Book Reviews

On Understanding What We Don't Know How We Know

Knowing and Being. Essays by Michael POLANYI. Marjorie Grene, Ed. University of Chicago Press, Chicago, 1969. xviii + 246 pp. $4.95.

But what we can't say we can't say, and we can't whistle it either.—F. P. RAMSEY

One of the goals of this new collection of 14 essays by Michael Polanyi was to represent the extraordinary dimensions of the author's intellectual map. In this the editor, Marjorie Grene, has succeeded admirably. The essays range from analyses of historical events (the Hungarian Revolution) and contemporary social phenomena ("modern nihilism"), through inquiries concerning the relation of science to government, to speculations on the nature of life and consciousness (with lots of other stops in between). The core of the book, however, is the group of essays on what Polanyi calls "tacit knowing." Grene somewhat tendentiously describes Polanyi's theory of the "structure of tacit knowing" as his "major discovery" (p. xvii); it is certainly his major philosophical thesis, and most of the essays in the book derive their specifically philosophical interest from their relation to it. I shall therefore comment only briefly on the book as a whole, and then pass directly on to an examination of this thesis.

The collection is, as I have said, wide-ranging; it is representative, and thorough where it needs to be, in the section on tacit knowing. I have only two very small bones to pick, which are, first, that I find Grene's introduction rather more obscure than the ideas it is intended to illuminate, which are themselves obscure enough; and second, that her valiant efforts as editor to minimize repetition have not succeeded well enough to prevent a rather numbing sense of *déja vu* at the repeated appearances not only of the same ideas, but often of the same sentences and paragraphs. But these are trifling points: it is in the main a good collection.

Now to the substantive issues. I shall discuss three questions concerning Polanyi's theory. First, what are the philosophical springs of the theory? Second, what are the phenomena which Polanyi describes as instances of "tacit knowing," and why does he describe them in that way? Third, what is his theory of these phenomena, what are his grounds for belief in that theory, and what is its explanatory force?

Polanyi's thesis emerged as a reaction against the most bizarre version of scientific rationalism, "the ideal of a strictly explicit knowledge" (p. 195), by which it must be understood that Polanyi means a theory of knowledge in which every sort of knowledge, and every aspect of every sort of knowledge, is to be analyzed in terms of explicit inferences. Thus the editor takes modern epistemology as demanding a "conception of wholly *explicit* truth" (Grene underlines "explicit"; I urge the reader to underline "wholly"); and Polanyi describes "modern positivism" as pursuing "the false ideal of a strictly explicit knowledge" (p. 195), which he denies by asserting that even in the most explicit sort of knowledge, that resulting from deductive inference, there are tacit elements, for example, those involved in understanding the language of the formulae ("the logic of language itself—the way language is used—remains tacit," p. 145). Now, to the extent that Polanyi's arguments are directed toward denying this Laplacian vision, they are irrelevant to a reasonable evaluation of the attempts of contemporary logicians and philosophers of science to analyze those elements of mathematical and scientific reasoning which *are* explicit. It would surely be Pickwickian in the extreme to take a logician to task for not including in his analysis of deductive inference an explanation of how we distinguish the letter *p* from the letter *q*.

We may now rescue the significance of Polanyi's analysis by disregarding its obsession with the demonic ideal of explaining *everything* in terms of "matter and motion" and pointing out that what is crucial is Polanyi's assertion that there are significant aspects of scientific reasoning which simply do elude analysis in terms of "explicit inference" (for example, the perception of a problem, the making of a discovery) and that these must, and can, be understood only by his theory of tacit knowing. This assertion, if true, would be very important indeed.

What, then, are the sorts of things which Polanyi claims *cannot* be understood in terms of explicit inference? They are all those things which we know, yet cannot tell: how I recognize a face, how I ride a bicycle, how I read an x-ray, how I use a tool, how I read, write, and speak. The grounds for the belief that such phenomena exist? Common knowledge, experiments in subception, and so on. Let us grant that they exist; the question is not whether we perform such activities by following explicit rules—as we obviously do not—but whether it is in principle impossible to understand such performances by explicit theories.

Polanyi characterizes such tacit knowings as acts of "tacit inference," whose main logical characteristic is that they are integrations from clues, rather than deductions from premises (p. 194). Let us accept that these acts are acts of "integration" rather than "analysis"; the question is, why must they be tacit? Why can there be no explicit logic of integration (or, at any rate, no explicit logic of these sorts of integrations)?

One argument on which Polanyi seems to rely rather heavily consists in his assertion that, in cases of tacit knowing, we are aware of the clues only "subsidiarily" and that if we attend to them we destroy the integration which is produced when we "rely on" them tacitly, and when our "focal awareness" is centered on the the whole which they constitute. Thus he remarks,

> ... the identification of the constituent motions of a skill tends to paralyze its performance. Only by turning our attention away from the particulars and towards their joint purpose, can we restore to the isolated motions the qualities required for achieving their purpose [p. 126].

Polanyi's evidence for this thesis consists in such examples as piano-playing and riding a bicycle. Now no one denies that this may be the case; the important question is whether it must be the case. For one who denigrates the standard

canons of inductive logic as thoroughly as Polanyi (who describes them as the "current Sunday school precepts of scientific method," p. 100), he seems to make his own inductions from these examples rather cavalierly and with no discernible attempt to recognize even those counterexamples which he provides. And, of course, there are plenty of counterexamples. It is often the case that attention to the "joint purpose" is counterproductive, and attention to the "particulars" crucial: an expert marksman with a pistol, for example, will never concentrate on holding a gun steadily on the target and shooting at it, let alone hitting it. He lets his eye take care of the alignment of sight and target automatically, and will concentrate on two things, his breathing and the pressure of his finger; and if he is not surprised when the pistol goes off he has lost, rather than gained, his concentration. Polanyi cites the case of the acrobat Blondin, apparently in the belief that the anecdote he offers supports his thesis, as it obviously does not:

> The famous tight-rope walker, Blondin, says in his memoirs that he would instantly lose his balance if he thought directly of keeping it; he must force himself to think only of the way he would eventually descend from the rope [p. 213].

Presumably, whatever "descending from the rope" is, it is not the "joint purpose" of the acrobat's balancing maneuvers; for *that* is obviously staying *on* the rope.

There is, however, another argument which Polanyi marshals which could meet these counterexamples, at the expense, however, of admitting that his first argument is no argument at all. For he says that the question of "attention" is not really crucial, and that the difference between "subsidiary" and "focal" awareness is a logical and not a psychological one:

> It is the *function* of a subsidiary item that counts in classing it as subsidiary. We may call it its *logical function.* When I see visual clues as a coherent object, the relation between any awareness of the clues to the knowledge derived from them is similar to that between premises and conclusions derived from them: it is a logical relationship. The clues enter here into a procedure of *tacit inference,* with integration replacing deduction [p. 194].

Now, we may perfectly well agree that there is a "logical relationship" which is common to that between premises and conclusion, parts and wholes, evidence and what it is evidence for, and so on. Call it what you will, the "from-to" relation, for example. The point is that the existence of such a relationship carries no implication whatever that the relation need be tacit, as is perfectly clear from its very similarity to the premise-conclusion relationship.

The last argument for the necessity of "tacit knowing" seems to be that if one had an explicit description of, for example, perceptual or vestibular integrative processes, one would not be able to utilize that description as a recipe for seeing or balancing:

> No explicit direction can make us see a pair of stereoscopic photographs as one solid image... We cannot learn to keep our balance on a bicycle by taking to heart that in order to compensate for a given angle of imbalance a, we must take a curve on the side of the imbalance, of which the radius (r) should be proportionate to the square of the velocity (v) over the imbalance: $r \sim v^2/a$. Such knowledge is ineffectual, unless known tacitly [p. 144].

It is not clear that this need always be true; only experiment can tell how adept we might become at balancing bicycles by reacting to computer outputs. Its truth, however, is irrelevant: for the question of understanding integrative processes is quite independent of our ability to utilize that understanding to perform them, though of course not independent of the capacity of our nervous systems to receive and process information in the way that our utopian neurophysiology would postulate it does to account for these processes.

Which brings us to our last question. Let us assume that there are phenomena which are ultimately tacit; that there is no way, shape, or form in which even our utopian neurophysiology could possibly explain, for example, stereoscopic seeing. In what sense does Polanyi's theory of "tacit integration" explain them? The theory seems to tell us four main sorts of things about them: first, that tacit knowing requires that the entities have an inexhaustible reality (p. 149); second, that, for example, "We know a comprehensive whole, for example, a dog, by relying on our awareness of its parts for attending focally to the whole" (p. 213); third, that "our perception of living beings consists largely in mentally duplicating the active co-ordinations performed by their function" (p. 150); and fourth:

> To the question how a child can learn to perform a vast set of complex rules, intelligible only to a handful of experts, we can reply that the striving imagination has the power to complement its aim by the subsidiary practice of ingenious rules of which the subject remains focally ignorant [p. 200].

I shall try briefly to indicate why these and similar descriptions do not constitute, individually or severally, an acceptable theory of tacit knowing. The first is consistent with an explicit epistemology (Hempel has said the same sort of thing many times), and so cannot be a part of a theory of *tacit* knowing except in the quite innocuous sense of being part of any theory of knowledge save for atomistic (in the Humean sense) or Laplacian ones. The second cannot be part of a theory of tacit *knowing* since it is consistent with the falsity of what is claimed. After all, it is just as true to say of me that I "rely on my awareness of its parts for attending focally to the whole" when I say of a fox that it's a dog as when I say of a dog that it's a dog. Of the third, it is, I think, generally acknowledged that copy theories of knowledge are hopeless for the simple reason that whatever mysteries there are about the phenomena are simply duplicated in, and not dispelled by, the "internal representation." (Cf. Wittgenstein: "If the mental image of the time-table could not itself be *tested* for correctness, how could it confirm the correctness of the first memory? [As if someone were to buy several copies of the morning paper to assure himself that what it said was true.]" [*Philosophical Investigations,* Macmillan, 1953, § 265]; and Heisenberg: "Democritus was well aware of the fact that if atoms should, by their motion and arrangement, *explain* the properties of matter—color, smell, taste—they cannot themselves have these properties" [*Physics and Philosophy,* Harper Torchbooks, 1962, p. 69].) Fourth, to say of a mystery that it is accomplished by "questing imaginations" and "powers" following unknowable "rules" no more tells us how it is accomplished than to say of opium that it has the dormitive virtue tells us how it puts people to sleep.

I have been severely critical of Polanyi's theory. What justifies this severity is the very great importance of the questions which Polanyi attempts to answer. He is quite right in his judgment that the traditions of logical positivism and logical empiricism have not provided a satisfactory analysis of anything: not induction, not explanation, not description, let alone "discovery." But Polanyi is not the only philosopher of science who is antipositivist, nor is the theory of "tacit knowing" the only alternative to the "Sunday school precepts" of traditional empiricism. I greatly admire Polanyi's single-minded

attempt to break a new path, I agree with not one jot of it, and I commend to the reader the works of philosophers, from the time of Mill and later, who have tried different paths through the wilderness: William Whewell, C. S. Peirce, Stephen Toulmin, P. K. Feyerabend, Michael Scriven, Hilary Putnam, the late Norwood Russell Hanson, and many, many others.

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Tools on a Grand Scale

Technological Change. Its Impact on Man and Society. EMMANUEL G. MESTHENE. Harvard University Press, Cambridge, Mass., 1970. 128 pp. $4.95. Harvard Studies in Technology and Society.

Mesthene is the director of the Harvard University Program on Technology and Society, a well-financed and ambitious enterprise which is presumably beginning to approach completion. One might expect, therefore, that this little volume would be a kind of interim report. It has a little of that quality. Mainly, however, it is a personal essay, reflective rather than empirical, on the impact of technical change on society, its possible evaluation, its impact on values and religion, and on economic and political organization. The essay indeed is short, comprising as it does only about 75 small pages, a considerable part of the volume being taken up by an excellent annotated bibliography. The word that most immediately comes to mind in describing it is "Emersonian." It has the judicious, rather lofty quality of Emerson's essays, and at the end of it one has the same slight feeling of emptiness. The great god Technology turns out to be neither all good nor all bad and indeed frequently overrated, so that he looks at the end a bit like a Boston-Unitarian Thor who hardly seems worth all the excitement. This, however, is a little unkind. There is a good deal of mature reflection in this book and it deflates gently a fair amount of popular nonsense on the subject. Technology is simply tools and ways of doing things. Social institutions are just "groups of people organized in certain ways to accomplish certain purposes" (p. vii), so there is no great mystery about all this and one wonders almost whether the question was worth studying.

By defining technology as tools, however, Mesthene in effect defines away what may be the real problem at issue, which is the life that human artifacts seem to possess almost independent of man himself. A tool is something always under the control of the tool user. The very thing which disturbs so many people about the burgeoning evolution of human artifacts is precisely that this process seems to be getting out of control and to be taking on an evolutionary life of its own, using the totality of human nervous systems as the field within which this evolutionary process proceeds. One sees this gap between the tool concept of technology and the social evolution concept most clearly perhaps in Mesthene's treatment—casual, it must be admitted—of military technology, which he looks upon with a surprisingly benign eye. Thus, he describes on page 32 "our most spectacular technological successes in America in the last quarter of a century" as "in national defense, in space exploration, and in the provision of consumer goods and services. These successes have provided protection for the nation, realization of an age-old human dream, and achievement of the highest standard of living ever enjoyed by man." There is not much feeling here for the appalling instability of a system of nuclear deterrence which makes the probability of almost total destruction in the next 25 or 50 years dangerously high. There is no sense that the space enterprise has diverted desperately needed resources from other things or that the higher standard of living goes hand in hand with desperate pockets of poverty, inadequate medical care, racial discrimination, and students burning down their own universities. There is no real discussion of the effect of technology on identity, or on those all too fragile integrative structures which hold society together. In its political theory the essay seems to rely on what is to my mind the rather naive assumption that improved collection or processing of information from the ruled will improve decisions of the rulers. This greatly underestimates the difficulty of the conflict-resolution tasks of political organization, especially as we move toward an age of declining growth.

This essay was clearly written before Earth Week and it does not reflect much of the anxieties about ecology and resource use which have been so prevalent this year. There is an implied expectation that technology will continue to increase human power and productivity almost indefinitely, without regard to the limited capacities of the planet. However, this is a very brief essay and one should not complain that it does not cover everything. The slightly querulous tone of this review indeed may be a tribute to the power of this essay to stimulate reflections which go far beyond its actual content.

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Interdisciplinary Earth Science

Hot Brines and Recent Heavy Metal Deposits in the Red Sea. A Geochemical and Geophysical Account. EGON T. DEGENS and DAVID A. ROSS, Eds. Springer-Verlag, New York, 1969. xii + 600 pp., illus. $32.

This book is a compilation of research papers from a variety of geological and oceanographic disciplines. Its approach is, therefore, problem-directed rather than subject-oriented. The purpose is to bring to bear, on a single problem, a diversified array of techniques and scientific backgrounds. The result is a coverage and a tone that is strongly reminiscent of the Apollo 11 conference in Houston, and the approach is one that probably will be applied more in the future in attempts to unravel complex interdisciplinary problems whose importance justifies the efforts involved. An advantage of such a treatment is that it may provide the only satisfactory insights into certain types of problems; a disadvantage lies in the great demands it makes on the reader's competence as a generalist. The present volume has these characteristics.

A proper framework is established from which to consider the origins and significance of the Red Sea brines. The regional stratigraphy, tectonics, and probable geologic history of the Red Sea and adjacent land areas are carefully documented and described, and a similar treatment is given to the physical, chemical, and biological oceanography of the Red Sea. It is likely that many readers will find the book valuable as a source and bibliography on these subjects, even if they are not interested in the hot brines per se. Similarly, the description of sampling and analytical techniques utilized in the various investigations is a useful summary of methodology for many areas of earth science.

The location of the brine basins and