Robert Feleppa Philosophy

Emics, Etics, and Social Objectivity

Robert Feleppa
Wichita State University, robert.feleppa@wichita.edu

CURRENT ANTHROPOLOGY Vol. 27, No. 3, June 1986

Emics, Etics, and Social Objectivity

by Robert Feleppa

IN THE BELIEF THAT ETHNOCENTRISM is incompatible with objectivity, anthropologists try to purge their methodologies of elements that will yield imposed conceptions instead of the cultural items it is their ostensible task to discover. Consequently, emphasis is often put on forms of qualitative analysis designed to reveal subject conceptions, feelings, motives, etc., so as to avoid the suppression of culture-specific particularities that can result from efforts to subsume social phenomena under scientific law. Advocates of "emic" analysis seek a form of understanding that is, to some extent, like that which subjects have of themselves and their world. Inquirer viewpoints, they argue, must be circumscribed in efforts to discover other viewpoints embodied in diverse cultural backgrounds. They are thus wary of excessive reliance on "etic" analysis—given, roughly, in terms of inquirers' imported conceptions.

The typical attitude underlying emic analysis is Weberian: emics should complement etics, the idea being that anthropology seeks to unify emic perspectives into a systematic, comparative theory of culture based in large part on etic theoretical notions. Yet some view emics and etics as innately conflicting and emphasize one to the exclusion of the other: some minimize or ignore emic analysis in the belief that it inhibits the development of a systematic culture theory, while others willingly sacrifice theory for emic understanding of the culturally specific.

The idea that emics and etics are complementary is attractive, but the incompatibility that extremists see is not without basis. Moreover, I shall argue that it is impossible to "purify" even the emic component of *all* forms of imposition. While sympathetic to efforts to combine etics and emics, I think that to be attainable the aims of emic analysis must be reconsidered. It is particularly important that we confront W. V. Quine's idea of the indeterminacy of translation, which lends strong support to etic extremism by showing how pervasive imposition is.

ROBERT FELEPPA is Assistant Professor of Philosophy at Wichita State University (Wichita, Kans. 67208, U.S.A.). Born in 1946, he was educated at H. H. Lehman College, City University of New York (B.A., 1973) and at Washington University, St. Louis (M.A., 1977; Ph.D., 1978). He has been a visiting assistant professor at the University of Minnesota–Morris. His research interests are the philosophy of social science and of language and social and political philosophy. His publications include "Hermeneutic Interpretation and Scientific Truth" (*Philosophy of the Social Sciences* 11:53–64), "Translation as Rule-governed Behaviour" (*Philosophy of the Social Sciences* 12:1–31), "Kuhn, Popper, and the Normative Problem of Demarcation," in *Philosophy of Science and the Occult,* edited by Patrick Grim (Albany: State University of New York Press, 1982), and "On Reproducing Social Reality: A Reply to Harrison" (*Philosophy of the Social Sciences* 16[1]). The present paper was submitted in final form 14 VIII 85.

Reconciling observer interests and objectivity is a matter of long-standing controversy in social theory and is by no means limited to the etics/emics problem. Its solution rests largely on the resolution of central problems in anthropology concerning the nature of culture and the methodological character of the discipline. Moreover, it presupposes solutions to still more general problems of meaning, reference, truth, and rationality (see Bernstein 1983 on the confluence of diverse literatures on imposition/objectivity issues). Rather than take this all on here, I will tailor my discussion to relevant specifics.

Ethnography and ethnology require adequate concepts and units of analysis, and the problem of unit identification ranges from the very "culture-bearing unit" itself (Naroll 1964:283) on down. For some, anthropology's comparative aims motivate emic analysis. Kay (1970:23) argues that "one has to isolate comparable units before one can engage in reasonable comparison. Hence the emphasis in ethnoscience on emics, so called, the analysis of a cultural system or subsystem in its own terms as a precondition to the comparison of different systems." But this concern cuts both ways. If intercultural variation is manifest in units of comparison, statistical and other comparative results may be compromised, thus giving reason to *avoid* use of emicly defined notions.

We should expect the foundational nature of emics/etics questions to issue in the incommensurability problem of which Thomas Kuhn has warned us. Thus I will sort out some variations in usage of "etic" and "emic" and the key points of dispute that underlie them. I do not attempt to give a comprehensive survey of the literature (indeed, save for Harris's controversial 1976 work, none seems to exist [Fisher and Werner 1978:197]). Nor do I intend to beg any of the incommensurability questions raised by Kuhn (who, as I think Bernstein [1983:84] correctly argues, does not intend to cut off rational discussion in interparadigmatic controversies).

ETICS AND EMICS

In the sense derived by Pike from the phonetic-phonemic distinction, emic analysis is simply a method of determining symbolic significance by elicitation to determine complementarity and contrast. The idea is that just as phonemes can be determined by systematically varying phonetic features of an expression in conjunction with queries to subjects regarding resultant changes in meaning, so the defining criteria for some thing can be determined by systematic querying to reveal the properties that cannot be removed from it without changing the meaning of the expression designating it. Etic notions, on the other hand, involve only the interpreter's imported conceptual apparatus, just as recognition of phonetic contrasts involves only the conceptions and measuring techniques of the

phonologist. Phonemic contrast is described using a phonetic metalanguage, and this, some contend, bears out the general interdependence of emics and etics (Frake 1962:76; Goodenough 1964:37).

However, this methodological conception of the contrast is not predominant. For one thing, emic analysis has ramified in a variety of ways, notably in the widely employed method of componential analysis and in the frame analyses employed in ethnoscience. Also, a distinction viewed by some as entailed by the methodological contrast has come to define it, namely, that etics is bound up with the cross-culturally valid, emics with the culturally specific. Further, emic analysis has come to be identified in terms of the adoption of subjects' viewpoints by anthropologists (see, e.g., Goodenough 1970:109–10; Harris 1968:571; Frake 1962:76; Keesing 1972:303). These latter senses of the contrast have become largely divorced from that specific to the phonological model. Emic analysis is typically emphasized by inquirers who view themselves as part of the Boasian tradition in anthropology, guided by concerns to avoid excessively molding inquiry in terms of preconceived, "Western" notions. Thus Boas (1943:311) remarks that "if it is our serious purpose to understand the thoughts of a people, the whole analysis of experience must be based on their concepts, not ours." In Pike's (1964:55) own terms, "the emic analysis of the emic units of human behavior must analyze that behavior in reference to the manner in which native participants in that behavior react to their own behavior and to the behavior of their colleagues." As Kay (1970:23) argues, "The very provenience of the emic/etic distinction, namely phonology, should make clear that the guiding spirit of an emic approach is to rid oneself of *pre*conceptions about universal structures so that the data may be analyzed objectively to reveal the true universal structures."

At times "emic" is applied to the social phenomena themselves rather than the methods for their analysis and therefore equated with "untranslatable" (e.g., Triandis 1976:229–30). However, the typical view is that ethnography hinges on accurate description of emic phenomena. That is, while cultural significance is typically not universally shared, it is regarded as potentially, and necessarily, sharable.

Adoption of the subject's point of view is sometimes taken to mean the actual sharing of particular concepts and rules by inquirer and subject—as is manifest, for instance, in Goodenough's efforts in his study of the Trukese to use what he takes to be the fundamental "emic primitive" of the *corporation* (which concept he claims combines elements of property ownership and kinship in ways specific to their culture). He stresses complementarity, arguing that emic notions, once incorporated in the ethnographic descriptive apparatus for some culture, become part of an "etic kit" usable by other inquirers (1951; 1970:esp. 70–72, 108–12; cf. Oliver 1955). Exactly what concept sharing entails is not clear; at times Goodenough speaks as if there were conceptual identity between his ethnographic terms and Trukese terms, while at other times he speaks more guardedly, e.g., of defining descriptions "in terms of whatever criteria enabled me to distinguish among the entitlements and transactions in a manner *consistent* with the distinctions the people of Truk *seemed* to be making" (1970:79, my emphasis). This may explain how it is that he stresses the values of emic/etic analysis and the need to generate results in componential analysis that have "cognitive validity" or "psychological reality" while nonetheless viewing the components of kinship as *etic* (and perhaps lacking source-language counterparts). These concepts enable inquirers to use source-language terms correctly—i.e., as the subjects do.

There are also significant mentalistic commitments in many emic-oriented methodologies (not manifest in earlier emic approaches, as Burling [1969] and Durbin [1972] point out). Goodenough's work exhibits these, though again claims to analyze culture construed as "the form of things that people have in mind, their models for perceiving, relating, and otherwise interpreting them" alternate with more guarded aims, e.g., to probe "what, for lack of a better term, we must call the minds of our fellow men" (1964:36, 39). Also, the unconscious as well as the conscious mind is emphasized in ethnoscience and other Boasian approaches. This is manifest perhaps in Goodenough's reference to componential concepts for which there are no source-language analogues and is clearer in the work of other writers. For instance, Fisher and Werner (1978: 200) cite the phonological roots of the etics/emics distinction to argue that just as "the phonemic attitude" is detectable "in the unguarded speech judgments of naive speakers who have a complete control of their language in a practical sense *but have no rationalized consciously systematic knowledge of it,*" so emics must go beyond what subjects say or consciously think (cf. Sapir 1949:47). Indeed, for all his admonitions against imposition, Boas, in emphasizing the fundamental role of linguistic structure in culture and in noting the unconscious nature of the laws governing speech behavior in primitive societies, laid the groundwork for emphasis on the unconscious by others—notably Sapir, whom Fisher and Werner cite here.

However, other emic focal points have emerged, some in reaction to the formalism and mentalism of earlier ethnoscience—the concern being that these elements might reintroduce the very imposition problems about which Boas, Sapir, Whorf, and others worried and whose avoidance seemed one of the key motivations for emic analysis in the first place. For example, Frake (1977) warns about the dangers of excessive rigor in the "eliciting frames" used in recent fieldwork. He views these often elaborate question sets as blinding inquirers to the social context that gives significance to questions and answers, as well as encouraging "platonistic" attribution of alien, rigid formal structure to social realities that are less structured and more fluid. He favors a more flexible dramaturgical analysis which minimizes the import of predetermined structure in question frames and supplants emphasis on elicitation or prodding with concerns to find "query-rich settings" which generate emicly significant question sets and units of context specification (cf. Geertz 1976). Similar concerns motivate proponents of more recently flourishing schools of sociolinguistics and symbolic anthropology. (Indeed, Frake and others have developed strong misgivings about the very employment of the terms "etic" and "emic": Geertz [1976], for example, opts to define the contrast as "experience-near" and "experience-distant.")

Some of these difficulties are discussed by Watson (1981), who criticizes etic psychological-conflict models, which he himself has employed, for relying on alien psychoanalytic categories and emphasizing unconscious motivation in a way that led him to overlook the conscious and positive strategies of his (Guajiro) subjects in adapting to urban life. His etic model, he argues (pp. 453, 458), depicted them as "helpless or incompetent," passively and "automatically" reacting to conditions defined in (etic) terms that were alien to them. His later model reveals, he claims, that what he earlier took to be simply a poor understanding of the city (Maracaibo) instead reflected differing loci of identification, namely, the various neighborhoods in which they lived. Also, he contends that emic analysis showed him the positive aspects of their tribal background in providing stabilizing reference points, while his earlier model showed the background only as obstructive to adaptation. His emic methodology analyzes "spontaneously recalled personal data" provided in his subjects' answers "to open-ended questions that did not call for an organization of a response beyond the subject's immediate and authentic interests and orientation" (p. 465).

Another important point of dispute, and the one that is my primary concern here, is the *methodological role* of emic analysis—something which is central to the controversy surrounding the work of Marvin Harris. Harris propounds a materialist strategy that emphasizes economic and biological factors and

reacts to what he perceives as the theoretically stifling features of "idealistic" approaches. He is as critical of conscious as of unconscious mentalisms, and thus many of his criticisms are aimed as much at structuralism as they are at ethnoscience. Also, while much of his concern is with the ontological status of mental entities (conscious or unconscious) and the epistemological barriers their privacy engenders, his main objections concern efforts to make translation and emic concept use fundamental aspects of ethnography. (Thus it is that he groups sociolinguists such as Hymes under the broad "idealist" rubric as well.) He relies minimally on emic data, often using them in measuring levels of "mystification" (roughly in Marx's sense) in the cultures he studies (1968:chaps. 16–18, 20; 1976; 1979:chaps. 1 [esp. pp. 32–45], 7, 9).

Divergences of usage abound in this controversy. Harris wishes to divorce the emic from the mental and thus defines the emics/etics contrast in terms of the "locus of reality" of concepts and claims. Emic notions are those about which the subject is the final arbiter and which are such as *must* be determined by elicitation; etic notions are ones whose appropriateness is dictated by the inquirer. If elicitation indicates that a subject employs a term that translates into some target-language or metalanguage expression, that expression is *emic*. Thus he objects to Goodenough's idea that emic concepts feed the etic kit, becoming etic simply because they are or may be cross-culturally instantiated. Notions that are real for subjects, he contends, are emic even if they recur cross-culturally. He also insists, in distinct contrast to Fisher and Werner, that emic notions have explicit source-language vehicles. With such variations go frequent charges and countercharges of misuse of "emics" and "etics" (Harris 1976: 343; 1979:39–45; Fisher and Werner 1978:200; Goodenough 1970:113–14).

IMPOSITION AND PRAGMATIC CRITERIA OF ADEQUACY

The main issue is Harris's emphatic denial that emics should play the *central* role that ethnoscientists intend for it (1979:32; cf. 41):

> The test of the adequacy of etic accounts is simply their ability to generate scientifically productive theories about the causes of sociocultural differences and similarities. Rather than employ concepts that are necessarily real, meaningful, and appropriate from the native point of view, the observer is free to use alien categories and rules derived from the data language of science.

In keeping with the spirit of this passage, Harris criticizes idealist emics on pragmatic grounds, citing a range of what he takes to be innate atheoretic and antitheoretic tendencies, e.g., the introduction of inflexibility by limiting the analyst's fund of basic organizing principles, the trivialization of research, and the confusion of etic and emic categories. This latter flaw, he argues, leads to failure of ethnoscientific efforts at quantitative analysis, to compromise of inquirers' critical perspectives through unreflective acceptance of subject accounts, and to imposition on subjects, under the guise of emic categories, of what are really alien, etic notions. These points are salient in his critiques of cognitivist and structuralist emphasis on the unconscious.[1]

These latter points are emphasized by another cultural materialist, Marano (1982), who argues that the windigo (or witiko) psychosis—an inexplicable craving by members of certain Algonkian tribes for human flesh—results from a confusion of certain etic psychological categories, such as "obsessive cannibalistic compulsion," with the windigo concept. Manifest also here, and paralleled in structuralist strategies, he argues, is a tendency to ignore data and "to overpower a very poorly known Witiko phenomenon with our own intellectual creations" (pp. 394–95). Thus concerns with imposition of the familiar seem to motivate members of both idealist and materialist camps. Watson, evidently an "idealist" (despite the fact that he cites Harris in his initial discussion of etics and emics), views the drawbacks of psychoanalytic categorization as an argument in favor of emics over etics (and one would expect Frake to be in agreement on this), while Marano perceives them as bolstering an etic orientation.

In contrast, Fisher and Werner see Harris's restriction of emics to conscious cognition as intimately related to his tendency to tie idealist emics to the antitheoretical descriptivist tradition they reject (1978:200; cf. Kay 1970:23–24). They add that Harris's efforts to *oppose* etics and emics—by in effect equating the latter with "the confused"—prevents them from being theoretically productive complements (1978:203–4; cf. Harris 1975:160–61; 1968:578). Moreover, they view Harris's own aim of measuring the function and mystification of subject ideologies as compromised by his excessive concern with prediction of behavior (p. 204). In a related vein, Kay (1970:28–29), in explicit contrast to Harris, stresses the predictive power of ethnoscience, citing a predictively successful statistical study of decision making and residence by Geoghegan (1969). However, Harris, in both his 1968 (which these critics address) and his 1979 work, stresses the need for etic units here to avoid unwanted interpersonal and intercultural variations in units, pointing to the units of residence studies (e.g., community organization, family organization, marital residence) as particularly prone to emic/etic confusions (1979:49).

Some of the disagreement here may stem only from Harris's refusal to allow an emic notion to become an etic one simply in virtue of its cross-cultural instantiation. However, his critics are concerned with the foundational emphasis he gives etics (which Fisher and Werner view as tantamount to the "emics of scientific observers" [p. 202n]), and the very appeal he makes here to pragmatic considerations may be the deeper source of the problems they perceive. There seems a significant tension between efforts to predict in terms of observer categories and desires to reveal *cultural* content. Fisher and Werner themselves stress theoretical productivity in criticizing Harris, but they measure productivity partly in terms of *cognitive* yield. They worry that Harris may leave us without a warrantable account of *subject* beliefs and conceptions (1978:205–8). In their view, Harris's etics perpetrates a wholesale, ethnocentric imposition of the conceptions of Western science which obscures rather than reveals social reality (p. 204n).

However, if the imposition implicit in Harris's pragmatic appeals is the problem, it is not one that idealists easily escape. Although some may view emic analysis (given its presupposition of accurate translation) as free of interpreter imposition, the basis for this faith is unclear, especially if it presumes that it is possible to mirror the semantic-cultural substratum. Fisher and Werner (1978:201) quote Campbell (1975:1120) as follows: "All scientific knowing is indirect, presumptive, obliquely and incompletely corroborated at best. The language of science is subjective, provincial, approximative, and metaphoric, never the language of reality itself." However, a questionable notion of "objectivity" may be operative here, one that has been criticized by many authors from Hegel to Kuhn and Gadamer.

[1] Harris is also criticized for not defining emics in terms of contrastive relationships within the cultural-linguistic context (Durbin 1972:385; cf. Harris 1976:341–42). But his stress on the *epistemological* thrust of Pike's work, viz., on who is the final judge of appropriateness, rather than on the contrastive method itself fits into general trends, already noted, in the literature. He is also criticized, indeed, for sailing too close to the idealist wind, as his definition of emics does not conform to Bloomfield's behaviorist phonemics and related noncognitive notions of emics (Burling 1969:826n). However, I think the terms of the idealist/materialist controversy best illuminate my key concerns here.

In particular, this citation is reminiscent of what Rudner (1973:126–27) calls the "reproductive fallacy," assuming that "the function of science is to reproduce reality"—a fallacy presupposed, he contends, by thinkers (he here criticizes Peter Winch) who see intrinsic shortcomings in the fact that "science distorts through abstraction from physical reality" (cf. his 1966: chap. 4 and Winch 1958). Characterizing Winch's view—in terms that make clear its kinship to some of the views I discussed earlier—as that "the only way in which such a social science investigation can achieve understanding is via the adoption by the social scientist of the teleology of the observed," Rudner argues that this is to *insist* that social inquiry give "a *reproduction* of the condition it investigates." Citing Einstein (from a similar context) to the effect that a soup recipe need not taste like soup, he argues that Winch places social inquirers in an untenable position analogous to that of a meteorologist whose accounts of tornadoes must actually reproduce them (1973:127–28; cf. Dennett 1978:191; White 1963:207). Why must *social* descriptions reproduce what they describe when other types (scientific or otherwise) need not?

Rudner explicitly challenges only the idea that reproductive understanding is *necessary* for adequate social description, but he leaves one with doubts as to its very possibility. It may be wiser to seek a notion of emics that acknowledges the role of observer interests, and, indeed, a number of anthropologists have pursued this line of reasoning.

One such line of argument is afforded by the fact that emic analysis has not always been tied to mentalistic aims. For instance, Burling (1964) counsels continued use of componential analysis without making "cognitive validity" a criterion of adequacy; instead he prefers that componential constructs be granted fictional status, as existing in inquirers' heads only. Others have applied pragmatic criteria in redefining rather than rejecting cognitivist aims. In a recent discussion of relativism and comparativism in psychological anthropology, Kiefer (1977:107) argues (in notable contrast to Winch) that determinations of similarity and difference in cross-cultural concept identification, as well as criteria of explanatory adequacy, are dependent on observer interests and not "given" in social phenomena. Similar sentiments emerge in a review of anthropological studies of cognition by Ember (1977), who argues for comparativism over descriptivism on related grounds. These reviewers prefer to replace empirically prior metaphysical theses supporting both viewpoints—to the effect that cultures are or are not unique in certain respects—by more empirically open ones.

A similar pragmatism with regard to structuralist analysis is offered by Caws (1974), who contends that inquirers' "explanatory models" need not be identical with those of their subjects, arguing, indeed, that "it is the scientist's representational (i.e., explanatory) model, the theory he constructs to account for the data and their interrelation, that confers objective structure on the system." He highlights "confer," claiming that "it would be quite accurate to say that until the explanatory model was constructed the system had no objective structure" (p. 7). Arguing that directional relations such as "north of" are objective matters of fact even though they do not exist until a directional grid is imposed on nature, he contends that similarly the translation of source-language strings as "north of" produces something objectively attributable to subjects—and likewise, generally, for social relations. Similarly, Wallace (1970:152) counsels "that kinship terminologies may only be reckoning devices, like systems of weights and measures, whose utility depends more on internal coherence than on their fit with the social system."

There are, however, notable objections to these attitudes. Brown (1974*a*:429) takes Wallace to task, arguing: "Systems of weights and measures, like all tools, are designed to meet certain requirements extraneous to their own internal logic. One would not . . . weigh letters in fractions of tons, nor concrete blocks in multiples of ounces." He asks how it could be that "kin terminologies in their capacity as linguistic tools do not similarly 'fit' the reality they are used to describe." Related problems for Caws's pragmatic rendering of structuralism are raised by Hanson (1976), who sees the following absurd implications in Caws's view: (1) kinship systems might possess properties, such as a skewing effect in cross-cousin terms, that identical but unanalyzed systems did not; (2) misdescribed systems would possess structures they did not really have; and (3) analytic models would exist prior to the structures they described. He argues (p.324) that inquirers do not *confer* structures but *give formulations of* structures—which have "objective existence in the regularity of usage by native speakers" and exist prior to formulation by inquirers. (His emphasis on the behavioral level relates also to a rather unsympathetic attitude toward mentalism and toward the emic focus of ethnoscience; see Hanson and Martin 1973:205–6.)

THE INDETERMINACY OF TRANSLATION

Fisher and Werner (1978:207) see a significant underdetermination of ethnographic theory by observation, quoting Popper (1961:423) to the effect that "almost every statement we make transcends experience . . . we *are theorizing all the time*." They view this as indicating a weakness in Harris's emphasis on predictability and as necessitating efforts to delve into (perhaps unconscious) cognition. However, it seems that they must acknowledge the similar underdetermination of the anthropologist's theory of the "conceptual models" of social subjects that moves Goodenough (1964:36) to remark that beyond observational strategies, one "depends largely on the aesthetic criteria to which scientists and mathematicians customarily refer by the term 'elegance.' "

Yet these elements exacerbate concerns that imposition will compromise objectivity. Goodenough's reliance on formal criteria gives factors "internal" to the anthropological community a *constitutive* influence on ethnography. But why is this variety of imposition any more acceptable than others? Emic analysts rest a lot on translation—but can it reliably provide a check against "excessive" appeal to those interests?

There is a deep tension here (a species of what Kuhn calls the "essential tension" between science's imposition of structure on reality and its aim to reveal objective truth about it, but the differentia of this species from natural science are significant). It is best considered, I believe, in terms of Quine's idea of the indeterminacy of translation (1960: chap. 2; cf. 1970*b*, 1981*a*). This is but one of several attacks by Quine and others on traditional and recent theories of meaning, but it is of particular interest here in that it comprises an extended anthropological example designed to show translation's ontological limits—limits which stem from the failure of observation alone to reveal meaning and culture.

At the heart of this account is the following widely discussed illustration: If it is compatible with behavioral evidence, a linguist will be correct in translating a source-language term *gavagai* as the English receptor-language concrete count-noun "rabbit" on the basis of the equivalence of stimulus conditions for affirmation and denial of the sentences *Gavagai* and "Lo, a rabbit." However, nothing in this behavioral evidence precludes attribution of divergent grammars to source-language-speakers which produce translations of *gavagai* as a concrete mass-term, an abstract term, etc.; only the familiarity of these grammatical notions (or, what amounts to the same thing, the familiarity of a physical-object ontology) to the ethnographic audience supports the choice. But thus maximizing the familiarity of the subjects' conceptual scheme (within evidential constraints), Quine (1960:72) argues, gives no basis for claims to having *discovered* how they refer, for to do this is to "impute our sense of linguistic analogy unverifiably to the native mind."

Actually, this example only establishes the indeterminacy (or what Quine calls the "inscrutability") of term reference and term sense, but it gives a good idea of the pattern of the more general argument (against sentence meaning). Quine's general point is that behavioral evidence (even all possible such evidence) leaves linguists room for choice among translation manuals. Reason dictates that linguists settle for those translation manuals that seem to work and make for the most elegant system. One measure of a manual's formal adequacy is its giving translations that facilitate a manual user's activities, something which would seem well served by, among other things, employing grammatical notions which are as familiar to the user as possible, provided this does not conflict with some other important desideratum such as its systematic taxonomization of linguistic data. Another important criterion of adequacy is that the manual translate subjects as believing true what is obvious—the so-called principle of charity. Yet while there is no good reason to attribute odd though empirically adequate grammars, ontologies, beliefs, etc., to the ethnographic subject, thus maximizing agreement between subject and manual user does not, Quine contends, establish that the chosen manual, as opposed to one of its empirically equivalent rivals, expresses the truth of the matter. The selection procedure involves all manner of projections of what is (say, grammatically) familiar to the linguist or to the receptor-language-speaker. (Quine's main concern is with the familiarity and charity principles, rather than the simplicity principle, though see Quine 1961.) The "homeward thrust," as Quine calls it, of translators' extra-empirical criteria of validation precludes objective recovery of the meaning of source-language expressions by their receptor-language counterparts.

In the present context, the point is that quality-identifications that depend on translation cannot be legitimate items for description, whether formal, quantitative, or otherwise. Emic phenomena *are* generally untranslatable—where "translation" involves the factual recovery of meaning and not simply the facilitation of intercultural interaction. (I say "generally" because Quine does allow for a relatively small set of empirically determinate translations.) Typically, meanings and natural synonymy relations are not proper objects of scientific study.

From these reflections Quine draws rather drastic consequences for emic (and, generally, linguistic-oriented) anthropology: Except for limited cases in which observational criteria serve as a basis for translation, translational claims are not warrantably assertable as true—however reasonable the criteria for their selection—and this indeterminacy compromises the acceptability of any further ethnographic hypotheses that rest on them. At one point Quine quite explicitly draws this dire anthropological conclusion, comforting the anthropologist with the suggestion that "much can be determined by leaving language alone and observing non-verbal customs and taboos and artifacts," while limiting the role of determinate linguistic data to providing, perhaps, "a general and undirected measure of [linguistic] remoteness in the sheer difficulty of intertranslation" (Quine 1970*a*:16).

This is a rather heavily etic view of things, and it is somewhat ironic that Kay (1970:19) opens one of the defenses of idealist emics we have been considering by quoting Quine as follows: "The familiar material objects may not be all that is real, but they are admirable examples." Kay sees in Quine's tolerance of the possibility that other things may exist beside physical objects an admission of semantic determinacy. Drawing the parallel that "the informant's most careful statements about the nature of his world may not be all the ethnographic data, but they are admirable examples," Kay articulates the following aim for ethnosemantics: "to discover some part of the system of meanings by which people organize the world. The goal is the raw cognition if you will, but since the major realization of this cognition is in the words people speak, semantics is considered an integral part of ethnography" (cf. Quine 1960:3). Yet Kay emphasizes here as the basis for ethnoscience a point which Quine's various attacks on meaning aim to refute; Quine's view is that there is no underlying semantic fact of the matter to reveal in the study of "raw cognition." Contrary to Kay's assumption of the "psychic unity of mankind" (p. 26) and to Goodenough's belief in the existence of subject mental models for organizing experience and behavior, Quine sees radical interpersonal variability in underlying cognitive structure, likening the conditioning of individuals to linguistic uniformity to the shaping of hedges to similar forms. External uniformity belies diversity in twig structure, just as uniformity in verbal behavior belies interpersonally variant learning histories (Quine 1960:8). Kay and Goodenough embrace what Quine calls the "museum myth" that there is in some sense a subsisting realm of meanings or ideas that serves to account for the facts of natural synonymy (a thesis to which, indeed, even Harris's emic notions and strategies succumb). And Quine's strategy for showing this involves demonstrating the general absence of factual synonymy relations whose description would call for the positing of meanings.

Indeed, it seems to be precisely the favored position that Quine gives, as in the remark cited by Kay, to physical objects—or, better, to the physical sciences as embodying the ultimate parameters of belief—that creates the problem for translation. Like any empirical systematization, translation manuals are underdetermined by their data. However, their underdetermination in a significant sense goes beyond that of physical theories: different, indeed mutually incompatible, translation manuals can be applied, and hence different belief systems can be attributed, to some source-language community, but no discrimination can be made between the manuals on the basis of anything that physical theory can say about the arrangements of microparticles and other bodies. Translational underdetermination is *additional* to physical underdetermination, but since physical theory is an "ultimate parameter," this takes translation outside its bounds. There can be factual differences about meaning (or anything else) only if there are differences in physical macro- or microstructure. Thus translation is more than underdetermined, it is indeterminate: it has no facts to describe. Translators can (of course) produce "right" answers, but they generally cannot warrantably say that they are *true* (see Quine 1981*a*:23; 1981*b*:98).

The common thread of pragmatism notwithstanding, Quine's account is at odds with pragmatically defined emics—adding to the burdens already imposed by critics such as Brown. However, even Brown's Wittgensteinian analysis (see his 1974*b*, 1976) succumbs to Quine's critique, and even the fictionalism of Burling is rendered problematic in that Quine views the "hocus-pocus" status of semantic reference as eroding ethnography.

IS EMIC ANALYSIS POSSIBLE?

Is Quine's indeterminacy thesis cogent? Does it have the full consequences for linguistic and cognitive anthropology that he claims it has? The first of these questions is a matter of longstanding controversy, and it would be well beyond the scope of this essay to try to resolve it. One central point of contention has been whether Quine succeeds in demonstrating a significant difference between the underdetermination of physical theory by data (which he claims does not entail indeterminacy of truth) and indeterminacy of translation. I doubt that he does, since it seems to me that his thesis that physics is an ultimate parameter does not suffice to establish indeterminacy. His physicalist thesis amounts, it seems, only to the claim that "nothing happens in the world . . . without some redistribution of microphysical states," that is, that there is factual change

only if there is physical change (1981*a*:23). The consequent case against translation stems, he claims, from the fact that two rival translation manuals can be "physically equivalent," i.e., "both manuals are compatible with all the same distributions of states and relations over elementary particles" (1981*b*:98). But, it seems to me, physical equivalence entails that there is no factual basis for the choice between the manuals only if it can be shown that there are *changes* in the translations dictated by either manual *individually* without corresponding physical changes. But the radical-translation case seems to concern only difference in *interpretations*. What physically baseless change has been shown to occur within either manual? What force does the claim that factual change presupposes physical change have for translation?

I believe that there is reason to see a difference that confers a special epistemic status on translational claims. My main concern, however, is that this feature not force us to exclude them from empirical inquiry but simply cause us to recast their justificatory role, and I would maintain this even if Quine's physicalist thesis were shown to be adequate to establish indeterminacy. It is over the second question that I want to emphasize my disagreement with Quine. I shall offer another account of the *origin* of indeterminacy, one which has closer kinship than Quine's to the worries of Sapir, Boas, and Whorf, and, more important, an alternative account of the *import* of indeterminacy. What I say will not hinge on displacing Quine's main arguments for indeterminacy.

TRANSLATION AS CODIFICATION

The problem Quine sees for anthropology lies in the fact that he views translational correlations as "fallen hypotheses." They purport to have warranted truth value but generally do not because there is nothing for them to describe. Their failure to be *premises* for explanation and prediction eliminates them, and any derivative claims, from legitimate scientific inquiry. Yet while failure of X to do A can mean that X simply fails, it can also mean that X's function is not to do A at all, and I believe the latter option applies to translation. That is, much as Rudner asks why social inquiry must reproduce reality, I shall ask why translation must describe it.

Hypotheses, laws, and observation statements form only one component, which I shall call the "descriptive" one, of scientific theories. There are also rules of inference and hypothesis acceptance, as well as theoretical definitions, which make up the "prescriptive" component of theories;[2] and although the adequacy of these elements still hinges in large part on the empirical success of theories, they are not "confirmed" in the ways that descriptive items are. Their relationship to the observational basis for theories is different. I suggest that we might benefit from considering the kinship of translational correlations to these prescriptive elements by regarding indeterminacy problems as indicating not that translations fail in a descriptive function but that their proper function is prescriptive. I believe we thus avail translation and emic analysis of a place in a scientific anthropology (cf. Feleppa 1982).

The important distinguishing feature is that prescriptive claims are validated by a process of *codification,* significantly distinct from the process of theoretical confirmation and operative, for instance, in the genesis and development of grammars and legal codes. (My exemplars of codification are presented in Lewis's [1969] game-theoretic analysis of convention and in Goodman's [1973] and Rawls's [1971] accounts, respectively, of inductive and ethical norms.) I consider translations prescriptive insofar as the patterns of their justification are codificational. That is, much as dictionaries cull rules of usage, grammar, etc., from antecedent practice in the hope of facilitating communication and other activities involving the use of that language, a translation manual primarily serves to facilitate coordination of intercultural activities and to expedite social inquiry insofar as it is a special sort of codification that serves similar aims, only now across three fairly distinct communities: the source and receptor communities and the community of social inquirers. Enhanced communication and related interaction are achieved in both cases. Receptor-language-speakers are enabled to comply with source-language conventions in virtue of their compliance with the dictates of a translation manual.

The crux of the difference is this: Translations are justified insofar as they remain in what Rawls calls "reflective equilibrium" with an ongoing and changing set of linguistic behaviors. The rules govern linguistic behavior, yet sufficiently broad or significant changes in usage in the source- or the receptor-language community (whether through conscious efforts at redefinition or neologism or through more unguided linguistic variation) can force modification of the rules. Linguistic rules (including translational ones) typically are not measured by their correct description of linguistic behavior and for that reason are best not viewed as hypotheses. Also, translations share with rules, and not with descriptive hypotheses, the important logical feature of being *violable* without being thereby *abridged* (as opposed to the mere refutability-in-principle one might demand of scientific hypotheses). This logical difference parallels the important difference in the manner in which rules as opposed to hypotheses are validated, and thus I am inclined to treat translational correlations as themselves rules, even though they can be stated as easily in descriptive as in prescriptive form.

Codification improves the functioning of an existing set of conventions by increasing the degree to which the expectations of speakers are enhanced and optimal coordination equilibria achieved. Users are rationally justified in following a manual's translational prescriptions only if they are reasonably sure that source-language-speakers will conform to certain conventions (whether explicitly acknowledged or not) in such a way as to fulfill their expectations. The important difference is that the conventions that source-language-speakers follow are clearly not the translational rules that manual users follow. Assuming for the sake of argument that the source community has a fairly well-codified language (and there is no reason to insist on this), what speakers follow is the codified rules of linguistic usage of that language. The translation manual allows receptor-language-speakers optimally to conform to source-language rules and reap all the practical benefits thereof without *consciously* following those rules. Thus translational codification is a more complex affair, but otherwise the basic dynamics are the same. While it is responsive to earlier established practices, the manual's structure is partly dictated by what facilitates the various tasks of manual users—it does not evidently describe preexisting semantic isomorphisms. As Quine concurs, the existence of such things, construed as *natural* objects or relations existing beneath the behavioral surface, is unsubstantiated.

The prescriptive character of translation is also reflected in the anthropological community's ethnographic role: Successful translation also facilitates the anthropologist's efforts to "get along" in professional-community activities of theory construction. Inquirers bring to the field a body of linguistic and other theoretical notions that have already met with success and for that reason are rationally preferable. That is, they operate with

[2] My rough grouping of theoretical definitions with rules of acceptance as "prescriptive" overlooks the fact that the former but not the latter occur at the same logical level as law statements in explanation. (Rules of acceptance refer to explanatory inferences, while definitions are parts of such inferences.) However, I think that this is permissible given my stress on relations between claims and their evidential support. Also, I don't think that I fall prey to criticisms (raised, e.g., in Stich and Nisbett 1980) regarding the grounding of reflective-equilibrium accounts of ethical and rational norms in the intuitions of competent practitioners.

a set of criteria of adequacy which dictates that, within the constraints of empirical adequacy, familiar grammatical concepts are to be used. This imported scheme comprises the basic elements of theory construction, and some degree of general agreement on them is essential for theoretical success. Yet such concepts and criteria are the products of codification: they are subject to revision, in the light of ongoing practice, and can be altered or violated at any time, without abridgment of procedural rules. Linguists are obliged to adhere to such conventions owing to the practical gains that stem from fulfilling the expectations and facilitating the work of others who seek to understand and incorporate their results.

The important point is that it is neither surprising nor perplexing that receptor-language or anthropological-community practices have a structural impact on a translation manual, since its purpose is now seen to involve "blending" receptor-language conventions with source-language ones. Also, being prescriptive does not make translation a special case, and thus this alone provides no basis for any contrast between the scientific and the emic: we need not accept the consequences Quine draws for linguistic-oriented anthropology. Though the status of translations as rules may provide a basis for following Quine in denying them truth value, they still have an empirically legitimate role, akin to that of technical definitions and rules of inference. They are among the dictates of what *ought* to be done in order to conduct social inquiry, although they are, in effect, part directives and part a sort of instrument for data gathering. The fact that, as parts of codifications, they are on occasion measured by the compliance with them of actual behaviors (at those times when we are inclined to amend the rule rather than simply regard the noncompliant behavior as deviant) and the fact that their validation involves determining rules that to a great degree do accord with actual subject behavior can lead one to think that translations are descriptions of something (existing or subsisting "within" the source language or "between" it and the receptor language). But they are not, and they need not be viewed as performing a descriptive function in anthropological theory. And recognizing this makes possible the avoidance of a number of needless perplexities concerning translational and ethnographic objectivity.

That there is a definitive answer to the question "What does the subject mean?" Quine does not call into question. Rather, he challenges the idea that there is a definitive answer to the question "What does the subject *really* (or *in fact*) mean?" I share both attitudes, but rather than see indeterminacy as undermining the key translational *premises* of linguistic ethnography I view translations as answering questions about the analytic framework for posing and answering factual questions.[3] There is no answer to the question whether emic units are *really* emic, but neither is there reason to deny them a place in scientific anthropology.

WHITHER EMICS?

I hope to relieve some of the "essential tension" between pragmatism, with its implicit commitment to imposition, and objectivity by eliminating the expectation that the meaning for only *one* party to the coordinative effort of translation must be expressed in the idiom of another. I shall apply this account to some of the particulars of the etics/emics controversy.

Emic analysis surely involves the demand that (1) the ethnographer develop (or learn) another symbol system, different from those familiar to anthropologists or to receptor-language-speakers, in order to account for the significance of source-language expressions or of any extralinguistic entities which are construed symbolically by source-language-speakers. But it also typically involves more than this: for one thing, it is usually taken to entail that (2) the symbol systems, meanings, rules, etc., of the source culture thus recovered be "really" those of the culture's members. Further, as we have seen, it is said to entail that (3) these very source-culture notions be somehow employed by the ethnographer in explanation. My view is that 1 is a reasonable demand but 2 and 3, even though they rest on legitimate concerns for descriptive accuracy, are not. These two demands are best eschewed unless it can be shown that they can be fulfilled, and my account provides no basis for considering success in translation such a demonstration. Perhaps some basis for belief in the "psychic unity" of mankind can be found, but I do not think it is provided simply by our success in communication and translation. Rejecting 3 is not tantamount to rejecting emic analysis if we define "emic analysis" in purely *methodological* terms of the use by inquirers of expressions that serve also as correct receptor-language translations of source-language expressions. (Perhaps a similar account can be given of "implicit" emic notions, but I will not try to deal with that difficult problem here.) In addition, emic analysis so defined suffers from no special conceptual problems that could provide an a priori basis for thinking that it will stifle theorizing or that it has no place in the accounting of facts. There is a place in scientific anthropology for the translations on which emic analysis is based. (I shall not present a case for the *necessity* of extensive, or even limited, emic analysis.)

For all his mentalistic remarks, Goodenough's initial definition of culture is not as committal: it simply makes learning language and culture a matter of learning rules that enable one to behave in ways acceptable to native speakers (1964:36; 1970:101, 110–11). Of course Goodenough intends to say more than this: his reasons for emphasizing getting along stem from a belief that something is overlooked in ethnographies which aim only to describe objective fact, and he views learning to get along as the key to maintaining a referential connection between an ethnography and the mental realm of culture it describes (see, e.g., his 1970:110–11). However, the status of scheme recovery as an emic criterion of adequacy is challenged by the problems delineated above (and, indeed, some of Goodenough's own formulations of his mentalistic theses are guarded). Also challenged is the idea that culture *is* a set of mental items. What culture is depends on what adequate anthropological schemes say it is: implicit in this definition may be the very exclusion of materialist viewpoints that Harris decries.

Perhaps we can say that simply enabling conformity to source-culture standards of appropriateness is enough. There is significant convergence between my view and Burling's that *all* one can hope for is something that enables manual users to get along with the source-culture's members, as well as with the members of the anthropological community, who place their own demands on the translation manual's and ethnography's character. However, I think I eliminate the appearance that a significant *concession* has been made: what we have is not "hocus-pocus" but a variety of a perfectly respectable mode of theory formulation. If it is asked why one should bother to add Goodenough's criterion of social conformity, if not to achieve his mentalistic aims, I believe other potential advantages can be cited, such as enrichment of the empirical base and the production of warranted and interesting results

[3] The adequacy of translational rules still hinges on compliance with them of certain states of affairs—even though as prescriptions they can be violated without being abridged. Codifications still answer, in their way, to what Quine calls the "tribunal of sense experience." Thus in referring to framework questions here, I do not mean to subscribe either to the logical-positivist thesis that framework choices are purely pragmatic (see Carnap 1950) or to the idea that some beliefs (so-called analytic ones) are true in virtue of meaning only—views that Quine (1976*a*) sees as bound up with the semantic assumptions that his indeterminacy thesis attacks.

not otherwise attainable. As for Frake's and Watson's concerns with context, I believe what I propose at least partly captures their concern that the source society's practices have structural impact on the questions asked by ethnographers and on the theories that result. We cannot "escape" imposition of the familiar, even with the most ardent effort to understand source-culture remarks and contexts. But an important distinction remains, I think, between analytic frameworks such as frames, whose predetermined procedures are highly selective of data types, and analytic strategies that involve more flexibility in adapting frameworks to cultural context. What Watson discovered through his emic strategies was that his subjects' behavior was interpretable, with empirical warrant, in ways that provide a plausible account of the resources available to the Guajiro in adapting to urban life—discoveries not possible, perhaps, within the constraints of a psychoanalytic model. The prescriptive character of his methodological assumptions and translational base notwithstanding, Watson was able to *discover* things that were not simply the logical entailments of these choices. Theoretical fruitfulness—measured in terms of testable answers to interesting questions—is the potential virtue. There are other frameworks, importable from one's "Western" background, that can be used to give satisfying accounts, but the special character of translation—particularly, the different compliance relationship involved in the validation of translations as opposed to hypotheses—precludes our having empirical warrant for *identifying* inquirer and subject frameworks.

Translation does not *reflect* preexisting structure, it *creates* a structure. And while this structure is causally connected, via its ties to observation, to source-language structure, observer and receptor-language community interests have a constitutive impact on it, since translation must serve those interests. Translation, like other forms of codification, *transforms* what it touches. But this alone entails no "descriptive distortion," since what occurs is the creation of a framework for description. (Moreover, these structures are not of the problematic "underlying" variety which Davidson [1973] rightly criticizes.)

Thus I agree with Harris that etic analysis does not depend on emic analysis, particularly where freedom from all "interpreter interference" is sought. *Both* etic and emic analysis depend on fruitfulness, and while this may speak, on occasion, in favor of emic analysis, it may favor "purely etic" analysis in other contexts. It is a pragmatically oriented pluralism that I advocate. All the schemes used to account for cultural behavior are in an important sense "those of the subjects"—and while some schemes, the emic ones, place additional demands upon themselves to employ expressions that adequately translate source-language expressions, this confers on them no special identity relation to source-language schemes.

While my rejection of the idea that the teleology of the observed *must* be adopted by inquirers or that social inquiry *must* be extensively concerned with cognition echoes Harris's complaints about the exclusionary attitude of idealists toward materialist paradigms, I do not share his evident skepticism about the potential yield of cognitive approaches. I see no reason that pragmatic defenses are not equally applicable, at least in principle, to the idealist disciplines—as Ember, Kiefer, and (in a more fictionalist vein) Burling contend. And if these are feasible, then there is no reason to think pragmatism will necessarily count against using emic units in quantitative analysis. Moreover, notions of "psychological reality" have themselves come under pragmatic reconstrual of late (see, e.g., Bresnan 1978:58–59), and this makes it difficult to say, solely on the basis of the considerations adduced here, that they are inherently flawed. In questioning Goodenough's mentalism, I challenge the criteria he offers for validating his claims. I do not deny that systematic unification of elements of, say, cognitive psychology, psycholinguistics, and cognitive anthropology might validate beliefs about psychological reality (see also Wallace 1965, Romney and D'Andrade 1964).

But what of the antipragmatic criticisms of Hanson and Brown? Is there no check on the impositions inquirers can make? Don't the practices of subjects somehow serve to define these limits? I believe that my account of things goes some way toward meeting these objections. I agree, as one reasonably should, that there is antecedent structure implicit in the behavior of ethnographic subjects, i.e., conventional behavior patterns (regardless of their degree of codification or reflective grasp by source-language-speakers) which are proper objects of description. But in order to establish descriptive hypotheses, I claim, the anthropologist must perform certain translational, prescriptive theory tasks in the process of establishing what Caws (1974:9) calls the "boundary conditions of his work." Many of these boundary conditions are fairly well established, before the particular fieldwork is done, by professional training. Yet it is also the case, as Caws puts it, that "inside the boundaries [the ethnographer] becomes, as the physical scientist does not, a participant in the determination of the structure he studies." Caws continues (pp. 9–10; cf. 1976):

> And this is because the structure was, in the first place, a product of minds like his own, and will continue in being only if sustained by such minds; by taking it as an object of inquiry he has lent his own being to it; future investigators who seek to understand it can reasonably be expected to take note of his conclusions as an integral part of the data for their own work. A society is, in the last analysis, nothing except what is said and thought about it, by those who observe it as well as by those who compose it.

With all this I agree, and I take it to be sustained by the analysis I have given. (Caws does not seem to view translation in the minimal way I do, but this divergence is not critical.) Once translation of, say, direction terms is successfully done (as measured, say, by navigational success), genuine facts about the source society's views of navigation—such as their means of deteremining direction—may be uncovered. There may be some arbitrariness and interest-relativity in the projection of certain concepts, but facts "not of the inquirer's making" are derivable from the behavior of the ethnographic subjects. And the objective loss is no more than that involved in theoretical definition in physics (i.e., none at all). At this level, surely, factually determinate error is possible. As for the prescriptive level, here too error is perfectly possible. (The similarities between codification and descriptive theory should not be overlooked; see n.3.) Should a broad change in the usage of the term translated as "seaward" occur in the source language, this must have an impact on the translation manual, whereas occasional deviations would not force this. Similarly important are changes in target-language usage and shifts in theoretical paradigm. For much the same reasons, the dangers of etic/emic contrasts' compromising units of comparison can be met with methodological care and rigor.

The resultant explanatory model or account cannot be said to succeed or fail in revealing or mirroring structures (whether they be conscious or unconscious, public or private). A good deal of the structure of one's explanation can be dictated by "internal" constraints, while still saying things pertinent, in virtue of the efforts made at translation, to the description of the source culture. Thus Caws's contention that the structure the anthropologist derives is also "conferred" is compatible with Hanson's claim that the structures of behavior exist prior to translation—though I would prefer to say that it is patterns of practice that precede translational codification. The dispute is resolved if one drops the "natural" presupposition that social scientists describe the components and relations of abstract semantic (or social) structures in doing linguistic analyses such as translation, kinship typologies, etc. Under my reconstruction, it is clearer why inquirer interests have the "constitutive"

effect Caws claims they do on the resulting system. (Indeed, while Hanson objects vigorously to this view, he may well be quite sympathetic to my related thesis. Hanson and Martin [1973:205] remark: "Analytic rules . . . are tools or codes which enable one to select proper behaviour in particular circumstances." They reject the idea that an emic, or what they call "internal," understanding can be expressed by any set of ethnographic inscriptions, though it can be *achieved* by an inquirer who becomes adept at getting along. What differences there are between their view and mine lie in their evident allegiance to Ryle's ordinary-language philosophy, which is in certain key respects at odds with Quinean indeterminacy—though my reconstruction may soften some of the contrasts.)

In summary, my main contentions are these: (1) Translation is distinct from description in virtue of being subject to codificational patterns of justification. (2) It thus expresses no facts but can have a place in the framework of fact expression. (3) Emic units of analysis, whose determination hinges directly on translation, cannot warrantably be shown really or factually to exist or subsist in the minds or discourse of source-language-speakers, but (4) they too can have a place in the framework of scientific, descriptive anthropology.

This is not to say that the question of redefining "emics" is fully answered. I have expressed various caveats about the limitations of my proposed solutions, and I should indicate in closing what tasks I think remain. Pivotal here is a more thorough analysis of what talking *about* culture entails, since we have seen several authors call for emic analysis to provide for referential ties between ethnographic description and culture—ties perceived as missing, say, in Harris's concern with the description and prediction of *behavior*. However, the interdependence of culture concepts and particular theoretical paradigms entails, I think, dealing with this matter on a paradigm-by-paradigm basis—something which is clearly beyond my present scope. Not to dodge this important issue altogether, I offer the following considerations: Anthropology has as an important aim the revelation of feasible ways of organizing experience and the social world that are different from those with which we are familiar. And emic analysis would seem suited to this aim. Moreover, it is hard to see how one can find good reason to rule out the full and varied range of "emic" or "emic/etic" approaches without looking at them in detail. Earlier I remarked (actually turning a well-known phrase of Quine's) that what a culture is is what an adequate account says it is, and I see no wholesale way of judging the adequacy of all these approaches. (Of course, we cannot make this point too glibly, since we have seen questions of what measures "adequacy" so intimately tied to central points of etic/emic controversy.) But unless some generic, intrinsic difficulty for emic analysis is revealed, promoting a pluralism of models seems, as I remarked earlier, the preferable route to take—if demonstrable explanatory or heuristic gains result.

Comments

by ROGER F. GIBSON, JR.
Department of Philosophy, Washington University, St. Louis, Mo. 63130, U.S.A. 5 XI 85

Feleppa's central topic is the etic/emic controversy. The first few pages of his discussion reveal just how muddied these waters are. It is regrettable, therefore, that he muddies them still further by introducing Quine's theses of inscrutability of reference and indeterminacy of translation. First, he misidentifies the "origin" of Quine's indeterminacy thesis as Quine's commitment to physicalism; second, he erroneously concludes that Quine does not succeed in differentiating indeterminacy of translation from underdetermination of physical theory; third, he appears both to reject and to accept Quine's indeterminacy thesis. This last point is explained, perhaps, by the fact that Feleppa agrees with Quine that most utterances about meaning and reference lack truth values but disagrees with what he (erroneously) takes to be Quine's explanation for this (i.e., Quine's physicalism). According to Feleppa, such utterances lack truth values because, contrary to appearances, they are prescriptive and not descriptive. If this is Feleppa's position, then it is puzzling why "it is particularly important that we confront W. V. Quine's idea of the indeterminacy of translation," especially since he correctly claims later that his response to the etic/emic controversy "will not hinge on displacing Quine's main arguments for indeterminacy." Why, then, include all this on Quine's controversial views regarding inscrutability, indeterminacy, and underdetermination?

Feleppa argues for "a pragmatically oriented pluralism" according to which "all the schemes used to account for cultural behavior are in an important sense 'those of the subjects'—and while some schemes, the emic ones, place additional demands upon themselves to employ expressions that adequately translate source-language expressions, this confers no special identity relation on source-language schemes." He defines " 'emic analysis' in purely *methodological* terms of the use by inquirers of expressions that serve also as correct receptor-language translations of source-language expressions." The stringency of this "additional demand" depends on how the word "correct" is to be understood in Feleppa's definition of "emic analysis." Does "correct" mean intensionally synonymous, or does it merely mean pragmatically justified? Feleppa does not say, but we may assume from other things he says that his intended meaning is closer to the latter than to the former. But, if so, then his notion of emic analysis is a wolf in sheep's clothing, for what is the difference between such nominally "emic" analysis and etic analysis? After all, *both* kinds of analysis impose alien structures on the culture/language being studied.

Feleppa circumvents this type of criticism by claiming that translation relations between source- and receptor-languages are not descriptive; rather, they are, he insists, prescriptive. If so, then the apparent conflict between etic and emic analyses is an illusion: yes, both kinds of analysis impose structure on the source-language, but neither kind is descriptive of source-language users' beliefs. Furthermore, both kinds of analyses are justified "codificationally."

But it is just this distinction between justification by empirical confirmation and justification by codification, justification of hypotheses and justification of rules, that needs clarifying in Feleppa's account. If Duhemian-Quinean holism is true—if it is true that any individual sentence of a theory may be held true come what may because it is theories as wholes rather than individual sentences which confront the tribunal of sense experience—it is useless to insist, as Feleppa does, that the characteristic difference between the two is that hypotheses contrary to experience are refuted but rules contrary to behavior remain unabridged. One is reminded here, too, of Quine's (1976*b*) discussion of legislative and discursive postulation.

by PAUL A. ROTH
Department of Philosophy, University of Missouri–St. Louis, St. Louis, Mo. 63121, U.S.A. 7 XI 85

How is one to know whether or not putatively emic categories are genuinely such? Feleppa's claim is that the process of translation precludes answering this question in a way fully satisfactory to those partial to emics. Translation requires a holistic approach; translation rules must be "in place" before most conversation can proceed. But once discourse is possible, what could establish that this is due to emic analysis and not, for

example, to propitious imposition? However, Feleppa concludes only that an emic analysis purified of all imposition is impossible.

Yet Feleppa's remark that "we cannot 'escape' imposition of the familiar" cuts deeper than he appreciates, and its implications for the methodological dispute are more radical than he acknowledges. Translation, as he notes, "*creates* a structure"; given this fact, however, it follows that interpretation must proceed via an accommodation of the behavior and utterances of others to a structure. Any notion of meaning as some additional free-floating product to which we might also adjust translation proves empirically empty. (For a full Quinean analysis of the indeterminacy thesis, see Gibson 1982:64–95.)

I suggest that the true import of Quine's analysis for the controversy which Feleppa surveys is, in fact, to reveal it as a pseudoproblem. (This was the positivists' term for problems for which no empirical evidence *could* exist by which to resolve them.) As urged above, *all* cases of translation are matters of imposition, for how are we to understand anyone—ourselves or strangers—except in terms of categories that make sense to use or are extensions of some that do? In cases of "successful" translation there is no *empirical* distinction between alleged imposition and purported insight (details of my views are found in Roth 1985).

But surely, someone might protest, fieldwork reveals the existence of people with views very different from our own, and we certainly can, and sometimes do, learn to understand (and perhaps accept) previously alien perspectives. My reply is that understanding just means that we have incorporated alien utterances and behaviors into categories comprehensible to us; again, there is no distinguishing here between imposition and discovery. We cannot reasonably assume that the acquisition of new perspectives involves *complete* abandonment of the antecedently familiar, for the old notions provide the only general framework for understanding that we possess. It is this perpetual epistemological dependence on the antecedently familiar which makes it pointless to attempt to distinguish between imposition of the familiar and a lifting of the cultural veil.

How, then, to do justice to the genuine concerns regarding cultural differences which underlie the pseudoproblem posed by the emic/etic controversy? Following a suggestion by Rorty (1982: 198), I would argue that the choice of methodology (e.g., choosing between a vocabulary which attempts to reflect interesting variations in behavior and a vocabulary that will help predict what these human objects will do) is a *moral* and pragmatic one. If we divest ourselves of artificially rigid notions of what it means to construct an explanation (which is so often tied to being able to predict) and an equally untenable fantasy that translation discovers rather than creates what we call meaning, then the investigation of human behavior can proceed unencumbered by the sort of pseudoproblem represented by the emic/etic debate.

by ANNE SALMOND
Department of Anthropology, University of Auckland, Private Bag, Auckland, New Zealand. 9 XII 85

Feleppa begins by recalling the distinction drawn in some modes of anthropological inquiry between "etic" and "emic" analysis: roughly, accounts based on inquirer's and subjects' conceptions. Most anthropological research is intercultural, however, and all requires dialogue. In practice, both subjects' and inquirer's notions of the world are on call in anthropological exchanges. The difference between etics and emics rests in attitudes of theoretical privilege: in etics, inquirer's interests and conceptions are cast as preeminent in analysis, and in emics, subjects' interests and patterns of practice are held in focus. Yet even in etics, subjects' answers (in speech or other forms of practice) to the anthropologist's questions are material to the project of explanation, and even in emics, the anthropologist's questions and project of understanding are material to the way that conversations and analysis proceed.

Quine's idea of the "indeterminacy of translation" rightly draws attention to the difficulty of claiming cross-linguistic retrieval of semantic fact, but to say that translators cannot ascribe truth to their translations does not help anthropologists much. Many of us are so struck by the interpretive difficulties of our craft that we do not make such truth claims in any case and are more interested in better or worse, rather than true or false, translations. I take it that this is part of Feleppa's point when he says, "Much as Rudner asks why social inquiry must reproduce reality, I shall ask why translation must describe it." He goes on to argue that while anthropological translation inevitably involves interpretive interests from both the source and receptor communities, facts "not of the inquirer's making" are derivable from the practice of ethnographic subjects, and error is possible; and in all of this I agree with him. What he does not say, however, is how success and error in translation can be demonstrated, just how translation conventions and description languages differ in anthropology, and what happens if notions of truth in the two communities are differently described.

The literature of ethnoscience and componential analysis suggests that some expressions are more readily translatable than others, at least in the sense that "getting along" in kinship terminology and botanical, zoological, and colour ascriptions is more readily tested than, say, notions of cosmology, ideas of trust, or, in Maori, for instance, conceptions of tapu, mana, *hau*, and the rest. Part of the trouble is that interpretive charity works two ways and that subject communities, just like philosophers, are inclined to translate others as "believing true what is obvious." Their notions of both truth and what is obvious may differ somewhat from ours, however, and errors (now no simple notion) in more subtle and difficult areas of cross-cultural discourse may take the anthropologist years to grasp. Perhaps one needs to learn a good deal about "getting along" before it is possible to enter into exchanges where some sorts of error can be discovered. Then there is the possibility of divergence of interpretive interests: perhaps the inquirer's ignorance or error on certain matters suits the source community very well.

By the end of this article, Feleppa has redefined emics as a project in codification and translation which goes along with the scientific establishment of intercultural fact. I am interested but not wholly convinced. "Emics" and "etics" are useful thumbnail-sketch terms for different interpretive attitudes, but I don't find them an adequate base for delineating anthropological theory. The question of "adequate" accounts in anthropology, too, will need a more stringent and comprehensive analysis of "getting along" in the practice of both source and receptor communities (and the possible contradictions between these two attempts) than emic anthropology has so far been able to offer.

Reply

by ROBERT FELEPPA
Wichita, Kans., U.S.A. 15 I 86

Gibson wonders why I claim that Quine's indeterminacy thesis ought to be confronted. Let me review the reasons.

The main relevance of Quine to this controversy lies in two points: (1) he raises serious challenges to those who would tie the success of emic analysis to the success of recovery of underlying meaning components; (2) he raises equally serious difficulties for those who would take a pragmatic turn in assessing anthropological methodology, if they believe emic analysis will hold up under such a shift. Even were I to disagree en-

tirely with Quine's position, it would be of value to draw it out in this anthropological setting, especially given the character of Quine's own radical translation arguments. However, I am partly in agreement with Quine, partly in disagreement (a reasonable state of affairs, though Gibson suggests otherwise in his third objection), and I thus endeavor to reconstruct the thesis in what I take to be a clearer and more fruitful way.

Gibson is also bothered by my effort to separate the question of the soundness of arguments for indeterminacy from the question of its consequences. I am motivated to do this out of a belief that the final verdict on Quine's account of the roots of indeterminacy is far from being in. The literature is full of divergent accounts of what Quine is up to, of attempted revisions, etc., and it seems to me that if these unresolved questions can be circumvented, so much the better. It is the consequences of Quine's thesis in which I am mainly interested. Also, it is important to note that many philosophers do not share Gibson's conviction that Quine has succeeded in differentiating indeterminacy of translation and underdetermination of physical theory. Gibson himself seems to think that the heart of the indeterminacy thesis lies in the demonstration that "there is no sense to the question of any one translation being the uniquely correct one" (1982:69). However, barring some further specification of the standards of "unique" correctness, the indeterminacy thesis has no force, for physical theories seem to admit of alternatives compatible with equivalent bodies of evidence as well. This is a problem of long standing in the literature on the indeterminacy thesis (for "classic" statements, see Rorty 1972 and Chomsky 1968). And it is here, it seems to me, that Quine's physicalism is of central relevance: I don't maintain that this alone constitutes the origin of the thesis (and I apologize if the necessary incompleteness and compression of my remarks in this connection creates this impression). Rather, it is one of its component premises, serving to differentiate the import of the multiplicity of right answers for physics as opposed to translation by giving reason to rule out reliance on criteria of selection in translation that could play no part in the warranting of beliefs about physical macro- and microstates.

He also is concerned that I do not adequately differentiate emic from etic analysis. I am not entirely sure what the thrust of this objection is. The two types are usually easy enough to distinguish from each other. If his worry is that I don't sufficiently explain why emic analysis ought to be done, I must point out that it was not my intention to do so. The main burden of the discussion of the indeterminacy thesis is to undercut certain considerations, derivable from the thesis, that might be offered against the feasibility of emic analysis. I aim not to answer the question (which cannot be done in a single essay) but to keep it open, leaving it to be addressed in pragmatic and, largely, intraparadigmatic terms.

As for his concerns about the usefulness of the codification/description distinction, let me say the following: First, I do not claim that "utterances [about meaning and reference] lack truth values because, contrary to appearances, they are prescriptive and not descriptive." I make no claim about an underlying linguistic form (an enterprise about which I have a skepticism similar to Quine's [1960:157–61]); rather, I offer a way of regimenting translational claims for the purpose of giving a philosophically less problematic account of translation and cultural description. Secondly, I see no reason Duhem-Quine holism should have the consequences Gibson claims. One can holistically construe codificational endeavors (indeed, I think one ought to) and yet delineate differences in the justificational patterns that apply to codificational and descriptive "wholes"—or, better, in the justificational patterns that respectively characterize our interrelated prescriptive and descriptive beliefs.

Roth may create the impression that there is more disagreement between our positions than actually exists. This may result from a misunderstanding of my intentions, which I shall try here to clarify.

Part of the problem, I think, is that Roth may be more satisfied with Quine's indeterminacy arguments than I am. He accepts that Quine presents substantially all the premises needed to establish indeterminacy (though he is not always satisfied with the various ways Quine presents his case) and suggests that I do not see how deeply indeterminacy cuts. However, as much as I draw on Quine, I express dissatisfaction with a key element in his thesis, and unless the difficulty is resolved the thesis, to my mind, does not cut at all. The problem, about which I make only suggestive remarks, lies in Quine's appeal to physics as an ultimate parameter in the determination of the possible totality of facts. Interestingly, Rorty (1982:201), in the essay Roth cites in supporting his efforts at fruitful redefinition of methodological contrasts, raises similar concerns:

> Quine . . . thinks there can be no "fact of the matter" about intentional states of affairs because different such states can be attributed without making a difference to the elementary particles. . . . But surely all that such irreducibility shows is that one particular vocabulary . . . is not going to be helpful for doing certain things with certain explananda (e.g., people and cultures).

At any rate, as I note, even if Quine's or some other account of the basis for indeterminacy proves viable, I believe the reconstruction will still serve to modify constructively its consequences for anthropology.

The cogency of my reconstruction aside, it provides a commodious setting for Roth's efforts (in, e.g., his 1985) to restate ethnographic divergence in terms of competing translation schemes—a compatibility he elsewhere acknowledges (1986). However, these harsh consequences it seems to me are ones Roth must take account of. In arguing, as he does in his 1985 work, for a pluralistic view of the consequences of indeterminacy, he places himself clearly in the camp of those who wish to recast methodological controversies and commitments on pragmatic as opposed to metaphysical grounds—a group against which I set Quine. Why are we to trust any ethnographic description if it rests on indeterminate translational premises? I offer one way of dealing with this problem, one which arises initially from general considerations about the particularly coordinative, prescriptive character of the translational enterprise.

Also, his remarks here leave unclear just what he takes the emics/etics controversy to involve. Even if we agree that a number of key metaphysical and epistemological presuppositions of the controversy are not cogent, this is not to relegate the entire controversy to the status of a pseudoproblem. I intended to show certain aspects of it to be pseudoproblems and believe I am in substantial agreement with Roth on this point. However, I believe significant "emic/etic" issues remain: Anthropologists still ask themselves whether they should try to translate subject notions into terms usable for ethnographic description, or how much of their efforts should be directed to this, or just how systematically central emic conceptions should be. They also wonder how much of their efforts at "emic" analysis should be aimed at the unconscious as well as the conscious mind—and the tensions expressed in the discussions of Frake, Watson, and Marano seem to have a point, even given acceptance of some form of indeterminacy. And even if imposition in the absolute sense is incoherent, we still need to make sense of its other forms—e.g., taking informant reports too ingenuously, overlooking data that may produce different impressions and more constructive attitudes and programs of action.

Now, in all fairness to Roth, I believe he is generally agreeable to the idea that there remain significantly answerable

questions regarding emic analysis. He argues (1985) that the (I would say rather heavily emic) orientation of Peter Winch can be justified, in pretty much Winch's own terms (though given more of a pragmatic emphasis by Roth), for its morally sensitizing us to other possible ways of organizing experience.

What I found most enlightening in Roth's account can be put as follows: A critic might respond to Roth's pragmatized Winch by saying, "We might well find new ways to categorize our experience, ones that produce morally praiseworthy results for ourselves, for our dealings with the society we describe, and yet wonder whether we have said anything true about its culture or whether we have distorted it in some fundamental way." In response, Roth puts the burden on the critic to show what the empirical or practical import of this talk about unrecovered truth can be. If it arises from a belief that the point of cultural description is to reveal the stucture of an underlying mental or propositional realm, then it must be recognized that belief in such things is not easy to justify.

I am encouraged that Salmond finds my sketch of prescriptive translational methodology generally acceptable. For the most part, I see her objections as displaying far greater experience with field translation and as insightfully suggestive of points I must develop further. The intention of my essay was only to remove certain philosophical impediments to the incorporation of translational results in emic (and, for that matter, etic) methodologies. (And though I believe Salmond does not intend to suggest otherwise, I think it worth reiterating that Quine's indeterminacy thesis, as it stands, presents serious philosophical obstacles to even the "hocus-pocus" view of translation to which she seems to subscribe.)

I am somewhat puzzled by her claim that I do not say how success and error in translation can be demonstrated. If this is only to call for a more detailed accounting of how behavior may or may not comply with the various rules that spring from the complex coordination problem I delineate, then I am in full agreement. Such accounting is an essential part of the intraparadigmatic study of criteria of adequacy for which I call in the latter part of my essay. I do, however, speak, in general terms at least, to the matter of how error is detectable. My characterizing translation as codification is aimed, in part, at capturing the idea that violations of translational rules, or of the expectations they produce, do not have quite the same logical or methodological consequences as obtain in the disconfirmation of descriptive hypotheses. As I remark, translations possess, with rules, the logical feature of being violable without thereby being abridged (a distinction which itself needs more extended formulation to establish clear contrasts with probabilistic laws). This naturally leads to the question, however, of when deviation is not tolerable and a rule must be amended or replaced. Yet although a precise general statement of conditions under which rules are clearly violated is impossible, reflection on more typical cases of codification (say, of legal, logical, or ethical practice) yields sufficient illustrative instances, e.g., laws being modified owing to gross noncompliance or philosophical codifications of scientific practice giving way in light of the regular failure of respectable scientific practice to conform to them. Similarly for translation: in its early stages inquirers are rightfully quick to condemn a rule formulation for noncompliance and less so inclined as the integrated translation manual meets with more success, though required sensitivity to shifts in source-language usage counsels against regarding even well-established rules as beyond reproach.

In reading this, one might be struck by the close similarities of my prescriptive account to the ordinary descriptive account that takes translations to be genuine hypotheses. Despite my efforts to contrast rules and hypotheses, I welcome this similarity: the account of error detection does not change all that much on my reconstrual. If it is asked, then, why one ought to bother to shift to a philosophical orientation that stresses codification, it is because it fares better when other problems, such as those Quine adduces, are before us. Rules can have a legitimate function in theories without being either true or false. I do not believe the same can as easily be said for hypotheses.

References Cited

BERNSTEIN, R. 1933. *Beyond objectivism and relativism: Science, hermeneutics, and praxis*. Philadelphia: University of Pennsylvania Press.

BOAS, F. 1943. Recent anthropology. *Science* 98:311–14, 334–37.

BRESNAN, JOAN. 1978. "A realistic transformational grammar," in *Linguistic theory and psychological reality*. Edited by M. Halle, J. Bresnan, and G. A. Miller, pp. 1–59. Cambridge: M.I.T. Press.

BROWN, C. H. 1974*a*. Psychological, semantic, and structural aspects of American English kinship terms. *American Ethnologist* 1:415–36.

———. 1974*b*. *Wittgensteinian linguistics*. The Hague: Mouton.

———. 1976. Semantic components, meaning, and use in ethnosemantics. *Philosophy of Science* 43:378–95.

BURLING, R. 1964. Cognition and componential analysis: God's truth or hocus-pocus? *American Anthropologist* 66:20–28.

———. 1969. Linguistics and ethnographic description. *American Anthropologist* 71:817–27.

CAMPBELL, D. T. 1975. On the conflicts between biological and social evolution and between psychology and moral tradition. *American Psychologist* 30:1103–26.

CARNAP, R. 1950. Empiricism, semantics, and ontology. *Revue Internationale de Philosophie* 11:20–40.

CAWS, P. 1974. Operational, representational, and explanatory models. *American Anthropologist* 76:1–10.

———. 1976. The ontology of social structure: A reply to Hanson. *American Anthropologist* 78:325–27.

CHOMSKY, N. 1968. "Quine's empirical assumptions," in *Words and objections: Essays on the work of W. V. Quine*. Edited by J. Hintikka and D. Davidson. Dordrecht: Reidel.

DAVIDSON, D. On the very idea of a conceptual scheme. *Proceedings of the 70th Meeting of the American Philosophical Association*, pp. 5–20.

DENNETT, D. C. 1978. *Brainstorms: Philosophical essays on mind and psychology*. Cambridge: M.I.T. Press.

DURBIN, MRIDULA A. 1972. Linguistic models in anthropology. *Annual Review of Anthropology* 1:383–410.

EMBER, C. R. 1977. Cross-cultural cognitive studies. *Annual Review of Anthropology* 6:35–56.

FELEPPA, R. 1982. Translation as rule-governed behavior. *Philosophy of the Social Sciences* 12:1–31.

FISHER, L. E., and O. WERNER. 1978. Explaining explanation: Tension in American anthropology. *Journal of Anthropological Research* 34:194–218.

FRAKE, C. O. 1962. "The ethnographic study of cognitive systems," in *Anthropology and human behavior*. Edited by T. Gladwin and W. C. Sturtevant, pp. 72–93. Washington, D.C.: Anthropological Society of Washington.

———. 1977. Plying frames can be dangerous: Some reflections on methodology in cognitive anthropology. *Quarterly Newsletter of the Institute for Comparative Human Development, Rockefeller University* 1(3):1–7.

GEERTZ, C. 1976. "From the native's point of view: On the nature of anthropological understanding," in *Meaning in anthropology*. Edited by K. H. Basso and H. A. Selby. Albuquerque: University of New Mexico Press.

GEOGHEGAN, W. H. 1969. *Decision-making and residence on Tagtabon Island*. University of California, Berkeley, Language-Behavior Research Laboratory, Working Paper 17.

GIBSON, ROGER F. 1982. *The philosophy of W. V. Quine*. Tampa: University of South Florida Press. [PAR]

GOODENOUGH, W. 1951. *Property, kin, and community on Truk*. Yale University Publications in Anthropology 46.

———. 1956. Residence rules. *Southwestern Journal of Anthropology* 12:22–37.

———. 1964. "Cultural anthropology and linguistics," in *Language in culture and society*. Edited by D. Hymes, pp. 36–39. New York: Harper and Row.

———. 1970. *Description and comparison in cultural anthropology*. Chicago: Aldine.

GOODMAN, N. 1973. 3d edition. *Fact, fiction, and forecast*. Indianapolis: Bobbs-Merrill.

HANSON, F. A. 1976. Models and social reality: An alternative to Caws. *American Anthropologist* 78:323–25.

HANSON, F. A., and R. MARTIN. 1973. The problem of other cultures. *Philosophy of the Social Sciences* 3:191–208.

HARRIS, M. 1968. *The rise of anthropological theory*. New York: Crowell.

———. 1975. 2d edition. *Culture, people, nature*. New York: Crowell.

———. 1976. History and significance of the emic-etic distinction. *Annual Review of Anthropology* 5:329–50.

———. 1979. *Cultural materialism: The struggle for a science of culture*. New York: Random House.

KAY, P. 1970. Some theoretical implications of ethnographic semantics. *American Anthropological Association Bulletin* 3(3, pt. 2):19–31.

KEESING, R. 1972. Paradigms lost: The new ethnography and the new linguistics. *Southwestern Journal of Anthropology* 28:299–332.

KIEFER, C. 1977. Psychological anthropology. *Annual Review of Anthropology* 6:103–19.

LEWIS, DAVID K. 1969. *Convention: A philosophical study*. Cambridge: Harvard University Press.

MARANO, L. 1982. Windigo psychosis: The anatomy of an emic-etic confusion. CURRENT ANTHROPOLOGY 23:385–412.

NAROLL, R. 1964. On ethnic unit classification. CURRENT ANTHROPOLOGY 5:283–312.

OLIVER, D. 1955. *A Solomon Island society*. Cambridge: Harvard University Press.

PIKE, K. 1954. *Language in relation to a unified theory of the structure of human behavior*. Pt. 1. Glendale, Calif.: Summer Institute of Linguistics.

———. 1964. "Towards a theory of the structure of human behavior," in *Language in culture and society*. Edited by D. Hymes, pp. 54–61. New York: Harper and Row.

QUINE, W. V. O. 1960. *Word and object*. Cambridge: M.I.T. Press.

———. 1961. "The problem of meaning in linguistics," in *From a logical point of view*, 2d edition revised, pp. 47–64. New York: Harper and Row.

———. 1970*a*. Philosophical progress in language theory. *Metaphilosophy* 1:2–19.

———. 1970*b*. On the reasons for indeterminacy of translation. *Journal of Philosophy* 67:179–83.

———. 1976*a*. "On Carnap's views on ontology," in *The ways of paradox and other essays*, revised and enlarged edition, pp. 203–11. Cambridge: Harvard University Press.

———. 1976*b*. "Carnap and logical truth," in *The ways of paradox and other essays*, revised and enlarged edition, pp. 118–21. Cambridge: Harvard University Press. [RFG]

———. 1981*a*. "Things and their place in theories," in *Theories and things*, pp. 1–23. Cambridge: Harvard University Press.

———. 1981*b*. "Goodman's ways of worldmaking," in *Theories and things*, pp. 96–99. Cambridge: Harvard University Press.

RAWLS, J. 1971. *A theory of justice*. Cambridge: Harvard University Press.

ROMNEY, A. K., and R. G. D'ANDRADE. 1964. "Cognitive aspects of English kin terms," in *Transcultural studies in cognition*. Edited by A. K. Romney and R. G. D'Andrade, pp. 146–70. *American Anthropologist* 66(3, Pt. 2).

RORTY, R. 1972. Indeterminacy of translation and of truth. *Synthese* 23:443–62.

———. 1982. "Method, social science, and social hope," in *Consequences of pragmatism (Essays: 1972–1980)*, pp. 191–210. Minneapolis: University of Minnesota Press. [PAR]

ROTH, P. 1985. Resolving the *Rationalitätstreit*. *Archives Européennes de Sociologie* 26:142–57. [PAR]

———. 1986. Pseudo-problems in social science. *Philosophy of the Social Sciences*. In press.

RUDNER, R. 1966. *Philosophy of social science*. Englewood Cliffs: Prentice-Hall.

———. 1973. Some essays at objectivity. *Philosophical Exchange* 1:115–35.

SAPIR, E. 1949. "The psychological reality of phonemes," in *Selected writings of Edward Sapir*. Edited by D. Mandelbaum, pp. 46–60. Berkeley: University of California Press.

STICH, S. P., and R. E. NISBETT. 1980. Justification and the psychology of human reasoning. *Philosophy of Science* 47:188–202.

TRIANDIS, H. C. 1976. "Approaches toward minimizing translation," in *Translation: Application and research*. Edited by R. W. Brislin, pp. 229–44. New York: Wiley.

WALLACE, A. F. C. 1965. "The problem of psychological validity of componential analysis," in *Formal semantic analysis*. Edited by E. A. Hammel, pp. 229–48. American Anthropologist 67(5), suppl.

———. 1970. "A relational analysis of American kinship terminology: An example of relations between process and structure in cognition," in *Cognition: A multiple view*. Edited by P. L. Garvin, pp. 145–53. New York: Spartan Books.

WATSON, L. C. 1981. "Etic" and "emic" perspectives on Guajiro urbanization. *Urban Life* 9:441–68.

WHITE, M. 1963. *Toward reunion in philosophy*. New York: Atheneum.

WINCH, P. 1958. *The idea of a social science and its relation to philosophy*. London: Routledge and Kegan Paul.