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Personal or Impersonal Knowledge? *

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Abstract

Reflections on the contrast between the titles of Popper's *Objective Knowledge* and Polanyi's *Personal Knowledge* led Haack to explore how Polanyi's ideas might be used to correct some of the distortions caused by Popper's refusal to allow any role in epistemology to the knowing subject, and thus to throw light on such questions as the relations between the knower and the known, between epistemology and psychology and sociology of knowledge, and between subjectivity and objectivity.

Key words: epistemology; philosophy of science; Karl Popper; Michael Polanyi; knowing subjects; personal judgment.

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* Historical note: This paper was written (on a typewriter!) in 1980, for presentation at a workshop on "Epistemologically Relevant Sociology of Science" organized by Donald T. Campbell. It was never previously published. Of course, Prof. Haack wouldn't write it the same way today; but scholars will notice, in this early piece, the seeds of her later ideas: in her *Evidence and Inquiry* (1993), about the relation of logical and causal elements in epistemology, for example, and the contributory relevance of results from psychology; and in her *Defending Science—Within Reason* (2003), the way scientific evidence is shared, and the usefulness to philosophy of science of epistemologically well-informed sociology of science.

Introduction

This paper began with some reflections on the contrast between the titles of Popper's *Objective Knowledge*, and Polanyi's *Personal Knowledge*.[1] The most attractive features of Popper's epistemology, to my mind, are its acknowledgment of the ubiquity of error, its concern for the growth of knowledge, and its picture of science as a cooperative enterprise. But Popper's overwhelming concern to maintain the objectivity of scientific knowledge, manifested in his exclusion of the knowing subject from the scope of epistemology, and his refusal to contaminate the autonomy of the theory of knowledge with the "subjectivism" of psychology or sociology, has the effect (as I have argued elsewhere)[2] of compromising the fallibilist, dynamic, public character which was the chief virtue of his theory. Polanyi, by contrast, stresses the personal character of scientific knowledge, and does not shun the aid of psychology and sociology; which suggested to me that some of his ideas might be used to correct the distortions caused by Popper's inhospitable attitude to the knowing subject.

I hope that my reflections may, eventually, throw some light on a nest of the most intractable issues in the theory of knowledge—the relations between the knower and the known (the subject and the content of knowledge), between epistemology and the psychology and sociology of knowledge, between subjectivity and objectivity. But my immediate object is more modest: to give a straightforward account of the main outlines of Polanyi's views,[3] and to bring out the most important points of contrast with Popper. Then in the last part of the paper, which is more ambitious and also more tentative, I shall concentrate my attention on four themes thrown into relief by this contrast: the role of the knowing subject; the relation between fallibilism and commitment; the active character of knowledge; and the ways in which we learn from each other.

1. Exegesis and Interpretation

When Polanyi describes scientific knowledge as "personal," part of what he wants to convey is that the traditional picture of the scientist as a strictly neutral observer impartially investigating objective facts will not stand serious scrutiny. The scientist is constantly obliged to exercise his personal judgment, e.g. as to the importance of a problem, the weight to be given to evidence, the likelihood that an observed correlation is not significant but coincidental, and so on. And such judgments are unavoidably a matter of discretion: no formal rule can tell a scientist, for example when to take an ostensibly falsifying result seriously and when to disregard it as probably the effect of some as yet undiagnosed experimental error. Scientific judgment is a matter of the appraisal of the worth of problems, of theories, of evidence; it is *evaluative*, not purely factual, and as much a matter of knowing how as of knowing that. It is, in short, a *skill*; and, like other skills—among which Polanyi

would also include perception and the understanding of language—it cannot be made fully explicit; it involves an unavoidably tacit component.

Scientific judgment is personal not only in the sense that it inescapably calls for the exercise of discretion, but also in the sense that it involves a *commitment* on the part of the scientist who makes it. The scientist is passionately committed to the problems he judges worthy of investigation, to the evidence he decides he must take seriously, to the theories he devises, and to the values of science itself.

But, though scientific knowledge is, in the senses just explained, "personal," it is also in an important sense, *public*. For, first, though the skill of scientific judgment cannot be encapsulated in formal rules or prescriptions, it can be taught, by senior and experienced scientists, to aspiring "apprentices." This is the way in which the scientific tradition is passed on, and the reason it must be respected even by those young scientists who will go on to do original and creative work which radically challenges the traditional wisdom. Even the mature scientist, furthermore, is subject to the judgment of his peers, especially those in a position to decide whether his work should be published or supported financially, whether he should be appointed to a professional post, and so on. Each scientist understands, and is competent to judge the worth of, only a relatively small part of the work done by other scientists; but a "chain of overlapping competences" enables the community of scientists to exercise an essentially decentralized authority, a sort of communal self-control. There is an interaction between tradition and authority, on the one hand, and creativity and criticism, on the other; in this, and in other respects, the structure of science is not radically different from the structure of, for instance, art or religion.

It would be quite easy to get the impression, from this, that Polanyi is proposing a radically subjectivist, authoritarian, sociological theory of knowledge which is fundamentally at odds with Popperian critical rationalism. But this impression is not, I think, altogether correct.

For Polanyi allows that scientific knowledge is *objective,* as well as personal. His account of truth has rather strong affinities with Strawson's version of the redundancy theory: to say that p is true is to assert, or reassert, p. Though he stresses the point that to believe that p just is to believe that p is true, he is nevertheless at pains to insist that the truth of p does not consist in my, or anyone's, or everyone's, believing it. Scientific theories aspire to represent the way the world is in a manner independent of the scientist's personal point of view; the

scientist who judges that a theory is true commits himself to the universalizability of that judgment. And scientific theories aspire to represent the truth about a world which is largely independent of us and our beliefs: an independence which is manifested in its ability to *surprise* us by not turning out the way our theories predicted.

A better interpretation, therefore, represents Polanyi as expressing skepticism about a number of traditional dualism, i.e., dichotomies often taken to be both exclusive and exhaustive. Most notably, Polanyi does not respect sharp dichotomies:

- between the *objective* and the *subjective*
- between *fact* and *value*
- between *knowing that* and *knowing how*, and
- between *science* and other human cognitive enterprises, such as *art* or *religion*.

Polanyi should not be seen as claiming that science, like art or religion, is subjective *rather than* objective, evaluative *rather than* factual, a matter of knowing how *rather than* knowing that. His claim is that science is subjective *as well as* objective, evaluative *as well as* factual, and a matter of knowing how *as well as* knowing that.

If this is right, it might be argued, Polanyi's apparent disagreement with Popper begins to look *merely* apparent. For it now looks as if we might understand Polanyi as saying that, while scientific knowledge in the state sense ("scientific judgment") is subjective, scientific knowledge in the content sense (*what* scientists know) is objective and with this, no doubt, Popper would concur. But this picture would also be misleading, for two main reasons: first, that it does not recognize the importance of the fact that Polanyi differs from Popper precisely in giving a central role in the theory of knowledge to the individual who knows or believes, and to interactions between knowing subjects (whereas Popper declines to take any interest in the theory of knowledge "in the subjective sense"); and second, that it disguises the significance of the fact that, while Popper insists that knowledge "in the subjective sense" is irrelevant to knowledge in the objective sense, Polanyi believes that we cannot fully understand what scientists know independently of a study of how they know it. So major disagreements remain: as to whether epistemology should be concerned with the states of knowing subjects as well as

with the content of this knowledge, and as to whether the study of the former is relevant to the study of the latter.

2. Comparative Remarks

In the light of this I can now bring out the main points of agreement and disagreement in their proper perspective.

First, both Popper and Polanyi are, in a sense, *realists*; in the sense, that is, that both hold that scientific knowledge in the content sense, what is known, is knowledge of a world which is, at least largely, independent of our beliefs about it, and which, in virtue of its independence, is capable of surprising us, of falsifying our beliefs about it. Popper's realism is manifested in his adherence to a correspondence theory of "absolute and objective truth," and his reliance on falsifiability as a criterion of the scientific, Polanyi's in his view of the scientist as seeking clues to the making out of a world the character of which does not depend on him, and producing theories for which he claims universality, independence of point of view.[5]

Secondly, both are fallibilists, urging that any our beliefs could turn out to be false, that we have no infallible ways of finding out about the world.[6] And both, again, place considerable emphasis on the growth of knowledge.[7] But in both these respects Popper is placed in considerable embarrassment by his official exclusion of the knowing subject, which means that he has to try to explain scientists' liability to error, and their ability to learn from and build on the work of their predecessors, in terms exclusively of the character of the problems, theories, etc., which constitutes his world 3; an attempt which, I believe, fails. Polanyi, on the other hand, because he gives a central role to knowing subjects and their interactions, can readily give a theory which is dynamic and fallibilist.

Though both Popper's and Polanyi's epistemologies are fallibilist, Polanyi's is committal in a way Popper's is not. Popper holds that scientists do not, or perhaps should not, believe the theories they devise; these theories are only conjectures, which they must try, and should even hope, to falsify. Polanyi, on the other hand, thinks that scientists are, at least sometimes, passionately committed to a belief in the truth of their theories; and that this is as it should be.[8] This contrast is connected with a further issue. Popper's philosophy is strongly anti-authoritarian, and it places great weight on criticism. Polanyi certainly allows room for criticism of the established wisdom, but he also gives an important role to tradition and

authority in science. And here, he thinks, the personal commitment of scientist of their theories plays an important part; for it will take determination to get a new theory listened to, new evidence looked at, new problems investigated, if conventional wisdom deems them improbable, insignificant or unimportant, and it would be unrealistic to expect such determination on behalf of a mere conjecture which the scientist is hoping to falsify.[9]

Popper draws a sharp distinction between the context of discovery and the context of justification. The ways in which scientific theories are discovered, according to Popper, are properly the concern of psychology or sociology; the proper concern of epistemology is the ways in which scientific theories are tested. Polanyi would not invest this distinction with the same significance; for he holds that the assessment of evidence for and against a theory is as much a matter of personal discretion, and as little amenable to strict, logical formulation, as the initial process of discovering the theory. This is another part of the reason for Polanyi's insistence on the central place of the knowing subject, and his *rapprochement* with psychology and sociology; for in his view, there is no way to divide up the study of scientific knowledge into the epistemologists' and the psychologists' concerns.

Because Polanyi is concerned with the knowing subject and the ways in which he learns about the world, as well as with what he learns, there is room in his epistemology for issues which Popper simply excludes: notably, the analogies between scientific judgment and the exercise of other, e. g., perceptual, skills, and the ways in which scientists learn from, criticize and control each other's work. I shall turn shortly to a more detailed consideration of Polanyi's contributions to these questions. For now, though, the point I want to bring out is that Polanyi's stress on the skillful character of scientific judgment and the element of often aesthetic evaluation he believes it to involve is one of the things that leads him to claim important affinities between science and, for instance, art or religion. Popper's whole strategy, by contrast, is motivated by the desire to draw a sharp line of demarcation between science and non science, "pseudo-science," or metaphysics.[10]

This leads me to a final, surprising, similarity. At the level of the choice between scientific and other (e.g., religious or magical) "world-views" Polanyi's position is that one can make a commitment, a choice, but not a commitment or a choice grounded on any neutral, rational grounds. He has chosen the scientific world-view, he says, but he couldn't give arguments that would rationally persuade someone

committed to a different world-view to shift his allegiance. More surprisingly, Poppet takes something like this position, too. In the end, he has to say, Critical Rationalism cannot be rationally defended, but must be accepted as the basis of an ethical decision.[11] In summary:

Polanyi	**Popper**
Scope of epistemology: knowing subjects *and* what is known	Scope of epistemology: what is known only ("knowledge" in subjective sense excluded)
Involved in justification as well as discovery of scientific theories—hospitable attitude to psychology, sociology	Context of justification only (context of discovery excluded)—hostility to psychology, sociology
Realism, objectivity	Realism, objectivity
Fallibilism	Fallibilism (but hard to express without knowing subjects)
Growth of knowledge	Growth of knowledge
Personal judgment	(impossible to express without knowing subjects)
Committal	Non-committal
Subjectivity, tacit dimension	World 3—all explicit
Skillful	(again, not applicable without knowing subjects)
Affinities between science, art, and religion	Sharp demarcation of science from non-science
Choice between science and other world-views: relativism	Critical Rationalism accepted by an ethical decision, not by rational argument

3. Polanyi and Pragmatism: Four Themes

It is worth observing that there are interesting similarities between some of Polanyi's ideas and some views held by the pragmatists.

Polanyi's skepticism about the dualisms of objective and subjective, fact and value, knowing how and knowing that, science and religion, is strongly reminiscent of Dewey, who is well- known for his hostility to traditional dualisms. (But I am not aware of any evidence that Polanyi has been influenced by Dewey, nor that he, like Dewey, has been influenced by Hegel.) Dewey attacks some of the very same dualisms that Polanyi does, and draws very similar conclusions; e.g., he stresses the active character of knowledge, the analogies between knowledge of the world and skill in manipulating the world, and even goes so far as to attribute the whole (as he believes) misguided quest for certainty to undue respect for the distinction between the practical and the theoretical, and an unjustified, elitist preference for the latter. (It is arguable, also, with Dewey as with Polanyi, whether some of the rejected dualisms might not be reinstated by means of a distinction in the subject–matter to which they apply—as the dichotomy of objective and subjective might be partially restored by maintaining the objectivity of knowledge in the content, and of the subjectivity of knowledge in the state, sense.)[12]

Polanyi's stress on the central role of the knowing subject, as the active character of learning, on the importance of interactions among knowing subjects, and on the need to respect tradition and authority, are strongly reminiscent of Peirce. (Again, I am not aware of any evidence that Polanyi has been influenced by Peirce; it is clear, however, that, like Peirce, he has been influenced by Kant in relevant ways.) Peirce attacks Descartes for the pernicious individualism of his epistemology, the false idea that self- consciousness is intuitive, and his hostility to tradition, his anti- authoritarian ambition to clear the ground of all his former beliefs and start afresh on the indubitable basis of intuitions concerning his own state of mind.[13]

These points of comparison are of more than historical importance; for the pragmatists, like Polanyi, offer a dynamic, fallibilist theory with a proper place for the knowing subject—a theory in short, of the very kind that the shortcomings of Popper's account suggest that we need.

These comparative remarks throw into relief four themes in Polanyi's work on which I want to concentrate in the rest of this paper, *viz.*:

(1) The central role of the knowing subject, and the *rapprochement* with psychology and sociology:

(2) The compatibility of fallibilism with commitment ;

(3) The active character of knowledge;

(4) The importance of the ways in which we learn from each other.

First theme: The knowing subject: epistemology and psychology.

I believe, like Polanyi, that the knowing subject must be given a central place in epistemology, and that psychological results can be relevant to epistemological theories. I shall not attempt to repeat, here, all the arguments I have given elsewhere (in "The Relevance of Psychology to Epistemology" and Epistemology *with* a Knowing Subject") in support of these beliefs. I want instead to say a little more about what is meant by the claims that psychology is relevant to epistemology to comment on the significance for the relevance of psychology, of the adoption of a causal theory of knowledge, and to answer an argument for irrelevance which I have not previously discussed.

Someone who holds that psychology is relevant to epistemology may be maintaining:

(i) that psychological results are sometimes sufficient to verify epistemological theses;

(ii) that psychological results are sometimes sufficient to falsify epistemological theses;

(iii) that psychological results are sometimes sufficient to probabilify epistemological theses;

(iv) that psychological results are sometimes necessary—and, with the help of other information, such as logical results, may be sufficient—to verify epistemological theses;

(v) that psychological results are sometimes necessary (etc., as in (iv)) to falsify epistemological theses;

(vi) that psychological results are sometimes necessary (etc., as in (iv)) to probabilify epistemological theses;

(vii) that psychological works may sometimes aid in the clarification epistemic concepts.

These alternatives are arranged, roughly, in order of decreasing strength; no doubt there are other alternatives that I have failed to think of.

In "Epistemics: The Regulative Theory of Cognition,"[14] Goldman supports (vii). He points out those psychologists have made distinctions, e.g., of occurrent versus dispositional beliefs, which represent an improvement on the rather coarse conceptual apparatus traditionally employed by epistemologists. (The distinction mentioned, I observe, promises to cut through some hitherto depressingly persistent philosophical disputes, notably the Case of the Nervous Examination Candidate who, some urge, knows the answer, but does not believe it, thus falsifying the principle that Kp—> Bp.)

In "Epistemology w*ith* a Knowing Subject" I argued for a version of (vi). Briefly my argument was that psychological evidence (e.g., such as might be supplied by work on cognitive dissonance) as to people's inability to hold inconsistent beliefs, was necessary, and, with the addition of certain logical results, sufficient, to probabilify a version of fallibilism: a version, namely, that said that if there are any logically self-guaranteeing proposition, it is psychologically possible to disbelieve them. (I should now want to classify versions of fallibilism in the following way: dogmatism claims, and fallibilism denies, that some beliefs are epistemically privileged, in the sense of being guaranteed against error, unwarrantedness, or ignorance; the version of fallibilism I was discussing was error and ignorance oriented, but my argument is, I suspect, capable of generalization to all ignorance-oriented versions.) [16]

The strongest of the relevance theses, (i), will be thought, by some, to be nothing less than a statement of the so- called genetic fallacy: the fallacy, that is, of supposing that the genesis of a belief could constitute or contribute to its justification. (The issue of course, is whether this alleged fallacy is really fallacious.) It is worthy of notice that a causal definition of knowledge seems actually to require (i). For on this definition x knows that p iff there is a causal connection of an appropriate kind between x's believing that p and the fact that p (the usual clauses to the effect that x believes that p and that p is true are redundant, in view of the reference to *x's believing that p* and *the fact that p* in the causal clause); and this refers precisely to the genesis of x's belief.

The traditional definition of knowledge as justified true belief (like variants thereon) is hospitable only to weaker forms of the relevance thesis. To say that x has good reasons to believing that p is, I suppose, to say something to the effect that x's reasons for believing that p are good reasons (or the reasons) for believing that p. To know whether x has good reasons, therefore, we need to know something about what x's reasons are—i.e., something causal about what gave rise to and/or sustains his belief—and something about what would constitute good reasons for believing what x believes, i.e., something logical about the evidence for p. (I am assuming that the reasons for x's belief are causes of x's belief; but not, of course, that all causes of a belief are reasons for that belief.) So even on this definition some weaker from of the relevance thesis—(iv), (v) or (vi)—seems plausible. These reflections perhaps shed some new light on Goldman's defense of the relevance of psychology.

Surprisingly, though, Goldman offers an explicit defense, not of (i) but of (ii). His argument for (ii) needs to be understood in the context of an idea that, though it is rarely explicitly formulated, I think often underlies hostility to appeals to psychology. The idea is that, while psychology is a descriptive enterprise the business of which is to describe how, as a matter of fact, we learn, think, perceive, etc., epistemology is a normative enterprise the business of which is to tell us how we ought to think, what we ought to believe. And so the attempt to derive epistemological conclusions from psychological premises rests upon a fallacious inference from an "is" to an "ought."

But the claim that psychology is (purely) descriptive, and the claim that epistemology is (purely) normative, is doubtful. Psychologists study, e.g., the conditions of reliable and unreliable perception, how to teach people skill in formal logic, what kinds of data conduce to good hypothesis formation…and so on. And epistemologists, whether they adopt the traditional or the causal definition of knowledge, are concerned with people's reasons for their beliefs. So even if it is a fallacy to argue from an "is" to an "ought," psychology might for all that be relevant to epistemology.

Goldman urges the relevance of psychology, in the sense of thesis (ii), on the grounds that psychology can rule out epistemological recommendations by pointing out our cognitive limitations. Since "ought" implies "can," the argument goes,

"can't" implies "not (ought)," so that normative conclusions can after all be derived from descriptive premises. So psychological results can be at least negatively relevant to epistemic theses. The question will perhaps be raised, whether "it is not the case that you ought to φ" counts as a normative statement. Certainly there is room to doubt the classification of statements into descriptive versus normative with respect, e.g., to disjunctive statements one disjunct of which is straightforwardly normative, as is only too apparent from discussion of the is/ought issue in the ethical literature. But this question is not crucial here; Goldman's argument suffices, however that question is answered, to show that psychological information about human cognitive limitations could properly lead us to reject some proposed epistemic recommendation (e.g., "believe all the consequences of anything you believe").

I am more sympathetic to the thesis that "ought" implies "*could*" than to the thesis that "ought" implies "*can*"; it does seem pointless to tell someone that they ought to do something if they not only *now can't*, but also *couldn't get themselves into a position where they could do it*. A weaker but still useful version of Goldman's argument would survive this modification. It is as we shall see an argument that is pertinent to the disagreement between Popper and Polanyi as to whether scientists should be committed to their theories.

Second theme: Fallibilism and commitment

Polanyi maintains that scientists may be, and often are, passionately committed to their theories; sometimes they have been willing to go to the stake for them. Popper, by contrast, seems to maintain that scientists theories are merely conjectures in the sense (not only that they are never conclusively justified or verified, but also) that scientists do not believe them, but only entertain them provisionally, until such time as they have been submitted to test and found wanting. Insofar as the issue here is the straightforwardly historical question, whether scientists ever do really believe their theories, there is, I think, little doubt that Polanyi is right; they do.

But there is also another, normative question at issue. For Popper clearly thinks that it is undesirable that scientists should believe, rather than just provisionally entertain, their theories; and Polanyi evidently holds, on the contrary, that a passionate commitment is proper, and perhaps even desirable. (It is interesting, in view of the considerations raised in the last section, to find Popper so ambiguous

on the question of whether he is describing how scientists do behave, or prescribing how they should.)

This disagreement between Polanyi and Popper raises an important, though usually neglected, question: what is the relation between fallibilism (the possibility that scientific theories will turn out to be false), revisability (scientists' willingness to give up their theories), and *commitment* (scientists' belief in their theories)?

The relation between fallibilism and revisability is relatively straightforward. If it is always possible that a scientific theory, however successful so far, should turn out to be false, then presumably, scientists should always be willing to give up or modify the theory if and when evidence that it is false becomes available. Fallibilism requires revisability.

Popper attaches considerable significance to the distinction between modifying a theory and rejecting it in favor of another, regarding the former strategy as methodologically dubious, falling under suspicion of conventionalism. It is worth noticing that the distinction relies upon an assumption of criteria of identity of theories, which however, have not been explicitly supplied, and might not be easy to supply. In any case, I doubt that the distinction has as much significance as Popper supposes; sometimes the best strategy, in the face of contrary evidence, is to modify the existing theory, but sometimes things get so bad that an entirely new theory is called for. When to adopt which strategy is a matter of what Pierre Duhem called good sense, and what Polanyi would call personal judgment.

The relation between revisability and commitment is less straightforward. The first point to be made is that there is no logical incompatibility between scientists' believing their theories, and their being willing to give up those theories should the evidence turn out unfavorably. It is after all, possible for people to stop believing things. It may be that when Popper suggests that commitment would lead to dogmatism he has at the back of his mind the idea that there is something paradoxical about really believing that p, and at the same time really believing that one's belief that p might be mistaken. But there is not; indeed, it is quite rational for me to believe that some of the things I believe are false—even though, since I know about the Paradox of the Preface,[17] I know that my believing that some of

the things I believe are false itself guarantees that some of the things I believe *are* false.

The issue seems to be, rather, a psychological one: is it true that people are more likely to be willing to give up a theory in the face of contrary evidence if they only entertain it, rather than really believe it, in the first place? ("Give up" would have, in the former case, to mean something other than "stop believing," but I don't think the problem of exactly what it would mean should detain us.) Here we seem to be faced with two conflicting arguments. In favor of Popper's view it could plausibly be claimed that people's ability to recognize unfavorable evidence as unfavorable is known to be hampered by the psychological set induced by their pre-existing beliefs, and hence that scientists are more likely to acknowledge contrary evidence, and revise their theories in the light of that evidence, if they do not believe them.[18] (It is notable that this takes the form of a *psychological* argument for Popper's *epistemic* recommendation.) But, on the other side, it might be argued that revisability is better safeguarded by different scientists believing different theories than by no scientist believing any theory.

This argument—of which there are hints in Polanyi's writing[19]—would be sociological in character, to the effect that the conservatism of the scientific community is such that it will take passionate commitment on the part of its advocates to get a hearing for a new theory. This seems to leave us in the unhappy position of having two plausible arguments; one favoring Popper's view, the other Polanyi's, and no very promising way of deciding between them. But I think progress can be made by turning back from the normative to the historical issue; if Polanyi is correct in claiming that a scientist often does passionately believe his theories—that, indeed, this is inevitable, given the investment of time, energy and reputation that serious scientific work demands—then the danger that scientists will fail to acknowledge evidence unfavorable to their theories is unavoidable. But this danger will be mitigated if other scientists believe different theories; for then they will be alert to the difficulties in his theory, and he also, to the difficulties in theirs.

So it looks as if revisability (and hence fallibilism) is compatible with commitment, and it seems plausible to suppose that revisability would be best secured by a pluralistic, differential commitment on the part of different members of the scientific community.

The structure of the argument of this section was as follows: given that scientists will believe their theories, it is pointless to recommend, as a way of guaranteeing their openness to evidence, that they merely entertain them, and better to find some other way of encouraging criticism. This is just what an endorsement of Goldman's response to the "is/ought" argument to would lead one to expect.

Third theme: The active character of knowledge

It takes rather little reflection on one's own experience to realize how much one depends on action for learning. Consider, for example, how much more quickly one learns the way to a certain place if one drives there oneself, instead of going as a passenger in a car driven by someone else; or what a world of difference then is between having understood the instructions for using a piece of equipment, and the mastery that comes with having used it a few times. And this is true not only in the case of severely practical knowledge (knowledge how to get to Bradford, how to use the flash attachment), but also is more theoretical cases; there is a comparable difference between having understood the rule of conditional proof, say, and the mastery that comes with actually having used it.

These purely commonsensical observations are supported by the results of psychological experiments. Kohler, for example, found that his subjects adapted quite quickly to wearing reversing spectacles, seeing the world the right way up, if they were obliged to get about the world wearing them; Held and Hein, that their purely passive kitten never learned to see, whereas his active partner was capable of normal perception.[2]

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And the thesis that learning requires activity has some interesting philosophical spin-offs. I am thinking especially of Bruner et al.'s criticism of one of Chomsky's arguments for the innateness of grammar. Chomsky argues that the linguistic competence the child eventually manifests is extraordinarily manifests is extraordinarily thick compared with the extreme thinness of the data available to him. Bruner et al. reply that Chomsky has underestimated the thickness of the input by concentrating only on the auditory data, and ignoring the vital fact that the child is not just subject to auditory input, but is also a party to frequent, intense interactions with adults, in which verbal activity is only a part, though a major and integral one.[21] (This work, incidentally, was apparently suggested by Dewey's ideas

about the role of action in learning. It also has affinities with Wittgenstein's stress on language as forming an integral, part of the way of life of its speakers.)

This example is germane to an argument Popper gives for the irrelevance of psychology: that while we can learn about a process (learning) by studying the product (objective knowledge) we cannot learn about the product by studying the process. It should be said that this argument sits very uneasily with Popper's professed interest in the growth of knowledge, which has, in consequence, to be confined to questions concerning increase of the content of world 3. Bruner's argument against Chomsky—though not exactly an argument to product from process—certainly suggest a more complex interaction between our knowledge of process and our knowledge of product than Popper's assumption of a strictly one-way traffic permits. For one thing, it reminds us that inferences to the process that must have yielded a certain upshot are subject to test by direct study of the process itself.

The thesis that one important way in which humans (and other animals) acquire knowledge of, learn about, the world, is by acting in and upon it, seems to me a very plausible one. (And, incidentally, also to be a thesis strikingly at home in a theory of belief as disposition to action as in Bain, Peirce and the other pragmatists.) It is a thesis prominent in Polanyi's writings, especially in the analogies Polanyi draws between scientific judgment, physical skills such as riding a bicycle or swimming, perception, and the understanding of language.

According to Polanyi, our perception of the world is a skilled activity. Its skillful character is perhaps most apparent in cases where an expert can perceive something which a layman cannot (e.g., the doctor reading an X-ray photograph) or in cases where there is an ambiguity in what is perceived (e.g., an ambiguous picture, making out a scene in poor light); but Polanyi believes that skill is also involved even in quite ordinary cases. The scientist's judgment that this evidence can be safely ignored as the result of some undiagnosed experimental error, or that this problem is significant and that trivial, he thinks, involves a skill analogous to that of the doctor who reads the X-ray picture as showing a shadow on the lung, or to that of the cyclist staying upright on his bicycle.

Central to Polanyi's account of skills is his distinction between focal and subsidiary awareness. When we perceive something, we are focally aware of what

we perceive in virtue of a subsidiary awareness of its background. The idea is, again, clearest in the case of perception of figures, such as the vase/profiles picture, which manifest figure/ground ambiguity, but it is intended to apply to perception quite generally. If we were to became focally aware of (what is now) the background, we should cease to see the figure; we should, e.g., see the vase rather than the profiles.

The distinction between focal and subsidiary awareness is extrapolated to other skills, such as riding a bicycle or swimming. There are, of course, mechanical principles which explain how one stays upright on a bicycle, say or how one stays afloat while swimming. But one need not be aware of those principles in order to ride a bicycle or swim; indeed focal awareness of—concentration of one's attention upon—those principles may inhibit the exercise of the skill. (Polanyi sometimes suggests that it must do so: a stronger, and more dubious, claim.)

Since, according to Polanyi, the exercise of scientific judgment is a sort of skill, the distinction between focal and subsidiary awareness applies here too. And here Polanyi draws a surprising, indeed alarming, conclusion: since focal awareness of the principles governing scientific judgment would inhibit the skillful exercise of that judgment, scientific knowledge has an ineliminably *tacit* component; it cannot be made fully explicit.[22]

Polanyi's stress on the active character of knowledge is congenial; the conclusion that there is something ineffable about scientific knowledge, however, is unwelcome. So it seems worthwhile to point out that the conclusion Polanyi draws does not follow from his analogy between scientific judgment and physical skills.

To make this clear, it is essential to distinguish between judgment in the state sense (judging) and judgment in the content sense (what is judged). It is plausible to think that there are analogies between scientific judgment and other skills only if "judgment" is taken in the state sense. Now if the scientist's exercise of his judgment is indeed relevantly like the cyclist's tacit knowledge, what would follow would be that, though there are principles governing the assessment of scientific problems, the evaluation of evidence, etc. it is not necessary that a scientist know these principles in order to exercise scientific judgment, and, indeed, attending focally to these principles could actually interfere with the exercise of the skill. It would not follow that meta-scientific principles (e.g., of inductive or abductive

logic) inevitably resist explicit statement; any more than it follows that there could not be mechanical principles stating explicitly what the cyclist knows tacitly or oenological principles stating explicitly what the wine connoisseur knows tacitly.

So it is possible to acknowledge the appeal of the analogy with skills without committing oneself to any disagreeably obscurantist conclusions about the ineffability of scientific knowledge.

In my comparative remarks I pointed out that, unlike Popper, who places considerable weight on the distinction between discovery and justification, Polanyi holds that the testing of theories, the evaluation of evidence, like the original discovery of theories, involves a large element of personal judgment. This contrasted Popper's picture, in which discovery is a matter of psychology and justification a matter of logic, with Polanyi's, in which both are a matter of psychology. What I am now urging is that the whole dichotomy of "psychology or logic?" be resisted: that *both* discovery *and* justification can usefully be studied *both* from the standpoint of psychology *and* from the standpoint of logic.[23]

To say this is not to say that I regard logic as no more than a descriptive characterization of actual inferential practices; I do not want to deny that logic is normative, an idealization. One way to express its normative character would be to say that it describes how on ideal reasoner would think. And this way of putting it has the virtue of raising some important, though difficult, questions about the fact that the scientific community regards some scientist's personal judgment as more reliable than others and in turn about how the scientific community is to be demarcated.

In the next section I shall be commenting on the virtues of Polanyi's stress on the dynamic of interactions within the scientific community; but it should be said, at this point, that his account of the demarcation of the scientific community is less than satisfactory. Since, however, I have no more satisfactory account to offer, I shall content myself, for now, with raising, and not answering, this question.

Fourth theme: Learning from each other

Polanyi and Popper agree on the public character of the scientific enterprise. This leads to Polanyi's concern with charting the ways in which scientific knowledge is passed on and controlled by the scientific community. Popper, however, excludes

the knowing subject, and a fortiori the knowing subjects who constitute the scientific community, from the scope of epistemology, and consequently can give no account of the nature and significance of interactions between cognitive agents except insofar as these are conducted indirectly, by way of world 3. The publicity of scientific knowledge can only be manifested in the (alleged) autonomy, and hence objectivity of world 3. But the autonomy of world 3 cannot, as I have argued in "Epistemology *with* a Knowing Subject," be reconciled with its character as a human creation, so that the publicity of scientific knowledge is not adequately assured.

From a certain point of view, it is difficult to overestimate the extent of our cognitive dependence on others. Consider, as an example, how I came to believe that the earth is round. I read it somewhere, or perhaps someone told me; and I know that practically everyone else, but for a few notorious cranks, believes it too. I do have some idea of some of the evidence for it (the appearance of the horizon, the fact that you don't fall off the earth as you cross the Atlantic), and of course I have seen many representations of the earth as round. Certainly, though, I do not have available to me anything like conclusive or adequate evidence for this belief. The others on whom I rely may or may not themselves be in possession of such evidence: they in turn may be relying on others who in turn may or may not be in command of adequate evidence,….etc. if I know that the earth is round, it is in part in virtue of someone else's possession of relevant evidence. In either case, I may have no direct evidence at all; this is so, e.g., with respect to my knowledge that whales are mammals. (It may be worth drawing attention to the fact that my cognitive reliance on other is by no means restricted to my accepting their testimony about events which they witnessed and I did not.)

Much recent work in epistemology seems not to take facts of this kind at all seriously. It traces the interconnections between beliefs, but doesn't acknowledge that my beliefs may depend upon yours. For either, as with Popper, the concern is exclusively with logical relations between propositions, or else, as with Lehrer,[24] the concern is exclusively with relations of support among the beliefs of a single knowing subject. From this point of view Polanyi's work, with its detailed attention to the dissemination of knowledge, its metaphor of the overlapping chain of competence by means of which the community of science keeps up its own standards, represents an important advance.[25]

But, it will be objected, the point of view from which this emphasis seems proper is questionable. For it takes for granted—what is not, of course, beyond philosophical dispute—that there are other people, that they have beliefs, and that their beliefs can somehow support mine. From the point of view of, say, the Carnap of *Der logische Aufbau der Welt*, other persons are nothing but logical constructions, and other persons beliefs nothing but even more complicated constructions, out of his sense data; any support there is for anything he believes must in the end derive from something else he believes, for support by means of something someone else believes just is, in the last analysis, support by means of something else he believes.[26] if solipsism were true an epistemology which placed emphasis on the ways I learn from other people would be hopelessly misguided.

Against this kind of objection, I think, attack is the best form of defense. Any epistemology which—like Carnap's or, paradigmatically, Descartes'—assumes from the outset a sharp contrast between my direct, intuitive, infallible knowledge of myself and my indirect inference to others, is itself hopelessly misguided just because it is bound to raise the problem of other minds in acute and obstinate form. (So the fact that Popper's philosophy of mind is pretty classically Cartesian[27] creates another difficulty for his official recognition of the public character of science.) To respond in this way is, of course, to issue a promissory note; for the response is only feasible if some plausible theory of the self can be given which does not require the fatal, Cartesian contrast.

G. H. Mead's theory of the social construction of the self is one such theory. Mead distinguishes between a biological individual, which is just a subject, and a conscious self, which is also an object to itself. A biological individual becomes a conscious self through an essentially social process, his participation in language and communication. Language, according to Mead, is a system of gestures, and a conscious self is an individual capable of responding to his own linguistic gesture as another hearer would, thus, "an object to himself." Interestingly, a major influence on Mead's theory, besides Darwin's work on the expression of the emotion, was Peirce's critique of Descartes pernicious individualism, and his suggestion that self-consciousness, far from being intuitive, derives from one's interactions with others.[28]

My object, in alluding to Mead's theory, is not uncritically to underwrite it, but only to illustrate what kind of theory could plausibly be thought to redeem the promissory note of the previous paragraph. Quine, I note, is sufficiently influenced by the pragmatists to see the virtue of conducting epistemology in terms of the cognitive community rather than the individual cognitive agent, and to stress the public, social character of the language in which knowledge is expressed: but he is also sufficiently influenced by Carnap to invest one's sensory surfaces—the *individual*'s sensory surfaces—with a special epistemic significance.[29]

The Cartesian epistemology is not only individualistic, but also, and foe related reasons, radically anti-authoritarian.[30] Descartes criterion of truth is personal; he will not rely on others beliefs. And it has often been supposed that anti-authoritarianism is a quintessentially scientific attitude; the scientist does not accept anything on authority but submits everything to the test of experience.[31] This is Popper's attitude: science grows by means of criticism and creativity; authority and tradition are anathema. Polanyi's view seems, at first glance, diametrically opposed, for it allows an important place to the collective but decentralized authority of the community of scientists. But this, once again, oversimplifies. For though Polanyi acknowledges the right of the scientific community to decide whether an individual's research is worthy of publication or financial support, he also acknowledge the fallibility of its decision.[32] One might describe this (by analogy with Peirce's Critical Common-sensism), as Critical Authoritarianism. Its object is to explain how the community of scientists manages to exercise its authority so as to discourage cranks, but at the same time to allow for the possibility of radical, creative criticism of the traditional wisdom.

I am not concerned, here, with the correctness or incorrectness of the details of Polanyi's account, but with the application of Polanyi's idea of the critical acceptance of authority to the general issue of our epistemic dependence on others. For here, as in the narrower issue of the structure of the scientific authority, there are conflicting requirements to be reconciled: on the one hand, the ubiquity of one's reliance on others, and on the other hand, the impossibility of surrendering one's own epistemic responsibilities. I have no realistic option but to rely, in substantial measure, on other people's beliefs; and it would be foolish to deny that I can learn from others. But others, like myself, are fallible; there have, after all, been times when just about everybody believed what I know (I think!) to be false, e.g.,

that the earth is fiat. Some people are better informed, more reliable than others; and I cannot escape the responsibility of deciding, in the end for myself, whose opinion I should respect, who is an authority on the subject I want to know about. The appeal of critical authoritarianism is that it suggests at least the outline of an account of how we can overcome some of our cognitive limitations by learning from each other, without suggesting that the community has mysterious cognitive powers that the individual lacks. If there were no truth in this idea, there would be little point in writing papers, or inviting the comments of others.

Notes

1. Popper, K.R., *Objective Knowledge*. Oxford U.P. 1972, hereafter OK; Polanyi, M., *Personal Knowledge*, Routledge and Kegan Paul 1958, hereafter PK.
2. Haack, S., "Epistemology *with* a Knowing Subject," *Review of Metaphysics*, XXXIII1979, 307-35.
3. As presented in PK; *The Study of Man*, Chicago U.P., 1959; *Knowing and Being*, ed. Grene, M., Routledge and Kegan Paul,1969, hereafter KB; The Tacit Dimension, Doubleday, 1966, Anchor, 1967, hereafter TD; and *Meaning* (with Harry Prosch), Chicago U.P.,1975.
4. For Popper's views on truth, see "Truth, Rationality and the Growth of Scientific Knowledge," and "Addenda" in *Conjectures and Refutations*, Routledge and Kegan Paul, 1964, hereafter CR; and "Philosophical Comments on Tarski's Theory of Truth" in OK; for falsifiability as the demarcation of science, *The Logic of Scientific Discovery* (1934; Hutchinson, 1959), hereafter LSD, §6, and On the Status of Science and Metaphysics" and "The Demarcation between Science and Metaphysics," in CR.
5. PK, pp. 300 ff.
6. Popper, "On the Sources of Knowledge and Ignorance," in CR; "Conjectural Knowledge," in OK; Polanyi, "The Republic of Science," in KB.
7. Popper, "Truth, Rationality and the Growth of Scientific Knowledge," in CR, "Epistemology without a Knowing Subject" and "Evolution and the Growth of Knowledge" in OK; Polanyi, "The Growth of Science in Society" in KB.
8. Cf. Levi, I "Truth, Fallibilism and the Growth of Scientific Knowledge, 1975, and Haack, S., "Descartes, Peirce and the Cognitive Community" now published in Eugene Freeman, ed., *The Relevance of Charles Peirce* (La Salle, IL: The Hegeler Institute, 1983), 238-63.
9. PK, ch.6.
10. Polanyi, PK, ch.6; Poppet, LSD 4 ff, and "On the Status of Science and of Metaphysics" and "The Demarcation between Science and Metaphysics," in CR.
11. Popper, *The Open Society and its Enemies*, Routledge and Kegan Paul, 1945, pp. 231-32 of 4th, revised edition, 1962; cf. Also Kekes, J., "Popper in Perspective," *Metaphilosophy* (8), 1977, 36- 61.

12. See Dewey, J., *Experience and Nature*, W. W. Norton.1929; and cf. Rorty, R., "Dewey's Metaphysics," in *New Studies in the Philosophy of John Dewey*, ed. Cahn, S., New England U.P., 1977, 45-74.
13. Peirce C. S. "Questions Concerning Certain Faculties Claimed for Man," *Journal of Speculative Philosophy* (2) 1869, 103–114, and "Some Consequences of Four Incapacities," *Journal of Speculative Philosophy* (2), 1868, 140- 57, reprinted in Hartshorne, C., Weiss, P. and Burks, A., eds., Cambridge, MA: Harvard U. P., 1931-58, 5.213–317. Cf. Also Haack, S., "Descartes, Peirce and the Cognitive Community" (note 8 above)
14. Goldman, A., "Epistemics: The Regulative Theory of Cognition, *Journal of Philosophy* (LXXV), 1978, 509- 23; cf. also abstracts of replies by Swain (pp. 523- 5) and Grandy (pp. 525 – 6).
15. See note 2 above.
16. Cf. Haack, S., "The Swings and Roundabouts Argument against Foundationalism: Goldman's critique of C. I. Lewis," unpublished.
17. Makinson, D., "The Paradox of the Preface," *Analysis* (2), 1964, 205-7.
18. Cf. Jervis, R., *Perception and Misperception in International Politics*, Princeton U.P., 1976, for some striking case- studies.
19. PK, ch.6.
20. See Kaufman, L., *Sight and Mind*, Oxford U. P., 1974, and Gregory, R. L. *Eye and Brain*, Weidenfeld and Nicholson, 1966, for accounts of these experiments.
21. Brinet, J., Caudill, E., and Ninio, A., "Language and Experience," in *John Dewey Reconsidered,* ed. Peters, R. S., Routledge and Kegan Paul, 1977, 18 – 34.
22. See PK, ch. 4, and TD.
23. Polanyi himself seems to come close to accepting the dichotomy of which I am urging revision. See especially his "Logic and Psychology," *American Psychologist* (23), 2968, 27– 43.
24. Lehrer, K., *Knowledge*, Oxford U.P.1974.
25. See especially "The Republic of Science" in KB.
26. Carnap, R., *Der logische Aufau der Welt* (1927) translated by George, R. A., as *The Logical Structure of the World*, Routledge, 1967. Carnap describes himself as a methodological solipsist.
27. Poppet, K. R., and Eccles, J. R. *The Self and Its Brain*, Springer International, 1977.
28. Mead, G. H., *Mind, Self and Society*, ed. Morris, Charles W., Chicago U.P., 1934; "The Social Self," *Journal of Philosophy, Psychology and Scientific Methods* (X), 1913, 374 – 80, reprinted in *Selected Writings*, ed. Reck, A. J., Bobbs- Merrill, 1964, hereafter SW, 240-66; "The Genesis of the Self and Social Control," *International Journal of Ethics* (XXV) , 1924- 5, 251-77, and in SW, 267- 93. There is an excellent summary and discussion of Mead's theory of the self—on which I have relief in large measure—in Thayer, H. S., *Meaning and Action*, Bobbs-Merrill 1968, ch. 5.

29. Cf. Margolis , J. "The Relevance of Dewey's Epistemology," in *New Studies in the Philosophy of John Dewey* (see note 12 above)
30. Descartes , R., *Discourse on Method* (1637), part II, in *Philosophical Works of Descartes* , ed. Haldane, E. S., and Ross, G. R. T., Cambridge U.P., 1911, vol. I., pp. 87 ff.
31. See e. g. Russell. B., *The Impact of Science on Society*, Allen and Unwin, 1950, pp. 110-111, quoted by Polanyi in KB, p. 94.
32. "The Republic of Science," in KB.

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