meaningless sense impressions are organismically integrated into meaningful wholes. For example, in the case suggested above by Kuhn, a layman with some familiarity with electronics might identify the meter as "some sort of electrical measuring device," while a child, seeing such a meter for the first time, might well imagine that it is a circular jewelry case somewhat like his mother's. Even among scientists, one scientist might recognize "an operating ammeter" but, without a schematic diagram and direct inspection of where the meter is connected, not be able to determine that the meter is monitoring the emitter current for transistor "T4" in a given audio amplifier. In effect, the given "tacit skills" of each particular observer, whether layman or scientist, allows each to make a particular set of meaningful interpretations to which the sensory clues point.

Furthermore, while tacit powers operate habitually and routinely, they are not "involuntary." An electronic engineer with the requisite skills might take a full hour before interpreting the functional role of the ammeter in the given circuit. This task would require attention and deliberate effort. Meanwhile, he might, upon the arrival of his daughter, take time out to imagine that the ammeter is "a special kind of jewelry box." This playful empathy requires skills which not all fathers possess. It would be out of the question, meanwhile, for his daughter to understand "the true purpose" of the ammeter in terms of the theoretically enhanced experiences which her father possesses.

Sensory recognition is "involuntary" to the degree that one cannot elect to see with a different body. Theory-informed recognitions, however, are voluntary in so far as someone with the requisite training can deliberately switch back and forth between paradigms while analyzing a given phenomenon. This observation will have importance later when considering how scientists can entertain a novel theoretical analysis even before they are committed to it.