

PERSONAL KNOWLEDGE AND THE COGNITIVE UNCONSCIOUS

Introduction

A theme that runs throughout the assembled works of Michael Polanyi whether he was writing as a physical chemist, a philosopher with epistemological leanings, or a social commentator is that the very essence of *knowing* something, no matter the domain, is predicated on a personal knowledge, an epistemic foundation acquired from experience with the world about and reflective of that world in a deep way. Although this theme emerges and re-emerges in many contexts, I want to focus for a moment on one particular essay entitled "Knowing and Being" which was published in 1961 in *Mind*. In this essay several classical Polanyi-ish themes are displayed in a setting that will allow me to develop what I hope will prove to be an interesting perspective on his overall metaphysical programme.

In "Knowing and Being," an essay which many of you may be familiar with, Polanyi is exploring what we now regard as a staple of cognitive psychology, the problem of, to use one of his favorite phrases, "knowing more than we can tell." He reiterates his anti-positivist conviction that much of our functional knowledge base is ineluctably private and personal. For Polanyi, this epistemic store, which he referred to as *tacit knowledge*, represents a kind of memorial core that functions to frame action, mold behavior, and guide conscious thought but itself is refractory to invasion by consciousness. Tacit knowledge is knowledge that resists communication to oneself and to others; it is knowledge that finds its expression, not in behaviors that are under the executive operations of consciousness or the social communicative processes of language, but in behaviors that are guided by deeper, implicit domains of mental representation. Polanyi worked and re-worked this theme in a variety of contexts over several decades and ultimately came to have a considerable impact in areas such as the philosophy of mind, and the history and philosophy of science. Surprisingly, however, he had little impact on psychology even though, as I will show, his arguments entail a particular model of mind which many contemporary cognitive scientists feel quite comfortable with.

I first discovered Polanyi quite by accident in the middle 1960's. Behaviorism was still a potent force within experimental psychology and the name Polanyi was hardly a household word. I had become intrigued with the mentalistic and non-behavioristic approach that was beginning to emerge and began rummaging around through libraries looking for heretical thoughts when I stumbled across a book titled *Personal Knowledge*. I found much of what Polanyi was arguing for fascinating and in one particular respect, downright eerie. His notion of tacit knowledge as a kind of epistemic larder lying outside of consciousness was precisely what I was working on for my thesis - only I had called it implicit knowledge, to distinguish it from the explicit, conscious knowledge that the early cognitivists like Jerome

Bruner and George Miller and their colleagues were working on (see, for example, Bruner, Goodnow, & Austin, 1956; Miller, Galanter, & Pribram, 1960). My interests, unlike Polanyi's, were specifically psychological and my focus was on the issue of implicit learning, that is, how one acquired this unconscious, personal knowledge base.

Over the intervening decades my approach developed independently of Polanyi's formulations but demonstrably in parallel with them. As I hope to show, my orientation invites a number of extensions of the original notion of personal knowledge and, within the context of contemporary cognitive science, suggests a rather novel parsing of mind - although it is one that I believe Polanyi would have been comfortable with. Later I will develop in more detail just how these extensions emerge and what the data base for them looks like. To anticipate, they will emerge from taking an adaptationist's stance in which the relationship between unconscious mental functions and consciousness is seen in the light of some very ordinary principles of evolutionary biology. In short, the argument of the primacy of tacit or implicit knowledge over conscious, communicable knowledge will turn out to be, not merely arguably but necessarily, correct.

Some issues **of mind and knowing**

First, however, I would like to touch on a number of the more intriguing suggestions that accompany the stance that Polanyi took on these matters. This will amount to an extended "aside" on matters of mind, the science of mind, and some issues involved in doing the science of mind. I hope you will bear with me; these issues will prove to be useful in garnering philosophical justification for the arguments to be developed below.

Let me return to Polanyi's 1961 essay on the question of personal, tacit knowledge. In the middle of a discussion of the relationship between tacit knowledge and the role it plays when doing science, Polanyi makes the following observation:

We can account for this capacity of ours to know more than we can tell if we believe in the presence of an external reality with which we can establish contact. This I do. I declare myself committed to the belief in an external reality gradually accessible to knowing, and I regard all true understanding as an intimation of such a reality which, being real, may yet reveal itself to our deepened understanding in an indefinite range of unexpected manifestations. I accept the obligation to search for the truth through my own intimations of reality, knowing that there is, and can be, no strict rule by which my conclusions can be justified.

This is quite a remarkable passage, for a number of reasons. The most obvious aspect of it is that Polanyi has staked out for himself, quite unambiguously, a position on an issue that has bothered more than a few in recent centuries, the belief in the existence of a knowable reality. He has also managed, seemingly quite calculatingly, to have wedded himself to a phenomenological point of view on this matter. For several reasons both of these points are of considerable interest to me. First, I too find myself committed to the belief in a knowable reality - although, as will become clear, my stance is more labored and my current resting

place less firm for me than it was for Polanyi. Second, I believe that Polanyi's stance on this matter glosses uncritically some fairly serious problems about how one is to capture the notion of an objective and knowable reality - although, as will become clear, my solution to them may not be entirely satisfactory either. Third, it will turn out that Polanyi's resorting to phenomenology with respect to the form and functions of tacit knowledge is not necessary; the notion of a cognitive unconscious possessed over a tacit knowledge base sits on a firm empirical foundation that has grown up over the past two decades. In the first part of this paper I want to pursue each of these themes in a little more depth.

1. **On the existence of a knowable reality:**

The thesis that there is an external knowable reality has not always been treated quite as kindly as it was by Polanyi. From the extreme subjective idealism of Berkeley to the more contemporary arguments of deconstructionists and social constructivists, it has often been argued that "reality" is a subjective notion that emerges either from singular phenomenal representations or from socially constructed representations, a kind of elaborated characterization that spins itself out by dint of the interactions between participants and their several sets of beliefs. This point of view has a good many supporters, particularly within the field of social psychology. As Cook put it in an overview of the issues in the study of person perception,

...accuracy of perception implies a reality to be perceived, and the current resurgence of phenomenological approaches to social psychology tends to deny any such reality (Cook, 1984, p. ix).

Similar themes are echoed elsewhere, for example:

...events in the social world may be as much effects of individuals' beliefs as they are causes of these beliefs." (Snyder, 1984, p. 294)

And:

Constructivism asserts that we do not discover reality, we invent it. (Hare-Mustin & Maracek, 1988, p. 455)

This point of view was captured about as succinctly as possible in a remark that has at one time or another been attributed to any number of people: "If I didn't believe it, I wouldn't have seen it."

Constructivism, to be sure, may take any of several forms and the "purity" of each is, in large measure, reflected by the discipline from which the particular theorist is working. In its most extreme form, found typically among social psychologists, sociologists and some social philosophers, constructivism stands as a direct challenge to Polanyi's program for if there is no objective reality to be known then all knowing is indeterminate and there is no way that one could argue that tacit knowledge is a reflection of an external reality. In its

more muted forms it does little violence to Polanyi's epistemology. Indeed, in many ways Polanyi anticipated the force of constructivist thinking in that he recognized that one's own particular biases and prejudices could contaminate the manner of one's knowing. In "Knowing and Being" Polanyi advanced the thesis that there are limits imposed on his capacity to know. The conception of knowledge as *persona!* knowing, in his words,

departs in two closely related respects from the ideal of a strictly justifiable knowledge. It accredits man's capacity to acquire knowledge even though he cannot specify the grounds of his knowing, and it accepts the fact that his knowing is exercised within an accidentally given framework that is largely unspecifiable. (Polanyi, 1961/1969, p. 133)

The "accidentally given framework" is, again quoting, "the cultural background of our knowing." (p. 134)

Of the varying perspectives on constructivist thought, the most pragmatic is that expressed by the thesis that the terms "reality" and "knowable" need to be dissociated. That is, there may be indeed a legitimate, external reality but the very act of "knowing" is inescapably subjective and social so that what is known is indeterminate in the degree to which it corresponds with "reality." This position appears at first blush to blend a bit more comfortably with the one Polanyi staked out for himself in that the *act* of knowing is where the subjective components are to be found and not in the external "reality" which may still be assumed to have objective metaphysical status.

Nevertheless, even by this more moderate constructivist analysis we are still stuck with a kind of social scientist's "uncertainty principle." For example, it is still not obvious that scientific "discoveries" may be treated as other than "inventions." That is, Newton's laws should be treated, not as "discoveries" about the material world but "inventions" that happen to give a reasonable account of the data base. Newton, therefore, should not be said to have discovered any particular property of reality, only that he put forward a formalization of his own thinking that fit with what was accepted by the scientific community at the time as characterizing "reality." The same goes for Einstein.

Just how far is this line of thought from Polanyi's? Indeed, is it still possible to consider seriously the possibility that both points of view could be making some kind of sense, independent of each other? Recall that Polanyi's line of argument is that not only does he maintain that there is indeed a coherent reality that is in principle knowable, but that, in his words, "all true understanding is an intimation of such a reality." The difficulty is that one component of Polanyi's position appears to be the antithesis of the social constructivist's in that he regards knowledge as *prima facie* evidence of reality; yet another component seems to admit of the legitimacy of the kinds of arguments that constructivists employ.

I find myself feeling, as I hinted above, not entirely uncomfortable with the overall thrust of Polanyi's line of thought here while at the same time maintaining grudging respect for some elements of social constructivism. What I want to argue here is that these two posi-

tions, while not entirely compatible, are not necessarily mutually unintelligible. My point will be that there are two "things" to be known here. The first is the external reality, the second is the subjective, private, tacit knowledge itself. Polanyi argued that the second (tacit knowledge) is a guide to the first (the external reality). I maintain that tacit knowledge has become itself part of a knowable external reality. In what follows I hope to be able to develop a defense of this proposition that has both philosophical and empirical elements.

Initially I will try to develop the philosophical arguments; in a later section I will put forward a more sharply empirical presentation that respects and substantiates much of Polanyi's perspective on tacit, personal knowledge that I believe does not do violence to the sometimes compelling arguments of the more moderate forms of constructivism. The line of argument that seems best fitted to my analysis is best thought of as a form of "representational realism." I will develop the proposition that our personal or tacit knowledge base is, as Polanyi intimated, the result of our interaction with our environment but I will stop short of suggesting that this epistemic or phenomenological representation is necessarily a wholly valid mirror of an external reality. I will also present parts of a growing data base that shows that tacit or personal knowledge is itself an object of scientific investigation. For those of you who are aware of the richness and subtlety of Polanyi's thought, it will come as no surprise that many of his speculations concerning the nature of tacit knowledge have been borne out by careful experimentation.

One of the reasons that I am taking this experimentalist stance here is that I find Polanyi's perspective too intensely woven in with his own particular belief systems about the world independent of any coherent data base to support it. Each time I re-read that passage I quoted earlier, I am struck anew at how "theological" it sounds, how intensely committed to a particular view of things Polanyi was and how far removed he was in this matter from any substantive empirical data base. Polanyi's commitment to personal belief is, of course, an essential element in his epistemology. But what I find paradoxical about it is that if you look at his scientific writings the very style of investigation is different. It is to this point that I want to turn next.

2. On "knowing" a knowable reality; a levels-of-analysis approach:

Many the difficulties entailed by the apparent conflict between objective and subjective perspectives on reality can be handled by taking a more pragmatic stance and seeing matters as dependent in part on their being several levels of analysis hidden in the dispute. It may help here to remind ourselves that Polanyi was originally trained in the medical and physical sciences.

Indeed, given his views on the matters at hand, it strikes me as unsurprising that Polanyi was primarily a natural scientist. For most natural scientists the opportunity to question an external, knowable reality rarely arises. There really isn't much purpose in the active examination of a physical world unless one takes as axiomatic the proposition that it exists independently of the observer-researcher and that its essential features and operating principles are "out there" no matter what particular beliefs or prejudgments one may have about

it. Early theologically driven models of gravity may have characterized a rock plummeting to earth as the return of earthly matter to its God-given proper location, and contemporary relativistic models may view it as following Einsteinian principles of shifts in the space-time manifold, but both are merely local characterizations of the same, in principle, knowable reality. What changed is our conceptualizations of events, not the events themselves.

Social scientists, a sub-species with which I am rather familiar, often find themselves feeling uncomfortable with this point of view, not because of any inherent crankiness on their part, but simply because they recognize that the objects of their researches often are not "out there" in quite the same sense that they are for the physical scientists. This is why, as I pointed out earlier, the strongest defenders of constructivism tend to be those trained in the social sciences.

The "social reality" established by a kind of interpretative conspiracy among the participants in a group, be it a cluster of intellectuals in a "think tank" or a professional basketball team, is manifestly an emergent property of the group. Reality here changes with shifts in the personnel, and its "knowability" changes with the beliefs and prejudgments that the social scientists trying to study it may have about it. The rock plummeting to earth seems to have a kind of transcendent existence that maintains 'tself no matter what the beliefs about its action may be; the apprehension of the social events taking place in this room right now are intimately dependent upon the beliefs about them that each of us may have. If you believe that I'm a serious scientist trying to bridge the domains of epistemology and cognitive psychology, your notions about my personal knowledge will be very different than if you believe that I am some crank with an eccentric perspective on problems of mind that empiricists would be better off staying away from. The social scientist's kind of reality seems to be knowable in ways fundamentally different from the physical scientist's.

Is there a problem here? Is it possible that an objective and knowable reality only exists in some domains? Does it make sense to argue that the physical world is "out there," palpable and knowable in a coherent fashion, but that the social world is a labile construct whose manifestation is utterly dependent on the constructions of the participants? My answer to all of these and many other related questions is a clear, "yes and no." "Yes," there are different forms of reality here but only in the sense that there are different levels of analysis that can be or are being carried out and that the very conceptualization of a "knowable reality" is dependent on the level of analysis. "No," in that there is no such thing as an inherently emergent, subjective reality whose existence is arguably of some other form from the knowable reality that Polanyi was so fond of.

To help me develop my argument here, let me resurrect one of the most sophomoric of questions, raised anew by each generation of Philosophy 101 students, usually at the local watering hole. This question has any forms but my favorite is, "If a tree falls in the forest and there is no one around to hear it, does it make a sound?" I do not wish to belabor the pros and cons of answers positive and negative that have been provided for this question; I will simply give you mine, which is, as above, a clear "yes and no."

If by "sound" you mean a pattern of condensations and rarefactions of molecules in a medium (what I'll call "Sound₁"), then it most certainly does make a sound. But, if by "sound" you mean a phenomenal auditory experience of an organism with requisite anatomical and neurophysiological apparatus to process the acoustic display (let's call this one "Sound₂"), then it most surely does not make a sound. Sound₁ feels like a natural scientist's sound; it is there and part of an objective external reality independent of the contributions of an observer and its proper ties are what they are in a pure, rather pristine way. Sound₂ feels like a social scientist's sound; its very nature is dependent on the organism or organisms whose anatomical, neurophysiological, and social interactive properties encode it, represent it, and interpret it. As a constructivist would argue, this sound will be one thing to a termite, something else to a woodpecker, and something very different to a human — indeed, it will become yet something else should there be several humans together to process it in a truly social manner.

This "levels of analysis" approach is, of course, a standard trick and surely old hat to most of us, but there are still these nagging questions: Where is Polanyi's knowable reality here? Which sound is constitutive? Would he have restricted us to the natural scientist's domain and excluded us from the social scientist's? Doesn't Polanyi have *really* serious problems here? Is he not hoist by his own petard? After all, if personal knowledge is *the* essential feature of human cognition and if personal knowledge is part of psychological experience, is it not then part of the subjective, evanescent domain of the phenomenologist? Has Polanyi, and those who would follow his line of thought, been locked out of any hope of making "sound" part of knowable reality?

Surely not - or else most of us would have given up this game long ago. The way out of this seeming paradox is to resort to the levels-of-analysis rubric once more. Both the objective, physical domain and the subjective, phenomenological domain are legitimate objects of investigation; both are parts of knowable reality. Typically, we have no difficulty in seeing the former in this light because we believe, as it were, in a realness that transcends our experience of it - and we have been warned off solipsism by our teachers. But we often have trouble with the latter because we are typically unacquainted with the point of view of the psychologist whose very mission is predicated on the proposition that the experience of the observer, whether hearing a tree fall or participating in a sensitivity training group, is part of the knowable reality. The trick is to recognize that even though one of these domains of existence is relatively stable whereas the other is notoriously labile, both are equivalent in their claims to be part of an objective and, in principle, knowable reality. Put another way, the moderate constructivist would not exclude Sound₂ from becoming part of a psychologist's knowable reality. He or she would simply remind you that it needs to be analyzed on a different level from Sound₁.

At this point you are probably wondering why I have gone off into this realm of discourse. There were several reasons. For one, I was bothered by the apparent paradox that Polanyi's position seemed to invite and I'd not seen any discussion of it elsewhere (although that, of course, may be my fault). For another, I have spent the last two decades or so

attempting to understand the notion of personal, tacit knowledge, not through philosophical argument but by trying to turn it into an object of scientific investigation, a candidate for entry into the domain of objective, knowable reality. Sound₁ has long been part of the empirical realm of the physical scientist; within the past century and a half Sound₂ became part of the empirical realm of the psychological scientist. Both, as such, are candidates for being dubbed pieces of a knowable reality. Michael Polanyi treated tacit knowledge as an object of philosophic musing; I want to show how it too, despite its inherent subjectivity, has become an aspect of a knowable reality. This leads me to my next issue:

3. The empirical foundation for the cognitive unconscious:

Polanyi, like so many scholars with vision, seemed to shift his very style of investigation when moving from the natural to the social sciences and when moving from the laboratory to the philosopher's armchair. His writings in his home disciplines are peppered with data, formal theory, and careful references to relevant literatures. His philosophical writings, on the other hand, are full of proclamations and notably few references are to be found; indeed there is often a sense of Polanyi speaking *ex cathedra*. For example, the essay "Knowing and Being" that I have been quoting from contains but a single reference and this is to a paper that is cited to support the idiosyncratic use of a technical term. Polanyi, the philosopher, wants us to accept his arguments that personal knowledge is an essential feature of the human mind while eschewing the kind of evidence that Polanyi, the physical chemist, would have insisted upon.

Well, for whatever it's worth, the past twenty-odd years of work on the problem of the cognitive unconscious has shown that many of Polanyi's intuitions concerning the form and function of personal knowledge were accurate. Polanyi may have written from a position that emerged from his own internal sense of what was involved in the process of knowing rather than from any established empirical literature, but his intuitions on these matters fit neatly with recent findings in the cognitive sciences. In the remainder of this paper I want to give a rapid overview of the empirical and theoretical work on the problems of implicit learning and tacit knowledge and relate them to the issues that were raised above.

Implicit learning and tacit knowledge

Three topics will be touched on here: (1) The acquisition of tacit or implicit knowledge, (2) The dissociation between implicit and explicit knowledge systems, (3) The primacy of the implicit. The first addresses ontology; it will cover the evidence for the existence of a tacit knowledge system and explore the manner in which such nonconscious knowledge is acquired and used. The second presents an overview of the evidence that implies that this implicit knowledge base is dissociable from that characterized by knowledge that is overt, explicit and communicable. The third puts forward the argument that the implicit knowledge systems are evolutionarily older and hence have epistemic primacy over the later emerging explicit systems that typify conscious mentation.

1. The acquisition of tacit knowledge

Polanyi maintained that personal knowledge could be taken as evidence for a knowable reality. In his view, a tacit, largely nonverbalizable epistemic base could be seen to have emerged in individuals who had been steeped in particular stimulus domains. A good physical chemist, to take an obvious example, seems to have acquired a deep, abstract, personal knowledge of his or her domain of expertise as a result of intense and extended experience with the varieties of manifestations that the objects of examination displayed. Similarly for a political scientist, a diagnosing physician, a linguist, or even a cognitive psychologist. The expert in each case was viewed by Polanyi as having acquired a mental representation that was richly isomorphic with the structure of the particular domain of reality with which he or she had had experience.

When we first began to look at this issue in our laboratory, it was clear that it would not do to try to study the experiences of real physical chemists or practicing diagnosticians. Instead we set up an arbitrary and calculatingly unreal stimulus domain. The point was to determine whether tacit knowledge would emerge in arbitrary stimulus domains that had no relationship at all to ordinary reality. This point is important. If you wish to watch the "pure" acquisition of tacit knowledge in a controlled laboratory setting, you must work with a stimulus environment about which your subjects have no pre-existing knowledge. We developed what is now called the artificial grammar learning experiment.

An artificial grammar (AG) is a set of syntactic rules that dictates the order that a set of symbols may take with respect to each other - much like the real grammar of a natural language. The rules and the symbols are arbitrary and, in the cases we have studied, without a semantic component. One can take a fairly simple AG with letters used as symbols and some examples of the strings of letters ("sentences") that it can generate. Various AG's of differing complexity have been used by us and other researchers and the symbols that instantiate the grammar have ranged from letters and geometric shapes to sounds of different pitches and lights flashing in various locations. The beauty of the procedure is that the results of the experiments have been consistent independent of the manner in which the grammar is instantiated.

In the prototypical study, subjects are given a subset of grammatical strings to memorize and then tested on their knowledge of the grammar acquired during the memorization phase. The testing consists of presenting the subjects with new strings, some of which are similarly grammatical and some of which violate the transition rules of the AG and hence are non-grammatical. The subjects are asked to determine the status of each new string. During the learning phase they are told only that they are in a memory experiment; no mention is made of rules for letter order. At the outset of the testing phase they are informed for the first time about the existence of rules and told that the learning stimuli were all examples of acceptable strings.

In this simple setting, implicit learning and tacit knowledge emerge in ways that mirror many of Polanyi's intuitions concerning tacit knowledge and support the representation realism interpretation offered above. Subjects gradually acquire a knowledge base about the strings that is a partial but largely valid reflection of the rules of the AG. This knowledge base is personal in that subjects are poor at providing coherent descriptions of what they know; the most common response of subjects when asked why they thought a particular test string was nongrammatical is, "I don't know; there was just something odd about it." Yet, this knowledge is sufficient to allow them to reliably make decisions about the rule-governed status of novel strings.

It's worth noting that in these experiments subjects' tacit knowledge base is nowhere near being a complete mapping of the reality that they have been working within. Like the scientist whose cognitions Polanyi described, our subjects have only a partial representation of the patterns and structures that the stimulus domain displays. If our subjects are analogs of real scientists (and we believe that in many ways they are), they are clearly in the early phases of scientific understanding. However, as Polanyi liked to point out, knowledge was being acquired long before philosophers arrived with their theories of epistemology and scientific glimmerings of external reality were made manifest long before formal models were developed. The implicit, tacit knowledge base emerges first; the conscious, overt explication of it takes time and energy and emerges only later and in an incomplete form.

This point was made clearly in an elegant experiment by Robert Mathews and his colleagues (Mathews, Buss, Stanley, Blanchard-Fields, Cho, & Druhan, 1989). Subjects were run in pairs, in what is known as a "yoked" design. For each experimental subject who saw structured stimulus sequences and made decisions about their "grammaticality," there were others who never saw the displays but were given verbal protocols from the experimental subject with whom they were "yoked." That is, at regular intervals the experimental subjects would stop and provide as complete a description as possible about what they were doing, what they thought they knew about the stimulus sequences, and how they were making their decisions. The protocols were then given to the "yoked" control subjects who were asked to make the same decisions using only the descriptions provided.

This is something like giving someone who is musically naive a musicologist's description of what a Beethoven string quartet sounds like and then asking them to discriminate examples of musical passages from Beethoven and Brahms without ever being given the opportunity to experience them. The results, however, were most intriguing. The "yoked" control subjects did, in fact, perform better than chance but below the level of the experimental subjects. Interestingly, the experimental subjects reached their asymptotic level of performance fairly early in the experiment, toward the end of day 2 of the experiment that ran over four successive days. The various "yoked" subject groups, however, did not improve their performance markedly until they were permitted to work with protocols prepared by experimental subjects from day 4. In other words, even though the experimental subjects reached their maximum performance levels on day 2, their ability to communicate their knowledge effectively to others did not develop for another two days.

In classical Polanyi'ish fashion, tacit knowledge preceded explicit, communicable knowledge; the experimental subjects clearly "knew more than they could say."

In addition to these studies, literally scores of other experiments have been run showing that implicitly acquired knowledge is used in problem solving, decision making, pattern learning, motor learning, and judging preferences (see Reber, 1989 for a review). Moreover, implicit knowledge has been shown to be abstract in nature and to generalize to new situations so that novel tasks in analogous settings can also be undertaken. Although there are, to be sure, various disputes in the study of implicit learning concerning the exact nature of the underlying memorial representation and the degree to which tacit knowledge lies outside of consciousness (see Brooks & Vokey, in press; Dulany, Carlson, & Dewey, 1984, 1985; Perruchet & Pacteau, 1990), few would dispute that the essential features of implicit learning are now well established. However, what would surprise many cognitive psychologists is the extent to which these findings dovetail with Polanyi's intuitions about how tacit knowledge operates in real world settings.

Collectively, these findings function as a kind of extended existence demonstration. In the next section I want to expand the scope a bit and introduce additional findings that suggest that these implicit, unconscious cognitive systems can be shown to be differentiated from the more commonly studied explicit and conscious.

2. The dissociation between implicit and explicit systems

A reasonable inference that one can make based on the above findings is that the cognitive processes that underlie the acquisition and use of tacit knowledge differ in fairly basic ways from knowledge that is conscious and communicable. Interestingly, Polanyi's writings reveal that he was thinking along similar lines. Indeed, the way in which he characterized personal knowledge, particularly in his critiques of Popperian positivism, suggests that he thought of the cognitive processes underlying the acquisition and instantiation of tacit knowledge as different in fundamental ways from those that underlie the acquisition and instantiation of knowledge that could be communicated. His often moving descriptions of individuals engaged in the process of actually "doing" science suggest that he thought of this, as well as many other cognitively interesting processes, as being made up of distinguishable modes of mentation.

As above, there now is a substantial literature that supports this proposal. It has been built up from a variety of sources using a wide array of experimental techniques and procedures. I would like to review some of this literature here to help develop my general theme.

In this literature, perhaps the most compelling findings come from studies carried out on individuals who suffer from any of several forms of neurological disorders such as amnesia, agnosia, and prosopagnosia. Most nonspecialists think of these disorders as characterized by a loss of memory (amnesia) or a loss in the ability to recognize objects (agnosia), or

faces (prosopagnosia). However, for over a century it has been suspected that the impairments that accompany these organic disorders were not quite so uniform or complete. In 1889 Sergei Korsakoff reported an incident with a patient suffering from the alcoholic amnesic syndrome that now bears his name. He had previously subjected the patient to a series of mild electric shocks. When he stopped by for a visit while carrying the ox containing the shock apparatus, the patient, who had no memory of the previous episode, reacted with suspicion saying that some doctor had been going around giving people shocks. Some years later, the French physician Claparede (1911) confirmed Korsakoff's observation with an amnesic woman. Upon first meeting with her he secreted a pin in his palm and pricked her when they shook hands. At a subsequent meeting she refused to shake hands with him claiming that he was known to carry pins around with him although she maintained vigorously that they had never met before.

Reports such as these suggested that some aspects of memory are maintained while others are lost. Korsakoff viewed those memories that were maintained as being "too weak" to have entered consciousness. However, it was not until recently that it was understood that this phenomenon had little to do with the relative strength or weakness of specific memories. Rather, it was the explicit, conscious memories that were lost in these disorders; the tacit, unconscious memories were typically maintained and at levels that were often indistinguishable from normal individuals. Korsakoff's and Claparede's patients had no conscious memory of the episodes but they still had unconscious memories and these memorial residues were sufficient to exert control over their behavior. This dissociative pattern has now been reported in literally hundreds of neuropsychiatric cases.

Milner and her colleagues (Milner, Corkin, & Teuber, 1968) reported that a densely amnesic patient showed near normal sensorimotor learning that required transfer of practice over days. Warrington and Weiskrantz (1968, 1974) reported that amnesics show normal word priming effects despite the fact that they have no conscious memory of having been exposed to the words the day before. Young, DeHaan and their colleagues (DeHaan, Young, & Newcombe, 1987; Newcombe, Young, & DeHaan, 1988; Young & DeHaan, 1988) have shown that patients with prosopagnosia so extreme that they do not recognize the faces of members of their own family may show virtually normal face recognition when asked to perform tasks that do not require conscious identification of the face. One task involved asking them which of two faces (one of which was a famous political figure) would make a good politician. Weiskrantz (1986) reported a case of a syndrome now known as blindsight in which patients with neurological damage to the visual system can pick up and use information about objects that they maintain that they cannot see. In the same vein, several studies with patients with aphasia and acquired dyslexia show virtually normal processing of linguistic materials provided that they are not asked to deal with them consciously. For example, Milberg, Blumstein and their colleagues (Blumstein, Milberg, & Shrier, 1982; Milberg & Blumstein, 1981; Milberg, Blumstein, & Dworetzky, in press) reported that aphasic patients showed essentially normal responses on lexical decision tasks where they were asked to respond as quickly as possible whether a given group of letters was or was not a word despite showing chance performance on the meanings of the very same words.

In addition, there are other studies that show that not only does implicit memory appear to be dissociable from explicit memory but that implicit learning shows a similar pattern. For example, Nissen and Bullemer (1987) found that Alzheimer's patients who performed poorly on tasks that required conscious control of problem solving techniques nevertheless performed normally on an implicit sequence learning task. Johnson and her colleagues (Johnson, Kim, & Risse, 1985) showed that amnesics learn to develop preferences for melodies based on Korean melodic patterns that they were unaware that they had heard before. Abrams and I (Abrams & Reber, 1988) showed that psychiatric inpatients and patients with chronic alcoholic syndrome showed intact ability for implicit learning of an artificial grammar despite being unable to discover simple letter-to-number rules when they were presented as explicit problems. Finally, in an extended series of studies, Glisky and her co-workers (Glisky & Schacter, 1989; Glisky, Schacter, & Tulving, 1986) showed that an amnesic patient could learn to operate a computer including learning to perform complex tasks such as data entry, the use of disk storage systems and even the writing of simple programs. The patient showed continuous daily improvement despite her claim that she has never seen a computer before.

Now, I appreciate that the connection between these studies and Polanyi's implied division between ordinary, communicable knowledge and tacit knowledge that resists efforts at explication may not be completely obvious. My purpose in introducing this literature was the same as in the preceding section, to show that there was an empirical basis for taking Polanyi's philosophical musings seriously. The validity of a point of view is often appreciated only by the discovery of converging operations and dovetailing lines of investigation. By focusing on the dissociation between the implicit and the explicit, these clinical cases provide some of the strongest evidence to show that the unconscious, tacit dimension has an independent reality that transcends the speculations of an insightful philosopher.

Moreover, there is a thread that runs through this literature that I believe to be of singular importance - perhaps you noticed it during my overview. In all of the cases reviewed, the unconscious, tacit memories and the capacity for implicit learning survived when the explicit and conscious functions were compromised by injury or disease. The suggestion here is that there is indeed something special about the unconscious, implicit systems, what I have called the "primacy of the implicit." In the final section of this paper I want to expand on this theme a bit. This will take us on a short journey into evolutionary biology.

3. On the primacy of the implicit

One of the standard heuristics of evolutionary biology is that both phylogenesis and ontogenesis are processes that are hierarchically structured (see Simon, 1962). Hierarchical systems display certain features, one of which is that the forms and structures that emerge earlier are more robust, less malleable, and less variable than those that come later. Hence we should expect to see particular relationships emerging between forms that are evolutionarily old and those that evolved in more recent times. To make sense of all of this we really

need only to recognize a simple truism: consciousness is a relatively late arriver on the evolutionary scene and was preceded by a considerable margin by sophisticated cognitive, information processing systems. Put simply, it makes good evolutionary sense that implicit systems should survive disorders that disrupt the functions of the explicit systems. The implicit, unconscious systems, being phylogenetically older, form the epistemic foundation for the more recently evolved conscious systems. There are other entailments of this line of reasoning that are rather interesting and, as I go through them, I hope you will see how they begin to make contact in perhaps unexpected ways with many of Polanyi's intuitions.

1. Implicit learning is a process that yields accurate memorial representations of the stimulus display. In order for the knowledge acquisition systems that underlie implicit learning to have adaptive value they must have the capacity to represent external reality accurately. An information processing system that failed to provide a veridical representation of the world about would be unlikely to aid in the survival of the organism. I had mentioned earlier that one of the links between implicit learning theory and Polanyi's overall thesis was the presumption of the point of view known as "representational realism." The intriguing entailment of the evolutionary stance is that representational realism can be seen, not only as a philosophically interesting position as presented above, but as one that follows as a natural consequence of a standard model of evolutionary biology.

2. Tacit knowledge is the key to the knowable external reality. This proposition, of course, follows naturally from the preceding. If implicit acquisition systems are mapping the stimulus inputs in a veridical fashion, then the resultant tacit knowledge base will be an accurate reflection of the external reality. Once again, we see that a conviction of Polanyi's for which he had little but his own intuitions to go on may now be seen as an entailment of evolutionary biology.

Interestingly, the various constructivist arguments I raised earlier concerning the degree to which this external reality may or may not ultimately be known still hold. The implicit acquisition systems that provide information about the external, physical world are the evolutionarily old ones. No matter how far back along the phylogenetic scale you go, all organisms, independent of their epistemic primitiveness, must be able to represent those aspects of the external environment that are germane to their survival. On the other hand, those processes that code information about social systems, about the patterns of interaction between members of one's own and other species, are evolutionarily much more recent. Hence, we should expect them to be more labile, to show greater variability and, hence, to be more easily compromised by environmental events. As I pointed out above, it is within the social domain that we find the strongest arguments in favor of constructivism.

3. Tacit knowledge resists invasion of consciousness. The literature on the relationship between the implicit and the explicit domains of knowledge, some of which I shared with you earlier, generally supports the conclusion that implicitly represented knowledge is, in some sense, "ahead of that which can be delivered unto consciousness. This proposition, coded by Polanyi as "knowing more than we can tell," is a natural consequence of the

evolutionary stance. However, this evolutionary stance carries additional entailments about the relationship between consciousness and mental states. Specifically, it implies that the degree to which we can become aware of our internal states and functions will be given by the point in evolutionary time when these states and functions first emerged.

To appreciate this point we need merely recognize that we are virtually completely blocked off from any conscious insights into neurological or vegetative functions; none of us has awareness of our occipital lobes or our livers. We do, however, have some glimmerings of the modes of operation of our limbic system which evolved later, this is particularly so in the manner in which it functions in settings that have emotional and motivational components. We have even greater awareness of those daily offices that involve our knowledge of social and cultural actions. It is much easier for us to be conscious of the manner or style of our interactions with others than it is to gain insight into our deeper emotional and motivational functions - as any psychotherapist can testify. Finally, we are even more cognizant of the operations and procedures we engage in when solving problems. As any experimental cognitive psychologist can tell you, subjects in explicit problem solving experiments are typically able to provide a full and rich description of the rules and heuristics they used in finding the solution.

Interestingly this evolutionary stance also has entailments that were not anticipated by Polanyi and which might actually have surprised him. For example, it predicts that there should be both phylogenetic and ontogenetic commonality for these implicit systems; that so long as we stay within the implicit domain, we should be able to look up and down the phylogenetic scale and across developmental levels and find commonality of function.

This is heady stuff but, as before, not outside of the existing data base. Recent work in the neurological basis of learning and conditioning suggests that the same basic processes of the detection of covariation operate from *Aplysia*, a large mollusk of modest intellectual capacity, up through *Homo sapiens* (Carew, T. J., Hawkins, R. D., & Kandel, E. R., 1983; Reber, in press; Rescorla, 1988). And, as Cleeremans and McClelland (in press) have suggested, this process is none other than simple association between events. In addition, recent studies on infants and young children suggest that implicit acquisition processes are intact at very young ages (Haith, Hazan, & Goodman, 1988; Roter, 1985). What distinguishes lower species from higher and infants from mature adults is not the underlying process which, in all cases, is the detection of covariation but the degree to which tenuous and subtle covariations can be detected. Newborn infants and mollusks need highly invariant associations; adults process subtle covariations between events, including the multi-variable interactions that characterize the kinds of sophisticated mentation that Polanyi was concerned with. Well, let me stop here before I get carried away on a wave of inference and outright speculation that I might one day regret: I had three goals in mind for this paper. First, I wanted to worry for a while about a problem that I believe has lurked unnoticed in Polanyi's writings, the inherent tension between his presumption of a knowable reality and the constructivists' suggestion that some elements of reality may not be quite so obviously knowable. Second, I wanted to praise Polanyi's intuitions concerning tacit,

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