Public Recognition, Vanity, and the Quest for Truth:
Reflection on "Polanyi vs. Kuhn"

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ABSTRACT Key Words: Thomas Kuhn, Robert Merton, Freud, James Conant, J. B. Rhine, Hilary Putnam, recognition, vanity, unconscious borrowing, unspecifiability, unaccountable element, ESP, paradigm shifts, sociology of knowledge, anticipated fruitfulness, phenomenology of discovery, scientific hierarchy, embodied knowing

After commending Moleski for his excellent study, I focus attention on three areas that merit further clarification: (a) that Polanyi's quest for public recognition was legitimate and not the effect of a runaway vanity, (b) that Kuhn's straining to define his dependence upon Polanyi was blocked by the unspecifiability clouding the discovery process and by his (mistaken) notion that Polanyi appealed to ESP to explain the dynamics of discovery, and (c) that Kuhn's success in gaining public recognition for his paradigm shift is understandable (just as is Polanyi's relative failure). In the end, I list five areas wherein Kuhn's account of scientific revolutions could be substantially improved by joining forces with Polanyi.

To begin, I want to give my great applause to Martin X. Moleski for having so carefully uncovered the threads of evidence regarding the question of dependence of Thomas S. Kuhn upon Michael Polanyi. Moleski taught me many things that I did not know, and he did a superior job of producing a tapestry that incorporated those things that I did know. Hence, he is to be highly commended for his painstaking sleuthing.

Moleski's study repeatedly exposes Polanyi's impulse to receive due recognition for his published ideas. Moleski also raises the question as to whether Kuhn is more dependent upon Polanyi than he was ever willing to acknowledge. My own reflections on these issues will be grouped under three rubrics: (a) Polanyi's quest for recognition; (b) whether Kuhn depended upon Polanyi; and (c)Polanyi's interesting hypothetical conjecture, picked up by Moleski, "If I join forces with Mr. Kuhn . . . ."

Polanyi's Quest for Recognition

Society is ever intent upon giving its applause and its awards to originators and, at the same time, to pour its scorn upon plagiarists. Having said this, one can also acknowledge that it must have been a keen disappointment to Polanyi (and to many of Polanyi enthusiasts, myself included) that his thought was largely overlooked and politely bypassed by the educated elite in general and by the philosophers of science in particular. His letter of February 15, 1967, to Poteat 9cited in Moleski, 13) is especially lucid in this regard. Polanyi expresses his hesitancy to accuse Kuhn of unattributed borrowing because he himself is unsure on this matter. While not overtly expressed, there can be little doubt that Polanyi was aware of the potential harm that could fall upon him if he publicly accused Kuhn. He could easily be judged as the spoilsport bent upon robbing Kuhn of his justly deserved recognition, or worse, as the false incriminator driven by his runaway vanity.

Moleski never asks whether the quest for recognition was a moral failing in Polanyi (and even in his disciples as well). One must ask whether Polanyi's quest for recognition conceals a dangerous "vanity" that gets fixated on social status rather than on the advancement of knowledge within one's profession. In principle, the advancement of knowledge and public recognition are entirely compatible. Polanyi himself notes that the eureka-experience that crowns years of hard work on a perplexing problem has the effect of encouraging the pioneer to publish his/her results and to gain due recognition for having enriched others by his/her endeavors (*PK*, 142-145). What may appear, upon first sight, as "vanity" or as the "unbridled hunger for recognition," consequently, might be merely the outer expression of "the inner need for assurance that one's work really matters" (Merton, 270):

> The need to have accomplishment recognized, which for the scientist means that his knowing peers judge his work worthwhile, is the result of deep devotion to the advancement of knowledge as an ultimate value. Rather than necessarily being at odds with dedication to science, the concern with recognition is ordinarily a direct expression of it (Merton, 270).

Hilary Putnam (b. 1926) is a well-known and well-published American philosopher and mathematician. He was enamored by logical positivism during his doctoral studies, yet he later went on to advocate various forms of modified realism and social activism. In his analysis of Kuhn, Putnam wrotre in 1974: "I believe that I anticipated this view about ten years ago when I argued that some scientific theories cannot be overthrown by experiments and observations *alone*, but only by alternative theories" (Putnam, 69). In making this claim, Putnam did not try to establish his priority nor did he infer that Kuhn was influenced by his earlier work. Nonetheless, Putnam exhibits the normal disappointment that a creative individual feels when he perceives that some of his "unpopular" ideas of ten years ago have made it big time due to the enterprising efforts of a comparatively young and inexperienced philosopher. If one reads between the lines of Moleski's study, one finds that Polanyi, like Putnam, must have experienced Kuhn's easy success in gaining public recognition for many "unpopular" ideas that they held in common as a bitter pill to swallow.

Whether Kuhn Depended upon Polanyi

What can we say about Kuhn's seeming inability to recall and to publicly acknowledge the depth of his dependence upon Polanyi? Might this be an instance of Kuhn wanting to enlarge his own claim to originality while defeating the suspicion that he may have borrowed (and disguised) ideas gained from Polanyi? Possibly. The danger here, however, is to imagine that Kuhn himself might have some absolute clarity on this issue and that he intentionally disguised his dependence.

To begin with, recall that Polanyi put forward the recognition that a Gestalt perception allows only a subsidiary awareness of the particulars. Polanyi enlarges upon this same model for helping understand how the process toward the emergence of a pioneering discovery takes place as a string of successive hunches that have been guided, all along the way, by dwelling in the problem while the manifold clues were held in subsidiary awareness. As a result, the discovery that crowned the straining toward a resolution, comes as a sudden Gestalt (or a series of Gestalten). Even upon reflection, a discoverer cannot exhaustively explain (even to him/herself) why the route chosen seemed more plausible than all the others and how the eureka-experience showed up (if and) when it did. This is what Polanyi refers to as "the problem of unspecifiability" (*PK*, 62; "unaccountable element" 1962, 1; "indeterminacies" 1968, 27-30).

More to the point, "If a set of particulars which have subsided into our subsidiary awareness lapses altogether from our consciousness, we may end up by forgetting about them altogether and may lose sight of them beyond recall" (*PK*, 62).

Applying this to Kuhn, he could, of course, deliberately lie about his borrowing from Polanyi. But, setting this prospect aside for the moment, we must allow that even when Kuhn strains to replay his route to discovery, he cannot entirely reconstruct in detail the lines of influence. Thus, in reading his response, we hear him acknowledge that he read Polanyi but that he was not forcefully impressed by him—thus, giving him the impression that he did not and would not borrow from him. Is this authentic remembering? Probably. Is it at the same time inauthentic since it covers up unconscious borrowing? Possibly.

The history of science knows of many instances of "unconscious borrowing" (Merton, 272-278). Scientists sometimes even make discoveries that "seemed to come to them out of the blue [but] had actually been formulated by them years before [and later found in a journal, a lecture, a conversation with a colleague] and then forgotten" (Merton, 276). Sigmund Freud, the master of the unconscious, puzzled over the fact that, as a young boy, he had read Ludwig Börne's study of "free association" as an active agent in creative writing. Freud acknowledged that at the time he was pioneering his use of "free association" in psychotherapy, he still had the volume of Börne on his bookshelf, yet "he could not remember the essay in question" (cited in Merton, 275). This inability to remember, however, can sometimes be a clue to a psychological block; hence, it does not preclude the possibility that Freud did make use of Börne's "free association" in his own pioneering therapy. Upon reflection, Freud generalized his position as follows:

> A scientific worker may sometimes ask himself what was the source of the ideas peculiar to himself which he has applied to his material. As regards some of them, he will discover without much reflection the hints from which they were derived, the statements made by other people which he picked out and modified and whose implications he has elaborated. But as regards others of his ideas, he can make no such acknowledgments; he can only suppose that these thoughts and line of approach were generated—he cannot tell how—in his own mental activity, and it is on them that he bases his claim to originality (Freud, cited in Merton, 275).

Within this context, it makes perfectly good sense for Kuhn to acknowledge some dependence yet to be unable to precisely define it since it was so inconsequential or incongruous with his main lines of thought. In the free-flowing interview in 1994, Kuhn thus recollects the following:

> We did read some Polanyi in the Conant course. Conant introduced him to the course, and I liked it quite a lot—I don't remember just what it was, except that I kept feeling terrible at those points where he spoke as though extrasensory perception[1] was the source of what scientists did. I didn't believe that. That ... gets into the tacit knowledge thing also. I don't know. But Polanyi was certainly an influence. I don't think a great big one, but it was helpful to me to have him out there.[2]

This does not appear to me to be a cover-up. Moleski is suspicious; yet, given the lapse of time and given the uncertainty still felt around the topic, I would judge that there is no falsehood here. In fact, I could safely apply

Polanyi's own words from an earlier inquiry, "I am sure that Kuhn was acting in good faith and might himself be anxious to clarify this matter" (cited in Moleski, 14).

Robert K. Merton did an extensive study of two hundred sixty four instances of "multiples"—i.e., cases where scientists working independently made similar discoveries in complete or relative ignorance of the work of the other. Since the sociology of science gives the greater recognition to the originator, Merton notes that little attention has been given to the study of multiples. For our use here, it seems apparent that "multiples" testify to the understandable high incidence that independent minds tackling the same perplexing problem within the shared intellectual environment might indeed arrive at similar solutions. Polanyi, himself, drew attention to this phenomenon:

> Two scientists faced with a similar set of facts will often hit on the same problem and discover the same solution to it. Coincident or nearly coincident discoveries by independent investigators are quite common. . . (*SFS*, 35).

If one gives due weight to multiples, then the act of discovery might be seen as an emergent possibility that resides within the group of like-minded individuals puzzling over the same constellation of problems. When one lets go of the notion that the act of discovery is always the incomparable "gift of genius," it might then be possible to consider multiples as the inevitable phenomenon that is bound to emerge among professional practitioners over the course of time. Multiples, moreover, indicate how the disposition to accept a novel discovery is already tacitly disseminated within the community poised to receive it. It also indicates how discoveries can be made and published without receiving any suitable notice; later, when a more renowned investigator arrives at the selfsame discovery, then and only then does public acceptance and recognition arrive for the one who first published.

Multiples invite quite a different perspective on the Kuhn-Polanyi affair. In fact, the prevalence of multiples encourages us to see Polanyi and Kuhn as arriving at many parallel lines in resolving the problem posed by the emergence of "scientific revolutions." Granted there are significant differences, Polanyi gave greater weight to the phenomenology of discovery; Kuhn gave greater weight to the sociology of recognition. Kuhn undoubtedly learned something from Polanyi but was entirely put off by what seemed to him, his reliance on "something very like ESP"[3] in scientific discovery. Hence, Kuhn was unsympathetic to the overall thesis of Polanyi, never read *Personal Knowledge* beyond what was required by his course work, and, accordingly, was authentically unable to recognize Polanyi as making a seminal contribution to his work in *The Structure of Scientific Revolutions*. On the other hand, Kuhn amply recognizes James B. Conant as having given him his start in the right direction.[4] Kuhn, therefore, is not opposed to showing gratitude where it is due.

"If I join forces with Mr. Kuhn . . . ."

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 that 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:

> 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 sustained traditions of personal knowledge and personal performance skills that underwent changes during the course of history. In all of this, Kuhn made only a marginal use of Polanyi.

Kuhn gained acceptance in wide circles in part because he harmonized with the prevailing intellectual climate of the 60s. The fields of psychology (especially behaviorism), sociology (especially the sociology of knowledge), and the philosophy of science (especially logical positivism) had been taken over by the distrust of explaining matters by appeals to interior states.[5] This, in my mind, is a key reason why Kuhn was received so easily while Polanyi was not. Kuhn, it must be remembered, remarked on multiple occasions that Polanyi made appeals to ESP when it came to explaining how discoveries were made and accredited. ESP is a polite word for "internal states of mind" that cannot be examined or verified and thus have no bearing upon the social arbitration as to whether a novel scientific paradigm was to be accepted or rejected.[6]

Here is precisely where Kuhn needs Polanyi. In the crudest terms, the sociology of knowledge of the 60s put forward a world in which primary and secondary socialization were used to explain how "common sense" was culturally determined and how the "scientific consensus" was maintained. In such an atmosphere, the social world of science was imagined to be populated by "symbolic universes" (theories) that were promoted and maintained by "conceptual machineries" (the scientific hierarchy):

> It is important to stress that the conceptual machineries of universe-maintenance are themselves products of social activity, as are all forms of legitimation. . . . Specifically, the success of particular conceptual machineries is related to the power possessed by those who operate them. The confrontation of alternative symbolic universes [Copernicus vs. Ptolemy] implies a problem of power—which of the conflicting definitions of reality will be "made to stick" in the society. . . . Which of the two will win, however, will depend more on the power than on the theoretical ingenuity of the respective legitimators. . . . He who has the bigger stick has the better chance of imposing his definitions of reality (Berger and Luckmann, 109).

Faced with such a "social construction of reality" (the title of the book), Polanyi and Kuhn were both rebels. Polanyi believed that the apprenticeships undertaken by aspiring physicists did serve to conform them into community-accredited forms of thinking and habits of judging; yet, the active scientist remains free to challenge, correct, and reform parts of the system. Kuhn, aware of his critics, tried to argue that proponents of a new paradigm argue for their altered system on the basis of its anticipated fruitfulness "to solve problems presented by nature" (Kuhn, 1970, 205) and as "a prelude to the possibility of proof" (Kuhn, 1970, 199). Moleski is quite helpful in noting that Polanyi had great difficulty with Kuhn on this very point and intimated that his resolution was "just plain nonsense" (Moleski, 16).

My own dissatisfaction with Kuhn comes directly from my ability to see (a) how Polanyi provides a greater depth of understanding to what they hold in common and (b) how Polanyi resolves issues that Kuhn cannot overcome within his system. In my earlier work (Milavec, 1993), I specified five areas of difficulty. I would summarize and revise them now as follows,

1. Kuhn's system forces him to project a crisis[7] at the root of every paradigm change as the stimulus for searching for alternatives. Polanyi would offer Kuhn his own critique of the heuristic value of doubting (*PK*, 269ff) which might allow Kuhn to speak more accurately as finding "intellectual dissatisfaction" rather than a "crisis" at the origin of inquiries into alternative paradigms.

2. Kuhn offers no explanation as to how scientists caught up in a crisis can initiate an explorative quest that can turn up attractive alternatives without being sent off into a maze of dead ends. At one point, Kuhn suggests something akin to a rudimentary phenomenology of discovery (Kuhn, 1970, 89f, 122f); yet, on the whole, Kuhn was burnt by accusations of using mystical mumbo-jumbo and, accordingly, he preferred the Darwinian model of random mutations and survival of the fittest.[8] Polanyi provides a sophisticated phenomenology of discovery that functions to insure that, even though the creative process cannot be exhaustively delineated, at least it can be perceived as having an internal direction from beginning to end (*PK*, 142-159; TD, 3-33; Polanyi, 1967).

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 also 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 would bring to Kuhn a much greater depth of analysis here.

4. Once a pioneering discoverer does come forward with his novel paradigm, it appears to me that Kuhn fails to provide any credible grounds whereby any other scientist might want to join him. Kuhn rightly notes that "if a paradigm is ever to triumph it must gain some first supporters, men who will develop it to the point where hardheaded arguments can be produced and multiplied" (Kuhn 1970, 158). Careful thinkers such as Imre Lakatos accuse Kuhn of suggesting that the social structure of paradigm changes reduces the quest for truth to "a band wagon effect" (Lakatos, 181; cf. response of Kuhn, 1970, 259-266). Polanyi would bring to Kuhn a much more sophisticated understanding of the intellectual gap that divides the adherents of conflicting paradigms and help define the grounds whereby adherents might embrace a new paradigm even prior to any experimental verification.

5. Kuhn remains entirely skeptical that a winning paradigm can ever be equated with "a better presentation of what nature is really like" (Kuhn, 1970, 206) or that "successive theories . . . approximate more and more closely to the truth" (Kuhn, 1970, 206). In brief, humans can never get outside of their bodies in order to compare "reality" as such with their "ideas" of reality. "Truth" and

"reality" are thus endangered species since, according to the sociology of knowledge, these are merely the linguistic tokens for sanctifying the reigning theories in the scientific hierarchy. But this is precisely where Polanyi begins—by taking bodily indwelling as the natural and the correct position for all knowing.[9] Based upon this, Polanyi examines the use of scientific theories as an extension of the indwelling within embodied knowing. Polanyi provides a foundation for a provisional realism that is the *sine qua non* for successful scientific research and a healing antidote to the British empiricism that has eroded our ability to accredit our bodily sensations.[10]

All in all, Polanyi had a decisive edge over Kuhn. Polanyi was a productive physical chemist and had nearly two hundred research papers to his name. Kuhn had none.[11] Kuhn received a doctoral degree in physics from Harvard in 1949, but near the end of his studies he immersed himself in the history of science and, for eight years (1948-1956), taught a revolutionary undergraduate program designed by James Bryant Conant, president of Harvard (1933-1953). His "General Education in Science" program explicitly aimed to give future policy makers a more interesting and more accurate social understanding of science by immersing thems in historical case studies wherein scientists disagreed over matters of scientific theory. When Kuhn was denied tenure at Harvard in 1955, he set himself to publishing the very thesis that he had been formulating while teaching. *The Copernican Revolution* came out in 1957 and *The Structure of Scientific Revolutions* appeared in 1962. In the preface, Kuhn speaks frankly about the importance of his experiences at Harvard:

> A fortunate involvement with an experimental college course treating physical science for the non-scientist provided my first exposure to the history of science. To my complete surprise, that exposure to out-of-date scientific theory and practice radically undermined some of my most basic conceptions about the nature of science and the reasons for its special success (1970, v).

Then, while still in his 40s, popularity overwhelmed him. Over a million copies of *The Structure of Scientific Revolutions* were sold in sixteen different languages. His book moved the popular imagination from seeing scientists as drones bent upon accumulating more precise data and more accurate theories to intelligent human beings locked in passionate conflict with each other over things that mattered deeply to them. Kuhn was a dynamic lecturer, and he had a compelling story to tell—one that went right back to his own conversion story that began when he taught Conant's (now defunct) experimental courses in the history of science.

Conclusion

At the end of his essay, Moleski remarks that "all that is good in Kuhn's position is found in Polanyi" (Moleski, 21). This is not a statement to be refuted since it is properly an act of admiration uttered by a devoted disciple. Can we not expect that a true disciple always understands and appreciates his master better than his rivals and, as a consequence, pushes for his excellence despite his comparative obscurity? Surely! So I will not subject Moleski's affirmation of admiration to a close scrutiny.

On the other hand, I do not share Moleski's suspicion that Kuhn was more dependent upon Polanyi than he was willing to acknowledge. My discussion above has endeavored to show that the issue of dependence does not have the clarity that Moleski seems to imply. The critical point for me, however, arrived when I read in Moleski's study how Kuhn, in the very act of straining to recall his dependence upon Polanyi, repeatedly emerged with his aversion to Polanyi's endorsement of ESP. Even in the free-flowing interview a year before his death in 1996, Kuhn was still (wrongly, of course) associating Polanyi's account of the discovery process with the bogus claims of "extrasensory" powers. In part he said, "I kept feeling terrible at those points where he [Polanyi] spoke as though extrasensory perception was the source of what scientists did." Then, a moment later, Kuhn admitted, "When I did try to read *Personal Knowledge*, I discovered that I didn't like it." Thus, in the end, Moleski's excellent study inclines me to register how Kuhn's aversion to Polanyi blocked him from relying upon Polanyi. Similarly, even unconscious borrowing appears only as a remote possibility.

In the final section of my paper, I summarized the soft spots of Kuhn's exposition when contrasted with that of Polanyi. In fairness to Kuhn, however, the relative simplicity of his "paradigm shifts" (coming out of Conant's case studies) and the acceptability of "paradigm shifts" by social constructionists (who would accuse Kuhn of not being a purist because he did occasionally refer to "interior states"[12]) was key to his success. Understanding this takes Moleski's investigation another step further for the disciples of Polanyi. How so? To begin with, there is no need to envy Kuhn's success, for the best of Polanyi was inaccessible to him. Conversely, the relative "failure" of Polanyi is intimately tied up with the rich sophistication of his thought and his persuasion that "interior states" (faith, tacit skills, intellectual passions, guiding intuitions, imagination) were decisive for making discoveries in science. It is no surprise, accordingly, why Kuhn (beyond a few footnotes) decided to distance himself from Polanyi. Likewise, it is no surprise that Polanyi cannot be explained in Kuhnian categories.

I cannot blame Kuhn for his limitations; I can only applaud him for how well he carried off a revolution in the popular imagination. Many of Kuhn's soft spots are due to his settling for simplistic generalizations. Yet, I cannot entirely fault him for this since it was the very simplicity of his themes that enabled them to be communicated effectively in an hour lecture. Most of all, not being a creative scientist himself, he could not be expected to be personally aware of many of the processes known tacitly by working scientists such as Polanyi. In the last line of his essay, Moleski faults Kuhn for having no taste for "purposes which bear upon eternity." For myself, however, I am inclined to say of him what my daughter learned almost as a refrain in her preschool—"He did the best that he could."

Endnotes

[1] Polanyi uncritically accepted the "extrasensory" and "psychokinetic" experiments that were pioneered by J.B. Rhine (d. 1980) in the 1930s. Unfortunately, Polanyi then proceeded to describe the workings of "scientific intuition" by linking it to the much more obscure and "extrasensory perception" (*SFS*, 35-38, 60; *PK*, 166). "ESP has yet to be demonstrated to the satisfaction of the scientific community and is often called a pseudoscience" (Sarah G. Stonefoot and Clyde Freeman Herreid, "Extrasensory Perception—Pseudoscience? *A Battle at the Edge of Science*," http, //www.sciencecases.org/esp/esp_notes.asp). Knowing this, it becomes understandable how Kuhn was repulsed by Polanyi's endorsement of ESP and, even a year before his death, he continued to associate Polanyi's account of the discovery process with the bogus claims of "extrasensory" powers.

In December of 1963, Polanyi was invited to write a new introduction for the University of Chicago Press reprint of *SFS*. In this introduction, Polanyi described how, in retrospect, he subsequently abandoned any hope of relying upon ESP to illuminate the discovery process despite his initial enthusiasm for this line of thinking,

> The testing hand, the straining eye, the ransacked brain, may all be thought to be laboring under the common spell of a potential discovery trying to emerge into actuality. I feel doubtful today about the role of extra-sensory perception in guiding this actualization. But my speculations on this possibility illustrate well the depth that I ascribe to this problem (*SFS*, 14).

James A. Hall narrates how, as a psychiatric intern at Duke University, he made it possible for Polanyi to present his ideas at a Grand Rounds meeting of the psychiatry department in 1964. Shortly thereafter, J.B. Rhine, whose institute of parapsychology was located in property adjoining Duke's east campus, invited Hall to accompany him to a public lecture given by Polanyi. Hall regarded Polanyi's notion of tacit powers as very favorable to Rhine's work and undoubtedly shared his insights with Rhine prior to the lecture itself. But then Hall reports that "instead of the appreciation I expected, Dr. Rhine had an immediate antipathy to Polanyi's thought for reasons that I still do not understand" (Hall, 16).

More recently , the magician, James Randi, has made it his mission to expose the hoax perpetrated by persons such as Uri Geller who claim to be able to perform remarkable feats due to their psi powers. Randi found that trained physicists examining Uri Geller while he bent spoons with only his powers of mind were easily hoodwinked into crediting his powers. When Randi later duplicated Geller's feats and then explained how he did it, the physicists quickly discovered how they had been artfully distracted from what they ought to have seen. The Amazing Randi has offered a one-million-dollar prize to anyone who can show, under proper observing conditions, evidence of any paranormal, supernatural, or occult power. To date, Randi has been able to see through every one of the would-be claimants for his prize. See James Randi, *The Truth About Uri Geller* (Buffalo, NY: Prometheus Books, 1982). See http //skepdic.com/geller.html for further details.

[2] Cited in Moleski, n. 41, p. 18

[3] This telling revelation comes from Moleski's paper. Kuhn is clearly influenced by the logical positivism and the sociology of knowledge of his day wherein interior states that defy public examination were categorically ruled out as having any bearing upon how scientists evaluated the merit of scientific discoveries. Kuhn, consequently, had strong reasons to distance himself from Polanyi, especially since scholars like Mark W. Wartoksky were inclined to lump together Kuhn and Polanyi as "emphasizing the subjective and irrational components in the contexts of scientific observation" (cited in Moleski, 15). See also Moleski's citation of Richard D. Whitley for a more pronounced instance of this confusion (pp. 16-17 and n. 39). While Kuhn included two footnotes in the expanded 1970 second edition of *The Structure of Scientific Revolutions* lauding Polanyi, it would appear that Kuhn might have regretted this since it seemingly gave more ammunition to his critics. This would help explain why Moleski found a distinct trend on the part of Kuhn to distance himself from Polanyi.

[4] I thank Moleski for bringing to my attention that the first edition of *The Stucture of Scientific Revolutions* was dedicated to "To James B. Conant, Who Started It." To this, I add that Kuhn invited Conant to write the preface to his first book, *The Copernican Revolution*. Conant willingly accepted. In both instances, Kuhn recognized his true mentor.

[5] See n. 3.

[6] It is interesting to note that Hilary Putnam, a philosopher who rigorously rejected the logical

positivism of his youth in favor of a modified realism, judged Kuhn as "radically subjectivistic" (Putnam, 70) when it came to explaining how a new paradigm gains acceptance over a prevailing paradigm.

[7] 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 evoking a crisis situation. 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).

[8] Moleski brings this out very nicely in his paper. Polanyi, of course, rebuffed the notion of making progress through the accumulation of blind mutations in both biological evolution and scientific development. Random mutations and survival of the fittest may have been satisfactory themes in the sociology of knowledge; yet, for Polanyi, they had no explanatory power whatsoever when it came to accounting for "emergence."

[9] 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 since they are causally related to a reality that is making its presence felt "out there." Heuristically, such projections are sense-giving and productively guide human interaction within one's environment. Working scientists who project the meanings that they discover while dwelling in their paradigms are similarly functioning spontaneously, necessarily and appropriately. Due to the "semantic aspect" of embodied knowing, the integrated meaning of the clues that originate within the organism appears "out there," i.e., at the focus of one's attention.

[10] The empirical school of British philosophers took great delight in undermining the reliability of the senses when it came to determining the actual nature of things. They did this under the mistaken conviction that science had disclosed the true (metaphysical) nature of reality that could be used to correct the mistaken judgments based upon our senses. According to this norm, the senses all suffered the terrible inadequacy of projecting bodily sensations (the so-called "secondary qualities") 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. This rock is not "heavy," its mass is being pulled toward the much larger mass of the earth.

[11] Moleski says, at one point, that "both Polanyi and Kuhn left their work in chemistry and physics to take up philosophy" (p. 21). This should not be taken to mean that Polanyi's mature scientific research over more than twenty years is somehow comparable to Kuhn's studies in theoretical physics without publishing a single research paper.

[12]Kuhn made social constructionists uneasy when he spoke of "the transfer of allegiance from paradigm to paradigm" as "a conversion experience" (1970, 151). Later, in explicating this, Kuhn states that "a decision of that kind can only be made on faith" – "faith that the new paradigm will succeed with the large problems that confront it, knowing only that the older paradigm has failed with a few" (1970, 158). This faith Kuhn calls "personal and inarticulate" (1970, 158). For a discussion of these points, see Gutting, 7-12, 18.

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