

inconclusive empirical resolution of the mind-body problem. But the materialist can stalemate Kripke's conclusions by formulating the identity thesis more carefully in terms of the distinction between rigid and nonrigid designation, admitting that the identity thesis is contingent, but only in the sense that minds are possibly distinct from nonrigidly designated bodies. This imposes a new terminology, but one to which the materialist can readily adapt to accommodate Kripke's objection. The reformulation of mind-body identity does not change the substance of the materialist's position, nor does it determine whether the identity thesis is true or false.

There is a straightforward transposition of reductive materialism in the Kripkean mode. The materialist can hold that actually  $D = \lambda Cx$ , but possibly  $D \neq \lambda Cx$ . It can then be stipulated Kripke-style that  $\lambda Cx = B^*$ . But from this it does not follow that possibly  $D \neq B^*$ . The inference requires the invalid substitution of the rigid designator ' $B^*$ ' for the nonrigid designator ' $\lambda Cx$ ' in the ineliminably intensional or referentially opaque modal context 'Possibly  $D \neq \lambda Cx$ '.<sup>17</sup> The intensionality or referential opacity of modal possibility contexts constitutes a final line of defense of nonrigidly formulated reductive materialism or mind-body identity theory against further Kripkean linguistic subterfuge. Materialism is ultimately secure from facile assaults involving rigid designators provided the theory is not explicitly committed to the logical possibility of rigidly designative mind-body dualism.

Kripke's argument therefore has different and weaker force than has sometimes been supposed. Kripke does not refute materialism, but only certain versions of reductive materialism inadvisably expressed in terms of rigid designation. Materialism does not require rigid designators to advance theoretical mind-body or mental-neurophysiological event identities, nor to acknowledge the empirical contingency of materialism or logical possibility of mind-body dualism. The mind-body identity problem like other genuine ontological issues resists stipulative semantic-philosophical resolution. The identity of mind and body may stand or fall as an empirical scientific hypothesis only by the accumulation and interpretation of experimental evidence.

<sup>17</sup> The intensionality or referential opacity of modal contexts is demonstrated by W.V.O. Quine, *Word and Object* (Cambridge: The MIT Press, 1960), pp. 191-232; "Three Grades of Modal Involvement", *The Ways of Paradox and Other Essays*, revised enlarged edition (Cambridge: Harvard University Press, 1976), pp. 158-164; "Reference and Modality". *From a Logical Point of View*, second revised edition (Cambridge: Harvard University Press, 1961), pp. 139-144.

## Meaning, Grammar, and Indeterminacy\*

by Stephen Neale\*\*

### Summary

It is a mistake to think that Quine's thesis of the indeterminacy of translation reduces to the claim that theories are under-determined by evidence. The theory of meaning is subject to an indeterminacy that is qualitatively different from the under-determination of scientific theories. However, there is no reason to believe that the indeterminacy thesis extends beyond translation and meaning, and hence no construal of the thesis prevents one from being a realist about grammars, construed as partial theories of mind.

### Résumé

C'est une erreur de croire que la thèse quinéenne de l'indétermination de la traduction se borne à affirmer que les théories sont sous-déterminées par l'expérience. La théorie de la signification est sujette à une indétermination qui est qualitativement différente de la sous-détermination des théories scientifiques. Il n'y a cependant, pas de raison de croire que la thèse de l'indétermination s'étend au-delà de la traduction et de la signification, et par conséquent aucune interprétation de cette thèse n'empêche d'être réaliste quant aux grammaires en tant que théories partielles de l'esprit.

### Zusammenfassung

Es ist ein Fehler zu denken, dass Quines These der Unbestimmtheit der Übersetzung auf die Behauptung, dass Theorien durch empirische Belege unterbestimmt sind, reduziert werden kann. Die Bedeutungstheorie ist einer Unbestimmtheit unterworfen, die sich qualitativ von der Unbestimmtheit wissenschaftlicher Theorien unterscheidet. Es gibt jedoch keinen Grund anzunehmen, dass die These der Indeterminiertheit sich über die Übersetzung und die Bedeutung hinaus erstreckt, und deshalb hindert uns keine Interpretation der These an einer realistischen Auffassung der Grammatik als einer partiellen Theorie des Geistes.

According to Chomsky, the linguist is not solely concerned with providing a rule system which will correctly predict which strings are, and which strings are not, well-formed in a given language or dialect; he is also interested in explaining, in part, the possibility of language acquisition. In particular, he is

\* The research for this paper was supported by NSF grant IST 83-14396 and by a grant from the Systems Development Foundation to the Center for the Study of Language and Information. I would like to thank Joan Bresnan, Alexander George, David Magnus, Julius Moravcsik, and W.V. Quine for comments. Special thanks are due to Dagfinn Føllesdal for many valuable suggestions.

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interested in the question of how it is that after finite exposure to a language in use, we come to have a non-conscious working knowledge of that language, knowledge that extends to such things as the syntactic structures of the sentences of the language. So to the extent that he is interested in human psychology, Chomsky argues, the linguist is interested in grammars that are, in a sense, partial theories of mind. Thus the word 'grammar' can be used, with systematic duplicity, for both cognitive structure and the linguist's theory of that structure.

W. V. Quine has been a staunch critic of Chomsky's realist conception of grammar, and Chomsky a staunch critic of Quine's thesis of the indeterminacy of translation. The indeterminacy thesis, Chomsky claims, reduces to the thesis that theories are under-determined by evidence, and so presents no special problem for the linguist that is not already encountered by, for instance, the physicist. Quine has replied to Chomsky, and Chomsky has replied to Quine, but neither, it seems to me, has quite understood the other. My aim in this paper is to determine exactly what indeterminacy is, and then establish just where it rears its head.

As originally stated, Quine's thesis is an objection to realist conceptions of *meaning*, and its extension to realist conceptions of *grammar* is not obvious. I have two main points to make here. First, Chomsky is just mistaken in thinking that the indeterminacy thesis is just a special version of the thesis that scientific theories are under-determined by evidence: the theory of meaning is subject to an indeterminacy that is qualitatively different from the under-determination of scientific theories. Second, there is no reason to believe that the thesis extends beyond translation and meaning, and hence no construal of the thesis that prevents one from being a realist about grammars.

### 1 Indeterminacy and Under-determination

One can be a realist about physics whilst maintaining that there will always be distinct theories compatible with all the available evidence. Non-trivial scientific theories are under-determined by the data at hand, in that no matter how much evidence is amassed in support of some hypothesis or other, there will always exist alternative descriptions incompatible with the original hypothesis yet perfectly compatible with the accumulated data.

On Quine's account, physical theory is under-determined by *all possible* observations; theories can be compatible with the totality of possible data yet be mutually at odds. As Quine puts it, such theories will be "logically

incompatible and empirically equivalent"<sup>1</sup>. The realist about physics can argue that even if there are different theories compatible with the same data, then even if we never come to *know* it, there can still be a fact of the matter, as to which, if any, of the proposed theories is *the* correct theory. Under-determination is thus an epistemological matter.

Theories of language, just like theories in the natural sciences, are also subject to under-determination by evidence. For instance, translation and theoretical physics are alike in at least this respect:

The totality of possible observations of nature, made and unmade, is compatible with physical theories that are incompatible with one another. Correspondingly the totality of possible observations of verbal behaviour, made and unmade, is compatible with systems of analytical hypotheses of translation that are incompatible with one another<sup>2</sup>.

But can a coherent realist position be adopted by the linguist as it can be adopted by, for instance, the physicist? This is the question on which Quine and Chomsky appear to be dead-locked. In *Word and Object*<sup>3</sup>, and other writings, Quine appears to answer this question in the negative. He presents what he calls the thesis of the *indeterminacy of translation*, which, he argues, is qualitatively different from the accepted thesis of under-determination, and prevents us from taking a realist stance on 'meanings':

Where indeterminacy of translation applies, there is no real question of right choice; there is no fact of the matter even to *within* the acknowledged under-determination of a theory of nature<sup>4</sup>.

Quine is less explicit when it comes to grammar, indeed no clear statement of the force of the indeterminacy thesis in this domain can be readily discerned in his works. Chomsky has argued that indeterminacy is a problem for neither grammar nor meaning because if linguistics is a part of the theory of nature, then the indeterminacy thesis as applied to the scientific investigation of language reduces to the observation that theories in this domain, just like physical theories, are under-determined by the available evidence<sup>5</sup>. Quine has charged Chomsky with missing the point of the thesis, insisting that "... the indeterminacy of translation is not just inherited as a special case of the under-

<sup>1</sup> W. V. Quine, 'On the reasons for the indeterminacy of translation', *Journal of Philosophy*, 67, (1970), p. 179.

<sup>2</sup> W. V. Quine, 'Reply to Chomsky', *Synthese*, 19, (1968) pp. 274-5.

<sup>3</sup> W. V. Quine, *Word and Object*, Cambridge: MIT Press, 1960.

<sup>4</sup> W. V. Quine, *op. cit.*, (1968), p. 275.

<sup>5</sup> Noam Chomsky, 'Quine's empirical assumptions', in Hintikka and Davidson (Eds.) *Words and Objections*, Dordrecht: Reidel, 1968.

determination of our theory of nature. It is parallel but additional"<sup>6</sup>. And in a 1972 paper Quine explicitly stated his doubts about realism in syntax by arguing that there can be no principled choice between extensionally equivalent grammars, posited by the linguist as partial theories of the mind<sup>7</sup>. To this Chomsky has retorted that Quine has not presented any argument for the view that "theories of meaning, language and much of psychology" are subject to under-determination that is "... qualitatively different in some way from the under-determination of theory by evidence in the natural sciences"<sup>8</sup>.

One cannot help but be struck by the failure in these exchanges, to separate the syntactic from the semantic. Chomsky is eager to defend the possibility of realism in the study of grammar, and Quine is principally concerned to demonstrate the impossibility of realism in the study of meaning. Quine is certainly also sceptical about grammatical knowledge, but as I hope to show, it really is important to keep these issues distinct. This has not hitherto been done and the result is that much of the debate between Chomsky and Quine consists in each talking past the other. In the rest of this section I want to look at two ways of understanding why indeterminacy is qualitatively different from under-determination.

Quine's most famous statement of the indeterminacy thesis appears in the second chapter of *Word and Object*: since (i) "... manuals for translating one language into another can be set up in divergent ways, all compatible with the totality of speech dispositions, yet incompatible with one another" (p. 27), and since (ii) there is no criterion with which to make any sort of choice between competing manuals satisfying certain basic conditions, then (iii) it makes no sense to say that any one manual is the correct one.

(i) simply expresses the point about under-determination with respect to translation. The difference between indeterminacy and under-determination comes in (ii) which, it should be noted, is not simply an epistemological claim. Quine's point isn't that we won't come across any data, or discover any criterion, that will enable us to discard all but at most one hypothesis; it is a metaphysical claim: there *isn't* anything to distinguish empirically equivalent theories of translation; all that can be asked of such a theory is that it cover the totality of observable facts of usage.

<sup>6</sup> Quine, *op. cit.*, (1968), p. 275.

<sup>7</sup> W. V. Quine, 'Methodological reflections on linguistic theory', in G. Harman and D. Davidson (Eds.) *Semantics of Natural Language*, Dordrecht: Reidel, 1972, pp. 442-454.

<sup>8</sup> Noam Chomsky, *Reflections on Language*, New York: Pantheon, 1975, pp. 179-204.

I want to emphasize two aspects of Quine's argument, each of which can be used to explain its overall motivation more clearly. First, Quine is clearly presupposing an empiricist view of evidence:

As always in radical translation, the starting point is the equation of observation sentences of the two languages by an inductive equating of stimulus meanings. In order afterward to construe the foreigner's theoretical sentences we have to project analytical hypotheses whose ultimate justification is substantially that the implied observation sentences match up<sup>9</sup>.

Second, Quine is concerned about the shared nature of language. Language, unlike nature, is in part a social arrangement; so whereas nature is there to be studied prior to investigation, language is itself part of the investigative apparatus, a partly social art.

Corresponding to each of these points is a way of understanding indeterminacy. The thesis can be seen as stemming from (a) concern about which facts are to be accounted for by a scientific theory, and from (b) concern about the social or shared nature of language.

(a) Consider the following interpretation of Quine's thesis given by Dagfinn Føllesdal<sup>10</sup>. Føllesdal argues that for Quine the indeterminacy of translation follows from his empiricist view of evidence rather than any ontological dogma. Even on Quine's account not all theories of nature compatible with the totality of evidence are equally illuminating. A theory of nature makes claims about the world, about the nature of concrete and abstract objects, their properties and their relations to other objects. Consequently, theories differing in ontology but making the same predictions with respect to possible evidence are still making different claims about reality. Notions such as simplicity, uniformity, redundancy, depth of explanation — assuming these to be intelligible notions — may guide the choice between competing theories.

Since a complete theory of nature should make reference to all and only the entities that exist, then, Føllesdal argues, "if all the evidence we can go on in translation is sensory evidence, which — just as in empirical science — only provides boundary conditions that can be satisfied by several different translation manuals" then we get indeterminacy. The argument that it is indeterminacy, not just under-determination, runs as follows:

<sup>9</sup> Quine, *op. cit.*, 1970, p. 180.

<sup>10</sup> D. Føllesdal, 'Meaning and Experience', in S. Guttenplan (ed.) *Mind and Language*, Oxford: O.U.P., (1975), pp. 25-44.

... the only entities we are justified in assuming are those that are appealed to in the simplest theory that accounts for all the evidence. These entities and their properties and inter-relations are all there is to the world, and all there is to be right or wrong about. All truths about these are included in our theory of nature. In translation we are not describing a further realm of reality, we are just correlating two comprehensive languages/theories concerning all there is<sup>11</sup>.

The point here is that there can be more than one translation manual compatible with everything there is to know about the states and distributions of all elementary particles, and hence no basis on which to select between such manuals. This interpretation of Quine's thesis is illuminating in that it avoids construing indeterminacy as the statement of any ontological dogma attributable to Quine's adherence to physicalism. If the theory of nature accounts for the totality of possible evidence, then there is no good reason to postulate entities purported to play explanatory roles in some other theory — for instance, a theory of understanding behaviour — when such entities are not seen doing any work within the overall theory of nature. Take intensional entities. Many philosophers feel quite comfortable with such entities; to borrow Paul Grice's newfangled ontological Marxism "They work therefore they exist". On Quine's account we should subscribe to the free-market counter: "They are not cost-effective therefore they do not exist." If the theory of nature makes no appeal to intensional entities then no sense can be made of their existence, we have no identity criteria for them.

(b) Indeterminacy can be explained in another way by emphasizing the social or shared nature of language. According to Quine, indeterminacy rears its head in the acquisition of one's native tongue in just the same way as it does in radical translation. Suppose  $\phi$  is a predicate expression in a language  $L$  used by a population  $P$ , then a new member of  $P$  learning the meaning of  $\phi$  must formulate and confirm hypotheses concerning which objects can be classified as being  $\phi$ , until at some stage he or she feels competent to use  $\phi$  without too much problem. On Quine's account, the new  $\phi$ -user has an inductively grounded stimulus meaning for  $\phi$ . (This is analogous to the case of translation where Quine's linguist has an inductive equating of stimulus meanings.) Of course we have the acknowledged under-determination to take into account. It is perfectly possible for two members of  $P$  to have incorporated  $\phi$  into their respective behavioural repertoires whilst having ascribed to  $\phi$  different applicability conditions<sup>12</sup>. That is, we might envisage these two folk using  $\phi$  in their

<sup>11</sup> *Ibid.*, p. 32.

<sup>12</sup> There are, of course, pertinent Wittgensteinian points concerning subjectivity to be taken into account here, but I must shy away from these for now.

talk exchanges without misunderstanding in their lifetimes, never encountering, indeed not being able to encounter, a critical example which would distinguish between their hypotheses concerning the meaning of  $\phi$ . The native speakers' respective theories are empirically indistinguishable and hence it makes no sense to say that there is such a thing as the correct usage of  $\phi$  beyond correlation with the same stimuli across  $P$ , hence it makes no sense to say that there is a fact of the matter as to the meaning of  $\phi$ . No one person can be construed as having grasped the correct set of conditions governing the usage of  $\phi$  since  $\phi$  belongs to  $L$ , which is the property of  $P$  not of any individual member of  $P$ . In other words, because of the social nature of language, under-determination in an empirical theory of translation just brings indeterminacy along with it.

These then, are two ways of understanding indeterminacy; but indeterminacy with respect to *meaning* only.

## 2 Indeterminacy and Grammar

The question of realism in the study of grammar is addressed head on by Quine in his paper 'Methodological Reflections on Linguistic Theory'<sup>13</sup>. In this paper Quine wants us to bear in mind a distinction between a set of rules *fitting* and a set of rules *guiding* linguistic behaviour:

Fitting is a matter of true description; guiding is a matter of cause and effect. Behaviour *fits* a rule whenever it conforms to it; whenever the rule truly describes the behaviour. But the behaviour is not *guided* by the rule unless the behavior knows the rule and can state it. This behavior *observes* the rule<sup>14</sup>.

Just as there can be physical theories which are at odds with one other yet consonant with the data, so too in linguistics there may be logically incompatible grammars which each *fit* a person's linguistic behaviour. On Quine's account such theories will be *empirically equivalent*. But of course, even abstracting away from performance factors, Chomsky is not just interested in constructing a grammar that *fits*, but a grammar that has some claim to mental reality. Chomsky is not of course claiming that a grammar *guides* linguistic behaviour in Quine's sense. For a start, the native speaker-hearer's knowledge of his tongue is non-conscious. The grammarian's task, on

<sup>13</sup> Quine, *op. cit.*, 1972.

<sup>14</sup> *Ibid.*, p. 442.

Chomsky's account, is to provide an explicit characterization of this tacit knowledge, what he calls the speaker/hearer's *linguistic competence*.

Indeed, Quine notes that Chomsky is not just concerned with fit, and not at all concerned with guidance. But the moral he seems to draw from this is that Chomsky's position makes little sense. It is, Quine says, a mere platitude that "... the native speaker must have acquired some recursive habit of mind, however unconscious, for building sentences in an essentially tree-like way"<sup>15</sup>. Chomsky's non-trivial claim, according to Quine, is imputing to the native speaker "... an unconscious preference for one system of rules over another, equally unconscious, which is extensionally equivalent to it"<sup>16</sup>. And this, Quine thinks, is a mistake: extensionally equivalent grammars account for the same data — they demarcate all and only the well-formed strings — hence there is no principled choice to be made between them.

Quine is not just making the epistemological claim that there is no uncontaminated criterion which will enable us to *distinguish* between extensionally equivalent grammars. He is making the metaphysical claim that there is *no fact of the matter* as to which is the *correct* theory. If two extensionally equivalent grammars *fit* the (abstracted, idealized) linguistic behaviour under consideration, then there is no more to be said; neither is any more correct than the other.

It is clear that Quine is making the *a priori* assumption that there is a proprietary body of data — the speaker/hearer's intuitions concerning well-formedness (cashed out in terms of assent/dissent conditions) — such that the most a linguistic theory can do is account for it. Thus any two grammatical theories which make the same predictions in this domain will, for Quine, be empirically indistinguishable, their extensions exhausting their empirical contents. Thus extensional equivalence and empirical equivalence are the same relation for Quine.

Quine must now confront two questions: (1) Why equate *extensional* and *empirical* equivalence? Or, to put it another way, what reason is there to suppose that we can stipulate in advance which data are relevant? (2) Why does the possibility of empirically equivalent grammars demonstrate anything over and above under-determination? It is not easy to consider these questions in isolation because of what appears to be a quite general disagreement between Quine and Chomsky concerning abstraction and realism. As far as Chomsky is concerned, there is no *a priori* reason to suppose that true statements about the mind/brain cannot be made at pertinent levels of abstraction from the

physical. Quine appears to be dubious about this, but it's hard to establish whether his doubts are a direct consequence of the indeterminacy thesis or whether they are attributable to some independent metaphysical position. This issue is the topic of section 4 so I propose, wherever possible, to skirt around it for the rest of this section. I will, for now, simply accept Chomsky's claim that we can make true statements about the mind/brain at a level of abstraction from the physical, and address the question of whether or not there can be any principled choice between competing grammatical theories.

If there is some principled way of choosing between what Quine calls extensionally equivalent grammars, it is open to Chomsky to claim that there are empirical differences between them. Indeed, if it were just a matter of accounting for empirical facts over and above those to do with well-formedness, the outcome would be straight-forward: Quine would simply admit that he was wrong in equating extensional and empirical equivalence, but then argue that the problem of indeterminacy persists. There would be additional facts to be accounted for, but still no principled choice could be made between grammars that account for both these and the facts concerning well-formedness. Quine would thus concede on question (1) and square up to face (2): a clean fight would be in order.

But matters are not this clear. First, let's get Chomsky's terminology onto the table. To the extent that a grammatical theory *T* correctly predicts which sentences are, and which sentences are not well-formed, *T* is said to be *observationally adequate*. An observationally adequate grammar of my English would predict that *The philosopher I admire is clever* is well-formed but that *The philosopher admires me is clever* is not, that *book* is a word but *bnook* is not, and so on.

But on Chomsky's account, two observationally adequate grammars need not be identical in their empirical predictions. The task of the grammarian is to characterize what the speaker/hearer tacitly knows about his language. Specifically, if we are to understand, in some interesting sense, just what our capacity to speak and understand a language consists in, it is not good enough to recursively delimit the set of well-formed strings of that language: what is further required is a recursive demarcation of the set of *structural descriptions* associated with each sentence, i.e. a characterization of the native speaker/hearer's implicit knowledge of the *structures* of the strings he or she will accept as grammatical. Therefore, Chomsky argues, observational adequacy is the minimum level of adequacy a particular grammar must attain.

A more important goal is that of achieving *descriptive* adequacy. A grammar is descriptively adequate to the extent that it is not only observationally adequate but also generates the right structural description for each sentence.

<sup>15</sup> *Ibid.*, p. 443.

<sup>16</sup> *Ibid.*

Thus, Chomsky can distinguish two types of equivalence: to the extent that two rule systems generate the same set of strings they are said to be *weakly* equivalent; to the extent that they also generate the same set of structural descriptions they are said to be *strongly* equivalent. On this account there can be facts which distinguish between what Quine calls extensionally equivalent grammars.

Now consider the following example from Quine<sup>17</sup>. We have constructed two observationally adequate grammars, *T1* and *T2* for a language *L*. According to *T1*, the immediate constituents of the sentence ABC are [AB] and [C]; according to *T2* they are [A] and [BC]. Since *T1* and *T2* are extensionally equivalent, then, Quine urges, it makes no sense to say that one of these, rather than the other, is the correct grammar for the language. Thus Quine appears to openly reject the goal of descriptive adequacy.

Chomsky's reply to this<sup>18</sup> is that the different structural descriptions assigned to ABC constitute empirical differences between *T1* and *T2*: the theories make different claims about what the speaker knows about sentence structure, hence they imply differences in cognitive structure. Accordingly, there should be evidence that will enable us to choose between the weakly equivalent *T1* and *T2*. Typically, facts to do with co-ordination, VP anaphora, distribution of pronouns, and so on are regarded by the linguist as reliable guides to constituent structure, and it is the first of these that Chomsky brings into play against Quine: if we find that both ABC and ADE are well formed and that so is ABC-and-DE, but that even though ABC and FGC are well formed AB-and-FGC is not, we will have found empirical support for *T2* on the grounds of simplicity and uniformity<sup>19</sup>.

Thus we can choose between extensionally equivalent grammars in much the same way that the physicist can choose between competing hypotheses that explain the same data. Theoretical goals of elegance, simplicity, uniformity, depth of explanation, and so on, are no more out of place in linguistics than they are in physics.

Furthermore, each area of inquiry will have its own internal constraints on theory construction, constraints imposed by the very nature of the facts to be accounted for. The ultimate aim of linguistics is to provide an account of how it is that on the basis of degenerate and limited stimuli, individuals manage to construct grammars sufficiently in accord with those of their linguistic neigh-

<sup>17</sup> *Ibid.*, p. 448.

<sup>18</sup> Chomsky, *op. cit.*, pp. 199-204.

<sup>19</sup> Quine has informed me that he is now prepared to accept that the results of certain psychological experiments may be seen as reliable guides to phrase structure. In particular, Quine mentioned Bever's famous 'click' experiments.

bours to make it possible to understand and produce a potentially infinite number of strings of a shared language. As Chomsky has repeatedly emphasized, in the absence of any remarkable leap in learning theory, this *poverty of the stimulus* constraint forces us to posit some very rich, task-specific, endogenously determined pre-linguistic state underlying the ability to acquire language: some element of our shared biological endowment which maps a course of experience into a particular grammar<sup>20</sup>.

Chomsky calls a theory that characterizes this initial state a theory of *Universal Grammar* (UG). An explanation of the structures of particular grammars will be provided by a theory of UG, in that it will explain the logical problem of language acquisition by resolving the tension between the diversity of existing languages and the poverty of the stimulus. To the extent that it makes it possible to formulate a descriptively adequate grammar from primary linguistic data, a theory of UG is said to be *explanatorily* adequate.

Of course an investigation into the nature of UG cannot take place without prior investigation into particular grammars. But there is a sense in which the higher goal may quite legitimately influence theorizing at the lower level of particular languages. As in the natural sciences, notions like simplicity, elegance, uniformity, and depth of explanation, although often vague, can guide the theorist in the formulation of empirical hypotheses, or, for example, in choosing between theories which appear to make the same empirical predictions in certain domains. It is by doing physics that one develops one's nose for a physical theory, and it is by doing linguistics that one develops one's nose for a good analysis of a certain body of data and the relation between the analysis and some more encompassing theory of grammatical structure, or one's nose for the sort of facts one should be swayed by when seeking to choose between weakly equivalent hypotheses.

Now suppose all the facts were in. We must now construe Quine as arguing that even when we have accounted for them all, there will still be logically incompatible theories that are compatible with all the data. But this is just empirical under-determination not indeterminacy: to demonstrate indeterminacy Quine must show that there is still no fact of the matter as to which of these competing hypotheses correctly characterizes what the speaker has internalized. We see that the issue has shifted very dramatically from language to psychology: even if there is indeterminacy when it comes to meaning, it in no way follows that there is indeterminacy in grammar. The linguist is constructing a theory of cognitive structure: so unless there is some reason to think that intentional entities must play a role in grammar, any argument against a

<sup>20</sup> See e.g. Noam Chomsky, *Rules and Representations*, Oxford: Blackwell, 1980.

realist conception of grammar will have to be an instance of a more general argument against the possibility of realism in psychology. This is the issue to be addressed in section 4.

### 3 Idealization

Before looking at the question of realism in psychology, I want to look briefly at one other way in which one might think there must be indeterminacy in grammar. As we have seen, Quine is quite prepared to accept that the native speaker of a language has acquired *something* — “a recursive habit of mind for building sentences in an essentially tree-like way”. But he is not prepared to go on and say that we can truly say that whatever it is that has been acquired is of such-and-such a form rather than some other. Chomsky’s position, on the other hand, is that since certain cognitive structures embody the native speaker/hearer’s ability to come to grips with an infinity of sentences, the aim of the linguist is to characterize in as clear and rigorous terms as possible just what this capacity or ability consists in. And to the extent that the linguist achieves this goal he is making true statements about the mind/brain.

Dialects of a language may differ from each other in a various ways. Consequently, we find that speakers of a common language may disagree as to the grammaticality of certain strings in that language. On methodological grounds, Chomsky therefore idealizes to the homogeneous speech community. It’s important to note that the idealization applies just as much to the study of ghetto languages and creoles as to the study of, let’s say, British RP English. In each case, meaningful work on the character of particular grammars is to be achieved by constructing models of an idealized system. Of course the actual grammars represented in the minds of the population of *L*-speakers do not need to be identical for there to be a meaningful characterization of what *knowing L* amounts to; after all we are assuming that *L* speakers do manage to communicate successfully using *L*, even where they speak non-identical dialects.

As Chomsky points out<sup>21</sup>, in order to seriously contest the idealization one has to accept one of the following two positions: (a) our cognitive constitution is such that we would be unable to acquire language in a homogeneous speech community, i.e. linguistic variation and inconsistency are pre-requisites to acquisition; or (b) although we could acquire language in a homogeneous speech community, those aspects of cognitive make-up that make this possible

<sup>21</sup> Noam Chomsky, *Rules and Representations*, Oxford: Blackwell, 1980, pp. 25-6.

play no role in the *actual* acquisition process, i.e. where there is linguistic variation.

Now given that on Chomsky’s account two individuals may, as a matter of fact possess grammars that do not generate exactly the same strings — each being, as it were, an approximation to the linguist’s ideal — the question naturally arises whether this fact gives rise to anything recognizable as indeterminacy. Chomsky’s concern is the grammatical competence of the native speaker/hearer, the set of well-formedness conditions represented in the mind of an individual; Quine is concerned with the language as a communal object (“Language is a social art”). So we might reconstruct a disagreement between Quine and Chomsky as a scope confusion, Chomsky arguing that every native speaker/hearer of *L* has a mentally represented grammar generating all and only the well-formed expressions of *L*, Quine arguing that it’s not the case that there is a grammar generating all and only well-formed expressions of *L* such that every speaker/hearer of *L* has this grammar mentally represented.

In a sense, then, there is an indeterminacy when it comes to the conditions guiding (in the non-conscious sense) the linguistic behaviour of the population of *L* speakers, but this is nothing Chomsky need be bothered about. Chomsky has consistently maintained that the notion of “language” is at a higher level of abstraction than the psychological notion of a “grammar”. An individual may possess knowledge of language in the sense that he has mentally represented a set of rules or constraints governing well-formedness. We can call this set of conditions the individual’s *grammar*, and the set of strings the grammar defines that individual’s *language*, or perhaps better, his or her *idiolect*. To the extent that idiolects overlap, we may say that speakers speak the same *dialect*; to the extent that dialects overlap, the same language. The first stage of linguistic enquiry is, Chomsky argues, to characterize, at some level of abstraction, the mature cognitive states attained in the process of language acquisition, i.e. to produce explicit synchronic descriptions of grammars of particular languages. Ideally the descriptions so provided will be models of the mentally real grammars represented in the brain. Accordingly, languages do not have the same ontological status as grammars. Whereas grammars are biological systems, which we are aiming to characterize at the level of abstraction we call “linguistics”, there is no good reason to suppose that the notion of “language” is naturally well-defined at all.

So the debate over the mental reality of grammars might simply reduce to the observation that there is indeed a fact of the matter when it comes to some given individual, but that there is a degree of indeterminacy when it comes to (e.g.) “the grammar of English”. This would not just be an epistemological point. But if this is what indeterminacy is all about, then in this domain it is an

*empirical* matter. In which case it's open to Chomsky to respond that while in principle we must allow for a range of indeterminacy, in practice it will be constrained by what we find out about cognitive structure; should it turn out that the mind/brain is structured in such a way that only a certain class of grammars is available to the language learner, then we might have good grounds for reducing "indeterminacy" in the syntax of a population of speakers. And as is well-known, Chomsky has provided some good reasons for thinking that this is indeed the case. Since there would seem to be no important philosophical point coming out of indeterminacy construed this way — unlike in the case of translation and meaning under the same interpretation — I propose to drop this line and simply note that on the grounds both poverty of the stimulus and purported functional organization, both Chomsky and Fodor have provided compelling arguments in favour of the view that those cognitive structures underlying our linguistic abilities are both domain-specific and largely innately determined<sup>22</sup>.

#### 4 Abstraction

Chomsky makes it clear<sup>23</sup> that he thinks his position with respect to the mental representation of linguistic knowledge is in no sense incompatible with the general thesis known as 'materialism'. We are asked to regard statements about those aspects of the mental that admit of scientific investigation as statements made at an appropriate level of abstraction — e.g. linguistics — about properties of physical systems of the brain. As Chomsky points out, the materialism/dualism debate tends to get marred by the presupposition that the notion of a physical body is quite clear and that the real problem is to establish just what the mental is. But we know from the history of science that the notion of a physical body is not rigid but evolving, and there is of course no reason to think that we have reached any sort of limit on what we can class as physical. Furthermore, current physics does not posit the existence of only observable entities; the statements of physical theory contain terms referring to idealized or abstract objects, so it would be rather misleading to talk of these objects as somehow qualitatively different from those figuring in the statements about the brain made at relevant levels of abstraction.

Simply because the statements of the theory of knowledge of language concern abstract objects and structures, the theory does not entail that the

<sup>22</sup> See Chomsky, *op. cit.*, 1980; J. A. Fodor, *The Modularity of Mind*, Cambridge, Mass.: MIT Press, 1983.

<sup>23</sup> See *Rules and Representations*, pp. 5-6.

knowledge in question does not admit of a material basis. That is, the fact that linguistic competence might be characterizable in abstract terms does not conflict with the claim that a materialist — assuming his position to be otherwise coherent — might want to make to the effect that mental phenomena are describable in physical terms, and that it is only by virtue of general structural properties of the brain that someone can be said to have knowledge of, for instance, English.

So it would seem that Chomsky sees no conflict between the following: (i) grammars are mentally real, (ii) they are, at least partially, characterizable in abstract terms, and (iii) all mental states might be, at least in principle, describable in a purely physicalistic terminology.

For Chomsky, a grammar is an abstract characterization of a cognitive structure, a structure which we can, by hypothesis, assume to admit of a material description. The particular cognitive capacity of the state in question can be viewed as obtaining by virtue of physical constitution. In other words, by leaving the gate open for materialism, Chomsky is making a very interesting claim to the effect that scientific theories of the mental can be constructed in areas where there is an almost total absence of connections between cognitive capacity and physical structure of the brain.

The indeterminist claim with respect to grammars here is that there can be nothing for a linguistic theory so conceived to be about. All truths about physical entities are accounted for by a comprehensive theory of nature; there is nothing left over for a theory of grammar to be about. One can construct observationally adequate grammars, even find empirical evidence to select between them, but at the limit there will still be empirically equivalent theories which are logically incompatible. And here we must stop: there can be no fact of the matter concerning the ontological status of a grammar and its posits since it is nothing more than an abstract characterization of a cognitive structure amenable to physical description.

The point is not that there are no truths concerning abstract objects, but rather that once we have a comprehensive theory of nature it is nonsensical to insist that there can be a fact of the matter concerning redundant entities, for instance the posits of a theory of the mental. By hypothesis, all the work that can be done by such entities has already been done by entities in the theory of nature. Consequently, there is no reason, so the argument would go, to believe in such entities, nor do we have identity criteria for them.

Hilary Putnam seems to think this is one way of understanding indeterminacy. On Putnam's account, "indeterminacy of translation is equivalent to indeterminacy of the transition from functional organization (in the sense of



machine table, or whatever) to psychological description”<sup>24</sup>. It is important to note that this is neither a fair restatement of Quine’s thesis nor implied by it. Quine has dispensed with meanings on the grounds that if a theory of nature makes no appeal to them no sense can be made of their existence, and hence there is no question of making a correct choice between theories that do appeal to them. But Putnam is talking about *psychological* not semantic theories, and the step from redundancy (hence indeterminacy) in the semantic to redundancy (hence indeterminacy) in the psychological is not obvious.

If psychology, like biology or chemistry, is regarded as a part of natural science, and the indeterminacy thesis does not preclude realism in natural science, then it doesn’t preclude realism in psychology. Suffice to say, if, for instance, biological laws are irreducible, one cannot have a complete theory of nature that appeals to physics alone. Likewise for psychological laws if there be such. In short, dispensing with meanings and theories about such entities does not necessarily involve dispensing with all entities posited by theories stated at levels of abstraction from physics, nor with the laws that relate such entities.

What one needs to demonstrate to get Putnam’s version of the indeterminacy thesis off the ground, is that psychology is not part of natural science. Thus one might be led to appeal to Davidson’s principle of the anomalism of the mental<sup>25</sup>. Davidson argues that if (i) at least some mental events interact causally with physical events, (ii) events related causally fall under strict deterministic laws, and (iii) there are no strict deterministic laws on the basis of which mental events can be predicted and explained, then we must conclude that any mental event that enters into any sort of causal relation with a physical event must itself be redescribable in a physical vocabulary. (As elsewhere in Davidson’s work, events are treated as particulars that can be referred to in different ways).

Premise (iii) is the principle of the anomalism of the mental. In context it amounts to the claim that we can have no scientific theory of the mental unless the mental is redescribed in physical terms. For current concerns we might put the point thus: for any two theories of the mental that (a) are stated in a mental vocabulary, and (b) accord with a theory of nature, there can be no principled choice between them. If something like this were true, Putnam might have an argument to the effect that even with a determinate physics we have an indeterminate psychology.

<sup>24</sup> Hilary Putnam, *Meaning and the Moral Sciences*, London: Routledge and Kegan Paul, 1978, p. 49.

<sup>25</sup> See Donald Davidson, ‘Mental Events’, in his *Essays on Actions and Events*, Oxford: O.U.P., 1980, pp. 207-227.

However, neither of Davidson’s discernible arguments for the anomalism of the mental is especially convincing<sup>26</sup>. His first argument is based on the openness of the mental:

Physical theory promises to provide a comprehensive closed system guaranteed to yield a standardized unique description of every physical event couched in a vocabulary amenable to a law.

It is not plausible that mental concepts alone can provide such a framework, simply because the mental does not, by our first principle [(i) above], constitute a closed system. Too much happens to affect the mental that is not itself a systematic part of the mental<sup>27</sup>.

The problem with this argument, as both Dagfinn Føllesdal and Lars Bergström have independently pointed out<sup>28</sup>, is that it does not at all follow from the fact that a system is open, that there can be no strict system-internal laws governing parts of the system. Indeed modern physics is characterized by a degree of openness yet we are not forced to conclude that there are no physical laws.

Davidson’s second argument for the anomalism of the mental apparently hinges on the indeterminacy thesis. The arguments is spread across pages 222-3 of ‘Mental events’; the important sections are reproduced below:

The heteronomic character of general statements linking the mental and the physical traces back to this central role of translation in the description of all propositional attitudes, and to the indeterminacy of translation. [Footnote omitted.] There are no strict psychophysical laws because of the disparate commitments to the mental and physical schemes<sup>29</sup>.

... when we use the concepts of belief, desire, and the rest, we must stand prepared, as the evidence accumulates, to adjust our theory in the light of considerations of overall cogency: the constitutive ideal of rationality partly controls each phase in the evolution of what must be an evolving theory. An arbitrary choice of translation scheme would preclude such opportunistic tempering of theory; put differently, a right arbitrary choice of a translation manual would be of a manual acceptable in the light of all possible evidence, and this is a choice we cannot make. We must conclude, I think, that nomological slack between the mental and the physical is essential as long as we conceive of man as a rational animal<sup>30</sup>.

<sup>26</sup> The following discussion of Davidson’s arguments for anomalous monism owes a lot to Dagfinn Føllesdal’s paper ‘Causation and explanation: a problem in Davidson’s view on action and mind’.

<sup>27</sup> Davidson, *op. cit.*, pp. 223-4.

<sup>28</sup> See Føllesdal, *ibid.*, and Bergström’s ‘Føllesdal and Davidson on reasons and causes’, in Włodzimierz Rabinowicz (ed.), *Tankar och Tankefel: Tillägnade Zalma Puterman*, 50 år, 1. oktober 1981 (Filosofiska Studier utgivna av Filosofiska Föreningen och Filosofiska Institutionen vid Uppsala Universitet, nr. 33), Uppsala, 1981, pp. 9-21.

<sup>29</sup> *Ibid.*, p. 222.

<sup>30</sup> *Ibid.*, p. 223.

At first this looks like an interesting argument given our current concerns: if it is valid then the indeterminacy of psychology would appear to follow directly from the indeterminacy of translation. But the argument does not hold up: in particular it fails to show why a theory of the mental is indeterminate rather than just under-determined. Indeterminacy of the mental does not follow from the fact that we can never have all the evidence in, for that is the situation just as much in physics as in psychology. The real problem here is that Davidson wants to infer *from* the indeterminacy of the mental to the anomalism of the mental; but he cannot establish the indeterminacy of the mental by appealing to the indeterminacy of translation alone. The missing premise is nothing less than the anomalism of the mental. Thus Davidson leaves us in the same place we were deposited by Putnam. Neither of his arguments demonstrate any inherent indeterminacy in theories of the mental.

The real weakness in Putnam's version of the indeterminacy thesis is that even if we assume, as Quine does, that mental states admit of purely physical description, we have no way of knowing in advance just what sorts of entities may come to be handled within a theory of the physical. If the history of science is any guide, the notion "physical body" is not a given, but rather more and more encompassing. We have no idea just how much future physical theories will be shaped by reflection on the nature of abstract entities, and *a priori* insistence that any abstract characterization of a structure — which may turn out on some construal to admit of a purely physical description — is as good or as bad as any other seems to be unwarranted. Thus we have seen no reason to suppose that investigation of the structure and nature of the brain may not proceed at levels of abstraction from the physical. Indeed discoveries about the brain's physical structure could in principle have a bearing on theories stated at more abstract levels, and these more abstract characterizations could ascribe properties to cognitive systems which are to be explained by a comprehensive physical characterization.

### 5 Conclusion

Once certain assumptions are made concerning the nature of evidence it is clear why Quine thinks indeterminacy rather than empirical under-determination infects theories of translation and meaning. Further, it seems that even without these assumptions a convincing case can be made for an empirical version of the thesis by emphasizing the shared nature of language, and this, I take it, is part of what Quine's original 'Gavagai' example was about.

But no convincing argument for indeterminacy in theories of cognitive structure seems to emerge from the epistemological or social considerations that lead to indeterminacy in translation and meaning. From the latter perspective indeterminacy is just an empirical matter and the dispute between Quine and Chomsky reduces to the scope misunderstanding outlined in section 3.

A more substantial issue emerges concerning the quality of statements that attribute abstract properties to complex systems admitting of independent physical descriptions. I have tried to show that attempts to deduce indeterminacy at the relevant level of abstraction have been quite circular, and suggested that there is no possibility of them being otherwise. To conclude, we should accept that there is indeterminacy in the realms of translation and meaning whilst realizing that this fact, by itself, does not force us to reject realism in the study of grammar.