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Social Sciences, Arts and Sciences, and Occam's Razor:

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Wolfson's effort and that of his fellow lexicographers is hard, long-lasting, and laudable. Roughly 50 percent of the anthropologists listed as such in North America will welcome the work. Some 20 percent will not. (That includes this commentator.) The rest will be, as usual, undecided.

Wolfson's nomological science is what anthropologists call *nomothetic*; it concerns the abstract and the universal. The term is contrasted with *idiographic*, which concerns the concrete and the individual. History is idiographic; some anthropologists are too, but the majority look for rules and norm—which is the nomothetic approach.

Anthropologists have created another, related pair of contrastive terms that I will need in this assessment: *emic* and *etic*. *Emic* describes the use of the language and symbols of the society being investigated, in ways that make sense to the members of that society but not necessarily to outsiders such as anthropologists. *Etic* describes the use of a language, methods, and manner of presentation in ways that make sense to fellow social scientists but not necessarily to members of the society being studied.

There was a time, say, just before the sixties, when most anthropologists, that is, people on anthropology payrolls and people applying for grants, thought that they had to be scientists à la physicists and mathematicians in order to be proper social scientists. In some anthropology pep talk we still hear, not as embarrassedly as before, about the need for formalized systems, for whatever-metrics, for total objectivity, which can be achieved only through formal, artificial languages.

I submit that such a need does not exist because anthropology (and to a lesser degree sociology) does not have to emulate formal science at all. The objectivity of anthropology is now in question—not only in the sense of whether objectivity is possible but, even more, whether it is desirable. If we want to understand people—mostly other people—and their culture, our first concern should be to look for methods which provide the most exhaustive information and not for parsimony and elegance. As byproducts the latter are fine; but they are not, and should not be, the main targets of the ethnologist's quest. (I write as an anthropologist, of course.)

Anthropology, sociology, and economics are usually units in the arts and sciences or liberal arts divisions of universities. Along with many other anthropologists, I believe that the idiographic approach is just as important as the nomothetic (nomological) approach—and usually far more interesting and informative about the proper subject of anthropology, which is people. I also believe that this group of social sciences is better affiliated with the arts part of the arts and science establishment.

The strife between substantivists (idiographers) and formalists (nomologists) in economic anthropology is, of course, the strife between Wolfson's antinomologists and nomologists. It is a healthy conflict, but I don't think either side can or should win. There is a hidden agenda here: Nomologists know that many antinomologists are afraid of figures and formulas; idiographers *know* that nomologists know that—regardless of whether they, the idiographers, are in fact afraid of formulas. Many are not. Idiographers can and occasionally do sit down for a couple of weeks crashing the wall of inhibitions, to learn the really quite simple techniques and notations separating the images held by the wider public of nomologists and idiographers. Among those idiographers who are not scared of figures and formulas, and among those who are, some fight the nomologists for ethical and/or aesthetic convictions—which is what I am doing here at this moment. I contend that the promised lexicon says nothing about societies and cultures that cannot be said idiographically and nomothetically, because not all nomothetic propositions require formal languages to express them. Many simple explanations and *all* complex sociocultural explanations, be they nomothetic or idiographic, are better expressed in ordinary language, since formal languages tend either to bypass exceptions or to ignore them as unimportant. The entire Chomskyan creed rose and fell over a period of less than three decades on that very count.

Now the anthropologist Marvin Harris, reporting on his wife's making a hamburger, listed a chain of some twenty actions (i.e. minimal etic action components), each of which is traceable on a videotape. Wolfson's *P5* sneak preview features *F-bel-(p)*, which may be read as "*Individuals which have F believe that p.*" But how is he going to deal with the following field note of mine?

High-caste males in this village will avoid directly worshipping low-caste goddesses unless the benefit thought to accrue supersedes purity-pollution considerations; or the low-caste goddess has been sufficiently upcasted.

There are similar notes from about half a dozen anthropologists today working along this general template. If my own note is written down in one or more of Wolfson's primitives, it is bound to be *longer* in type count than the statement in ordinary language. The ordinary-language statement can be read idiographically by focusing on the clause "in this village"; or nomologically by tabulating or schematizing the "will avoid" (i.e., tend to avoid) phrase in the statement.

To compound the misery, Wolfson quotes Feyerabend of 1962, where he averred that the meanings of the terms constituting the axioms of a theory are totally dependent on that theory. But the mature Feyerabend rejected this erstwhile pedantry; the main thrust of his magnum opus in the same Minnesota series (*Against Method*) is the rejection of any distinction between observation (experience?) and theory:

*Abolish the distinction between a context of discovery and a context of justification, and disregard the related distinction between observational terms and theoretical terms. Neither distinction plays a role in scientific practice. Attempts to enforce them would have disastrous effects.*¹

1. P.K. Feyerabend, *Against Method*, Minnesota Studies in the Philosophy of Science (Minneapolis: University of Minnesota Press, 1970), p. 165.

When Wolfson rebukes Peter Winch for claiming that understanding social phenomena depends entirely on experiencing them, he is right by the mature Feyerabend's count; and when Rudner charged that Winch commits the "reproductive fallacy," Rudner was wrong—not because Winch does not do so but because *reproductive fallacy* is, like the psychiatric labels *schizophrenia* and *paranoia*, a term of persuasion and insult; it does not describe any state of affairs.

A science of human behavior is not necessarily nomological. The kind of science that analyzes human behavior may be idiographic or nomothetic; it is part of the arts and science program of advanced Western thinking traditions. Preference for the one does not abridge the scientific legitimacy of the other. Anthropology and sociology, if viewed as both humanities and social sciences, remain science regardless of their assignment to one or the other category, or to both. If formalized languages are the criterial instrument of science, then history, classical philology, and all other branches of learning which aggregate knowledge through and about the use of natural languages would not be sciences; but this is egregious nonsense.

Wolfson says that only the proof of the *impossibility* of a scientific (read formalized) treatment of social research would disenfranchise the nomologist. But this is not so. The nonnomologist is not ipso facto an antinomologist. By Feyerabend's motto "Everything goes," the idio-grapher leaves the nomologist be. Both share a wider domain—social science; neither has to invade or legislate the other domain.

If we grant that a scientific theory is a linguistic entity—which I do—then there must be no implicit or explicit exclusion of theoretical types, idiographic or nomological. Chomsky thought that linguistic utterances are preprimed messages generated by a transmitter and received by a receptor. His more dogmatic followers excluded consideration of the unpredictable, hence unformalizable, components of real language. Steve Tyler neatly put it when he wrote that "the transformationalists give us either a spurious account of speech or at best an account of spurious speech."² On the anthropological side, ethno-semanticists (e.g., Conklin, Frake, Goodenough) have constructed elegant and exhaustive charts generating kinship terminologies, exhausting the lexicosemantic extensions of kinship nomenclature. These are, however, the excrescences of the nomothetic doctrinarian; they have little to do with actual people and actual languages. Neither postulates nor axioms nor the resultant theorems exhaust or even tap the sociocultural universe.

Entitates non sint multiplicanda praeter necessitatem ("Do not multiply entity beyond necessity"): Occam's razor enjoins us not to introduce more items or arguments than are needed to explain something. It seems to me that Wolfson's effort violates this rule. If artificial languages are made in terms of natural languages, as Wolfson suggests, then the former must either be purely heuristic or redundant. In mathematical discourse, no natural-language interpretation need interrupt a

2. Steven Tyler, *The Said and the Unsaid: Mind, Meaning and Culture, Language, Thought, and Culture* (New York: Academic Press, 1978), p. 140.

chain of formal notations. But in the social sciences, including the special case of organizational behavior, this is not the case. For a *theory* of organizational behavior, which Wolfson's lexicon wants to aid, a formal language—even the bare seven primitives—might be one feasible aid. For the *analysis* of organizational behavior, however, this entire apparatus may be an unneeded additional entity.

Suppose a nomologist using Wolfson's primitives and an idiographer using natural language want to generate a theory for monastic behavior. In an analysis of monastic organizations (not in a *theory* of such organizations), not one but several natural languages are involved (Latin, Greek, Church Slavonic, Sanskrit, Tibetan, Chinese, etc.). Now terms denoting packets of monastic organization prove to be virtually untranslatable, say, between Tibetan and Russian Orthodox organizations. Although there is some institutional overlap, a terminological or lexical leveling fails; it provides some very superficial, impressionistic brackets, but it ignores substantial differences, as I showed long ago.³

In studying such monastic institutions the idiographer has no problem; he needs common sense and philological skills. What about a *theory* of monastic behavior in cross-cultural perspective? A general-systems approach doesn't work cross-culturally and never claimed to. But I submit that a nomological approach, using "primitives" or other tools, cannot do the job either: The notation would become too cumbersome and complex to cover the corpus that establishes a theory of organizational behavior—a theory shared by institutions with a recognizably common theme across widely separate cultural lines.

If social scientists feel squeamish about their scientific status, their attention should be drawn to Gödel's theorem. With a slight modification for the purpose of social science, it puts a damper on the scientific fervor of nomologists. The theorem: *A completely formalized system does not provide axioms that are provable within the system itself.* Basic axioms postulated by any notation or formalization will remain either ambiguous or incomplete. In order to have an interesting and important investigation of cultural behavior (of which organizational behavior is one case), the results of that investigation must be, at some point, counterintuitive; that is, unexpected results must occur. If a formalized language could describe counterintuitive processes, and if such a formalized presentation could be briefer and more parsimonious than the relevant presentations in natural languages, then I would cede superiority to such an auxiliary language. The seven primitives as I read them do not provide notations for the counterintuitive, and I do not see how they could. They therefore seem to limit the analysis of sociocultural events and sequences; they do not allow for innovations and mutations which account for the counterintuitive.

Wolfson complains about the multiple or equivocal meanings of terms used by social scientists resulting in "vast umbras and penumbras of unclarity." Fair enough, but the remedy lies in improving the natural-language diction of the social scientists, not in abolishing it or relegating it to some sort of secondary status. If a poet writes bad poetry, and if a powerful critic convinces him and the audience that it is bad poetry, the critic might recommend that the poet write better poetry but not that

3. *New Encyclopedia Britannica*, 4th ed., s.v. "monasticism."

he switch to mathematics. At least in cultural anthropology as practiced in North America, there is a keen awareness of terms being fuzzy and the need to reach some consensus on only one meaning of each term. This has been accomplished for many ineluctably woolly terms in use by the fathers of this recent science and by their less wary progeny. Thus *primitive* as the epithet of a society now means "small-scale, band-type society with simple technology not exceeding hunting and gathering or swidden agriculture." Each of the constituent terms is defined in natural language in the anthropologists' dictionaries—by which I mean the glossaries anthropologists append to their larger opera. Wolfson is of course right when he suggests that all sorts of ideological investments prevent or deflect the reduction of fuzzy terms to terms with single meanings; but again, the remedy lies in overcoming these investments and in creating sober terms with consensually defined meanings. When March and Simon, as quoted by Wolfson, worried about defining "formal organizations" like United States Steel or the country grocery store, their worry was incompletely informed: *Only* Euclidean mathematical and (theoretical) physical terms are definable; "formal organizations" are not. But they can be analyzed and described exhaustively; and in the social sciences, an exhaustive description including the exhibition of probable causal nexuses is all we can get by way of definition.

I am worried about Wolfson's sanguinity: that the definitional systems implied by or contained in the lexicon might be "a test-bed for infant theories in the social sciences . . . and aid in the further formalization and development of such theories." Formalization and development are not coordinate terms. Formalization arrests development. Like structuralism in French anthropology, it spells out, presents, or otherwise encompasses a single frame, or at best a number of immediately linked consecutive frames, in an ongoing process analogous to that in a filmstrip or videotape. Formalization quite literally arrests development. For a formal grid to cover a process rather than a stationary scene, it would have to change its formal terms at each consecutive node, which would again complicate the spelling far beyond any analysis in a natural language.

A social theorist, so Wolfson suggests, would start from scratch like a theoretical physicist rather than "start out in terms of humors or fluids, proclivities or tastes, as occurred in natural science before Newton, and as is too often the case in the social sciences now." No, sir, as is *too seldom* the case in the social sciences now; because too many social scientists still insist on aping—badly—the physical and natural scientist, Gödel and the mature Feyerabend notwithstanding. If the social scientist starts from scratch, he starts from nothing; in good scholastic language, *ex nihilo fit nihil*. The physicist starts out from totally different scratch. His building blocks are intellectual constructs to begin with, and many of the results he achieves are, by his own effort and admission, further intellectual constructs. We hope that the social scientist deals with people and societies, not with intellectual constructs. But let there be intellectual constructs for the social scientist: So long as they are ancillary to the real job on hand—which is understanding society (an intellectual construct) and societies (people), culture (an intellectual construct) and cultures

(people and their seen and unseen works)—they are welcome and essential heuristic devices in the social scientists' enterprise. But they must not become ends in themselves, overtly or covertly. Intellectual constructs hardly ever become ends in themselves overtly. They do not target on the study of people but turn into a display of esoteric skills.

This state of the art also brings about trivialization, cognitive redundancy, and ellipsis. Formalized statements tend to trivialize the sociocultural object, since what makes the object complex is outside the ken of a purely nomothetic thrust. There are, succinctly put, no laws for exceptions; but sociocultural life consists very largely of exceptions to rules either cherished or imagined or stipulated. There is cognitive redundancy when the formalization represents a system which has already been analyzed in a natural language; cognitive redundancy is the modern name for Occam's razor. I have talked about ellipsis at the beginning of this commentary: What obstructs an elegant formalization in the form of minor events, or side roads and bypasses, is a major part of the emic situation, which the etic statement cannot ignore without peril; and a formal lexicon, of course, is the most completely etic statement thinkable.

Though I am moderately opposed to Binswanger's *Lebensphilosophie* and the whole German philosophical anthropology (I regard it as bad philosophy and as no anthropology), I do go along with Binswanger's notion that all abstractions are transpositions and simplifications of reality. I feel that Wolfson's strenuous scheme goads toward a simplification of sociocultural facts—as do all formal taxonomies. If cultural anthropology is a holistic science, anthropologists must rid themselves of their craving for the inapplicable purity of the physical sciences and mathematics. The mathematical-physical model is as bad for anthropology as the medical model is for psychiatry. The ambiance of arts and sciences is wider than the well-informed groping of the cultural anthropologist, idiographic or nomothetic, as well as the scientist's abstraction and formalization. In fact, the boundary of arts and sciences lies exactly around these two provinces of possible thinking.

In fine, Godspeed to Wolfson's lexicon. I may even buy it, mainly in order to ostensively show to my students that there are many ways to do social science.