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Translation as Rule-Governed Behaviour*

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The problems of radical translation occupy a central place in a number of long-standing controversies in philosophy and anthropology. The philosophical difficulties here concern the accurate recovery of speaker meaning in translation, in light of the following two problems: (1) the radical translator's unwanted possession, in principle, of too many right answers—i.e., the availability in principle of empirically equivalent, yet divergent manuals of translation for a given society; and (2) the *prima facie* undesirable, yet perhaps inescapable need to impose what is grammatically and ontologically familiar to the linguist and to the target language community upon the source language community in translating their discourse. In short, these problems make clearly problematic whether translations meeting standard criteria of adequacy can ever be said to reveal what the source language speakers *really* mean. As many anthropologists, especially those in the 'language and culture' tradition generated by Boas, Cassirer, Sapir, and Whorf, take the recovery of such culturally specific significance as central to their discipline, worries about how to select translation manuals and related ethnographic systematizations (such as kinship organizations and disease taxonomies) that have demonstrable 'cognitive (or psychological) validity' seem to run very deep indeed. And such concerns cannot but be deepened by the fact that much reflection in the philosophical community on these problems, particularly as embodied in the work of W. V. Quine, is *against* the objective determinability of meaning in translation. For while anthropologists generally worry about how to validate claims about meaning and synonymy in light of these methodological difficulties, presuming them to be surmountable in principle, Quine cites just these problems in order to denigrate the various current notions of meaning and synonymy—by rendering illusory the 'underlying' semantic (or psychological) reality they purport to reveal.¹

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1 I shall focus mainly on the indeterminacy/inscrutability arguments presented in Quine's *Word and Object* (Cambridge, Mass. 1960), Chap. 2 (hereafter cited as *WO*); 'The Problem of Meaning in Linguistics', in *From a Logical Point of View*, second edition, revised (New York 1961), pp. 47-64 (hereafter cited as *PML*); 'Philosophical Progress in Language Theory', *Metaphilosophy*, 1, 1970, 2-19 (hereafter cited as *PPLT*).

Yet much as these shared concerns with translational multiplicity and interpreter imposition have been discussed in the philosophical and anthropological literature, there is no general agreement as to their ultimate consequences. The cognitive validity problem, raised with respect to the widely employed componential analysis approach, remains unsettled—with many seeing the problems as reaching a dead end and thus providing one of a number of reasons for seeking new methodological initiatives. While within the philosophical community, despite general agreement that Quine has raised deep and serious problems for the various theories of meaning, few thinkers are satisfied that he has clearly identified the deeper conceptual problems or that he has cogently produced translational indeterminacy from the methodological shortcomings he cites. My own view is that Quine has brought the key difficulties to light, and further that he is correct in claiming that translational hypotheses are not warrantably assertible as true or false (what I shall mean here in calling them ‘indeterminate’); however, I derive indeterminacy from the initial Quinean considerations differently. On my view, the problems Quine cites can be resolved, and indeterminacy adequately explained, by viewing translation not, as Quine and others do, as a *descriptive* endeavour at hypothesis formation, but instead as a *prescriptive* task aimed at the formation and codification of *rules*, evolving more along the lines of a Rawlsian ‘theory’ of justice rather than of a theory of nature.

Owing to the close affinities between my thesis and Quine’s, I shall begin my argument by critically expounding his arguments for the indeterminacy, or inscrutability (as is his stricter usage), of reference. These arguments are pivotal to certain formulations of Quine’s more general thesis of *sentence* indeterminacy (see especially *WO*, chapter 2) and bring out the significance and character of the multiplicity and imposition points most clearly—more so, indeed, than other formulations of the thesis that bypass the inscrutability thesis (although these formulations seem generally to rest on close analogues of the multiplicity and imposition points).² Thereafter I shall bring these points, as well as certain

2 The role these two points play in the general indeterminacy thesis varies depending on which formulation one is considering (and, indeed, there have been many put forth by Quine and his commentators). Although Quine explicitly claims that he does not view the extended ‘gavagai’-example as itself *entailing* sentence indeterminacy, he does credit it with providing the setting for the more general thesis, ‘helping the reader to reconcile the indeterminacy of translation imaginatively with the concrete reality of radical translation’ (‘On the Reasons for Indeterminacy of Translation’, *Journal of Philosophy*, 67, 1970, 182). In this and other formulations of the general thesis, Quine cites the underdetermination of scientific theories and the attendant underdetermination of translation manuals as a key point in the argument (that is, just as the best possible theory of nature must admit of empirically equivalent but mutually incompatible alternatives, so must an ideally successful translation manual admit of empirically equivalent alternatives that yield target translations which are incompatible with target

insights drawn from David Lewis' *Convention: A Philosophical Study*³ and elsewhere, to bear on my suggested reconstrual of translation and indeterminacy. In so doing, I will focus attention on certain concrete methodological problems of translation and ethnography that relate closely to Quine's thesis (owing, in part, to a common heritage in the writings of Boas *et al.*) and to my own view. Such limitations of scope seem to me more productive than any attempt to discuss or indicate the full, broad range of complex ramifications of Quine's or my own view for the philosophy of language, epistemology, anthropology, linguistics, etc.

I

The first point of the inscrutability thesis is that on the basis of empirical adequacy alone (i.e., on the basis only of the 'stimulus meanings' of sentences), no uniquely correct scheme of reference can be determined (see *WO*, Chap. 2). We are free to confer upon the native any number of conceptual schemes, each of which (to employ Quine's well-worn example) renders a different target-language translation of the source-language term 'gavagai'—i.e., as 'rabbit', 'rabbit-stage', 'rabbit-fusion', etc. The second point (which seems patently necessary to make for a special *indeterminacy*, and not simply underdetermination, of reference) is that the additional, and rationally impeccable criteria of adequacy which are applied to the selection from these feasible alternatives of a 'correct' translation (and, derivatively, of a correct translation manual)—yielding in the present example 'rabbit' as the proper target-language translation—necessarily involve the imposition on the source-language community of a grammar and ontology that are familiar to the western linguist (and to western target-language speakers).⁴

Who knows but what the objects to which ['gavagai'] applies are not rabbits after all, but mere stages, or brief temporal segments of rabbits? In either event the stimulus situations that prompt assent to [the sentences] 'Gavagai' would be the

translations produced by the first manual, for the same set of source-language strings). Clearly, this is another type of 'multiplicity'-point. The imposition-point does not surface quite as clearly in the above-cited article, but it is implicit, I believe, here, and explicit in Quine's recent 'On Empirically Equivalent Systems of the World' (*Erkenntnis*, 9, 1975, 322), as a reference to the imposition manifest in the translator's (again, rationally irreproachable) adherence to a *charity* or *fair translation* principle, which bids us 'maximize the agreement between the native and ourselves on questions of truth and falsity, other things being equal'.

3 David Lewis, *Convention: A Philosophical Study*, Cambridge, Mass. 1969. Hereafter cited as *Cvn*.

4 I shall view translation as comprising *three* languages (and three corresponding communities): the *source* language being translated, the *target* language into which the source language is translated, and the *linguistic metalanguage* in which translational correlations are couched (and which mentions, rather than uses, the source and target strings).

4 Robert Feleppa

same as 'Rabbit'.... When from the sameness of stimulus meanings of 'Gavagai' and 'Rabbit' the linguist leaps to the conclusion that a gavagai is a whole enduring rabbit, he is just taking for granted that the native is enough like us to have a brief general term for rabbits and no brief general term for rabbit stages or parts. [WO, pp. 51-52.]

Indeed,

The whole [individuative] apparatus is interdependent, and the very notion of term is as provincial to our culture as are those associated [individuative] devices. The native may achieve the same net effects through linguistic structures so different that any eventual construal of our devices in the native language and vice versa can prove unnatural and largely arbitrary. [WO, p. 53.]

Furthermore, in applying Kenneth Pike's reflections on the imaginary Kalabans⁵ to a radically divergent conclusion (here a version of the more general indeterminacy thesis), Quine remarks:

... as the sentences undergoing translation get further and further from mere reports of common observations, the clarity of any possible conflict between translation and fact decreases; the lexicographer comes to depend increasingly on a projection of himself, with his Indo-European *Weltanschauung* into the sandals of his Kalaban informant. He comes also to turn increasingly to that last refuge of all scientists, the appeal to the internal simplicity of his growing system. [PML, p. 63.]

Quine's conclusion that reference and meaning are indeterminate varies sharply with other landmark figures in the language and culture tradition as well, while nonetheless issuing from similar concerns:

Usener, Cassirer, Sapir, and latterly B. L. Whorf have stressed that deep differences of language carry with them ultimate differences in the way one thinks, or looks upon the world. I should prefer not to put the matter in such a way as to suggest that certain philosophical propositions are affirmed in the one culture and denied in the other. What is really involved is difficulty or indeterminacy of correlation—less sense in saying what is good translation and what is bad—the farther we get away from sentences with visibly direct conditioning to nonverbal stimuli and the farther we get off home ground.⁶

As these passages illustrate, at the core of Quine's attack on the determinacy of reference (and of a number of formulations of the more general thesis) is the following argument: the translator's imposition of the grammatically and ontically familiar can be defended on the grounds that its application will maximize agreement and optimal coordination of intercultural intercourse. As these are prime objectives of translation, imposition, where it is a necessary supplement to 'purely' empirical

5 See Kenneth L. Pike, *Phonemics: A Technique for Reducing Languages to Writing*, Ann Arbor 1947, pp. 68ff.

6 Quine, 'Meaning and Translation', in Reuben A. Brower, (ed.), *On Translation*, New York 1966, pp. 171-72.

determinations of stimulus meaning, is rationally irreproachable. For what possible value could there be in employing a needlessly complex or alien grammar if a manual of translation that incorporates a familiar grammar serves the task equally well? Yet the choice of grammar and lexicon that application of the imposition principle dictates is thus partially, but critically, dependent on the linguist's suiting translations of the source language to the antecedent patterns of *two other* language communities. That is, the translator must make translations that are ideally harmonious with the accepted grammatical practices of the target-language speakers as well as of the theoretical metalinguistic community of translators and linguists (the latter two vary quite markedly in their accepted practices, even where, as in Quine's example, they share elements of the same natural language [English]). And while there is no good reason to attribute odd beliefs or grammars to the native, thus maximizing agreement does not eliminate the possibility that the rejected alternatives might nonetheless express the truth of the matter. They may reasonably be rejected on this basis, but they are not thus *falsified*.

The point is that if we examine the criteria of adequacy the linguist is *justified* in employing, these criteria are seen to provide no warrant for asserting that the best existing manual expresses the truth about what the native is referring to (and, more generally, about what the native means). The linguist's well-proven choices of translational hypotheses are rationally warranted to be sure, but they are not warrantably assertible *as true*. His methodology provides no way to focus exclusively upon the source language or its speakers' meanings, for the translation manual is first and always an instrument of *intercultural* coordination. And from this epistemological point there follows, on Quine's view, the ontological implication that there is no 'fact of the matter': for a Quinean empiricist can only entertain as possible fact that which he can in principle determine to be true. But if we must impose on the native, then to suppose we have thus determined his real belief or true meaning is only to 'impute our sense of linguistic analogy unverifiably to the native mind' (*WO*, p. 72: the pronoun 'our', again, refers to members of both the target- and metalanguage communities). The problem of inscrutability arises because a number of divergent conceptual schemes are coming into contact: and as this problem does not afflict the physical sciences (insofar as they do not presume to recover symbolic meaning), Quine thus has a basis for challenging the objectivity of reference (and intension) consistently with his scientific-realist attitude toward theoretical entities in physics.

Hence Quine agrees with anthropologists who view the multiplicity of 'right' answers in translation and ethnography as presenting a serious problem of objectivity, and further agrees that overcoming such multiplicity

licity by imposition of the familiar precludes one's being able to lay claim to discovery of the native's actual grammar. But Quine disagrees with the traditional, and probably majority, view in arguing that the lesson to be learned from this is not that imposition must somehow be avoided and multiplicity somehow eliminated, so that the real meaning may be recovered, but rather that these interrelated problems, and particularly the unavoidability of imposition, show that empirical methodology cannot answer questions as to the nature of *real* reference, belief, or meaning, and hence that such notions are empirically empty.

Now I agree with Quine that it is impossible warrantably to assert that one's translation of a radically foreign referential (and conceptual) scheme expresses the facts about the structure it possesses or which it imposes on the world. I agree that reference is indeterminate, or inscrutable, in the sense that the lexical hypotheses on which referential determination is based are, by and large, not warrantably assertible as true. But, as I indicated at the outset, I justify this belief differently from Quine. Before moving to the exposition of my own position, however, I should like to look briefly at a number of problems in Quine's thesis which have motivated the reconstrual I am about to offer.

First of all, the argument outlined above seems to rest on a questionable appeal to the very notion—of a real grammar or conceptual scheme—that it seeks to undercut. How can one claim on the one hand that there is a referential scheme and then, in the interests of showing its inaccessibility argue that it is impossible in principle ever to ascertain what such a scheme might be, and furthermore conclude that talk of such a scheme is empirically meaningless? If the argument requires a reference to such a scheme in its initial premiss, isn't the argument then self-vitiating, rendering its own premiss questionable because meaningless? And even if one reformulates the argument (more in the spirit of Quine's frequent admonitions to construe 'inscrutability' as entailing there being 'nothing there to scrute') as a *reductio* of the assumption that there exists a real scheme in the native mind, we are still left with the puzzling claim that the linguist must worry that in choosing one grammar from a set of empirically equivalent, but otherwise divergent ones that he *might thus be wrong*. That is, why should we take seriously the question 'Who knows but what the objects to which "gavagai" applies are not rabbits after all, but mere stages, or brief temporal segments of rabbits?' (See above.) If the point of the argument is to show us that in the proposed example we cannot have a rational basis for any 'discovery' that 'rabbit-stage' is after all correct, then what empirical force is there to the claim that 'gavagai' might (really?) apply to rabbit stages or whatever? What empirical sense do we make of the claim that the translator *might* thus be wrong, given that other steps in the argument show us that there can be no empirical basis to any claim to the effect

that the translator *is* wrong in choosing from among a set of alternatives that are underdetermined in Quine's strong sense? If I can have no empirical basis for saying that I am wrong, what basis can I have for saying that I might be wrong? Clearly, it is the *reductio* assumption that provides the basis for this possibility, and thus a mentalistically inclined linguist in search of underlying 'criteria in mind' must perhaps be concerned about it. But does the argument show that *anyone*, even, say, a 'naturalized' epistemologist such as Donald Davidson, must share it? If one maintains this broader thesis, that even those who prefer more behaviourally scrutable interpretive constructs must take seriously this possibility of error, is one not *resting*, once again, on the assumption that is ostensibly being challenged?

Now the point of this first criticism is not to present an airtight argument that is ready to meet every Quinean rejoinder—although I do not think the *WO*, *PML*, or *PPLT* discussions provide any clear clue as to how this objection might be met.⁷ My intention is only to indicate that they leave obscure an important transcendental basis for the inscrutability argument—one that justifies claims, independently of any empirical basis, to the effect that we might be wrong in making certain choices of translation. We must be wary that in accepting premisses such as this that we are not implicitly adopting as our standard of correctness the very inscrutable scheme we argue is illusory. This danger also threatens if we too easily grant Quine that imposition of the familiar *ipso facto* precludes warranted assertibility—anthropologists may take this step more casually if, indeed, they agree with Whorf that there is a real scheme to be recovered. But Quine cannot make this move so easily, and this point is easily obscured by the commonality of heritage and concern that he shares with the language and culture tradition.

More directly pertinent to Quine's relationship to this anthropological tradition is the following, additional problem: one seeming consequence of the general indeterminacy thesis is that anthropologists must get by with very little linguistic data in doing ethnography and ethnology. Except for those cases involving source-language strings whose meaning is entirely determinate on the basis of stimulus meaning, most translational hypotheses are indeterminate, and thus comprise the acceptability of any further translational or ethnographic hypotheses

7 Cf. Quine's supplemental considerations presented in the latter half of 'Ontological Relativity and Other Essays, New York 1969, pp. 26-68, which support inscrutability by reference to an 'at home' case in which he endeavours to show the impossibility of objectively determining the uniquely correct referential scheme or ontology of theories. Although full treatment of these arguments is beyond my present scope, I should mention in passing that I have objections to the appeal there to the 'ultimately inscrutable ontology' that theories purportedly have (and which forever escapes the various regimentations one might offer) (pp. 50-51), which are closely parallel to the objections I have offered here to the radical-translation formulation.

which rest on them. Quine suggests this very consequence in his concluding remarks in *PPLT*, noting that 'much can be determined by leaving language alone and observing non-verbal customs and taboos and artifacts', while minimizing the role of indeterminate linguistic data to that of providing, perhaps, 'a general and undirected measure of [linguistic] remoteness in the sheer difficulty of intertranslation' (*PPLT*, p. 16). But if indeterminacy thus plagues the 'eliciting operations' essential to componential analytic, structuralist, transformational, and other approaches that are the current offspring of the language-and-culture tradition, then acceptance of indeterminacy seems to wed one to the methodological commitments of clearly opposed, nonlinguistically oriented anthropological approaches. However, I do not think the substantial, though mixed successes of the linguistically oriented approaches can be so easily written off, and one task of the reconstruction I shall propose will be to reinterpret some of the main objectives of componential analysis so as to provide philosophical justification for it despite indeterminacy (the argument being feasibly extendable to other linguistic approaches as well, although I shall not pursue this broader thesis here). The methodological commitment of my brand of indeterminacy is less exclusive, and accords well, as I shall indicate, with the current views of a number of members of the language-and-culture camp.

II

My account of inscrutability of reference—which, I should note, is intended to serve to establish the more general indeterminacy thesis as well—shall proceed as follows: after a preparatory discussion in the present section, I shall discuss a number of considerations, general and particular, concerning the setting of radical translation and the logical character and function of translational hypotheses. My aim will be to establish the following thesis as to the proper basis for contrast between translation and physical theorizing: that it lies in the differing compliance relationships that obtain between physical hypotheses (and theories) and their confirming and disconfirming instances on the one hand, and translational 'hypotheses' (and manuals of translation) and their compliant or noncompliant instances on the other—the former manifesting the *law*-governance of compliance-classes, the latter manifesting conventional, and particularly *rule*-governance of compliance-classes. Important differences, I shall argue, between the lawlike and rulelike orientations are manifest not only in the types of regularities that the translator and physicist systematize, but also in the purposes and standards of adequacy of each sort of systematization. The central task of physical theory is to describe regularities in a way that permits conceptual simplicity, enlightening explanation, and correct prediction.

In translation, although much is required and effected in the way of explanation of symbolically significant behaviour within a culture, and although successful translation enhances predictability of verbal and non-verbal behaviour, the main burden of translation is to 'coordinate' behaviour—behaviour involving groups of rational individuals whose behaviour, qua symbolic behaviour, is rule-determinate rather than law-determinate.⁸

Translation hypotheses, on my view, inasmuch as they function in just the way that rules governing the usage of words do, are not in any literal sense hypotheses (they are neither true *nor* false). But this follows not for the reasons Quine offers, but rather because the translator's linguistic correlations are themselves rule-like codifications of rules of usage, cross-culturally projected from observed source-, target-, and meta-language behaviour. (The notion of codification here is essentially Goodman's, as shall be seen.) Now granting the highly problematic character of convention and the other varieties of conformative behaviour, I believe David Lewis' analysis is sufficient for present purposes, and I shall employ it to show that as linguistic conventions may be adequately construed as arising out of the need to solve coordination problems, and as these conventions are in fact constitutive of complex coordination equilibria, the analytical 'hypotheses' which serve to correlate the linguistic conventions and rules of one language with those of another themselves represent coordination equilibria and are thus themselves conventions.⁹ However their initial developmental stages might seem indiscernible from, or at least intimately parallel to, those of physical hypotheses, the fact that their systematic interrelationships are patterned by the conventional relationships among the verbal strings that they serve both to reveal, and especially to codify in a broader intercultural context, makes them 'conventional' in a way that physical hypotheses are not.

The bulk of Lewis' book is devoted to bringing the apparatus of rational choice theory to bear on the analysis of convention, and it is this part of his work that most concerns me here. (His subsequent analysis of *linguistic* conventions in fact diverges quite radically from Quine's and my own views on a number of key points.) In essence, the coming to be, continuation, and passing away of conventions is primarily determined,

8 This is of course not to say that rule-determinate behaviour is nomologically inexplicable.

9 Cf. Quine's 'Truth by Convention', in *The Ways of Paradox and Other Essays*, revised and enlarged ed., Cambridge, Mass. 1976, pp. 71-106, in which he argues against the view that certain 'analytic' or 'logically true' sentences are 'true by convention'. Lewis takes issue with a number of Quine's points, while defending the analytic-synthetic distinction that Quine rejects (*Cvn*, pp. 2-3, *passim*). In my application of Lewis, I shall endeavour to steer clear of this and other points of conflict between him and Quine.

on Lewis' view, by their efficacy in achieving maximal coordination among persons, who are thus motivated to coordinate out of rational self-interest. Hence, the rigour of rational choice theory makes it ideal for the proposed analysis. Lewis defines 'convention' in terms of complex 'higher order' expectations which rational (i.e., utility maximizing) persons form as a result of the gradual evolution of behaviour-patterns in which, in David Hume's terms, 'the actions of each of us have a reference to those of the other, and are performed upon the supposition that something is to be performed on the other part' (*Cvn*, p. 4). Rational parties form conditional preferences for performing certain types of actions in certain sorts of situations, the condition being that all others perform that action if they do (*Cvn*, p. 69). Performance of that action turns out to be best for all, providing all perform it, and combinations of actions which best yield optimal results for all are coordination 'equilibria', i.e., solutions to such so-called 'coordination problems' in which all parties turn out to be best off in the sense that 'no one would have been better off had any one agent alone acted otherwise' (*Cvn*, p. 14).

Now all behaviour is regular behaviour in the precise sense that all behaviour is (at least in principle) nomologically explainable, and a central task of Lewis' account is to distinguish conventional behaviour from other 'regular' behaviour. And the main feature to which Lewis appeals (as shall I, even moreso) is that conventional behaviour is essentially *contravenable* (and thus conventions govern behaviour in a way that is different from, and 'additional' to, the way in which scientific laws 'govern' behaviour). This becomes evident on examination of Lewis' list of criteria of conventionality (which, for present purposes, I shall leave in rough form, ignoring a number of subsequent changes made in *Convention* and elsewhere).¹⁰

The following are necessary conditions for a regularity *R* in behaviour being a convention in a recurrent situation *S* among the members of a population *P* in any instance of *S*:

- (1) everyone conforms to *R*;
- (2) everyone expects everyone else to conform to *R*;
- (3) everyone prefers to conform to *R* on condition that the others do, since *S* is a coordination problem and uniform conformity to *R* is a coordination equilibrium in *S*;

and further that (1) - (3) are common knowledge in *P* (*Cvn*, p. 42). This last feature of 'common knowledge', embracing as it does certain notions of 'potential', 'irremediably nonverbal' knowledge, and knowledge 'confined to particular instances' (*Cvn*, pp. 63ff.), is perhaps problematic, but I think serviceable for present purposes. Such knowledge can arise from explicit agreement, from the salience of some one type of

10 Cf. e.g. Lewis' 'Languages and Language', in K. Gunderson (ed.), *Minnesota Studies in the Philosophy of Science*, Vol. VII, Minneapolis 1975, pp. 3-35.

action (due perhaps to visible similarities borne to previous equilibria), precedent, or perhaps the sheer inertia of past conformity. The process of conformity through time can begin by explicit agreement, but may also arise from 'an exchange of manifestations of a propensity to conform to a regularity' (as when two rowers gradually coordinate the rhythm of their strokes) (*Cvn*, pp. 57f., 87f.).

Two emendations to this initial list do bear mentioning here: the first is that it is necessary only that *nearly* everyone conform to a given regularity in behaviour in order for the regularity to be a convention, thus allowing for violations and exceptions. Secondly, another condition is added to the list, one which demands the possibility of deviation from a given convention, rather than simply permitting it:

- (4) everyone (or nearly everyone) would prefer that everyone (or nearly everyone) conform to R' , where R' is some possible regularity in the behavior of members of (population) P in (situation) S , such that no one in any instance of S among members of P could conform both to R' and to R . (*Cvn*, p. 76).

That is, another regularity that is, in a broad sense, incompatible with the first could have served in its place. It is important to note that the preference for R over R' is rationally grounded in the fact that others are expected to perform the former and not the latter. One is nearly always free to perform an action which is not in conformance with the dictates of convention: such alternatives could be such either as to be conventions in their own right, were they accepted by nearly everyone, or such as to be for some reason not viable as conventional alternatives, though performable by individuals in isolated cases (a type of violability not fully accounted for by Lewis, though cited in the analyses of 'rule' offered by Winch, Wittgenstein, and others [see below]). But if we presume a rational preference for maximization of utility among the members of a group, we see that the tendency to coordination equilibria that results selects unique solutions from sets of equally adequate alternatives.

Lewis excludes action out of obligation and sanction from the class of conventions. However, these features are, I believe, proper components of many linguistic conventions, and a suitable definition of a 'rule of usage' can be derived from (1) - (4) by amending (3) and (4) as follows:

- (3') Almost everyone prefers that almost everyone conform to R , or be obliged to conform to R , on condition that almost everyone conform to R , or on condition that failure to conform to R will lead to punishing sanctions;
- (4') Almost everyone would prefer that almost anyone conform to R' , or be obliged to conform to R' , on condition that almost everyone conform to R' , or on condition that failure to conform to R' would lead to punishing sanctions, where R' is some possible violable

regularity in the behavior of members of *P* in *S*, such that no one in any instance of *S* among members of *P* could conform both to *R'* and to *R*.

A rule of usage, then, will comprise (1), (2), (3'), and (4') as necessary conditions. (Subsequently I shall speak of linguistic rules, rules of usage, and rule-governed linguistic behaviour, without considering whether certain of these rules might in fact conform to Lewis' narrower definition.) Evidently these formulations are rough, and they certainly do not exhaust the list of necessary conditions (or comprise sufficient ones) for linguistic rules or conventions; however, enough has been said, I believe, to establish a groundwork for my thesis.

III

With these points in mind, let me turn now to some general considerations bearing on the prescriptive character of translation.

The language communities which the linguist confronts can manifest a variety of levels of systematicity in their rules of usage. Conventional (i.e., rule-like) regularities might be utterly uncoded, though uniform enough, and 'tacitly' followed to a degree sufficient to allow the communication needed to survive. Or the culture might have gone so far as to have achieved a written language and perhaps so far as to have codified grammatical rules and lexicographical equivalences. Indeed, there might even be a well established antecedent practice of translation (in the strict, intercultural sense—as opposed to Quine's broader usage) among certain members of the community, e.g., merchants. The linguist approaches this community initially in an effort to correlate the uttered strings of the source language with the target language on the basis (following Quine) of stimulus meaning, something Quine regards as an inductive, descriptive task, which is properly characterized as one of hypothesis formation.

Yet even here we must note an important difference, namely, that the regular affirmation of certain sentences in the presence of certain sorts of stimulations, as well as the other violable regularities in usage, are (contravenable) rule-governed regularities, and not matters of (uncontravenable) law-like regularity—the application of an expression to an object or to other expressions or its various equivalence and substitutability relations, are to a large degree the result of conventional coordination. Coming up with a right answer (i.e., a member of the admittedly nonunit set of right answers) in any simple case of translation depends on certain coordination equilibria being reached. For example, a simple coordination problem is manifest in the linguist's endeavour to prompt affirmative responses to presentations of stimuli; while a number of promptings will comprise a more complex coordination equilibrium, such as discovering an expression in the source language which has the

same stimulus meaning as some expression in the target language. The latter sort of coordination equilibria—complex ones—would be preferred for their facilitation, in turn, of equilibria in other coordination problems (e.g., those which might be instanced by rabbit hunting by linguist and natives).

In general (for the field translator, and for colleagues who may use the manual), analytical hypotheses comprised in a translation manual will serve to coordinate action cross-culturally (and such 'hypotheses' will be tested by their ability to help effect coordination). They help accomplish this specifically by helping to map the inscriptions that reflect rule-governed patterns of verbal behaviour in one language onto inscriptions reflecting such verbal patterns of another language. In terms of their (*interlingual*) function and the test of their adequacy, then, they seem quite analogous to the various sorts of codifications of usage that might be developed *within* either language. *Intralingual* antecedent practice would admit of much individual and temporal variation, but there would be enough consistency of usage and use to permit, and presumably enough complexity to make rationally preferable, certain grammatical and lexicographical systematizations (what Quine calls regimentations) of usage. Once developed, these would come, in turn, to possess a normative force over particular questions of usage, spelling, etc., so that they would, indeed, affect future practice, but they would remain malleable to future variations in usage that were extensive enough. The important point of contrast here is that it is rational preference to conform which primarily underlies whatever fixity codifications have, while the evidential grounding of physical theories, for all their codificational elements, avails them of a primary basis that is fairly independent of the changing tides of preference.

The notion of 'codification' I am employing here is expounded most clearly by Nelson Goodman, with respect to the problem of justifying inductive procedures in science, and it has more recently been extensively applied to ethical contexts by John Rawls.¹¹ The accepted rules of induction (and, for that matter, deduction) are culled from the antecedent practice of working scientists, that is, from the set of inferences which scientists regard as acceptable. The culling and formulation are reconstructions rather than simply descriptions of that practice. By applying these reconstructed, systematized, codified rules in the assessment of the validity of any inductive argument for acceptance of a

11 Cf. Nelson Goodman, *Fact, Fiction, and Forecast*, 3d ed., Indianapolis 1973, pp. 62ff. (hereafter cited as *FFF*); Richard Rudner, 'Nelson Goodman', *The Encyclopedia of Philosophy*, New York 1967, Vol. 3, pp. 370f.; John Rawls, *A Theory of Justice*, Cambridge, Mass. 1971, pp. 20ff., 577ff., and *passim* (hereafter cited as *TJ*). My usage of 'theory', restricted as it is to *descriptive* theories comprised entirely of hypotheses, varies somewhat with Rawls' usage; however, I do not believe the difference here is of any great consequence.

theory, those arguments which scientists find clearly intuitively acceptable prior to application of the rules, should be ruled valid on the basis of the rules; and those which are clearly intuitively unacceptable should be determined invalid. The general rules of induction (or, indeed, of any inductive practice) are designed to assess particular inductive inferences; yet at the same time they are derived from reflection on antecedent inductive practice, and are answerable to that practice: if the rule counts as valid an argument that scientists uniformly regard as clearly invalid, the scientists will keep the inference and amend the rule. In this vein, Goodman continues:

This looks flagrantly circular. I have said that deductive [and inductive] inferences are justified by their conformity to valid general rules, and that general rules are justified by their conformity to valid inferences. But this circle is a virtuous one. The point is that rules and particular inferences alike are justified by being brought into agreement with each other. *A rule is amended if it yields an inference we are unwilling to accept; an inference is rejected if it violates a rule we are unwilling to amend.* The process of justification is the delicate one of making adjustments between rules and accepted inferences; and in the agreement achieved lies the only justification needed for either. [FFF, p. 64.]¹²

Indeed, one of the main purposes of this or any other sort of codification is to improve on the uncoded antecedent practice. By explicitly formulating and incorporating into a coherent and generally acceptable set the rules that govern some practice (be they rules of a game, of legal practice, or of right action), a number of advantages are gained: of particular importance is the enhanced ability to form expectations of what others, known to be following the same rules, will do—this, of course, is the very advantage that convention-formation (and convention-formulation) manifests on David Lewis' view. The additional insights that Goodman's earlier reflections on codification provide is that the very stability of the codified rules depends on how well they square with the *ongoing* practice they codify (not simply the temporally antecedent practice from which they are derived). That is, one employs the rules to govern the practice, and is always willing to modify the rules if they diverge too much from that practice. As long as principles and practice generally coincide, they thus remain in what Rawls calls a state of 'reflective equilibrium'. However, Rawls further points out:

12 Although I shall not discuss this here, whether the circularity of reflective equilibrium is benign or not warrants more discussion than is given by either Rawls or Goodman. (See, e.g., Norman Daniels, 'Wide Reflective Equilibrium and Theory Acceptance in Ethics', *Journal of Philosophy*, 76, 1979, 256-81; and Michael Friedman, 'Truth and Confirmation', *Journal of Philosophy*, 76, 1979, 361-81.) Yet while this *general* issue is unsettled, I do not believe the circularity problems that affect the accounts of inductive and moral justification present any overwhelming difficulties for my extension of this notion of justification to translation (cf. my remarks at the conclusion of Sec. III).

this equilibrium is not necessarily stable. It is liable to be upset by further examination of the conditions which should be imposed on the [imagined] contractual situation [by reference to which principles of justice are determined] and by particular cases which may lead us to revise our judgments. [TJ, p. 20f.]

The open possibility of future 'disequilibrium' is particularly important to keep in mind in the case of linguistic codifications such as grammars, dictionaries, and translation manuals. For while a dictionary may *indicate* (a term I am here employing generally, so that descriptions and prescriptions are both counted as indicators) word usage, linguistic practice is of course very much subject to change. Even though there is good reason (again, the increased likelihood of achievement of coordination equilibria) for stability of these codifications, and for the attendant stability they confer on usage; gradual shifts, or consciously prescribed variations (e.g., technical languages), may nonetheless occur. And if these are extensive enough, the codifications themselves must be altered.

Moreover, I think it is illuminating to note that these linguistic conventions are prescriptive: they are such as *ought* to be followed within the source culture, as well as by outsiders who have dealings with it (it is a general shortcoming of Lewis' book that he fails to differentiate the genesis from the justification of conventions). The linguist, whose main purpose in developing a field manual is to enable speakers of the target language, other ethnographers, etc. to communicate successfully with source language speakers, thereby effecting coordination equilibria in a wide range of contexts, ought then to endeavour to develop his manual of translation so that its users may conform ideally to rules and conventions of linguistic usage operative within the source culture. As, at least, a 'partial joiner' of the source society, he and any manual user should seek to fulfill the expectations of the members of the source society. The very reasons that justify our saying that manual users ought to conform to the rules of usage of the source society, also justify our saying that manual users ought to conform to the dictates of the manual: for unless they have fully mastered the source language, they can only conform to the former rules by following the manual. Obviously problems can arise regarding the potential conflict between conformity to certain of the source culture's conventions when such behaviour conflicts with the moral dictates of one's own culture or some target culture. The difficulties that underlie the well-known debates over ethical and cultural relativism, however, do not I believe significantly affect that present context in any general way. If moral dictates are not violated, and if prudential dictates strongly favour conformance to rules of usage, then one ought to conform to them.¹³

13 This is not to say one may not judge certain linguistic practices in the source-language community immoral, or seek to change them. The point here is that *initial* conformity

IV

Now thus far I have argued only that it is cogent to claim that a linguist or manual user *ought* to employ a manual of translation in order to be able to conform when necessary to the source society's linguistic conventions. However, it still remains to be shown that 'employing the manual' amounts to adhering to a set of *translation rules*; i.e., that the manual is, generally speaking, prescriptive. I have not yet established this latter claim since one could argue that all that has been established in showing that the manual *ought* to be employed for some purpose is that it *should* be employed *as a descriptive theory* for that purpose. In short, we still might want to call the manual entries descriptive hypotheses *about* the source-society rules, descriptive hypotheses about how best to map source culture rules onto target culture ones. The patterns of behaviour on which these hypotheses were based would admit of much more 'free' variation (e.g., momentary or gradual shifts in usage, by individuals or groups) and this would no doubt affect the logical form of the nomological generalizations that might be involved. One would not, for example, demand unfailing deductive predictability of individual, specific affirmative native responses to promptings to rabbit-stimuli, for the usage of a term like 'gavagai' might come to shift in ways requiring 'lower valued' statistical generalizations correlating such usage with shifts in the regular application of terms to the animals an English-speaking person would call rabbits. One might think, however, that on the face of it, such variability might call only for a 'loosening' of the logical relations between hypotheses and prompted responses, in whatever ways such relations ordinarily obtain among statistical laws and the evidential statements supporting them in any reach of scientific inquiry.

However, I think two more related points will serve to establish that analytical hypotheses are more cogently to be construed as rules and *not* as hypotheses at all, and that, correspondingly, translation manuals are more cogently to be construed as *codifications* of rules and *not* as scientific theories. Most importantly, I am claiming that the form of the manual is not the one that Quine appears to be committed to in arguing for the indeterminacy of translation—and that this misperception or misconception on Quine's part accounts for some of the difficulties and puzzles which have characterized interpretations (including Quine's own) of the significance of his results.

The first point to note, then, is that the variability of usage, and the fixity of usage, are both relative to the interrelated choices and preferences of speakers of a language. Because the rules (unlike laws) can be violated and changed, and because rational preference among alternatives is a key element which may figure in favour of or against such

to linguistic conventions is preferable, and, indeed, necessary for the discovery and eventual reform of any linguistically conditioned injustice.

violation or change, rules possess what David Lewis and others take as a defining characteristic of having to admit of contravenability, against the choice of which latter course, rational preference, presumably, usually dictates. But where preference plays a role in *change* (or constitution) of rules and conventions, it can, of course, play no such role relative to immutable scientific law. Granted the important role of entrenchment, familiarity, and simplicity of predicates in the *formulation* of both laws and rules, still the logical structure and semantical character (e.g., the *truth or falsity*) of lawlike statements and empirical generalizations, and the logical relationship of hypotheses at the various levels of physical theory to such laws and generalizations and to observation-statements, are both determined by a disparate semantical status which the formulations of theories and hypotheses have (in contrast to the semantical status of rules): formulations of laws are to fulfill the function of *making claims about what is the case* in contrast to the function of formulations of rules which is, speaking broadly, to indicate what should be, but which might *not* be the case *without abridging the acceptability of the rule*. So, scientific laws (i.e., true lawlike statements) and scientific hypotheses (deducible from them and, perhaps, other true statements) will, of course, not fulfill important criteria of conventionality: first, they will be 'conformed to' always and everywhere, even where no one prefers conformity to them; hence they violate the third criterion of conventionality (or rule likeness) noted above. Second, failure of an observational statement (implied in a scientific theory to hold), cannot be 'withstood' by any proposed scientific law. Either the law or the counterinstance must be 'rejected'; we cannot accept both (the point applies *mutatis mutandis* for statistical laws as well). If a state of affairs (including, in the statistical case, ensembles of events) holds that is regarded as a contravention of a law, the law must be amended (i.e., rejected). Precisely such *violability* is, however, a defining characteristic of conventions.

Although it is, indeed, partially covered by Lewis' fourth criterion of conventionality, and although it is at least implicit in his qualification of several of the formulations of criteria of conventionality (i.e., qualified as apply to *almost* everyone), the violability criterion is not clearly or rigorously or fully captured by Lewis' definition of convention. In particular, one would want to insist on the inclusion of a condition which unequivocally implies that alternative behaviour be possible which would not be such as to constitute an alternative convention. There are many alternative actions one could take that would constitute violations of a convention, but which could not constitute alternative conventions. There are many alternative actions one could take that would constitute violations of a convention, but which could not themselves ever become stabilized, conventional behaviour patterns among a rationally-self-interested group of people.

What Quine calls 'analytical hypotheses', on the other hand (and this goes, presumably, for other translational hypotheses as well), may well be construed as meeting the criteria for rules. For, the 'correctness' of a sentence, "'gavagai' means (is to be translated as) 'rabbit'" will depend upon continuing, *rule-governed* regularities at least among target- and source-language users, regarding utterance of certain expressions in the presence of certain animals. This will, in fact, be (in the sense of Lewis' analysis) a matter of choice and preference and coordination of behaviour. If, for example, the source-language speakers evolve a second term for gray-haired rabbits, applying the first only to white-haired rabbits, an emendation in some original analytical hypothesis may be required. And, just as any source-language speaker might violate the convention of applying 'gavagai' to rabbits, and just as other conventions of rabbit-naming must be available, so may a manual-user choose not to translate an expression according to the manual's dictates, or choose not to use 'gavagai' in a context that calls for indicating the presence of a rabbit, and so must other translational conventions be available to the user. Indeed, one very good example of a viable alternative translational rule to "'gavagai' means 'rabbit'" is "'gavagai' means 'rabbithood'" (or "'gavagai' means 'rabbit-stage'", etc.). Although one could, with suitable emendations throughout one's manual of translation, get along in the source society equally well using either convention, one of them might be preferred over the other in contexts of translating source strings into target strings, or in using such translations to determine native beliefs. As it stands, the preference of anthropological translators is (let us say) to use 'rabbit' as the target string, but they might well prefer 'rabbithood' if all or almost all other translators did. Hence alternatives to existing translation correlations are available owing to the potential variability of choice of conventions or rules of usage in three different contexts: that of the source society, that of the target society, or that of the linguist's own technical, anthropological society. Further, it might plausibly be argued that as translation manuals are so constructed that one conforms to source-language usage if and only if one conforms to the translational correlations and to the target-language usage, any violation of a source culture rule, by anyone, is *ipso facto* a violation of some corresponding convention, under a suitable description, for target-language speakers (and translators) dealing with the source culture. I.e., anyone's failing to conform to the convention of ascribing the term 'gavagai' to rabbits (that is, a convention which we are now stipulating to be stated entirely in the source language) is also violating the following convention:

Persons should apply source-term 'gavagai' to animals which comply with the condition of being the referent of conventional usage of the target-term 'rabbit'.

Hence certain translational correlations may be construed as violable simply because source language usage is violable.¹⁴ But any translational correlation is violable, and any is such as to admit of a fully viable alternative. Indeed, actions in conformance with viable alternatives are simply a subset of the class of nonconformists to a convention: as Lewis defines them, alternatives to conventions are such that one cannot conform to both a convention and any such alternative.

Again, with target- and source-language communities, choice, preference, and coordination will underlie the rule-like regularities in behaviour from which the linguist develops a manual, and these features will be manifest in the *occasional violations* that the linguist will note in the utterances of informants and which the linguist can get the informant to correct or which the linguist can justifiably write off without advisement as violations once the manual is sufficiently developed. Also, alternative ways of describing things are possible, which are not to be followed unless others follow them, for otherwise communication is likely to fail; but some alternatives which could become generally acceptable will be seen to emerge as conventions as usage evolves. The character of translation-manual entries will be responsive to these rule-governed regularities within target and source speech communities considered in isolation from one another. And the manual entries will be further dictated by the conventional agreements of translators.

In summary, then, my first point in arguing to the rule-like character of translation is that translation manuals and the 'hypotheses' that comprise them fulfill the conditions of *rule-likeness*, generally, in possessing certain key characteristics of violability and of being replaceable by equally viable alternatives (the class of whose compliant behaviours must be different). They also possess that characteristic feature of codifications, namely of being 'responsive' to variations in the antecedent practice of language use by source- and target-language speakers, and the antecedent practice of translators themselves, without being *descriptions* of any of those practices. They are in 'reflective equilibrium' with the intuitions of proper usage of members of each of those three communities and function as instruments of coordination.

14 However, such a rule would not be part of a typical translation manual, as the latter specifies rules applicable only to its *users*, and which could only be violated by them. That is, monolingual source-language speakers could violate a rule of the sort considered here in the text, though they could not know what the rule was; however, they could not violate a rule of the form 'Translate *S* in the source language as *T* in the target language' consciously or unconsciously. This brings out the fact that the full account of rule-compliance and reflective equilibrium of translation involves complexities not encountered in the sorts of cases discussed by Goodman, Lewis, or Rawls—insofar as the parties to conventional coordination need not be privy to the conventions with which their behaviour complies. These complexities certainly merit attention, but do not affect my general thesis.

My second point also can be seen to evolve from a reconstruction of certain of Quine's views, and requires, in particular, subscription to the broadened notion of 'translation' that he employs. That is, on Quine's view, the regularities in usage which a *grammar* codifies are in fact regularities in antecedent practices of *translation*: for all interpretation, communication, construal, etc., that goes on *within* a language community is construable as *intralinguistic* translation. Granting this, then if the native's own, and the target speaker's own, grammar and lexicon are to be regarded as codifications (and I am arguing that they do meet the requisite criteria), and if these *are* codifications of antecedent translational practice, how can we fail to regard translation manuals as belonging to the same genus they do (i.e., as being codifications *of* rules rather than descriptive theories)? They enable speakers of one natural language to understand and respond to speakers of another language, in just the way that a dictionary might well enable a child to understand and respond to the otherwise unfathomable talk of elders. Further, once adopted and put to extensive use in *intercultural* communication and interaction, the manuals will in their turn undoubtedly have an influence on the general character of the coordination problems that speakers of each language will face; e.g., they will allow this insofar as they allow cultural interchange and the posing of new sorts of coordination problems to members of each culture. As such changes will, in turn, be bound to have an effect on linguistic usage, the manuals themselves will then play a role in transforming each language. Nor is it entirely unlikely that translation manuals might come to have a normative effect on target or source usage, in the way that their respective grammars do.

These considerations lead me to view 'analytical hypotheses' (and 'translational hypotheses' in general) not as hypotheses at all, but as *rules*, in the sense delineated above. Moreover, they are taken to be rules which justifiably possess a normative force; since conformity to them is the 'outsider's' means to conformity to source society linguistic conventions, and conformity to the latter is clearly 'dictated by reason', where one's purpose is to 'get along' in the source-language community. Thus, formulations of translations are best construed as having the canonical form of *imperatives*, rather than declaratives.¹⁵ Such a form clearly indicates that they are not statements of fact, but prescriptive statements, directing manual users to make source-language utterances and inscriptions which will optimally effect coordination of activities involving source-language speakers and themselves. Though typically construed as assertions, sentences such as "'gavagai" means "rab-

15 Lewis himself is unclear on the canonical form he views statements of conventions as having, and this is directly related to the earlier-noted conflation of issues concerning the description of the *genesis* of conventions with questions concerning the justification of their adoption.

bit"', "'rabbit' is the meaning of 'gavagai'", or "'rabbit' and 'gavagai' have the same meaning', are best taken to be construed, after reflection on the problems of indeterminacy and the codifying function of these metalinguistic sentences, as the imperative 'Translate "gavagai" as "rabbit"'. Of course this is not to say that instances of the former types of locution need be abandoned in practice, just so long as one is not misled by their apparent form into thinking that by using them one is *stating* facts about what source-language speakers mean (or, for that matter, into thinking that one is ontically committed, by virtue of their use, to the existence of meanings as entities shared or meaning as entities transmitted¹⁶).

In the context of field translation, these rules function in the following way. Through careful study of source-culture linguistic, and related nonlinguistic behaviour, the linguist compiles a manual of translation which, ideally, will enable all target-language speakers who use the manual to communicate successfully. There will be no failures to coordinate activities or to fulfill certain expectations, traceable to faulty translating or misuse of words. Presumably the linguist's field methodology will be such as to test the developing rules of translation as exhaustively as possible to insure that they will enable a user to employ source-language terms in ways that insure that the coordination equilibria that depend on verbal communication will not fail to be achieved on account of linguistic failure. The mark of a good translation manual will be that it will contain rules which, if adhered to by target-culture users (who must then also be privy to the linguist's metalanguage), will enable them to conform to rule-governed regularities of speech behaviour operative in the source-language community.

Analytical hypotheses are not, then, in an important sense, hypotheses at all, i.e., they are neither true nor false nor to be accepted as true or false. But this is not so because they cannot be *right* or *wrong*, acceptable or unacceptable (an untenable view which is often, and unjustifiably attributed to Quine), nor simply because of the multiplicity and imposition problems that Quine cites; rather, it is because they are conventions and/or rules. These two problems are symptomatic of our taking a prescriptive task to be a descriptive one, and they cease to afflict us once the recommended shift of view is made: we should expect there to be a variety of incompatible translation rules or manuals available to us, for such availability is a necessary characteristic of rules; we should expect our resultant systematization to reflect the imposition of elements familiar to the metalinguistic and target-language communities,

16 With regard to this latter point, except for providing a more commodious prescriptive setting, I offer no further substantive argument against ontic commitment to intension than does Quine in his insistence on noncommittal regimentations of intensional contexts.

because it is a codification that is sensitive to the antecedent linguistic practices of three language communities and which is geared to effect coordination of linguistic and nonlinguistic behaviour among members of those three language communities.

V

Now I would like to apply the results of the foregoing discussion to a methodological problem in anthropology concerning the epistemological status of translation and ethnographic data that shares much of the substance of Quinean indeterminacy. The problem arises with respect to a method of translational and ethnographic inquiry (developed by Ward Goodenough), called *componential analysis*, that is designed to reveal the intension (in C. I. Lewis' and Rudolf Carnap's sense), or the 'components', of source-language expressions through techniques of contrast/complementarity determination derived from phonology.¹⁷ (This approach has been applied both by itself and in combination with other approaches.) It concerns whether or not one can demonstrate that the taxonomies delineated by empirically successful componential analysis *actually* comprise the source-language speakers' conceptual scheme.

This problem of *cognitive* (or *psychological*) *validity*—which plagues other related and more recently developed methodologies as well¹⁸—has a genesis similar to that of the indeterminacy problem examined above: it develops both from a concern to avoid the imposition of western preconceptions on ethnographic subjects (a concern echoed throughout the language-and-culture tradition, from Boas onward), and from a generally acknowledged availability (in practice and in principle) of alternative yet divergent translations, and other ethnographic systematizations, in field work. Of concern in discussions of cognitive validity is first whether it is a necessary criterion of adequacy, and, if the answer to this is yes, how its fulfillment is to be demonstrated—i.e., how the imposition and multiplicity problems are to be circumvented. I shall focus attention on the prior question only, since in acceding, for my own reasons, to the indeterminacy of translation, I reject cognitive validity as a cogent criterion of adequacy.

Goodenough and other componential analysts envision an integrated linguistic and ethnographic methodology—one designed to enable the *description* of culture, viewed as a *mental* phenomenon, i.e., as 'the

17 See, e.g., Goodenough's 'Componential Analysis and the Study of Meaning', *Language*, 33, 1956, 195-216; and 'Cultural Anthropology and Linguistics', in Dell H. Hymes (ed.), *Language in Culture and Society: A Reader in Linguistics and Anthropology*, New York 1964, pp. 36-39. The influence of Lewis and Carnap is mediated by Charles Morris.

18 E.g., there are anthropologists and linguists who dispute whether transformational approaches can ever achieve more than 'descriptive adequacy' (maximal simplicity and empirical adequacy).

forms of things that people have in mind, their models for perceiving, relating, and otherwise interpreting'. Such description is to be achieved through *eliciting operations* (carefully constructed questioning under controlled stimulation, essentially the procedure outlined by Quine in *W/O*) that, ideally, eventually enable the ethnographer to discriminate stimuli in just the way the subjects do. Components are a relatively small but powerful set of concepts that (cross-culturally) provide the essential characteristics of source language terms. (E.g., in English, 'son' is differentiated from 'father' through application of the component *membership in +1* (from ego's) *generation* to the former and of *membership in -1 generation* to the latter; while 'mother', which shares the component *+1 generation* with 'father', differs by virtue of possession of the *female sex* component.) What the cognitive validity criterion demands of the set of components is that they enable the linguist to understand the criteria that source-language speakers use in organizing their conceptual scheme—everything, all genera and differentiae, are to be accurately reflected: the inquirer must, according to Goodenough, 'find the conceptual elements out of which linguistic units are built'. Determination of source-culture conception and discrimination is essential: as Charles O. Frake emphasizes, 'The distinctive "situations", or "eliciting frames", or "stimuli", which evoke and define a set of contrasting responses are cultural data to be discovered, not prescribed, by the ethnographer', whose main objective, in A. F. C. Wallace's words, 'is to define the taxonomic system itself—that is, to explicate the rules by which users of the terms group various social and genealogical characteristics into concepts'.¹⁹

Yet Goodenough admits that the field linguist's predictive success, and the ability of manual users to get along with the source society, can only partially validate an ethnography. Beyond this, an ethnography, like any scientific theory, is to be measured in terms of its formal properties:

What is required is to construct a theory of the conceptual models which [observed material phenomena] represent and of which they are artifacts. We test the adequacy of such a theory by our ability to interpret and predict what

19 Goodenough, 'Cultural Anthropology and Linguistics', p. 36; 'Componential Analysis and the Study of Meaning', p. 196; Frake, 'The Ethnographic Study of Cognitive Systems', in Gladwin and Sturtevant (eds.), *Anthropology and Human Behavior*, Washington, DC 1962, p. 83; Wallace, 'Culture and Cognition', *Science*, 135, 352. Goodenough's insistence on the cognitive validity of componential analysis closely parallels his espousal of the 'ethnoscience' approach to ethnography: i.e., his insistence that the ethnographer use culturally specific, 'emic' concepts that are in conscious use by the society under study in describing its culture. (See his *Property, Kin, and Community in Truk*, Yale University Publications in Anthropology, No. 46, New Haven 1952, and his *Description and Comparison in Cultural Anthropology*, Chicago 1970.

goes on in a community as measured by how its members, our informants, do so. A further test is our ability ourselves to behave in ways which lead to the kind of responses from the community's members which our theory would lead us to expect. Thus tested, the theory is a valid statement of what you have to know in order to operate as a member of the society and is, as such, a valid description of its culture. Its acceptability beyond this depends largely on the aesthetic criteria to which scientists and mathematicians customarily refer by the term 'elegance'.²⁰

So, for Goodenough, what it is that one has to have in order to get along with the source society is a theory about the conceptual models of the members of that society. The theoretical predicates of the ethnographer's theory refer to the members' theoretical (in a broadened sense) constructs. Roughly, it is a theory of their theory of nature, a theory of the nonobservational, theoretical constructs of their theory.

And yet in citing this crucial role of elegance, Goodenough is allowing into his method of inquiry just the undesired imposition of preconceptions that, on Quine's view, renders such constructs (and theoretical sentences) indeterminate. As noted above, the appeal to elegance and simplicity is an internal one on Quine's view and thus precludes the warranted assertibility of claims about native constructs (a point which is reinforced if Goodenough's notion of 'elegance' involves the extended application of familiar metalinguistic concepts—e.g., kinship components—in a new ethnography). Quine's adaptation of the Kalaba-example (from Kenneth Pike, an ardent supporter of the cognitive validity criterion) is aimed precisely at showing that sameness in the discrimination of stimuli in the eliciting situation cannot be demonstrated.

If we could assumed that our Kalaban speaker and our English speaker, when observed in like external situations, differed only in how they say things and not in *what* they say, so to speak, then the methodology of synonymy determinations would be pretty smooth; the narrowly linguistic part of the causal complex, different for the two speakers, would be conveniently out of sight, while all the parts of the causal complex decisive of synonymy or heteronymy were open to observation. But of course the trouble is that not only the narrowly linguistic habits of vocabulary and syntax are imported by each speaker from his unknown past. [*PML*, p. 61.]

Further, as with the general intensionalist position that Quine criticizes, the imposition problem is abetted by the multiplicity problem—this latter being cited by the anthropologist Robbins Burling in a critique of the cognitive validity criterion, entitled 'Cognition and Componential Analysis: 'God's Truth or Hocus-Pocus?''²¹:

any of hundreds of thousands of logically alternative solutions might predict which term can be used in some situation, but the success of that prediction

20 Goodenough, 'Cultural Anthropology and Linguistics', p. 36.

21 *American Anthropologist*, 66, 1964, 20-28. (Hereafter cited as *CVCA*.)

does not demonstrate that the speaker of the language uses the same scheme, or indicate whether or not all speakers use the same one. [CVCA, p. 24.]

Burling agrees that the ethnographer must seek to enable himself and others to 'get along' in the society, but raises the multiplicity problem (as well as noting the inherent vagueness of the western anthropologist's own intensions) to criticize the further claim that 'God's truth' proponents wish to make. He takes to task two proposed taxonomies of disease terms and botanical terms, by Frake and Harold C. Conklin, respectively,²² constructing an alternative to Conklin's scheme that accords fully with the presented data and charging that

[s]tudents who claim that componential analysis or comparable methods of semantic analysis can provide a means for 'discovering how people construe the world' must explain how to eliminate the great majority of logical possibilities and narrow the choice to the one or few that are 'psychologically real'. I will not be convinced that there are not dozens or hundreds of possible analyses of Subanun disease terms until Frake presents us with the entire system fully analyzed and faces squarely the problem of how he chooses his particular analysis. In the meantime, I will doubt whether any single analysis tells us much about people's cognitive structure, even if it enables us to use terms as a native does. [CVCA, p. 26.]

Indeed, Burling proceeds to a somewhat stronger conclusion, that componential analysis, and other linguistically oriented techniques, provide no answer to a basic Whorfian puzzle: 'The language patterns were there to be sure, but how, except through intuition, could one tell whether the patterns corresponded to anything else?' And thus structural semantics provides no additional insight into human cognition. However, he does not thereby devalue such approaches altogether:

This conclusion may sound harsh, but it does not imply that 'structural semantics' is useless. There is a real problem of formulating rules which will predict the use of terms, or to put it another way, of specifying the relationship between terms, on the one hand, and events and situations in our extralinguistic experience, on the other. In still other words, it is legitimate to try to specify precisely what terms 'mean'. The exercise of carrying out a formal analysis, moreover, is certainly useful in checking the completeness and adequacy of one's data, in exactly the same way that writing up a grammatical statement may make one aware of previously unimagined possibilities, whether or not these are attributed to the speaker. If nothing else, a precise statement of the objects to which terms are applied is certainly a help to someone wishing to learn the language or use behavior which will be effective. [CVCA, p. 28.]

22 Frake, 'The Diagnosis of Disease Among the Subanun of Mindanao', *American Anthropologist*, 63, 1961, 112-32; Conklin, 'Lexicographical Treatment of Folk Taxonomies', in F. W. Householder and S. Saporta (eds.), *Problems in Lexicography*, Supplement to *International Journal of American Linguistics*, 28, Indiana University Research Center in Anthropology, Folklore and Linguistics, Publication 21, Bloomington, Ind. 1962, pp. 119-41.

On Burling's view, the value of grammatical analyses is not in their enabling one to discover 'something about the language which is "out there" waiting to be described and recorded' or 'some "psychological reality" which speakers are presumed to have', but rather, as maintained by proponents of the 'hocus-pocus' viewpoint, only in their enabling one to find 'a set of rules which allow us to use terms the way others do', and 'which somehow take account of the observed phenomena' (ibid.).

In fact, stripped of their mentalistic baggage, Goodenough's own definitions of language and culture reflect a remarkably similar, 'hocus-pocus' character:

As I see it, a society's culture consists of whatever it is one has to know or believe in order to operate in a manner acceptable to its members, and do so in any role that they accept for any one of them themselves.

Indeed, we may define a language in precisely the same terms in which we have . . . defined a culture. It consists of whatever it is one has to know in order to communicate with its speakers as adequately as they do with each other and in a manner which they will accept as corresponding to their own.²³

In light of these definitions, Burling's challenge seems very much to the point. If 'describing' culture and language involves nothing more than learning what one must in order to get along, what reason is there to further insist that only the rule-system of the source society can serve this purpose (or, for that matter, that there is some one system that all of its members follow)?

The argument would be further supported by consideration of the philosophical points that motivate the choice of canonical idiom. After all, if the goal and test of the ethnography or translation manual is simply its enabling target society members initially unfamiliar with the source society best to get along with the members of the latter, what need is there to worry about the fact that alternative formulations would equally meet all cogent criteria of adequacy? And, more to the point, if one considers carefully, as does Quine, the very *purposes* of the manual and ethnography, which, by their very nature, are in part conditioned by the needs of the *target* language speakers, and by the somewhat different needs of the *linguistic metalanguage* speakers, one will see that manuals of translation and other ethnographical semantic analyses cannot possibly achieve the task of *intercultural* coordination, while at the same time revealing what is going on 'inside' the source culture, in isolation from other cultures. So much of the manual of translation is determined by the explicit codifications of usage among speakers of the target language (as well as of 'anthropologese'), that it makes no sense to ask that the manual come to capture the native's 'inner meanings' as well.

23 Goodenough, 'Cultural Anthropology and Linguistics', pp. 36, 37.

All the empirical and formal criteria of adequacy that one could demand of it necessarily involve its successful and maximally efficient employment by 'outsiders' to the source society. It is written by and for them: the ethnographer, *qua* ethnographer, is collective bargaining agent, out to best coordinate the interests of members of two societies, in keeping with the established procedures of his own anthropological society. Of course anyone, including the ethnographer, can use the manual and ethnography as a stepping stone to becoming a fully participating member of the source society, perhaps fully forgetting all traces of one's former life. This is perfectly possible, but it is not the task of an ethnography to *reproduce* this process: its task is to say useful things to target- and metalanguage speakers who have some interest in the source culture's doings.

If one agrees with Goodenough that in doing ethnography one is taking one's target-society and anthropological audiences into the source culture, and telling the members of those audiences what they have to know in order to get along in the source society, then I think one must disagree with him that one is thus also telling the audience what the source-language speaker's concepts really are.

VI

Let me now summarize the present stage of these investigations. What I have argued is that 'recovery' of symbol systems (particularly, referential schemes) in radical translation is a matter of codification rather than description. I have been led to this view by consideration of Quine's indeterminacy thesis, especially insofar as it endeavours to contrast translation and scientific theorizing by appeal to the imposition and multiplicity problems. The nature of these and other grounds for the indeterminacy and inscrutability theses has not been clearly articulated by Quine, thus creating, in some quarters, the illusion that the point of indeterminacy is that no rational criteria are applicable to translation, and suggesting the less extreme, yet still deeply disturbing conclusion that a wide range of linguistically oriented approaches to ethnography and translation must be rejected (the latter consequence seemingly one Quine accepts). I have sought to clarify the basis for these distinctions between translation and theorizing in such a way as to preserve some of the important consequences of this and other of Quine's theses regarding meaning, while giving some insight into the nature, possible yields, and criteria of adequacy of field translation. A cogent basis for these distinctions, I have argued, is a contrast between the rule-like regularities the linguist *codifies*, which are themselves constitutive of his own codification, as opposed to the lawlike regularities that the physicist *describes*.

In citing the violability of rules as the key logical feature that differentiates them from laws, I strike a chord that is familiar to those acquainted

with Peter Winch's arguments for methodological separatism in the social sciences—a thesis which calls both for the replacement of the hypothetico-inferential-test method of the physical sciences by one more akin to the philosophical method of conceptual analysis, and for the demonstrable recovery of the source-society's rules as a product of this method.²⁴ To neither of these theses do I subscribe. With regard to Winch's latter point, my view seems quite contrary to his in that I hold that the product of translation is itself codificational, comprising a special set of rules couched in a metalanguage that only *mentions*, and does not *use*, the target- or source-language strings in question; and which furthermore is directed to the coordination of linguistic activity of three communities, and thus cannot serve to 'reflect' the 'inner' character of just one of them. The rules one learns from the translation manual are *not* the rules of the society, but rules that enable an outsider (or a bilingual) to get along.²⁵ Ideally, these rules will enable one to get on *as well* as if one understood the native's rules. (My view accords better with those that take interpretation to be *mediation* rather than recovery or description.) With regard to Winch's commitment to conceptual analysis, I am not certain of the exact degree of compatibility between us. I do not see that my view entails that there is a need for methodological retrenchment in social science. I have argued only for acceptance of a canonical form or construal of what field linguists and ethnographers produce—with the intention only of dispelling certain pseudo-problems that stem from confusions in the philosophical background they share with certain philosophers of language. I take what I have shown serves to *preserve* the values of componential analysis in the face of indeterminacy. Indeterminacy is perhaps inescapable, but, as Burling concurs, structural semantics is not thus necessarily compromised. Although I do thus undercut certain considerations, concerning purported drawbacks of standard methodologies, that seem to militate in favour of separatisms such as Winch's or the ethnoscientists', if *other* grounds can be provided for seeing explanatory advantages in these approaches, my thesis would be neutral with respect to them.²⁶ I wish them all the empirical success they *can* have.

It is also important to point out that I do *not* claim that my view of translational 'hypotheses' holds as well for *all* anthropological claims. It is not directed at ethnographical or ethnological hypotheses made along

24 Cf. Peter Winch, *The Idea of a Social Science and Its Relation to Philosophy*, New York 1967, Chap. 1; L. Wittgenstein, *Philosophical Investigations*, London 1953, I, pp. 215-37.

25 See esp. Richard Rudner, 'Some Essays at Objectivity', *Philosophic Exchange*, 1, 1973, 115-35.

26 That is, the employment of emic notions in an anthropological metalanguage can be regarded (like components and the taxonomies they delineate) as justifiable 'hocus-pocus'.

with, and perhaps with the aid of, translational codifications. My point is that there are certain aspects of anthropological theory-building activity which are codificational in nature, and do not of themselves involve the production of a theoretical product. For instance, I do not contend that the ethnographer formulates no hypotheses at all, or that there is no value in the ethnographic hypotheses that are built upon a translational or semantic-analytical base—but only that this base is itself codificational, and hence that its construction need involve no ‘penetration’ into what is demonstrably the source-society’s mental or cognitive realm.

The anthropologist’s task is not only a matter of codifying, and the important thing is not to confuse, in theoretical reflections, its codificational aspects with its hypothetical-descriptive ones. Once the codificational facets of the anthropologist’s work have been settled, some of their generated products may be used in hypothesis formation and theory construction. Field translation is a process of codification which yields target-language translations of native remarks, which translations may be used in communication with source-language speakers in order to recover important data concerning, say, farming practices, procedures in rituals, cultural history, etc. And a good codification will be such as to yield accurate information regarding these things. Moreover, codification functions also in the background (in Quine’s sense) of the anthropologist’s metalanguage, used in hypothesis formulation, which contains certain technical predicates which themselves arise from a process of codification—i.e., the selection, reconstruction (clarification, disambiguation) of antecedent linguistic practice of some natural background language to the metalanguage (in Goodenough’s and Quine’s case, English). Terms preserve something of their former usage, while at the same time their usage comes to be governed by more rigorous conventions and rules suited to the anthropologist’s tasks. (This is precisely the process Quine calls ‘regimentation’.) This codificational feature is one that the anthropologist shares with any other scientist: for the physicist, ‘work’ has a very precise meaning, which is nonetheless not entirely divorced from ordinary English antecedent usage; while the anthropologist employs such clarified technical notions as ‘peasant’ and ‘agriculture’, which have also developed through a process of codification, construction, and definition. With this metalanguage thus settled, the anthropologist may formulate and test hypotheses, say, to the effect that past agricultural practices have led to soil depletion, altering the means of subsistence, with certain effects on social structure, customs, etc. These are fully legitimate hypotheses whose status as such is in no way threatened by what I have said regarding translation. A comparative linguist may determine certain cross-cultural universals of language that demand for their determination certain fixed conventions of translation (such as the employment of

as fixed an individuating apparatus as is empirically feasible) yet which do not follow as logical consequences of those chosen conventions. An ethnographer may discover all manner of facts about ongoing customs, social stratifications, remembered and recorded history, etc., through queries asked in a source language that is understood in the sense delineated above.

How well my considerations and conclusions square with Quine's position is not easy to tell. Certainly I disagree with his stated position with regard to the possible yields of translation in ethnography. I believe my reconstruction provides a clearer basis for a distinction of the sort Quine wishes to draw between translation and theory, even if it is not the basis he has in mind. Also, it provides a clearer basis for arguing that successful translational correlations need not involve the social inquirer with recovery of the 'real' belief of subjects or cause him to worry as to whether their cognitive structures have been accurately 'captured' or 'reproduced'.

Even if one found reason to differ with my thesis that translational correlations are rules, and, indeed, were one to insist *contra* Quine and myself that they are genuine hypotheses having truth value, one might still draw similar conclusions regarding real belief from the perplexities of Quine and Burling. That is, even though translations might plausibly be regarded as having truth value, the fact that they are, in essence, correlations aimed to maximize intercultural coordination is not to be overlooked in deriving inferences as to *underlying* belief or meaning—in moving from warrantably assertible correlational claims to supposedly assertible descriptive claims that involve an illicit shift of focus to a semantic realm that is peculiar to the source-language community. One might feasibly regard the former as determinate, as true about ordered pairs of source and target strings, yet regard the latter as indeterminate. (This could well be Burling's view.)

Yet if it is the coordinative features of translation that lie at the heart of the various indeterminacy problems, I believe we have a basis for further differentiating translation and physics; indeed, for construing translational claims not as hypotheses at all—but not so much because of the point Quine stresses, i.e., their failure to be about *anything* (which doesn't seem warranted by the considerations he gives, since the sort of position sketched immediately above seems to accord with them, while preserving the factuality of translation). Rather, it is failure to see the resultant prescriptive character of translation that, in my view, creates perplexity and confusion. This is the fundamental point—more fundamental, in fact, than the absence of translational truth value which both Quine's and my view entail: that is, were one to prefer a canonical form of the following sort, 'Given criteria *a*, *b*, *c* (or the objective of "getting along"), the best translation of *S* is *T*', whose instances might plausibly

be said to have truth value, I would not regard this as a substantial disagreement with my position (unlike the view sketched in the previous paragraph).²⁷ Also, focus on the coordinative, and derivatively the prescriptive, character of translation makes the *generalizability* of the point evident. Like Quine I take the radical translation problem to apply to interpretation generally, and even though I have focussed on the anthropological context, I do not take the radical differences in cultural background among the parties to radical translational coordination (or the exact number of distinct linguistic backgrounds involved) to be the essential point. Rather, it is the supremacy of coordination over description, something common to both inter- and intralingual translation that is the key concern—and I suspect that at the core of a good many of the ‘special’ problems of interpretation is that it is asked to do more than arbitrate or mediate, but that is perhaps all it can do.

27 It is important in this connection not to miss the Quinean reconstructive spirit of my regimentation of translational correlations. I do not claim that anthropologists are or ought to be collecting data and validating their claims differently, or that the ‘real meaning’ of their claims is what I suggest. I am simply offering a construal of the translational product that I hope eliminates confusion and concern with certain pseudoproblems.