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LOGICAL FORM AND NATURAL LANGUAGE*

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I

The last half dozen years have witnessed growing talk about a new synthesis. Linguists and logicians, it is said, are finding common ground in the investigation of the logical form of sentences in natural language. Adverbs, mass terms, proper names, action sentences, belief sentences, quantifiers, comparatives and more have all come in for scrutiny. Early results have been intriguing. Even more enticing though are the claims that out of the synthesis will come illumination of our tacit metaphysical or ontological assumptions, and perhaps even the solution of major philosophical problems.

But what precisely is logical form? How are we to go about finding the logical form of a sentence? And if we find it, how are we to know *that* we have found it? How are we to know we have gotten an account of logical form *right*? These are questions commonly skirted in studies aimed at setting out the logical form of one or another area of language.¹ They are the questions I propose to take up here.

A central theme will be that there is not a single notion of logical form abroad. Instead there are two quite distinct notions, each paired with a theory laying claim to the title of a theory of logical form. Thus much of what follows will be aimed at pulling the two sorts of theories apart, setting out their goals and strategies, and assessing the insight that might reasonably be expected from each. But my aim is not entirely expository. I also have a pair of axes to grind. For it is my view that the pair of programs to be analyzed are not merely two possible approaches to constructing a theory of logical form, they are the only two. Alternative programs fall into two categories. Some are variations on the basic ideas of the programs sketched, the rest are untenable. Nothing in the present essay is aimed at establishing this contention, though in another paper I have

tried to take on what I suppose is the most prominent putative alternative.² Ultimately, I suppose, the contention is beyond proof. But if it is true it magnifies the importance of my second contention. That is the claim that much of the recent talk of synthesis is rooted in confusion. Linguists and logicians are engaged in fundamentally different projects. And while insights and technical developments in each field may suggest innovations in the other, their basic goals diverge. Actually I will urge a more radical thesis. On my view, the linguist engaged in the empirical study of natural language has no legitimate use for the notion of logical form; his use of such terms as 'logical truth' and 'entailment' is, at best, Pickwickian. And the logician, whose claim to these terms is solid, has no abiding interest in natural language. Indeed, if we construe natural language narrowly, excluding the technical locutions forged by logicians, then there may well be *no* logical truths in natural language.

II

We begin with what I will call the *Quinean program*. The basic ideas behind the program are older than Quine. One of them, the notion that logical truths are simply a species of empirical generalization, found an exponent in J. S. Mill. The other, the idea that ordinary language is inadequate to the purposes of logic, traces to Frege. Both ideas had still earlier advocates. But in recent years the pair have been aired most avidly by Quine.³ Thus the label. Still, I would not be taken to imply that all of what I call Quinean would be endorsed by Quine. Indeed I will note several places where Quine's views appear to clash with the view I am calling "Quinean."

"Logic", Quine writes, "chases truth up the tree of grammar."⁴ Unpacking the epigram will give us a good start at unfolding the program. Let us start with a few reflections on truth.

Central to our notion of truth is the following principle:

For any sentence of our language, *S*, the result of substituting a name of *S* for '*s*' and *S* itself for '*p*' in the schema '*s* is true if and only if *p*' yields a truth.

The principle is not a definition of truth. Nor would it do as an explanation of the notion to one who feigned not to understand it.⁵ But no matter.

My claim is only that the principle expresses something true and obvious about the notion of truth. Actually, a bit of hedging is called for. We cannot let *S* be *any* sentence of our language, but only any of its 'eternal' sentences, i.e. any of those sentences whose truth value remains constant from utterance to utterance. It is only these cases where truth can reasonably be attributed to sentences at all. Also, if we are to avoid paradox, we had best restrict our principle to sentences which do not themselves contain such semantic terms as 'true' and 'false'.

It is by virtue of this principle that when we attribute truth to a sentence we are talking about the world by indirection.⁶ Sometimes such indirection is pointless, but not always. The utility of the truth predicate, on the Quinean view, stems from the opportunity it affords to generalize in a direction unavailable to simple quantification. Noticing that Tom is mortal, Dick is mortal and Harry is mortal, we may generalize the observation and advance the thesis that all men are mortal. In so doing we pass judgment on all the objects of the universe: each is mortal, if human. No need to talk about sentences nor to attribute truth. Suppose, now that we go on to observe that Tom is clever or Tom is not clever, that Clarence is a crow or Clarence is not a crow, that snow is white or snow is not white. Here too there is a generalization to capture. But quantification alone is inadequate to the task. 'Tom is clever or Tom is not clever' shares no predicates with 'Clarence is a crow or Clarence is not a crow'. What they share is a certain linguistic structure. The generalization to be ventured is that all sentences with that structure are true. Here we are quantifying over sentences and attributing truth to those of a particular structure. But while quantifying over sentences our generalization still passes judgment, though obliquely, over the objects of the world – including Tom and Clarence and snow. In Quine's eyes, it is by facilitating such generalizations that the truth predicate earns its keep.

The utility of the truth predicate is not restricted to generalizations couched in terms of grammatical structure. Similarity of authorship is another dimension for generalization in which the truth predicate proves practically indispensable. How else, short of reproducing each of his pronouncements, might we advance the thesis that everything the Pope says is true? Note that advocacy of such a generalization in no way commits us to the further thesis that each of the Pope's statements is true *because* it is uttered by the Pope. Analogously, and the point is an impor-

tant one, the claim that each sentence of a certain grammatical structure is true does not carry with it the further claim that the sentences in question are true *because* they have that structure.

This said, we can make a first pass at setting out the Quinean program. The logician, as the program would have him, is an empirical scientist. Like the rest of us, he starts his theorizing already having a substantial body of beliefs. And like other scientists his goal is the discovery of generalizations and theories. Unlike other scientists, however, the logician's generalizations exploit the truth predicate. Noticing that many sentences formed by inserting a single declarative into both blanks of

(1) _____ or it is not the case that _____.

are true, he hypothesizes that all are. The hypothesis is a candidate for classification as a law of logic. Of course this preliminary picture of the logician's project is much too simple. But before adding complications, let us note a feature of the project that will be retained as our account grows more complex. *The justification of logical laws parallels the justification of scientific law.* Both logic and science begin with the body of beliefs that the theorist takes to be obvious. These support generalizations which in turn support particular instances. It is via this latter route, for example, that we declare living men to be mortal, and lengthy, multiple embedded logical truths to be true.

We have been talking as though the discovery of laws were the aim of the logician and the scientist. But theories, not laws, are the business of sophisticated science. The Quinean parallel between natural science and logic carries over nicely when our focus turns on theory. The standard account of empirical geometry proves a convenient illustration. 'Pure' geometry can best be viewed as a physically uninterpreted theory; its axioms and theorems make essential use of various predicates (including, perhaps, 'x is a line', 'x is a point', 'x is between y and z') to which we attach no physical significance. "Applied geometry" gives a physical interpretation to the terms, then attempts to test the total theory by testing the truth of a sampling of theorems. We need not insist that the theorems are to be tested by a straightforward appeal to observation or measurement. Rather, the correct applied geometry is the one which, combined with further theory, yields the simplest, most explanatory total physical theory.

Pure geometry finds its analogue in what might be called 'pure logic'. Its axioms and theorems contain such predicates as 'x is a sentence', 'x is the negation of y', 'x is the disjunction of y and z', etc. The pure theory, however, does not tell us which strings of words are sentences, which pairs of strings are related as sentence and negation, etc. Thus in pure logical theory 'sentence', 'negation' and the rest are (linguistically) uninterpreted. The axioms of the pure theory may be stated in a variety of ways. One idea, perhaps the simplest, has the axioms spelling out the ways in which the truth (or satisfaction) conditions of compound sentences depend on the truth (or satisfaction) conditions of their components. A simple axiom on this pattern might be

- (2) For any three sentences, S_1 , S_2 , and S , if S is the disjunction of S_1 and S_2 , then S is true if and only if S_1 is true or S_2 is true or both S_1 and S_2 are true.⁷

Among the theorems, of particular interest are those that attribute truth to all sentences of a certain structure – say, to every sentence which is a disjunction of a sentence and its own negation – for, when the theory is interpreted it is these sentences that the theory will advance as logical truths.

Interpreting the theory is the job of a logical grammar. To turn the trick a logical grammar must specify which sequences of sounds or symbols are to be taken as sentences, and it must attribute to sentences structure enough to interpret the otherwise uninterpreted predicates employed in the pure theory. That is, it must tell us which sentences are disjunctions (and what their disjuncts are), which sentences are negations of which, which sentences are universal quantifications, etc. Enriched with an appropriate grammar, the logical theory (let us call it an *applied logic*) will entail not only that every disjunction of a sentence with its negation is true, but also that 'Tom is tall or it is not the case that Tom is tall' is such a disjunction, and is true.

How is an applied logic to be tested? Here again the answer parallels the one given for applied geometry. We test an applied logic by assessing the explanatory and predictive power of the total theory that results from meshing an applied logic with the theories of the several sciences. As in the case of applied geometry, there is no need to insist that such an assessment will be simple or straightforward.⁸

If we are to make this analogy between the testing of an applied logic

and the testing of an applied geometry stick, we must handle a problem that arises over the notion of 'meshing' theories when one of the theories being meshed is an applied logic. A critic might put the problem as follows: "Talk of meshing a pair of non-logical theories (a physical theory and an applied geometry, say) is clear enough. Meshing amounts to no more than conjoining the postulates of the two theories. The result will be a new theory whose theorems include some sentences which were not theorems in either of the component theories. And hopefully some of these new theorems will be interesting or useful. But what empirical purpose is served by conjoining an applied logic to a physical theory? Since logical truths are entailed by anything, they were all among the theorems of the unsupplemented theory. Adding a logical theory adds nothing."

Part of the critic's complaint is beyond dispute. The scientific utility of an applied logic cannot derive simply from the fact that it entails each of the logical truths, for this is true of any theory. To understand what logic contributes to the overall utility of scientific doctrine we must look elsewhere. The place to focus, I think, is on the situation of what might be called the pre-logical scientist – the able and informed practitioner of the science of his day who has no explicit logical theory. This is not, of course, to say that the imagined scientist is irrational or incapable of reasonable argument and inference. Quite to the contrary. I assume that the pre-logical scientist can and does reason, argue and infer with considerable success. But he has no explicit theory to guide him, no general account of what follows deductively from what. Nor need he even be aware of the distinction between inductive and deductive argument. So described, the prelogical scientist is not an improbable philosophic fiction. Prior to the flourishing of logic in the second half of the nineteenth century, scientists could hardly have avoided being pre-logical.

But while the absence of an explicit logical theory will surely not cripple a scientist, it will sometimes prove inconvenient. When reasoning gets complex it is often unclear whether belief in one sentence, S_1 , justifies belief in another, S_2 . The theorist who is in doubt about such an inference is, we may suppose, equally in doubt about the conditional sentence whose antecedent is S_1 and whose consequent is S_2 . It is just here that an explicit logical theory facilitates scientific inference. For when the inference in question is deductively valid, the logical theory provides the

justification for the conditional. Of course further reasoning will be required to show that the conditional follows from the axioms of the applied logic. But this is no problem; our theorist can reason well enough. The view I am urging is a quite traditional one. An applied logic contributes to the sciences by justifying inferences whose validity is otherwise in doubt. Less traditional is the Quinean view that the logical theory itself is justified just the other way round, via the predictive and explanatory power of the theory that results when the inferences sanctioned by the logic are countenanced as correct in the rest of the sciences.

Let us now reflect on a few likely steps the Quinean logician will take in an effort to carry out his project. We can skip the complex interplay of inspiration and hard work that leads from hints like (1) to something approximating the familiar logic of quantifiers and truth functions. Imagine rather that our theorist has formulated one or another version of pure quantification theory and has a grammar which interprets the theory by attributing appropriate structure to some subset of ordinary English sentences. Which sentences will these be? The logician can be fairly liberal in the expressions his grammar interprets as unstructured or atomic predicates, taking care only to avoid non-extensional predicates, ambiguous locutions and any others that may lead to falsehood when substituted into a valid sentence schema. The situation is different for quantifiers and connectives. If we take seriously the idea that the theory is to attribute truth to sentences, then, to begin with at least, the grammar had best classify as quantifiers and connectives only those cramped and fussy locutions of less than ordinary English that are maximally hedged against ambiguity. To be avoided are expressions which, singly or strung together, might leave their containing sentence true on one reading and false on another. Thus, for example, the logician's grammar would do well to avoid 'or' as an interpretation of disjunction, opting rather for the more cumbersome but less ambiguous '_____ or _____ or both'.

A next obvious move is to reduce the awkwardness of the sentences classed as logical truths by abbreviating quantifiers and connectives into more manageable form. A more radical step is to introduce into the object language expressions intended as interpretations of one or another of the constructions employed by the pure logical theory, where the expression introduced cannot plausibly be held to abbreviate *any* expression in the pre-existing language. Arguably, the introduction of the horseshoe as an

interpretation of the material conditional was just such a move. Perhaps even the standard notations for universal and existential quantification with their careful attention to scope and cross reference are best thought of as similarly reformist in their origin. It is hard to think of any pre-existing usage which can be so freely self-embedded without risk of serious ambiguity. But arguing over examples is not to the point. The division between abbreviation and new coinage is a vague one of no particular importance.⁹

It might be objected that there is something illicit about adding vocabulary to the object language, that it is akin to tampering with data. To fabricate expressions which behave in accordance with our logical theory, the complaint continues, is no more legitimate than fabricating fake fossils in an effort to confirm an anthropological theory. The protest, however, is based on a confusion. Language is not the subject matter of the Quinean logician's theory. Indeed, what makes logic unique among the sciences is that it has no unique subject matter. It shares vocabulary and thus subject matter with each of the sciences, and generalizes across them. Thus the logician's introduction of new vocabulary is no more objectionable than the analogous move on the part of the physicist or the biologist.

As we have noted, the most convenient interpretation for the constructions mentioned in pure logical theory will be those carefully hedged or even perhaps blatantly artificial expressions that keep ambiguity at a minimum. But now let us reflect on sentences constructed from less contrived locutions. Consider, for example, sentences built from commonplace idioms like 'each', 'some', 'any', 'every', 'all', 'if', etc. Which of these are logical truths? The answer I would urge is that *for the program we have been recounting, none of them are*. These are the sentences that are simply left behind in the gradual evolution of scientific theory. The history of science abounds with analogues – terms that were dropped from the language of theory with the development of more precise, more readily quantified or less ambiguous notions. Thus, for example, 'warm' and 'cold' dropped out of serious science, to be replaced by talk of temperature and amounts of heat. Sentences that speak of warmth are no longer taken as truths of physical theory. Similarly, talk of uniform and difform motion yielded to talk of velocity and acceleration, and talk of simultaneity (*simpliciter*) yielded to talk of simultaneity relative to an inertial

frame. My claim, then, is that on the Quinean account the logical truths, strictly construed, are to be found only among those contrived sentences generated by a logical grammar.

There is no hiding the fact that the view I am trying to pin on the Quinean theorist is, in the eyes of many, a radical one. If my reading of the view is correct, then, with a few possible exceptions, no sentence in ordinary language is a logical truth; and, given the obvious account of entailment as logical truth of the appropriate conditional, no sentence in ordinary language entails any other. In some places at least, Quine himself seems to resist this conclusion. Thus he writes: "This much can be said for the linguistic theory of logical truth: we learn logic in learning language." Three paragraphs later he adds that the "truths or beliefs [acquired when a child learns language] are not limited to logical truths."¹⁰ On the view I am urging, however, knowledge of logic is no more a product of language learning than knowledge of physics. In the course of acquiring his language the child may well come to know a few snippets of physics, a few relatively elementary approximations to physical truths. So too he may come to know a few rough approximations to logical truths. But most of the truths of physics are not even statable in the language the child learns, nor are most of the truths of logic. Quine's words suggest that the history of logic can be viewed as a history of making explicit what was (tacitly?) known all along. The earliest logicians came to know logic when they learned their language. But it was not until the time of Frege that we had a decent explicit account of what we knew. Plainly such a view squares poorly with the central tenet of the program I have been calling Quinean. That program views logic as continuous with empirical science. The history of logic (like the history of physics or biology) is marked by the formulation of laws and theories invoking new concepts and using new vocabulary to express them.

It might well be argued that there is another, less radical strategy, equally compatible with the spirit of the Quinean program. Rather than maintaining that ordinary language harbors no logical truths, why not try to enrich logical grammar? The idea is to have logical grammar generate and attribute logical structure to as many sentences of the vernacular as possible. By expanding both our logical grammar and our pure logical theory, we would aim to specify which natural language sentences are logical truths and to give an account of entailment in ordinary language.

The strategy has evident attraction, enhanced no doubt by the promise of avoiding the claim that there are no logical truths in ordinary language. But it is not a proposal that a Quinean logician can endorse. There are a pair of reasons. First is the already stressed danger of ambiguity. The idioms that in ordinary language approximate the work of regimented quantifiers and variables cannot be counted upon to compound into complex sentences with only a single reading. The more complex the sentence, the greater the chance of trouble. We might try to construct the enlarged logical grammar so as to filter out the ambiguous sentences in ordinary language, leaving the rest. But to do so would involve considerable complication of the grammar with no evident gain toward accomplishing the theorist's goals. Moreover, and this is the second reason for rejecting the project, given the goals of the Quinean theorist, the whole exercise is pointless. His goal is the production of the best theory adequate to the needs of empirical science. If this can be done within the confines of a regimented fragment of partially fabricated English, there is no call to go further. Indeed, the push on the Quinean theorist, like that on other empirical theorists, is toward less theory rather than more. The physicist records a success when he can construct a theory equal in explanatory power to the previously received doctrine but sparser in assumptions. The logician is comparably successful when he can replace his current theory with another which, while equally adequate to the purposes of science, invokes a simpler logical grammar, making do with a sparser regimented language. There is no cause to protest if the truths of logic cannot be formulated in the language of the marketplace, for neither can the truths of physics.

The view we have been elaborating invites a misunderstanding that we would do well to guard against. In claiming that there are no logical truths or entailments among the idioms neglected by his grammar, the logician is not urging that we adopt his newspeak for the purposes of daily communication. Nor is he suggesting that serious science can only be carried out in the regimented language. The relation between ordinary and regimented language is rather to be viewed on the model of the relation between everyday language and the most precise and theoretically *au courant* scientific idioms. The former is sufficient for scientific discourse so long as it is obvious to all concerned how the speaker would express himself within the confines of the latter. And, failing this, we still

need not resort to technical paraphrase if the difference between various plausible technical paraphrases makes no difference to the matter at hand. Thus the scientist continues to speak of the sun dropping beneath the horizon and of distant events occurring simultaneously. Recourse to a canonical language, like recourse to technical terminology, is mandatory only when the alternative is breakdown of communication.

Some may be tempted to view the ease with which we acquire skill in paraphrasing from natural language to canonical as evidence that the latter somehow exhibits the underlying structure of the former. We will consider this notion of underlying structure in some detail in the sequel. Here we need only note that the mere fact that we readily acquire the ability to paraphrase hardly supports an inference to underlying structure. If it did it would support as well the claim that sentences in the technical jargon of the special sciences exhibit the underlying structure of more commonplace sentences. For once a science has been mastered we show a comparable ability to paraphrase ordinary language into the technical jargon of theory. But surely it is absurd to suggest that in constructing a technical vocabulary for a new science we are uncovering the buried structure of old sentences.

Three loose ends remain in this sketch of the Quinean program: a warning against a possible misunderstanding, a few reflections on logical truth abroad, and some comments on how much truth is logical truth.

First the misunderstanding. Our persistent assumption has been that our imaginary logician will come up with a theory close to familiar quantification theory. And, as is no secret, Quine himself has long championed quantification theory as logic enough for empirical science. But this view is quite independent of the program I have been calling Quinean. The program is a specification of goals, an answer to the question: What is it to get a logical theory right? And, of course, it could turn out that the theory which best fulfills the goals we have sketched might be in fundamental ways different from the standard logic of truth functions and quantifiers.

Thus far everything we have said about logical truth has been domestic. We have imagined how a theorist goes about formulating his theory in his own language; and we have imagined that his language is English. But what is to be said of logical truth in other tongues? How do we discover the logical truths in French or Old Norse? In principle, I think, the answer

is simple. The logical truths in exotic tongues are just the sentences whose translations in English are logical truths. And if our standards of translation are decently high, there will, in all likelihood, be *no* logical truths in Old Norse or everyday French. This should not be surprising, for neither are there any Old Norse sentences whose English translations are the abstruse truths of recent physical theory. To state the truths of logic (or of physics) in French or in Old Norse requires the same sort of warping of the vernacular that was required for English. Should it be protested that the notion of translation is uncomfortably obscure, I would be the first to agree. But I do not think there is any special problem with the translation of logical truths. Logic abroad is no less scrutable than biology or physics.

Finally there is the problem of drawing boundaries. Given a body of presumed truth, how much of it is to be regarded as *logical* truth, and how much as belonging to the special sciences or to the scattered and unsystematic reservoir of commonplace truth? It might be thought that the matter could be settled by noting how much of our theory made use of the truth predicate since, as we have seen, such use is among the hallmarks of logic. This idea loses its attraction when we realize that any fully explicit theory can be reworked so as to invoke the truth predicate. But if the boundary separating logical truths from their fellows proves hard to draw in a non-arbitrary way, the program we have sketched provides some solace. It suggests that the boundary *is* arbitrary and also unimportant. Drawing the line is a project of a piece with saying where physics stops and chemistry starts, a job more pressing for deans than for theorists. Of course, for the most part we have little trouble deciding whether a given body of doctrine is plausibly considered a part of logic. Our intuitions, I would speculate, rest in part on precedent and in part on the feeling that resort to the truth predicate should be a last resort. Other things equal, the less logical truth the better. Where the two principles conflict, there is no evident way of resolving the question, and no reason to worry if it remains unresolved.

III

The project which, by my lights, is the principal alternative to the Quinean program would develop a theory of logical form on the model of the theories produced by generative grammarians.¹¹ Indeed, some of the

programs' advocates view a theory of logical form as a proper part of a generative grammar. A few observations on the goals of grammatical theory will prove a convenient opening for our discussion of what I will call the *semantic program*.

What I will call a *descriptive grammar* aims to be an idealized theory of linguistic intuitions. Linguistic intuitions are simply the pre-theoretic (or 'intuitive') judgments that, with a bit of prodding, speakers can be brought to make about the linguistic properties and relations of the expressions in their language. A completed theory of linguistic intuition would, as a minimum, be expected to predict accurately just which judgments a competent speaker will make. A more ambitious theory might go on to explore the psychological mechanisms that underlie a speaker's capacity to judge expressions as he does. A linguist's grammar will do neither. Rather it is intended as a central sub-component in some larger theory which *can* be expected to predict intuitions accurately. The idea is that a theory predicting actual intuitions is best built in pieces, one piece specifying an idealized class of intuitions, and the other pieces explaining the deviation between idealized intuitions and those actually reported. Some of the deviations, we would expect, are due to general limitations on memory. Others might be best explained by psychological factors as yet poorly understood. As with any idealized theory, descriptive grammar requires a certain amount of informed speculation. The theorist will sometimes be required to rely on dubious intuitions or even to ignore apparently negative data with the hope that as the theory develops things will fall into place.¹²

Now the starting point of the semantic program is the observation that in addition to intuitions about syntactic properties (like grammaticality or syntactic ambiguity) and relations (like the active-passive relation or the relation between a verb and its subject) speakers also have tolerably uniform intuitions about semantic properties (like analyticity) and relations (like entailment and synonymy). Thus we may try to construct an idealized theory of semantic intuitions parallel to our idealized theory of syntactic intuitions. Or, going a step further, we might try for an integrated theory aimed at handling both syntactic and semantic intuitions together.

Before setting out to build a theory, the theorist would do well to decide just which sorts of intuitions are 'semantic' intuitions and thus

which sorts of intuitions his theory aims to capture. Unhappily there is no complete standard list. But, on reflection, this need not seem a difficulty. For as long as there is fair agreement that (most of) a certain set of intuitions are the proper domain for the semantic program, we can treat the inclusion of any other types of putatively semantic intuitions as an empirical problem. Such intuitions are to be treated as semantic if, without undue complications, they can be handled by the theory designed to handle the less questionable sorts of intuitions. Among this latter group, intuitions of entailment will surely play a prominent role, as will intuitions of synonymy, of contradictoriness and of semantic ambiguity. Some would add intuitions of analyticity and perhaps intuitions of logical truth, either as a subset of the analytic sentences or as a distinct class. It is by virtue of the first and last items on this brief list that the semantic program's advocates lay claim to the notion of logical form. For surely, they maintain, a theory detailing logical truths and entailments in a language is a theory of logical form for that language.

Thus far we have been talking as though semantic intuitions were judgments about certain properties and relations of *sentences*. But matters are more complicated. In many cases our intuitions of entailment, for example, cannot be construed as judgments that one *sentence* entails another. Rather they are judgments that *in one sense* (or on a certain *interpretation* or *reading*) a given sentence entails a second (on one reading). Thus we intuit that in one sense 'Some banks were submerged' entails 'Some financial institutions were under water'. While in another sense it does not. And on one reading 'Every girl was kissed by some boy' entails 'One boy kissed all of the girls'. These are the intuitions the semantic theorist aims to predict. To turn the trick, the most obvious strategy is to identify senses with suitable theoretical constructs within a descriptive grammatical theory. In the 'interpretative semantics' of Katz, Chomsky and others these are semantically interpreted underlying phrase markers. In 'generative semantics' they would appear to be sentences in some vastly enriched version of the languages designed by intensional logicians. In either case the entities serving as senses are mapped (usually many-many) to sentences in the language at hand. Entailment is then defined over senses. The idea is to have one sense entail a second only when speakers intuit that a sentence mapped to the first entails (on some reading) a sentence mapped to the second. Synonymy and analyticity are

handled similarly – all of this, of course, within the boundaries of idealization. Ambiguity and anomaly are proper properties of sentences. Those sentences intuited to be ambiguous are mapped to more than one sense; those intuited to be anomalous are mapped to none. The several semantic properties and relations are themselves nomologically linked. Thus, for example, it is to be expected that for each ambiguous sentence there will be two or more additional sentences each synonymous with the ambiguous sentence (on a reading) but commonly not synonymous with each other (on any reading).

Evidently senses construed in this way are plausibly viewed as theoretical entities of a growing psychological theory. And it is natural to wonder how senses might play a part in a broader psychological theory aspiring to more than prediction and explanation of syntactic and semantic intuitions. Harman has proposed that senses might serve as the objects of belief and propositional attitudes, that believing, wanting, hoping and such might be taken as relations between persons and senses.¹³ This is not the place to debate the merits of Harman's suggestion. But should it prove viable, it might suggest yet another move more central to our current concerns. The basic idea is this. If senses are the objects of beliefs, then perhaps we can use data about beliefs to test conclusions about entailment. More specifically, suppose it were discovered that generally persons who believe sense S_1 believe sense S_2 (but not conversely). Is this not positive evidence for the hypothesis that S_1 entails S_2 (but not conversely)? The answer, I think, is yes and no. Yes, if belief in S_2 is not generally accompanied by belief in S_1 , this is some reason to think that S_2 does not entail S_1 . And no, if belief in S_1 is generally accompanied by belief in S_2 this is scant evidence that the former entails the latter. Generally people who believe that one rabbit exists believe that two do. But surely the sense that is the object of the first belief does not entail the sense that is the object of the second. Entailment, as the semantic program construes it, is tighter than a nomological connection between beliefs. The evidence that decides between the weaker and the stronger connection is data about *intuitions*. An analogous point may be made about analyticity and logical truth. A semantic theorist may well take disbelief as evidence against the analyticity of a sense, though he could hardly take belief as evidence in favor.

In our account of the semantic program the notion of a sense has taken

on a prominent role. Its prominence may lead some to hope that the results of a Quinean theorist might be merged with the results of a semantic theorist, indeed that the two programs might probe to be different paths to the same end point. For, it might be argued, the postulation of senses and the construction of a canonical language share a common motivation. In each case we seek linguistic structures which, while related to ordinary sentences, do not share the ambiguity of ordinary sentences. And in each case it is these structures over which entailment and logical truth are defined. The thought that the product of a Quinean theorist's endeavor might *be* a semantic theory is reinforced by the generative semanticist's proposal to identify senses (or deep structures) with sentences in an enriched regimented language. Could it not turn out that the regimented language of generative semantics ideally suits the purposes of the Quinean theorist? And if so, could it not also turn out that the entailment relation the semantic theorists defines over senses is the same relation that the Quinean defines over regimented sentences?

Indeed it could. Stranger things, as they say, have happened. But there are a pair of reasons for skepticism. One centers on the intended scope of the two sorts of theories, the other on claims to truth. Let us focus first on scope. In writing a descriptive grammar an empirical linguist is concerned to describe as much of the language as he can. If his theory neglects some corner of the language, his theory is incomplete; the larger the corner, the greater the defect. The same is true for the semantic theorist. His theory is no less descriptive than the empirical grammarian's. If there is an area of the object language for which speakers have intuitions, and if his theory does not describe them, then his theory is incomplete. Not so for the Quinean theorist. For his purposes less is more. Other things (read: scientific utility) being equal, the more limited the logical grammar, the better. Of course it might happen that the Quinean theorist could get away with no less a canonical language than suits the needs of the semantic theorist, or, taking the opposite perspective, that the semantic theorist needs no more senses than the Quinean theorist's canonical language provides. But surely such a coincidence would be little short of miraculous.

The point about truth begins from the observation that the Quinean theorist is committed to the truth of the sentences he labels 'logically true'. He has as much reason to believe them as he has to believe the sentences of received physical theory. Indeed, it is much the same reason.

The semantic theorists need make no similar commitment to the sentences (or senses) he labels 'logically true' or 'analytic'. His concern is to provide an idealized description of intuition and if the proposal to take senses as the objects of belief pans out he may be committed to the claim that his subjects *believe* analytic senses to be true. But nothing in his theory commits the semantic theorist to agreeing with his subjects. Though obvious enough, this fact is sometimes obscured by the otherwise unobjectionable practice of theorists using themselves and their colleagues as the principal source of data for a semantic theory. To see how all this relates to the prospective merger of the two programs, imagine that at a given stage in the evolution of a Quinean theory its account of entailment, logical truth and the rest matches substantially the then current account of the identically labeled notions advanced by semantic theorists. Were such a situation to arise there is every reason to believe that it would be unstable. For suppose that on-going research among Quineans turned up an alternative theory whose overall scientific utility was pretty clearly superior. Plainly such a discovery would not alter the intuitions and beliefs of the linguistic community at large. Thus the accepted Quinean theory would change while the accepted semantic theory would not. And, of course, the story can be turned around, imagining a gradual evolution of the vernacular and with it a gradual evolution of intuitions, provoking no change in Quinean theory.

The observations of the preceding paragraph have implications beyond the potential merger of the Quinean and semantic programs. They point to a certain systematic misrepresentation in our account of the semantic program, a misrepresentation that follows the lead of the usual presentations of semantic theories. The problem is the semantic theorist's choice of the terms 'logical truth', 'entailment', 'analytic sentence' and some others to label the classes of sentences and the relations among sentences specified by his theory. The misrepresentation is clearest in the case of 'logical truth' for, as we have lately noted, there is no reason to believe that the sentences so labeled in semantic theory are *true*. Similarly, 'analytic' is standardly parsed as 'truth by virtue of meaning'. But the semantic theorist has no reason to believe the sentences he calls 'analytic' are true at all, let alone true by virtue of meaning. Nor has he any argument that as he uses the word 'entailment' truths entail only truths.

At issue is more than a matter of suitable terminology. What is at stake

is not the labels the semantic theorist chooses but the truth of the sentences selected, however labeled. The semantic theorist uses evidence about intuitions (and perhaps about beliefs) to build a theory which entails that certain sentences are logical truths. If, in this theory, 'logical truth' is viewed as a technical term in which 'truth' occurs syncategorematically, there is no complaint. There is cause for protest only if the semantic theorist goes on to claim that his 'logical truths' are true. To substantiate the claim he needs an argument that data about intuitions and beliefs can yield conclusions about truth. In the absence of such an argument – and I see little prospect of a serious argument being constructed – we must conclude that semantic theory, as here construed, *is not a theory of logical form at all*. It tells us nothing about logical truth and nothing about entailment, save in the Pickwickian sense in which logical truths need not be true and entailment need not preserve truth.

To endorse this conclusion is not to patronize semantic theory. Semantic theory, as here characterized, falls squarely within the boundaries of psychology; it is that branch of psychology concerned to describe and explain a complex system of judgments and intuitions characteristic of natural language users. And if, as suggested, semantic theory should prove to be systematically related to the study of beliefs and other propositional attitudes, the combined total theory would still plainly count as psychology.

There is no novelty in the observation that a theory of semantic intuitions (with or without ties to a theory of belief) sanctions no inference to the truth of sentences favored by intuition.¹⁴ Yet few who pursue the semantic program have taken it to heart. Part of the blame, I suspect, can be laid to one or another version of a bad argument invoking truth conditions. In outline the argument goes like this: by observing patterns of belief and disbelief (or of assent and dissent) we can determine the way certain constructions affect truth conditions. Suppose, for example, that a given construction in some exotic tongue works by adding the word 'neg' to the beginning of another sentence, thereby forming a new sentence. And suppose further that speakers commonly believe (or assent to) a sentence S when and only when they disbelieve (or dissent from) 'neg' $\neg S$, and that they believe 'neg' $\neg S$ when they disbelieve S . This, the argument holds, is enough to establish that the 'neg' construction functions logically as negation, or, more precisely, that

- (2) $(S_1) (S_2)$ if $S_2 = \text{'neg'} \neg S_1$ then S_1 is true if and only if S_2 is not true.

Suppose now that we find another construction, the 'dis' construction forming a sentence from two others, where the pattern of belief and disbelief indicates that the construction functions as inclusive disjunction, i.e. that

- (3) $(S) (S_1) (S_2)$ if $S = \text{'dis'} \neg S_1 \vee S_2$ then S is true if and only if S_1 is true or S_2 is true or both.

From (2) and (3) we can conclude that every sentence of the form 'dis' $\neg S \neg$ 'neg' $\neg S$ is true. Plainly, the argument concludes, psychological evidence, evidence about speakers' beliefs, can yield conclusions about the truth of sentences in the speakers' language, though the sentences in question are not sentences about speakers' psychological states. Indeed, the sentences are logical truths.

The trouble with this argument comes in the inference from evidence about beliefs to the conclusions (2) and (3). Soberly viewed, the inference is a simple non-sequitur. The imagined evidence may support generalizations like

- (2') $(S_1) (S_2)$ if $S_2 = \text{'neg'} \neg S_1$ then S_1 is *believed* true if and only if S_2 is not *believed* true.

and

- (3') $(S) (S_1) (S_2)$ if $S = \text{'dis'} \neg S_1 \vee S_2$, then S is *believed* true if and only if S_1 is *believed* true or S_2 is *believed* true or both.

But without further premises there is no argument from (2') and (3') to (2) and (3). We are back where we started, looking for an argument from belief to truth.

Another version of this untenable argument casts it in terms of translation. Here the argument goes something like this: In the case imagined two paragraphs back, the evidence about beliefs suffices to show that 'neg' is best translated into English as 'not' and 'dis' is best translated as 'or'. So if $\text{Tr}(S)$ is the English translation of sentence S in Exotic, 'not' $\neg \text{Tr}(S)$ will be the translation of 'neg' $\neg S$. And $\text{Tr}(S)$ is true if and only if 'not' $\neg \text{Tr}(S)$ is not true. Finally, assuming that a sentence is true in Exotic if and only if its translation into English is true, we can infer back to (2). Analogous reasoning yields (3). So we can get from evidence about beliefs to conclusions about truth after all.

This version of the argument helps itself to a healthy serving of assumptions about translation and the evidence for translation. But even if we leave these assumptions unchallenged, the argument will not do what it purports to. For central to the argument is a premise about truth conditions of *English* sentences. This is the assumption that for any Exotic S , $\text{Tr}(S)$ is true if and only if 'not' $\neg \text{Tr}(S)$ is not. It has surely not been shown that *this* premise can be supported by evidence about belief or intuition. On the view I am urging, the premise rests squarely on the sort of support a Quinean might offer. Evidence about belief may support conclusions about translation, but we have found no argument that it will support conclusions about truth or truth conditions.

One might well wonder why anyone should suppose it at all plausible that evidence about belief and intuition would support conclusions about truth and truth conditions. The answer, I think, traces back to a failure of nerve at the very beginning of modern empiricism. The classical empiricists were not thorough-going in their claim that all knowledge arose from experience. In one guise or another they exempted certain privileged sorts of knowledge, including knowledge of logic. With Kant the explanation of certain sorts of *a priori* knowledge came to rest on the notion of analyticity, which was itself explained in terms drawn from the study of language. And in the Vienna circle's sophisticated empiricism this idea grew to a full-blown linguistic 'explanation' of all *a priori* knowledge. Thus if beliefs, intuitions (and linguistic behavior) are the principal data for linguistics, they must be the principal data for a theory of logical form as well. Underlying the present paper is the view that all of this has been a monumental mistake. I have urged that elaborating an account of logical truth and entailment is no part of a linguists' proper job. The fact that a sentence is a logical truth is no more a fact about language than the fact that a sentence is a physical truth. Recognizing this would serve to direct linguists toward more profitable pursuits while yielding a clearer picture of the place of logic among the sciences.

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NOTES

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¹ A conspicuous exception is Donald Davidson in whose writings these questions come in for considerable attention.

² Cf. my 'Davidson's Semantic Program', to appear.

³ See, for example, 'Truth by Convention' and 'Carnap and Logical Truth', both reprinted in W. V. Quine. *The Ways of Paradox* (New York, 1966); *Word and Object* (Cambridge, Mass., 1960); *Philosophy of Logic* (Englewood Cliffs, N.J., 1970).

⁴ *Philosophy of Logic*, p. 35.

⁵ Cf. S. P. Stich, 'Dissonant Notes on the Theory of Reference', *Nous* IV (1970) 4.

⁶ It is easy enough to capture this informal idea more precisely. Suppose we have a theory whose theorems include (perhaps infinitely many) sentences of the form:

S is true

where ' S ' is replaced by some standard name of a sentence. We may then append to the theory an infinite number of axioms each of the form:

S is true if and only if p

where ' S ' is replaced by any standard name of a sentence and ' p ' is replaced by the sentence itself. Now each sentence whose truth was entailed by the original theory will itself be a theorem of the enriched theory. *Logical theory, as described below, is to be viewed as tacitly enriched with just such an axiom schema.*

⁷ There are, of course, other ways of presenting a logical theory. One idea is to begin by specifying that every instance of certain sentence schemata is true, then add certain transformations on schemata which preserve this property. Another is to pair sentence schemata with set theoretic sentences, adding that every instance of a given schema is true if and only if the associated set theoretic sentence is true. The analogy I am urging between geometry and logic can, I think, be pressed equally well for logical theories developed in these ways.

⁸ In a number of passages Quine seems to urge quite a different idea on how a logical theory is to be tested. (Cf., for example, 'Carnap and Logical Truth', pp. 105-6 in *The Ways of Paradox*.) The alternative suggestion would have a theorist test the theorems of an applied logic against the pre-theoretic store of beliefs he takes to be obvious. If the theory is correct, each sentence it classifies as a logical truth must either seem obviously true to the theorist or at least must be potentially obvious, i.e. he must be able to bring himself to see it as obvious via a series of steps each of which is in turn obviously acceptable. However, this account of the testing of an applied logical theory will not fit comfortably with the program I have been calling Quinean. There are a pair of reasons. The first is a practical problem. In most applied logical theories likely to be of interest there will be infinitely many sentences classified as logical truths. Thus there will be logical truths containing more than n words, for any natural number n . And it is hard to see what the notion of potential obviousness comes to for sentences that would take more than a lifetime to read. Second, and more important, is the fact that on the suggested account of the verification of a logical theory, the central Quinean analogy between empirical science and logic breaks down. An applied geometry is not to be tested by the intuitive obviousness of its theorems, but by the predictive and explanatory power of the theory that results when the geometry is joined with physics. And if the axioms and theorems of the best geometry are neither obvious nor potentially obvious, obviousness be damned.

⁹ The logician's introduction of wholly new vocabulary into his object language serves to reinforce the observation in the previous footnote concerning the justification of

logical theory. Insofar as logical truths make essential use of self-consciously artificial vocabulary, we cannot imagine the logician testing his theory by appeal to the body of sentences pre-theoretically believed true. If logical truths are coeval with logical theory, they could be neither accepted or rejected pre-theoretically. So, insofar as the output of a logical grammar departs from pre-existing usage, justification must take the indirect route, weighing the general empirical utility of the proposed logical theory, new vocabulary and all.

¹⁰ *Philosophy of Logic*, pp. 100–101.

¹¹ The program I have in mind is embraced fairly explicitly in, for example, J. J. Katz and J. A. Fodor, 'The Structure of Semantic Theory', *Language* 39 2 (1963); J. J. Katz and P. Postal, *An Integrated Theory of Linguistic Descriptions* (Cambridge, Mass., 1964); N. Chomsky, *Aspects of the Theory of Syntax* (Cambridge, Mass., 1965); J. J. Katz, *Semantic Theory* (New York, 1972). I would argue that it is also the program behind such works as G. Lakoff, 'On Generative Semantics', in L. Jacobovits and D. Steinberg (eds.), *Semantics: An Interdisciplinary Reader* (Cambridge, 1970), and G. Lakoff, 'Linguistics and Natural Logic', *Synthese* 22 (1970).

¹² For more on this view of grammar, see my 'Grammar, Psychology and Indeterminacy', *Journal of Philosophy* LXIX (1972) 22; 'What Every Grammar Does', *Philosophia* 3 (1973) 1; and 'Competence and Indeterminacy', in *Testing Linguistic Hypotheses: Papers From the University of Wisconsin-Milwaukee Linguistics Group Third Annual Symposium* (ed. by David Cohen and Jessica R. Wirth), Washington, D.C., 1975.

¹³ Cf. Gilbert H. Harman, *Thought* (Princeton, N.J., 1973), Ch. V.

¹⁴ Essentially the same point is made in Quine's reply to Grice and Strawson, *Word and Object*, pp. 66 ff.