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What Do We Mean by "Scientific?"*

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Abstract—There exists no simple and satisfactory definition of "science." Such terms as "scientific" are used for rhetorical effect rather than with descriptive accuracy. The virtues associated with science—reliability, for instance—stem from the functioning of the scientific community.

When a question is used as the title of a talk (Bauer, 1981), it is a reasonable assumption that the question is being put rhetorically: the speaker intends to provide the answer. But that is not exactly my intention here. I will say something about answers that have been suggested: but I shall maintain that the question remains open. I shall argue that no answer exists that is both short and empirically accurate: and none that is consensually accepted among those who discuss such matters. I shall also argue that the term "scientific" is itself most commonly used as a rhetorical device, to add force rather than substance to the point being pressed.

It is philosophers, of course, who have thought for the longest time about the nature of scientific knowledge. I was fortunate to hear, a few weeks ago, a splendid summary (Laudan, 1983) of that philosophical quest: Larry Laudan, himself a contemporary philosopher of science of note (Laudan, 1977, 1981), concluded that there is no epistemological criterion by which one can characterize science. Let me spend a little time on that, since the conclusion usually seems unpalatable to practising scientists when they understand its implications.

Until the middle-to-end of the 19th century, science was acknowledged to be characterized by the certainty of its knowledge. Philosophers, natural philosophers, scientists—and often the same individuals could properly be called any one of these—agreed that science equalled infallible knowledge. But that stance has proved to be untenable. For example, as science progresses, one can look back and note that the purportedly infallible science of yesterday has been replaced: making it plausible, indeed likely in the extreme, that today's science will also be found wanting in the future.

It is not just that theories change while certified facts and definite knowledge accumulate. Facts turn out to be slippery rather than graspable, and they change significantly as theories change—as our ways of looking at things change. Facts are theory-laden is how the philosophers put it (for example, Brown, 1979). Consider, for example, the "facts" about the structures and

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properties of atoms and nuclei: hardly had the existence of atoms become "hard fact" than the fact turned out not to be atoms as indivisible entities: nor were all atoms of a given element the same—not even in their chemical behavior. And what facts about elementary particles would one care to regard as infallibly established? As not subject to change in the future?

So philosophers came to realize that the facts and theories of science are not synonymous with certainty; and practising scientists and philosophers came more and more to go their separate ways, learning from one another less and less. As one result, practising scientists as a class still believe that science is synonymous with truth and certainty; a belief that humanists and social scientists commonly and pejoratively describe as "scientism."

Then came attempts to show that the scientific method (if not facts or theories) has some special virtue, leading progressively to greater certainty, closer to truth if not actually getting there. Those attempts continue, but face great difficulties: first, because there exists no agreement on what a satisfactory characterization of "the scientific method" might be: second, because the suggested attributes of "the scientific method" are not accurately descriptive of what scientists actually do: third, because none of the empirically accurate descriptions of what scientists do can be logically proven to have epistemological significance.

Consider the sort of things said about the scientific method: rational, impartial, strict regard for accuracy and controlled experiment, empirically based, looking to reproducibility and verification; careful, consistent, cautious, and so on. And those presumed attributes also define the popular stereotype of the good scientist, Martin Arrowsmith (Lewis, 1925)—careful, objective, disinterested, modest, shy of publicity, unconcerned with personal advancement or possessions, naively idealistic.

Then think about the way scientists describe the best examples of successful practice in science: creative, original, new; daring, surprising; splendidly correct; elegant. . . . Does work of that sort really come from carefully disinterested people obsessed with strict accuracy and reproducibility? For my part, I think it happens only rarely. I have known some first-rate scientists, and they were usually ready to ignore many results in favor of others about which they had hunches, ready to defend their theories vigorously despite the lack of sufficient proof, ready to jump intuitively to conclusions . . . and I have known a few Arrowsmiths, marvelously educated and conversant with the specialist literature, consulted by all on account of their judiciousness—who published hardly at all, and never anything of note. The Nobel prize went to Watson and Crick, not to the systematic, judicious, erudite Erwin Chargaff—which seemed unfair to a number of people, including Chargaff himself, who has written in very bitter tones about the manner in which Science nowadays is not as it ought to be (Chargaff, 1963, 1977, 1978).

Confidently will I wager to find examples of competent science that contradict any of the suggested attributes of "the scientific method." For instance, there is the use of hypotheses. Twenty-five years ago, having just begun postdoctoral research in chemistry, I happened to encounter a political scientist who told me that the scientific method consisted in setting up hypotheses and testing them. That shocked me: my own mentor in research had never said anything like that. His style had been, "Let's try this and see what happens"; or, "If we look at many substances by this technique, something interesting will turn up"; and the mentor, Bruno Breyer, attained a very respectable international reputation, primarily as the pioneer of an important technique in electrochemistry (Breyer & Bauer, 1963).

Is the scientific method a matter of careful experimental control of the variables? No, excellent science has come from those who are simple observers: taxonomists, paleontologists, field biologists, astronomers. Is the scientific method a matter of being systematic in collecting and categorizing? Hardly, unless one wishes to include philologists and historians and philatelists and numismatists and many others under the rubric of "science." And too many sciences are non-mathematical to permit rigorous quantification to serve as a criterion.

One hears it said that the scientific method consists in going where the data lead one; that it consists in discarding theories when the evidence is contradictory. But is there anyone here, anyone in the world of science, who has not put aside data that did not fit the purpose at hand? We all make judgments about which data to keep and use as meaningful, and which to discard as probably misleading; and those judgements, as Polanyi (1967) has forcefully argued, are largely tacit judgements which cannot be reduced to a formula derived from some precise specification of a scientific method.

I hope that I have aroused a suspicion that we use the terms "science" and "scientific" without knowing precisely what we mean. But we do use those terms, and I think some meaning does attach to them. Let me speculate about what that meaning is.

Some disciplines are universally agreed to be sciences—physics, for example, or chemistry, or biology. A chemist knows a good deal about such things as atoms and molecules, about their properties, about chemical reactions; he knows how to obtain more such information in reliable ways; he has useful theories; he knows how chemists work, and often he knows something of how chemistry has developed over the years and decades, even centuries. I think that a chemist also assumes that chemistry is an exemplar of "Science," and that "Science" has the same characteristics that he has learned to associate with chemistry. But the practical characteristics with which a chemist is most familiar are not all the matters of methodology and epistemology that concern the philosophers of science and the others who seek to understand "Science" as an entity in itself. Thus when a chemist (or any other practising scientist) talks about the characteristics of Science, he is talking about a different thing than the philosopher or historian. I suggest, for instance, that when a scientist describes a fact or theory as having been certainly established, he means a different sort of certainty—a more limited one—than the philosopher who discusses what "certainty" involves.

Even beyond that, not all chemists have the same notion of what "science" is, because different sorts of chemists have learned rather different things—and have learned rather different things to be important. I still remember the seminar given 30 years ago by one of my fellow students, a theoretician. His widely acclaimed dissertation consisted of bringing under consideration a previously neglected factor in the calculating of certain properties of molecules. His calculations gave results that differed from the measured values by a greater margin than did previous calculations: but his work was nevertheless seen as an advance, because he had made one of several previously neglected factors amenable to calculation, and everyone had faith that theory and experiment would eventually come to agreement when all the other significant factors had also been brought to mathematical heel. For the moment, however, experimental verification was simply not seen as of importance: the abilities to conceptualize, to quantify, and to solve technical problems of calculation were the significant "scientific" virtues. I, on the other hand, was seeking reliable values for the quantum yields of certain reactions. My ability to rationalize any given experimental result by a suggested reaction mechanism found no favor with my mentor, who reminded me that the essence of my job in "science" was to be sure of every aspect of technique and apparatus—I must achieve precise reproducibility, and precise concordance with previous results on model systems. Surely, then, I learned that accuracy and careful experimentation are paramount in "science," while my friend learned that theoretical understanding and mathematical virtuosity are paramount. Such differences, I suggest, occur in every sub-field and sub-sub-field of what we call "science," and lead every scientist to have a somewhat different notion of what science "actually" is.

So there are multiple meanings associated with the terms "science" and "scientific" and "scientific method" and "scientific knowledge." Most or all of those meanings have some truth to them, but no single one of them represents the whole truth, and, most important perhaps, all of those meanings are seriously erroneous and significantly misleading if they are used without qualification—if one speaks of "science" globally instead of making clear that one speaks of but one aspect of some part of science. But in practice this mistake of over-generalization is almost always made. We take our particular understanding of the sub-discipline in which we work, or of philosophy or of history, and generalize that as referring to "Science" in *toto.*

Analogous situations are common enough. We talk of "civil rights," and of "economic justice," and of "democracy," and of "patriotism," and so on; and there is general agreement that those are very worthwhile things. But when specific applications are proposed, it turns out that different people have rather different conceptions of what those words mean. To some, for example, "economic justice" means a flat rate of income tax, whereas to others it means a steeply graduated tax. Both sides talk of their proposals as embodying "justice" because that is universally seen as a good thing: it is a useful rhetorical device, to clothe one's personal opinions in generalities that have wide approval.

It seems to me that something similar occurs when we talk publicly about "Science." In our society, "Science" symbolizes man's power over Nature, and "Science" symbolizes Truth. Even those who attack "Science" believe those things: they attack Science for using its power incorrectly or unethically, not for being without that power; they attack Science for neglecting or rejecting some supposed truths, not for incorrectly claiming to embody truth. And so "science" and "scientific" are immensely powerful rhetorical and polemic devices, and they are so used—misused and abused.

I will hardly be contradicted, I take it, if I criticize such usage in the domain of everyday life and popular culture—when I point to the advertisements that seek to sell toothpaste by talking of "scientific proof' obtained through "scientific tests." (Note, incidentally, the power of the word. Would not the same sense be conveyed by simply talking of *tests* that *prove* something? No, the adjective "scientific" carries a supposed guarantee of certainty.) But I aim to take no such cheap shots: my target is this rhetorical use by *scientists*, on subjects in or close to science.

Consider the public arguments over the nature and place of so-called "creation science." Note, first, how the creationists seek intellectual respectability by attempting to bend the term "science" to their own ideological, political, social ends. But then ask, is the scientific community well served when leading figures in it claim that evolution is not a theory but a scientific fact? (Gould, 1981; Kornberg, 1981; Leakey, 1982; Sagan, n.d.) I think not. Leaving aside the technicality that the claim is incorrect, I deplore these attempts to invoke the authority of Science to settle arguments. After all, the public is often told that science is openminded and holds no theories as unquestionable—often by the same individuals who are thus dogmatic on specific issues.

Some wise things have been said and written about the proper role of scientists in public arguments. Alvin Weinberg (1972) pointed out that such arguments are often carried on as though purely technical or scientific issues were at stake, and that scientists on both sides of the issues attempt to invoke the authority of science on their side: when, in point of fact, the questions are actually ones of values—what Weinberg calls "trans-scientific" rather than scientific questions. Relevant examples are the debate between Oppenheimer and Teller about the feasibility of a crash program to construct a hydrogen bomb; or the controversy about the effects on human beings of exposure over a long time to very low levels of radiation; or, about the designing of an anti-missile missile-system. Weinberg argued cogently that the experts should restrict themselves to offering the best possible elucidation of the technical matters without coupling that to the question of what course of action the society should take. A similar point has been made by advocates of a so-called Science Court (Anonymous, 1976). Michael Polanyi (1967) put the matter thus: "Laymen normally accept the teachings of science not because they share its conception of reality, but because they submit to the authority of science. Hence, if they ever venture seriously to dissent from scientific opinion, a regular argument may not prove feasible. It will almost certainly prove impracticable when the question at issue is whether a certain set of evidence is to be taken seriously or not." (By "a regular argument," Polanyi meant the disciplined way in which technical disagreements within science are settled among experts—through controlled experiment, logical argument, refereeing of published contributions, and so on.)

That brings me to what is most germane to the present occasion: controversies about claimed anomalies which are different from the sorts of anomalous results that are routinely obtained and routinely handled within the various disciplines. For our present purpose, "anomalous phenomena" connotes claims usually made by individuals who are not necessarily expert in the relevant technical disciplines: and the claims themselves tend to be ones that seem quite incompatible with existing disciplinary knowledge. In the arguments about such claims, examples abound of ex *cathedra* statements designed to discredit a particular claim not through reasoned discussion but by invoking the authority of science. I will spare you the most outrageous instances in order to emphasize how insidiously we can fall into this sort of behavior.

In a quite moderately phrased, well referenced, soundly argued piece (Krupp, 1981) criticizing the pyramidologists, Atlantists, and gods-from-outer-spacers, I found the following sentence: "The archeologist tries. . . to avoid subjectivity and misinterpretation through systematic, scientific acquisition of information." I wondered what role "scientific" played there; other than to emphasize that archeologists are scientists, and their work scientific—to be believed, true.

The foreword (Kurtz, 1981) to a recent anthology debunking pseudo-science is entitled, "Believing the Unbelievable: The Scientific Response." And the text begins thus: "A dispassionate observer of the current scene can only be astonished by the rapid growth of bizarre belief in recent years among wide sectors of the public." The author, who happens not to be a dispassionate observer, and who is also not a scientist, seeks here to serve as a spokesman for Science; and talks of "the scientific outlook in which knowledge is based upon careful methods of inquiry and verification." Yet in that very article, he makes undocumented and unproven assertions about a host of subjects. I see here the same technique as that employed by the creationists: the use of the terms "science" and "scientific" as rhetorical devices to serve ends that are ideological, and therefore personal.

Now if science is anything at all, it surely is not a personal matter. I said earlier that no epistemological or methodological generalizations properly characterize science; now I shall suggest that some sociological principles do apply. I tried to present a paradox: that the best science—original, daring, elegant, and so on—is not the result of strict adherence to the scientific method as usually described; that those who do adhere to that method most usually produce only banalities and trivialities. The apparent paradox dissolves if one recognizes a distinction between individual behavior and organized or institutional behavior.

For the present purpose, this distinction parallels that made by historians and philosophers of science who noted that discovery and justification occur in different contexts. Individuals who do scientific work can employ any style they choose, and success or failure are not necessarily determined by that style: wild hunches sometimes pay off, and the most routine measuring of parameters sometimes results in a marvelously unexpected novelty; and the individuals themselves may be flamboyant, immodest, overambitious, unethical, and so forth—not at all Arrowsmithian. But the work that is done does not really become Science until it is acceptable to the scientific community. The tests applied before work is so accepted serve to weed out the unreliable, unsupported, and erroneous: other individuals, with other styles, examine what has been done—and what is finally accepted thereby becomes relatively independent of style, having run the gauntlet of several different styles along the way. And, of course, the scientific community agrees that, in the testing of candidates for entry into science, it is proper to employ logic, to demand clear evidence, to avoid unexplained contradiction, and to respect the validity of existing knowledge, methods, and theories. A criticism commonly made of Science is that it resists strikingly new claims. That, of course, is to be expected under the scheme just described. Candidates for entry must be judged against what has already been incorporated; the more strikingly the claimed novelty contradicts existing scientific knowledge, the more overwhelmingly must the novelty be supported by the evidence if it is to be communally accepted (Barber, 1961).

So what is accepted into science, and particularly what remains accepted for long periods of time, *seems as though* it has been obtained by work carried out objectively, impartially, carefully, reproducibly, and the rest. But the virtues reside in the system: of awarding of degrees, of refereeing, of awarding tenure—of using communally enforced judgements and standards. No individual working outside that system could produce science as good as he can produce within the system.

Unfortunately, we are not always conscious of that. It is easy for me to assume that my expertise as a chemist makes me also an expert scientist, and that I can be "scientific" about anything to which I turn my attention. And so it is easy for me to label "unscientific" or "pseudo-scientific" anything that appears to be badly done or wrong. This is really the point to which my talk was intended to lead. We have chosen to call ourselves the Society for Scientific Exploration, and I hope that we will be conscious of the virtues that the term "scientific" can connote: the proffering to others of evidence and ideas and approaches in a manner that can be convincing to those others on grounds of logic, careful examining of assumptions, documentation, and so on; and the willingness to have criticisms made in similar fashion, and to respond to those criticisms through the re-working of arguments and the re-examining of evidence and re-thinking of ideas and approaches.

The virtues I have just enumerated are characteristic not only of good science, but of good work in any intellectual endeavor—historical scholarship, for example. Marcello Truzzi (1971, 1977) has suggested that the investigation of anomalous phenomena ought to be referred to as proto-scientific, since it is analogous to what transpired in the early days of what are now established sciences. The point is a good one. Established disciplines, it seems to me, deal in three kinds of things: method, knowledge, and theory. Typically, research is carried on by accepting orthodoxy in two of those and seeking novelty in the third. Quantum theory, for example, was a respectable new theoretical venture because it was firmly grounded in well-established orthodox knowledge—the spectral distribution of black-body radiation—obtained by well-established orthodox spectroscopic method. By contrast, anomalous phenomena in our sense are not grounded in orthodoxy in any of the three aspects of method, knowledge, or theory. Typically, there is disagreement about the claimed knowledge: mistaken identifications, hoaxes, or fraud are often suggested as explanations. And typically the method depends initially on human testimony, which is given short shrift in the natural sciences and treated with the greatest skepticism in the social sciences. And, of course, orthodox theory typically offers no obvious explanation for anomalous phenomena.

So we must be clear about the fact that we are cut loose from the safeguards that are usual in established disciplines, and in the natural sciences in particular. That in our own disciplines our judgments and intuitions are likely to be quite good does not assure that they will be good over anomalous matters. In expressly pursuing the exploration of anomalies, we are attending to matters which the most relevant disciplines have—at least for the moment—judged to be so implausible as to be not worth pursuing; yet we stand ready to pursue them. Clearly, we cannot use our disciplinary knowledge, methods, and theories in the ways to which we have been trained. We will succeed, I believe, only if we can learn to deploy the virtues by which earlier proto-sciences became the well-established scientific disciplines of today, the virtues I enumerated earlier. We should seek to make the Society for Scientific Exploration an effective community, behaving institutionally as *though* we were individually impersonal, objective, openminded, and logical.

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