QUINE

"ONTOLOGICAL RELATIVITY"

Ontological Relativity and Other Essays

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Ontological Relativity

I

I listened to Dewey on Art as Experience when I was a graduate student in the spring of 1931. Dewey was then at Harvard as the first William James Lecturer. I am proud now to be at Columbia as the first John Dewey Lecturer.

Philosophically I am bound to Dewey by the naturalism that dominated his last three decades. With Dewey I hold that knowledge, mind, and meaning are part of the same world that they have to do with, and that they are to be studied in the same empirical spirit that animates natural science. There is no place for a prior philosophy.

When a naturalistic philosopher addresses himself to the philosophy of mind, he is apt to talk of language. Meanings are, first and foremost, meanings of language. Language is a social art which we all acquire on the evidence solely of other people's overt behavior under publicly recognizable circumstances. Meanings, therefore, those very models of mental entities, end up as grist for the behaviorist's mill. Dewey was ex-

plicit on the point: "Meaning . . . is not a psychic existence; it is primarily a property of behavior." [1]

Once we appreciate the institution of language in these terms, we see that there cannot be, in any useful sense, a private language. This point was stressed by Dewey in the twenties. "Soliloquy," he wrote, "is the product and reflex of converse with others" (170). Further along he expanded the point thus: "Language is specifically a mode of interaction of at least two beings, a speaker and a hearer; it presupposes an organized group to which these creatures belong, and from whom they have acquired their habits of speech. It is therefore a relationship" (185). Years later, Wittgenstein likewise rejected private language. When Dewey was writing in this naturalistic vein, Wittgenstein still held his copy theory of language.

The copy theory in its various forms stands closer to the main philosophical tradition, and to the attitude of common sense today. Uncritical semantics is the myth of a museum in which the exhibits are meanings and the words are labels. To switch languages is to change the labels. Now the naturalist's primary objection to this view is not an objection to meanings on account of their being mental entities, though that could be objection enough. The primary objection persists even if we take the labeled exhibits not as mental ideas but as Platonic ideas or even as the denoted concrete objects. Semantics is vitiated by a pernicious mentalism as long as we regard a man's semantics as somehow determinate in his mind beyond what might be implicit in his dispositions to overt behavior. It is the very facts about meaning, not the entities meant, that must be construed in terms of behavior.

There are two parts to knowing a word. One part is being

[1] J. Dewey, *Experience and Nature* (La Salle, Ill.: Open Court, 1925, 1958), p. 179.

familiar with the sound of it and being able to reproduce it. This part, the phonetic part, is achieved by observing and imitating other people's behavior, and there are no important illusions about the process. The other part, the semantic part, is knowing how to use the word. This part, even in the paradigm case, is more complex than the phonetic part. The word refers, in the paradigm case, to some visible object. The learner has now not only to learn the word phonetically, by hearing it from another speaker; he also has to see the object; and in addition to this, in order to capture the relevance of the object to the word, he has to see that the speaker also sees the object. Dewey summed up the point thus: "The characteristic theory about *B*'s understanding of *A*'s sounds is that he responds to the thing from the standpoint of *A*" (178). Each of us, as he learns his language, is a student of his neighbor's behavior; and conversely, insofar as his tries are approved or corrected, he is a subject of his neighbor's behavioral study.

The semantic part of learning a word is more complex than the phonetic part, therefore, even in simple cases: we have to see what is stimulating the other speaker. In the case of words not directly ascribing observable traits to things, the learning process is increasingly complex and obscure; and obscurity is the breeding place of mentalistic semantics. What the naturalist insists on is that, even in the complex and obscure parts of language learning, the learner has no data to work with but the overt behavior of other speakers.

When with Dewey we turn thus toward a naturalistic view of language and a behavioral view of meaning, what we give up is not just the museum figure of speech. We give up an assurance of determinacy. Seen according to the museum myth, the words and sentences of a language have their determinate meanings. To discover the meanings of the native's words we

may have to observe his behavior, but still the meanings of the words are supposed to be determinate in the native's *mind*, his mental museum, even in cases where behavioral criteria are powerless to discover them for us. When on the other hand we recognize with Dewey that "meaning . . . is primarily a property of behavior," we recognize that there are no meanings, nor likenesses nor distinctions of meaning, beyond what are implicit in people's dispositions to overt behavior. For naturalism the question whether two expressions are alike or unlike in meaning has no determinate answer, known or unknown, except insofar as the answer is settled in principle by people's speech dispositions, known or unknown. If by these standards there are indeterminate cases, so much the worse for the terminology of meaning and likeness of meaning.

To see what such indeterminacy would be like, suppose there were an expression in a remote language that could be translated into English equally defensibly in either of two ways, unlike in meaning in English. I am not speaking of ambiguity within the native language. I am supposing that one and the same native use of the expression can be given either of the English translations, each being accommodated by compensating adjustments in the translation of other words. Suppose both translations, along with these accommodations in each case, accord equally well with all observable behavior on the part of speakers of the remote language and speakers of English. Suppose they accord perfectly not only with behavior actually observed, but with all dispositions to behavior on the part of all the speakers concerned. On these assumptions it would be forever impossible to know of one of these translations that it was the right one, and the other wrong. Still, if the museum myth were true, there would be a right and wrong of the matter; it is just that we would never know, not having access to the museum. See language naturalistically, on the other hand, and you have to see the notion of likeness of meaning in such a case simply as nonsense.

I have been keeping to the hypothetical. Turning now to examples, let me begin with a disappointing one and work up. In the French construction "ne . . . rien" you can translate "rien" into English as "anything" or as "nothing" at will, and then accommodate your choice by translating "ne" as "not" or by construing it as pleonastic. This example is disappointing because you can object that I have merely cut the French units too small. You can believe the mentalistic myth of the meaning museum and still grant that "rien" of itself has no meaning, being no whole label; it is part of "ne . . . rien," which has its meaning as a whole.

I began with this disappointing example because I think its conspicuous trait—its dependence on cutting language into segments too short to carry meanings—is the secret of the more serious cases as well. What makes other cases more serious is that the segments they involve are seriously long: long enough to be predicates and to be true of things and hence, you would think, to carry meanings.

An artificial example which I have used elsewhere [2] depends on the fact that a whole rabbit is present when and only when an undetached part of a rabbit is present; also when and only when a temporal stage of a rabbit is present. If we are wondering whether to translate a native expression "gavagai" as "rabbit" or as "undetached rabbit part" or as "rabbit stage," we can never settle the matter simply by ostension—that is, simply by

[2] Quine, *Word and Object* (Cambridge, Mass.: MIT Press, 1960), §12.

repeatedly querying the expression "gavagai" for the native's assent or dissent in the presence of assorted stimulations.

Before going on to urge that we cannot settle the matter by non-ostensive means either, let me belabor this ostensive predicament a bit. I am not worrying, as Wittgenstein did, about simple cases of ostension. The color word "sepia," to take one of his examples,[3] can certainly be learned by an ordinary process of conditioning, or induction. One need not even be told that sepia is a color and not a shape or a material or an article. True, barring such hints, many lessons may be needed, so as to eliminate wrong generalizations based on shape, material, etc., rather than color, and so as to eliminate wrong notions as to the intended boundary of an indicated example, and so as to delimit the admissible variations of color itself. Like all conditioning, or induction, the process will depend ultimately also on one's own inborn propensity to find one stimulation qualitatively more akin to a second stimulation than to a third; otherwise there can never be any selective reinforcement and extinction of responses.[4] Still, in principle nothing more is needed in learning "sepia" than in any conditioning or induction.

But the big difference between "rabbit" and "sepia" is that whereas "sepia" is a mass term like "water," "rabbit" is a term of divided reference. As such it cannot be mastered without mastering its principle of individuation: where one rabbit leaves off and another begins. And this cannot be mastered by pure ostension, however persistent.

Such is the quandary over "gavagai": where one gavagai

[3] L. Wittgenstein, *Philosophical Investigations* (New York: Macmillan, 1953), p. 14.
[4] Cf. *Word and Object*, §17.

leaves off and another begins. The only difference between rabbits, undetached rabbit parts, and rabbit stages is in their individuation. If you take the total scattered portion of the spatiotemporal world that is made up of rabbits, and that which is made up of undetached rabbit parts, and that which is made up of rabbit stages, you come out with the same scattered portion of the world each of the three times. The only difference is in how you slice it. And how to slice it is what ostension or simple conditioning, however persistently repeated, cannot teach.

Thus consider specifically the problem of deciding between "rabbit" and "undetached rabbit part" as translation of "gavagai." No word of the native language is known, except that we have settled on some working hypothesis as to what native words or gestures to construe as assent and dissent in response to our pointings and queryings. Now the trouble is that whenever we point to different parts of the rabbit, even sometimes screening the rest of the rabbit, we are pointing also each time to the rabbit. When, conversely, we indicate the whole rabbit with a sweeping gesture, we are still pointing to a multitude of rabbit parts. And note that we do not have even a native analogue of our plural ending to exploit, in asking "gavagai?" It seems clear that no even tentative decision between "rabbit" and "undetached rabbit part" is to be sought at this level.

How would we finally decide? My passing mention of plural endings is part of the answer. Our individuating of terms of divided reference, in English, is bound up with a cluster of interrelated grammatical particles and constructions: plural endings, pronouns, numerals, the "is" of identity, and its adaptations "same" and "other." It is the cluster of interrelated devices in which quantification becomes central when the regimentation of symbolic logic is imposed. If in his language we could ask the native "Is this *gavagai* the same as that one?" while making appropriate multiple ostensions, then indeed we would be well on our way to deciding between "rabbit," "undetached rabbit part," and "rabbit stage." And of course the linguist does at length reach the point where he can ask what purports to be that question. He develops a system for translating our pluralizations, pronouns, numerals, identity, and related devices contextually into the native idiom. He develops such a system by abstraction and hypothesis. He abstracts native particles and constructions from observed native sentences and tries associating these variously with English particles and constructions. Insofar as the native sentences and the thus associated English ones seem to match up in respect of appropriate occasions of use, the linguist feels confirmed in these hypotheses of translation—what I call *analytical hypotheses.*[5]

But it seems that this method, though laudable in practice and the best we can hope for, does not in principle settle the indeterminacy between "rabbit," "undetached rabbit part," and "rabbit stage." For if one workable overall system of analytical hypotheses provides for translating a given native expression into "is the same as," perhaps another equally workable but systematically different system would translate that native expression rather into something like "belongs with." Then when in the native language we try to ask "Is this *gavagai* the same as that?" we could as well be asking "Does this *gavagai* belong with that?" Insofar, the native's assent is no objective evidence for translating "gavagai" as "rabbit" rather than "undetached rabbit part" or "rabbit stage."

This artificial example shares the structure of the trivial earlier example "ne . . . rien." We were able to translate "rien"

[5] *Word and Object,* §15. For a summary of the general point of view see also §I of "Speaking of Objects," Chapter 1 in this volume.

as "anything" or as "nothing," thanks to a compensatory adjustment in the handling of "ne." And I suggest that we can translate "gavagai" as "rabbit" or "undetached rabbit part" or "rabbit stage," thanks to compensatory adjustments in the translation of accompanying native locutions. Other adjustments still might accommodate translation of "gavagai" as "rabbithood," or in further ways. I find this plausible because of the broadly structural and contextual character of any considerations that could guide us to native translations of the English cluster of interrelated devices of individuation. There seem bound to be systematically very different choices, all of which do justice to all dispositions to verbal behavior on the part of all concerned.

An actual field linguist would of course be sensible enough to equate "gavagai" with "rabbit," dismissing such perverse alternatives as "undetached rabbit part" and "rabbit stage" out of hand. This sensible choice and others like it would help in turn to determine his subsequent hypotheses as to what native locutions should answer to the English apparatus of individuation, and thus everything would come out all right. The implicit maxim guiding his choice of "rabbit," and similar choices for other native words, is that an enduring and relatively homogeneous object, moving as a whole against a contrasting background, is a likely reference for a short expression. If he were to become conscious of this maxim, he might celebrate it as one of the linguistic universals, or traits of all languages, and he would have no trouble pointing out its psychological plausibility. But he would be wrong; the maxim is his own imposition, toward settling what is objectively indeterminate. It is a very sensible imposition, and I would recommend no other. But I am making a philosophical point.

It is philosophically interesting, moreover, that what is indeterminate in this artificial example is not just meaning, but extension; reference. My remarks on indeterminacy began as a challenge to likeness of meaning. I had us imagining "an expression that could be translated into English equally defensibly in either of two ways, unlike in meaning in English." Certainly likeness of meaning is a dim notion, repeatedly challenged. Of two predicates which are alike in extension, it has never been clear when to say that they are alike in meaning and when not; it is the old matter of featherless bipeds and rational animals, or of equiangular and equilateral triangles. Reference, extension, has been the firm thing; meaning, intension, the infirm. The indeterminacy of translation now confronting us, however, cuts across extension and intension alike. The terms "rabbit," "undetached rabbit part," and "rabbit stage" differ not only in meaning; they are true of different things. Reference itself proves behaviorally inscrutable.

Within the parochial limits of our own language, we can continue as always to find extensional talk clearer than intensional. For the indeterminacy between "rabbit," "rabbit stage," and the rest depended only on a correlative indeterminacy of translation of the English apparatus of individuation—the apparatus of pronouns, pluralization, identity, numerals, and so on. No such indeterminacy obtrudes so long as we think of this apparatus as given and fixed. Given this apparatus, there is no mystery about extension; terms have the same extension when true of the same things. At the level of radical translation, on the other hand, extension itself goes inscrutable.

My example of rabbits and their parts and stages is a contrived example and a perverse one, with which, as I said, the practicing linguist would have no patience. But there are also cases, less bizarre ones, that obtrude in practice. In Japanese there are certain particles, called "classifiers," which may be explained in either of two ways. Commonly they are explained as attaching to numerals, to form compound numerals of distinctive styles. Thus take the numeral for 5. If you attach one classifier to it you get a style of "5" suitable for counting animals; if you attach a different classifier, you get a style of "5" suitable for counting slim things like pencils and chopsticks; and so on. But another way of viewing classifiers is to view them not as constituting part of the numeral, but as constituting part of the term—the term for "chopsticks" or "oxen" or whatever. On this view the classifier does the individuative job that is done in English by "sticks of" as applied to the mass term "wood," or "head of" as applied to the mass term "cattle."

What we have on either view is a Japanese phrase tantamount say to "five oxen," but consisting of three words; [6] the first is in effect the neutral numeral "5," the second is a classifier of the animal kind, and the last corresponds in some fashion to "ox." On one view the neutral numeral and the classifier go together to constitute a declined numeral in the "animal gender," which then modifies "ox" to give, in effect, "five oxen." On the other view the third Japanese word answers not to the individuative term "ox" but to the mass term "cattle"; the classifier applies to this mass term to produce a composite individuative term, in effect "head of cattle"; and the neutral numeral applies directly to all this without benefit of gender, giving "five head of cattle," hence again in effect "five oxen."

If so simple an example is to serve its expository purpose, it needs your connivance. You have to understand "cattle" as a mass term covering only bovines, and "ox" as applying to all bovines. That these usages are not the invariable usages is beside the point. The point is that the Japanese phrase comes out as "five bovines," as desired, when parsed in either of two

[6] To keep my account graphic I am counting a certain postpositive particle as a suffix rather than a word.

ways. The one way treats the third Japanese word as an individuative term true of each bovine, and the other way treats that word rather as a mass term covering the unindividuated totality of beef on the hoof. These are two very different ways of treating the third Japanese word; and the three-word phrase as a whole turns out all right in both cases only because of compensatory differences in our account of the second word, the classifier.

This example is reminiscent in a way of our trivial initial example, "ne . . . rien." We were able to represent "rien" as "anything" or as "nothing," by compensatorily taking "ne" as negative or as vacuous. We are able now to represent a Japanese word either as an individuative term for bovines or as a mass term for live beef, by compensatorily taking the classifier as declining the numeral or as individuating the mass term. However, the triviality of the one example does not quite carry over to the other. The early example was dismissed on the ground that we had cut too small; "rien" was too short for significant translation on its own, and "ne . . . rien" was the significant unit. But you cannot dismiss the Japanese example by saying that the third word was too short for significant translation on its own and that only the whole three-word phrase, tantamount to "five oxen," was the significant unit. You cannot take this line unless you are prepared to call a word too short for significant translation even when it is long enough to be a term and carry denotation. For the third Japanese word is, on either approach, a term: on one approach a term of divided reference, and on the other a mass term. If you are indeed prepared thus to call a word too short for significant translation even when it is a denoting term, then in a backhanded way you are granting what I wanted to prove: the inscrutability of reference.

Between the two accounts of Japanese classifiers there is no question of right and wrong. The one account makes for more efficient translation into idiomatic English; the other makes for more of a feeling for the Japanese idiom. Both fit all verbal behavior equally well. All whole sentences, and even component phrases like "five oxen," admit of the same net overall English translations on either account. This much is invariant. But what is philosophically interesting is that the reference or extension of shorter terms can fail to be invariant. Whether that third Japanese word is itself true of each ox, or whether on the other hand it is a mass term which needs to be adjoined to the classifier to make a term which is true of each ox—here is a question that remains undecided by the totality of human dispositions to verbal behavior. It is indeterminate in principle; there is no fact of the matter. Either answer can be accommodated by an account of the classifier. Here again, then, is the inscrutability of reference—illustrated this time by a humdrum point of practical translation.

The inscrutability of reference can be brought closer to home by considering the word "alpha," or again the word "green." In our use of these words and others like them there is a systematic ambiguity. Sometimes we use such words as concrete general terms, as when we say the grass is green, or that some inscription begins with an alpha. Sometimes on the other hand we use them as abstract singular terms, as when we say that green is a color and alpha is a letter. Such ambiguity is encouraged by the fact that there is nothing in ostension to distinguish the two uses. The pointing that would be done in teaching the concrete general term "green," or "alpha," differs none from the pointing that would be done in teaching the abstract singular term "green" or "alpha." Yet the objects referred to by the word are very different under the two uses; under the one use the word is true of many concrete objects, and under the other use it names a single abstract object.

We can of course tell the two uses apart by seeing how the word turns up in sentences: whether it takes an indefinite article, whether it takes a plural ending, whether it stands as singular subject, whether it stands as modifier, as predicate complement, and so on. But these criteria appeal to our special English grammatical constructions and particles, our special English apparatus of individuation, which, I already urged, is itself subject to indeterminacy of translation. So, from the point of view of translation into a remote language, the distinction between a concrete general and an abstract singular term is in the same predicament as the distinction between "rabbit," "rabbit part," and "rabbit stage." Here then is another example of the inscrutability of reference, since the difference between the concrete general and the abstract singular is a difference in the objects referred to.

Incidentally we can concede this much indeterminacy also to the "sepia" example, after all. But this move is not evidently what was worrying Wittgenstein.

The ostensive indistinguishability of the abstract singular from the concrete general turns upon what may be called "deferred ostension," as opposed to direct ostension. First let me define direct ostension. The *ostended point,* as I shall call it, is the point where the line of the pointing finger first meets an opaque surface. What characterizes *direct ostension,* then, is that the term which is being ostensively explained is true of something that contains the ostended point. Even such direct ostension has its uncertainties, of course, and these are familiar. There is the question how wide an environment of the ostended point is meant to be covered by the term that is being ostensively explained. There is the question how considerably an absent thing or substance might be allowed to differ from what is now ostended, and still be covered by the term that is now being ostensively explained. Both of these questions can in principle be settled as well as need be by induction from multiple ostensions. Also, if the term is a term of divided reference like "apple," there is the question of individuation: the question where one of its objects leaves off and another begins. This can be settled by induction from multiple ostensions of a more elaborate kind, accompanied by expressions like "same apple" and "another," if an equivalent of this English apparatus of individuation has been settled on; otherwise the indeterminacy persists that was illustrated by "rabbit," "undetached rabbit part," and "rabbit stage."

Such, then, is the way of direct ostension. Other ostension I call *deferred*. It occurs when we point at the gauge, and not the gasoline, to show that there is gasoline. Also it occurs when we explain the abstract singular term "green" or "alpha" by pointing at grass or a Greek inscription. Such pointing is direct ostension when used to explain the concrete general term "green" or "alpha," but it is deferred ostension when used to explain the abstract singular terms; for the abstract object which is the color green or the letter alpha does not contain the ostended point, nor any point.

Deferred ostension occurs very naturally when, as in the case of the gasoline gauge, we have a correspondence in mind. Another such example is afforded by the Gödel numbering of expressions. Thus if 7 has been assigned as Gödel number of the letter alpha, a man conscious of the Gödel numbering would not hesitate to say "Seven" on pointing to an inscription of the Greek letter in question. This is, on the face of it, a doubly deferred ostension: one step of deferment carries us from the inscription to the letter as abstract object, and a second step carries us thence to the number.

By appeal to our apparatus of individuation, if it is avail-

All interpretation (semantics) is deferred? (to level of abstract objects?) Where is meaningfulness? (on sentence or abstraction)

able, we can distinguish between the concrete general and the abstract singular use of the word "alpha"; this we saw. By appeal again to that apparatus, and in particular to identity, we can evidently settle also whether the word "alpha" in its abstract singular use is being used really to name the letter or whether, perversely, it is being used to name the Gödel number of the letter. At any rate we can distinguish these alternatives if also we have located the speaker's equivalent of the numeral "7" to our satisfaction; for we can ask him whether alpha *is* 7.

These considerations suggest that deferred ostension adds no essential problem to those presented by direct ostension. Once we have settled upon analytical hypotheses of translation covering identity and the other English particles relating to individuation, we can resolve not only the indecision between "rabbit" and "rabbit stage" and the rest, which came of direct ostension, but also any indecision between concrete general and abstract singular, and any indecision between expression and Gödel number, which come of deferred ostension. However, this conclusion is too sanguine. The inscrutability of reference runs deep, and it persists in a subtle form even if we accept identity and the rest of the apparatus of individuation as fixed and settled; even, indeed, if we forsake radical translation and think only of English.

Consider the case of a thoughtful protosyntactician. He has a formalized system of first-order proof theory, or protosyntax, whose universe comprises just expressions, that is, strings of signs of a specified alphabet. Now just what sorts of things, more specifically, are these expressions? They are types, not tokens. So, one might suppose, each of them is the set of all its tokens. That is, each expression is a set of inscriptions which are variously situated in space-time but are classed together by virtue of a certain similarity in shape. The concatenate $x^\frown y$ of two expressions x and y, in a given order, will be the set of all inscriptions each of which has two parts which are tokens respectively of x and y and follow one upon the other in that order. But $x^\frown y$ may then be the null set, though x and y are not null; for it may be that inscriptions belonging to x and y happen to turn up head to tail nowhere, in the past, present, or future. This danger increases with the lengths of x and y. But it is easily seen to violate a law of protosyntax which says that $x = z$ whenever $x^\frown y = z^\frown y$.

Thus it is that our thoughtful protosyntactician will not construe the things in his universe as sets of inscriptions. He can still take his atoms, the single signs, as sets of inscriptions, for there is no risk of nullity in these cases. And then, instead of taking his strings of signs as sets of inscriptions, he can invoke the mathematical notion of sequence and take them as sequences of signs. A familiar way of taking sequences, in turn, is as a mapping of things on numbers. On this approach an expression or string of signs becomes a finite set of pairs each of which is the pair of a sign and a number.

This account of expressions is more artificial and more complex than one is apt to expect who simply says he is letting his variables range over the strings of such and such signs. Moreover, it is not the inevitable choice; the considerations that motivated it can be met also by alternative constructions. One of these constructions is Gödel numbering itself, and it is temptingly simple. It uses just natural numbers, whereas the foregoing construction used sets of one-letter inscriptions and also natural numbers and sets of pairs of these. How clear is it that at just *this* point we have dropped expressions in favor of numbers? What is clearer is merely that in both constructions

we were artificially devising models to satisfy laws that expressions in an unexplicated sense had been meant to satisfy.

So much for expressions. Consider now the arithmetician himself, with his elementary number theory. His universe comprises the natural numbers outright. Is it clearer than the protosyntactician's? What, after all, is a natural number? There are Frege's version, Zermelo's, and von Neumann's, and countless further alternatives, all mutually incompatible and equally correct. What we are doing in any one of these explications of natural number is to devise set-theoretic models to satisfy laws which the natural numbers in an unexplicated sense had been meant to satisfy. The case is quite like that of protosyntax.

It will perhaps be felt that any set-theoretic explication of natural number is at best a case of *obscurum per obscurius;* that all explications must assume something, and the natural numbers themselves are an admirable assumption to start with. I must agree that a construction of sets and set theory from natural numbers and arithmetic would be far more desirable than the familiar opposite. On the other hand our impression of the clarity even of the notion of natural number itself has suffered somewhat from Gödel's proof of the impossibility of a complete proof procedure for elementary number theory, or, for that matter, from Skolem's and Henkin's observations that all laws of natural numbers admit nonstandard models.[7]

We are finding no clear difference between *specifying* a universe of discourse—the range of the variables of quantification—and *reducing* that universe to some other. We saw no significant difference between clarifying the notion of expression and supplanting it by that of number. And now to say more partic-

[7] See Leon Henkin, "Completeness in the theory of types," *Journal of Symbolic Logic* 15 (1950), 81–91, and references therein.

ularly what numbers themselves are is in no evident way different from just dropping numbers and assigning to arithmetic one or another new model, say in set theory.

Expressions are known only by their laws, the laws of concatenation theory, so that any constructs obeying those laws—Gödel numbers, for instance—are *ipso facto* eligible as explications of expression. Numbers in turn are known only by their laws, the laws of arithmetic, so that any constructs obeying those laws—certain sets, for instance—are eligible in turn as explications of number. Sets in turn are known only by their laws, the laws of set theory.

Russell pressed a contrary thesis, long ago. Writing of numbers, he argued that for an understanding of number the laws of arithmetic are not enough; we must know the applications, we must understand numerical discourse embedded in discourse of other matters. In applying number, the key notion, he urged, is *Anzahl:* there are *n* so-and-sos. However, Russell can be answered. First take, specifically, *Anzahl.* We can define "there are *n* so-and-sos" without ever deciding what numbers are, apart from their fulfillment of arithmetic. That there are *n* so-and-sos can be explained simply as meaning that the so-and-sos are in one-to-one correspondence with the numbers up to *n*.[8]

Russell's more general point about application can be answered too. Always, if the structure is there, the applications will fall into place. As paradigm it is perhaps sufficient to recall again this reflection on expressions and Gödel numbers: that even the pointing out of an inscription is no final evidence that our talk is of expressions and not of Gödel numbers. We can always plead deferred ostension.

[8] For more on this theme see my *Set Theory and Its Logic* (Cambridge, Mass.: Harvard, 1963, 1969), §11.

It is in this sense true to say, as mathematicians often do, that arithmetic is all there is to number. But it would be a confusion to express this point by saying, as is sometimes said, that numbers are any things fulfilling arithmetic. This formulation is wrong because distinct domains of objects yield distinct models of arithmetic. Any progression can be made to serve; and to identify all progressions with one another, e.g., to identify the progression of odd numbers with the progression of evens, would contradict arithmetic after all.

Peano Systems (1,4,8,... e.g.)

So, though Russell was wrong in suggesting that numbers need more than their arithmetical properties, he was right in objecting to the definition of numbers as any things fulfilling arithmetic. The subtle point is that any progression will serve as a version of number so long and only so long as we stick to one and the same progression. Arithmetic is, in this sense, all there is to number: there is no saying absolutely what the numbers are; there is only arithmetic.[9]

II

I first urged the inscrutability of reference with the help of examples like the one about rabbits and rabbit parts. These used direct ostension, and the inscrutability of reference hinged on the indeterminacy of translation of identity and other individuative apparatus. The setting of these examples, accordingly, was radical translation: translation from a remote language on behavioral evidence, unaided by prior dictionaries. Moving then to deferred ostension and abstract objects, we

[9] Paul Benacerraf, "What numbers cannot be," *Philosophical Review* 74 (1965), 47–73, develops this point. His conclusions differ in some ways from those I shall come to.

found a certain dimness of reference pervading the home language itself.

Now it should be noted that even for the earlier examples the resort to a remote language was not really essential. On deeper reflection, radical translation begins at home. Must we equate our neighbor's English words with the same strings of phonemes in our own mouths? Certainly not; for sometimes we do not thus equate them. Sometimes we find it to be in the interests of communication to recognize that our neighbor's use of some word, such as "cool" or "square" or "hopefully," differs from ours, and so we translate that word of his into a different string of phonemes in our idiolect. Our usual domestic rule of translation is indeed the homophonic one, which simply carries each string of phonemes into itself; but still we are always prepared to temper homophony with what Neil Wilson has called the "principle of charity." [10] We will construe a neighbor's word heterophonically now and again if thereby we see our way to making his message less absurd.

The homophonic rule is a handy one on the whole. That it works so well is no accident, since imitation and feedback are what propagate a language. We acquired a great fund of basic words and phrases in this way, imitating our elders and encouraged by our elders amid external circumstances to which the phrases suitably apply. Homophonic translation is implicit in this social method of learning. Departure from homophonic translation in this quarter would only hinder communication. Then there are the relatively rare instances of opposite kind, due to divergence in dialect or confusion in an individual, where homophonic translation incurs negative feedback. But what tends to escape notice is that there is also a vast mid-

[10] N. L. Wilson, "Substances without substrata," *Review of Metaphysics* 12 (1959), 521–539, p. 532.

region where the homophonic method is indifferent. Here, gratuitously, we can systematically reconstrue our neighbor's apparent references to rabbits as really references to rabbit stages, and his apparent references to formulas as really references to Gödel numbers and vice versa. We can reconcile all this with our neighbor's verbal behavior, by cunningly readjusting our translations of his various connecting predicates so as to compensate for the switch of ontology. In short, we can reproduce the inscrutability of reference at home. It is of no avail to check on this fanciful version of our neighbor's meanings by asking him, say, whether he really means at a certain point to refer to formulas or to their Gödel numbers; for our question and his answer—"By all means, the numbers"—have lost their title to homophonic translation. The problem at home differs none from radical translation ordinarily so called except in the willfulness of this suspension of homophonic translation.

I have urged in defense of the behavioral philosophy of language, Dewey's, that the inscrutability of reference is not the inscrutability of a fact; there is no fact of the matter. But if there is really no fact of the matter, then the inscrutability of reference can be brought even closer to home than the neighbor's case; we can apply it to ourselves. If it is to make sense to say even of oneself that one is referring to rabbits and formulas and not to rabbit stages and Gödel numbers, then it should make sense equally to say it of someone else. After all, as Dewey stressed, there is no private language.

We seem to be maneuvering ourselves into the absurd position that there is no difference on any terms, interlinguistic or intralinguistic, objective or subjective, between referring to rabbits and referring to rabbit parts or stages; or between referring to formulas and referring to their Gödel numbers. Surely this is absurd, for it would imply that there is no difference between the rabbit and each of its parts or stages, and no difference between a formula and its Gödel number. Reference would seem now to become nonsense not just in radical translation but at home.

Toward resolving this quandary, begin by picturing us at home in our language, with all its predicates and auxiliary devices. This vocabulary includes "rabbit," "rabbit part," "rabbit stage," "formula," "number," "ox," "cattle"; also the two-place predicates of identity and difference, and other logical particles. In these terms we can say in so many words that this is a formula and that a number, this a rabbit and that a rabbit part, this and that the same rabbit, and this and that different parts. *In just those words.* This network of terms and predicates and auxiliary devices is, in relativity jargon, our frame of reference, or coordinate system. Relative to *it* we can and do talk meaningfully and distinctively of rabbits and parts, numbers and formulas. Next, as in recent paragraphs, we contemplate alternative denotations for our familiar terms. We begin to appreciate that a grand and ingenious permutation of these denotations, along with compensatory adjustments in the interpretations of the auxiliary particles, might still accommodate all existing speech dispositions. This was the inscrutability of reference, applied to ourselves; and it made nonsense of reference. Fair enough; reference *is* nonsense except relative to a coordinate system. In this principle of relativity lies the resolution of our quandary.

It is meaningless to ask whether, in general, our terms "rabbit," "rabbit part," "number," etc., really refer respectively to rabbits, rabbit parts, numbers, etc., rather than to some ingeniously permuted denotations. It is meaningless to ask this absolutely; we can meaningfully ask it only relative to some background language. When we ask, "Does 'rabbit' really refer

to rabbits?" someone can counter with the question: "Refer to rabbits in what sense of 'rabbits'?" thus launching a regress; and we need the background language to regress into. The background language gives the query sense, if only relative sense; sense relative in turn to it, this background language. Querying reference in any more absolute way would be like asking absolute position, or absolute velocity, rather than position or velocity relative to a given frame of reference. Also it is very much like asking whether our neighbor may not systematically see everything upside down, or in complementary color, forever undetectably.

We need a background language, I said, to regress into. Are we involved now in an infinite regress? If questions of reference of the sort we are considering make sense only relative to a background language, then evidently questions of reference for the background language make sense in turn only relative to a further background language. In these terms the situation sounds desperate, but in fact it is little different from questions of position and velocity. When we are given position and velocity relative to a given coordinate system, we can always ask in turn about the placing of origin and orientation of axes of that system of coordinates; and there is no end to the succession of further coordinate systems that could be adduced in answering the successive questions thus generated.

In practice of course we end the regress of coordinate systems by something like pointing. And in practice we end the regress of background languages, in discussions of reference, by acquiescing in our mother tongue and taking its words at face value.

Very well; in the case of position and velocity, in practice, pointing breaks the regress. But what of position and velocity apart from practice? what of the regress then? The answer, of course, is the relational doctrine of space; there is no absolute position or velocity; there are just the relations of coordinate systems to one another, and ultimately of things to one another. And I think that the parallel question regarding denotation calls for a parallel answer, a relational theory of what the objects of theories are. What makes sense is to say not what the objects of a theory are, absolutely speaking, but how one theory of objects is interpretable or reinterpretable in another.

The point is not that bare matter is inscrutable: that things are indistinguishable except by their properties. That point does not need making. The present point is reflected better in the riddle about seeing things upside down, or in complementary colors; for it is that things can be inscrutably switched even while carrying their properties with them. Rabbits differ from rabbit parts and rabbit stages not just as bare matter, after all, but in respect of properties; and formulas differ from numbers in respect of properties. What our present reflections are leading us to appreciate is that the riddle about seeing things upside down, or in complementary colors, should be taken seriously and its moral applied widely. The relativistic thesis to which we have come is this, to repeat: it makes no sense to say what the objects of a theory are, beyond saying how to interpret or reinterpret that theory in another. Suppose we are working within a theory and thus treating of its objects. We do so by using the variables of the theory, whose values those objects are, though there be no ultimate sense in which that universe can have been specified. In the language of the theory there are predicates by which to distinguish portions of this universe from other portions, and these predicates differ from one another purely in the roles they play in the laws of the theory. Within this background theory we can show how some subordinate theory, whose universe is some portion of the background universe, can by a reinterpretation be reduced to another subordinate theory whose universe is some lesser portion. Such talk of subordinate theories and their ontologies *is* meaningful, but only relative to the background theory with its own primitively adopted and ultimately inscrutable ontology.

To talk thus of theories raises a problem of formulation. A theory, it will be said, is a set of fully interpreted sentences. (More particularly, it is a deductively closed set: it includes all its own logical consequences, insofar as they are couched in the same notation.) But if the sentences of a theory are fully interpreted, then in particular the range of values of their variables is settled. How then can there be no sense in saying what the objects of a theory are?

My answer is simply that we cannot require theories to be fully interpreted, except in a relative sense, if anything is to count as a theory. In specifying a theory we must indeed fully specify, in our own words, what sentences are to comprise the theory, and what things are to be taken as values of the variables, and what things are to be taken as satisfying the predicate letters; insofar we do fully interpret the theory, *relative* to our own words and relative to our overall home theory which lies behind them. But this fixes the objects of the described theory only relative to those of the home theory; and these can, at will, be questioned in turn.

One is tempted to conclude simply that meaninglessness sets in when we try to pronounce on everything in our universe; that universal predication takes on sense only when furnished with the background of a wider universe, where the predication is no longer universal. And this is even a familiar doctrine, the doctrine that no proper predicate is true of everything. We have all heard it claimed that a predicate is meaningful only by contrast with what it excludes, and hence that being true of everything would make a predicate meaningless. But surely this doctrine is wrong. Surely self-identity, for instance, is not to be rejected as meaningless. For that matter, any statement of fact at all, however brutally meaningful, can be put artificially into a form in which it pronounces on everything. To say merely of Jones that he sings, for instance, is to say of everything that it is other than Jones or sings. We had better beware of repudiating universal predication, lest we be tricked into repudiating everything there is to say.

Is not Jones?

Carnap took an intermediate line in his doctrine of universal words, or *Allwörter*, in *The Logical Syntax of Language*. He did treat the predicating of universal words as "quasi-syntactical"—as a predication only by courtesy, and without empirical content. But universal words were for him not just any universally true predicates, like "is other than Jones or sings." They were a special breed of universally true predicates, ones that are universally true by the sheer meanings of their words and no thanks to nature. In his later writing this doctrine of universal words takes the form of a distinction between "internal" questions, in which a theory comes to grips with facts about the world, and "external" questions, in which people come to grips with the relative merits of theories.

Should we look to these distinctions of Carnap's for light on ontological relativity? When we found there was no absolute sense in saying what a theory is about, were we sensing the infactuality of what Carnap calls "external questions"? When we found that saying what a theory is about did make sense against a background theory, were we sensing the factuality of internal questions of the background theory? I see no hope of illumination in this quarter. Carnap's universal words were not

just any universally true predicates, but, as I said, a special breed; and what distinguishes this breed is not clear. What I said distinguished them was that they were universally true by sheer meanings and not by nature; but this is a very questionable distinction. Talking of "internal" and "external" is no better.

Ontological relativity is not to be clarified by any distinction between kinds of universal predication—unfactual and factual, external and internal. It is not a question of universal predication. When questions regarding the ontology of a theory are meaningless absolutely, and become meaningful relative to a background theory, this is not in general because the background theory has a wider universe. One is tempted, as I said a little while back, to suppose that it is; but one is then wrong.

What makes ontological questions meaningless when taken absolutely is not universality but circularity. A question of the form "What is an *F*?" can be answered only by recourse to a further term: "An *F* is a *G*." The answer makes only relative sense: sense relative to the uncritical acceptance of "*G*."

We may picture the vocabulary of a theory as comprising logical signs such as quantifiers and the signs for the truth functions and identity, and in addition descriptive or nonlogical signs, which, typically, are singular terms, or names, and general terms, or predicates. Suppose next that in the statements which comprise the theory, that is, are true according to the theory, we abstract from the meanings of the nonlogical vocabulary and from the range of the variables. We are left with the logical form of the theory, or, as I shall say, the *theory form*. Now we may interpret this theory form anew by picking a new universe for its variables of quantification to range over, and assigning objects from this universe to the names, and choosing subsets of this universe as extensions of the one-place predicates, and so on. Each such interpretation of the theory form is called a model of it, if it makes it come out true. Which of these models is meant in a given actual theory cannot, of course, be guessed from the theory form. The intended references of the names and predicates have to be learned rather by ostension, or else by paraphrase in some antecedently familiar vocabulary. But the first of these two ways has proved inconclusive, since, even apart from indeterminacies of translation affecting identity and other logical vocabulary, there is the problem of deferred ostension. Paraphrase in some antecedently familiar vocabulary, then, is our only recourse; and such is ontological relativity. To question the reference of all the terms of our all-inclusive theory becomes meaningless, simply for want of further terms relative to which to ask or answer the question.

It is thus meaningless within the theory to say which of the various possible models of our theory form is our real or intended model. Yet even here we can make sense still of there being many models. For we might be able to show that for each of the models, however unspecifiable, there is bound to be another which is a permutation or perhaps a diminution of the first.

This is linguistic, as though to approach to ontology

Suppose for example that our theory is purely numerical. Its objects are just the natural numbers. There is no sense in saying, from within that theory, just which of the various models of number theory is in force. But we can observe even from within the theory that, whatever 0, 1, 2, 3, etc. may be, the theory would still hold true if the 17 of this series were moved into the role of 0, and the 18 moved into the role of 1, and so on.

Ontology is indeed doubly relative. Specifying the universe

of a theory makes sense only relative to some background theory, and only relative to some choice of a manual of translation of the one theory into the other. Commonly of course the background theory will simply be a containing theory, and in this case no question of a manual of translation arises. But this is after all just a degenerate case of translation still—the case where the rule of translation is the homophonic one.

We cannot know what something is without knowing how it is marked off from other things. Identity is thus of a piece with ontology. Accordingly it is involved in the same relativity, as may be readily illustrated. Imagine a fragment of economic theory. Suppose its universe comprises persons, but its predicates are incapable of distinguishing between persons whose incomes are equal. The interpersonal relation of equality of income enjoys, within the theory, the substitutivity property of the identity relation itself; the two relations are indistinguishable. It is only relative to a background theory, in which more can be said of personal identity than equality of income, that we are able even to appreciate the above account of the fragment of economic theory, hinging as the account does on a contrast between persons and incomes.

A usual occasion for ontological talk is reduction, where it is shown how the universe of some theory can by a reinterpretation be dispensed with in favor of some other universe, perhaps a proper part of the first. I have treated elsewhere [11] of the reduction of one ontology to another with help of a *proxy function:* a function mapping the one universe into part or all of the other. For instance, the function "Gödel number of" is a proxy function. The universe of elementary proof theory or

[11] Quine, *The Ways of Paradox* (New York: Random House, 1966), pp. 204 ff.; or see *Journal of Philosophy*, 1964, pp. 214 ff.

protosyntax, which consists of expressions or strings of signs, is mapped by this function into the universe of elementary number theory, which consists of numbers.

The proxy function used in reducing one ontology to another need not, like Gödel numbering, be one-to-one. We might, for instance, be confronted with a theory treating of both expressions and ratios. We would cheerfully reduce all this to the universe of natural numbers, by invoking a proxy function which enumerates the expressions in the Gödel way, and enumerates the ratios by the classical method of short diagonals. This proxy function is not one-to-one, since it assigns the same natural number both to an expression and to a ratio. We would tolerate the resulting artificial convergence between expressions and ratios, simply because the original theory made no capital of the distinction between them; they were so invariably and extravagantly unlike that the identity question did not arise. Formally speaking, the original theory used a two-sorted logic.

For another kind of case where we would not require the proxy function to be one-to-one, consider again the fragment of economic theory lately noted. We would happily reduce its ontology of persons to a less numerous one of incomes. The proxy function would assign to each person his income. It is not one-to-one; distinct persons give way to identical incomes. The reason such a reduction is acceptable is that it merges the images of only such individuals as never had been distinguishable by the predicates of the original theory. Nothing in the old theory is contravened by the new identities.

If on the other hand the theory that we are concerned to reduce or reinterpret is straight protosyntax, or a straight arithmetic of ratios or of real numbers, then a one-to-one proxy function is mandatory. This is because any two elements of

such a theory are distinguishable in terms of the theory. This is true even for the real numbers, even though not every real number is uniquely specifiable; any two real numbers x and y are still distinguishable, in that $x < y$ or $y < x$ and never $x < x$. A proxy function that did not preserve the distinctness of the elements of such a theory would fail of its purpose of reinterpretation.

One ontology is *always* reducible to another when we are given a proxy function f that is one-to-one. The essential reasoning is as follows. Where P is any predicate of the old system, its work can be done in the new system by a new predicate which we interpret as true of just the correlates fx of the old objects x that P was true of. Thus suppose we take fx as the Gödel number of x, and as our old system we take a syntactical system in which one of the predicates is "is a segment of." The corresponding predicate of the new or numerical system, then, would be one which amounts, so far as its extension is concerned, to the words "is the Gödel number of a segment of that whose Gödel number is." The numerical predicate would not be given this devious form, of course, but would be rendered as an appropriate purely arithmetical condition.

Our dependence upon a background theory becomes especially evident when we reduce our universe U to another V by appeal to a proxy function. For it is only in a theory with an inclusive universe, embracing U and V, that we can make sense of the proxy function. The function maps U into V and hence needs all the old objects of U as well as their new proxies in V.

The proxy function need not exist as an object in the universe even of the background theory. It may do its work merely as what I have called a "virtual class," [12] and Gödel has

[12] Quine, *Set Theory and Its Logic*, §§2 f.

called a "notion."[13] That is to say, all that is required toward a function is an open sentence with two free variables, provided that it is fulfilled by exactly one value of the first variable for each object of the old universe as value of the second variable. But the point is that it is only in the background theory, with its inclusive universe, that we can hope to write such a sentence and have the right values at our disposal for its variables.

If the new objects happen to be among the old, so that V is a subclass of U, then the old theory with universe U can itself sometimes qualify as the background theory in which to describe its own ontological reduction. But we cannot do better than that; we cannot declare our new ontological economies without having recourse to the uneconomical old ontology.

This sounds, perhaps, like a predicament: as if no ontological economy is justifiable unless it is a false economy and the repudiated objects really exist after all. But actually this is wrong; there is no more cause for worry here than there is in *reductio ad absurdum,* where we assume a falsehood that we are out to disprove. If what we want to show is that the universe U is excessive and that only a part exists, or need exist, then we are quite within our rights to assume all of U for the space of the argument. We show thereby that if all of U were needed then not all of U would be needed; and so our ontological reduction is sealed by *reductio ad absurdum.*

Toward further appreciating the bearing of ontological relativity on programs of ontological reduction, it is worth while to reexamine the philosophical bearing of the Löwenheim-Skolem

[13] Kurt Gödel, *The Consistency of the Continuum Hypothesis* (Princeton, N.J.: The University Press, 1940), p. 11.

theorem. I shall use the strong early form of the theorem,[14] which depends on the axiom of choice. It says that if a theory is true and has an indenumerable universe, then all but a denumerable part of that universe is dead wood, in the sense that it can be dropped from the range of the variables without falsifying any sentences.

On the face of it, this theorem declares a reduction of all acceptable theories to denumerable ontologies. Moreover, a denumerable ontology is reducible in turn to an ontology specifically of natural numbers, simply by taking the enumeration as the proxy function, if the enumeration is explicitly at hand. And even if it is not at hand, it exists; thus we can still think of all our objects as natural numbers, and merely reconcile ourselves to not always knowing, numerically, which number an otherwise given object is. May we not thus settle for an all-purpose Pythagorean ontology outright?

Suppose, afterward, someone were to offer us what would formerly have qualified as an ontological reduction—a way of dispensing in future theory with all things of a certain sort S, but still leaving an infinite universe. Now in the new Pythagorean setting his discovery would still retain its essential content, though relinquishing the form of an ontological reduction; it would take the form merely of a move whereby some numerically unspecified numbers were divested of some property of numbers that corresponded to S.

Blanket Pythagoreanism on these terms is unattractive, for it

[14] Thoralf Skolem, "Logisch-kombinatorische Untersuchungen über die Erfüllbarkeit oder Beweisbarkeit mathematischer Sätze nebst einem Theorem über dichte Mengen," *Skrifter utgit av Videnskapsselskapet i Kristiania,* 1919. 37 pp. Translation in Jean van Heijenoort, ed., *From Frege to Gödel: Source Book in the History of Mathematical Logic* (Cambridge, Mass.: Harvard, 1967), pp. 252–263.

merely offers new and obscurer accounts of old moves and old problems. On this score again, then, the relativistic proposition seems reasonable: that there is no absolute sense in speaking of the ontology of a theory. It very creditably brands this Pythagoreanism itself as meaningless. For there is no absolute sense in saying that all the objects of a theory are numbers, or that they are sets, or bodies, or something else; this makes no sense unless relative to some background theory. The relevant predicates—"number," "set," "body," or whatever—would be distinguished from *one another* in the background theory by the roles they play in the laws of that theory.

Elsewhere [11] I urged in answer to such Pythagoreanism that we have no ontological reduction in an interesting sense unless we can specify a proxy function. Now where does the strong Löwenheim-Skolem theorem leave us in this regard? If the background theory assumes the axiom of choice and even provides a notation for a general selector operator, can we in these terms perhaps specify an actual proxy function embodying the Löwenheim-Skolem argument?

The theorem is that all but a denumerable part of an ontology can be dropped and not be missed. One could imagine that the proof proceeds by partitioning the universe into denumerably many equivalence classes of indiscriminable objects, such that all but one member of each equivalence class can be dropped as superfluous; and one would then guess that where the axiom of choice enters the proof is in picking a survivor from each equivalence class. If this were so, then with help of Hilbert's selector notation we could indeed express a proxy function. But in fact the Löwenheim-Skolem proof has another structure. I see in the proof even of the strong Löwenheim-Skolem theorem no reason to suppose that a proxy function can be formulated anywhere that will map an indenumerable ontology, say the real numbers, into a denumerable one.

On the face of it, of course, such a proxy function is out of the question. It would have to be one-to-one, as we saw, to provide distinct images of distinct real numbers; and a one-to-one mapping of an indenumerable domain into a denumerable one is a contradiction. In particular it is easy to show in the Zermelo-Fraenkel system of set theory that such a function would neither exist nor admit even of formulation as a virtual class in the notation of the system.

The discussion of the ontology of a theory can make variously stringent demands upon the background theory in which the discussion is couched. The stringency of these demands varies with what is being said about the ontology of the object theory. We are now in a position to distinguish three such grades of stringency.

The least stringent demand is made when, with no view to reduction, we merely explain what things a theory is about, or what things its terms denote. This amounts to showing how to translate part or all of the object theory into the background theory. It is a matter really of showing how we *propose*, with some arbitrariness, to relate terms of the object theory to terms of the background theory; for we have the inscrutability of reference to allow for. But there is here no requirement that the background theory have a wider universe or a stronger vocabulary than the object theory. The theories could even be identical; this is the case when some terms are clarified by definition on the basis of other terms of the same language.

A more stringent demand was observed in the case where a proxy function is used to reduce an ontology. In this case the background theory needed the unreduced universe. But we saw, by considerations akin to *reductio ad absurdum*, that there was little here to regret.

The third grade of stringency has emerged now in the kind of ontological reduction hinted at by the Löwenheim-Skolem theorem. If a theory has by its own account an indenumerable universe, then even by taking that whole unreduced theory as background theory we cannot hope to produce a proxy function that would be adequate to reducing the ontology to a denumerable one. To find such a proxy function, even just a virtual one, we would need a background theory essentially stronger than the theory we were trying to reduce. This demand cannot, like the second grade of stringency above, be accepted in the spirit of *reductio ad absurdum*. It is a demand that simply discourages any general argument for Pythagoreanism from the Löwenheim-Skolem theorem.

A place where we see a more trivial side of ontological relativity is in the case of a finite universe of named objects. Here there is no occasion for quantification, except as an inessential abbreviation; for we can expand quantifications into finite conjunctions and alternations. Variables thus disappear, and with them the question of a universe of values of variables. And the very distinction between names and other signs lapses in turn, since the mark of a name is its admissibility in positions of variables. Ontology thus is emphatically meaningless for a finite theory of named objects, considered in and of itself. Yet we are now talking meaningfully of such finite ontologies. We are able to do so precisely because we are talking, however vaguely and implicitly, within a broader containing theory. What the objects of the finite theory are, makes sense only as a statement of the background theory in its own referential idiom. The answer to the question depends on the background theory, the finite foreground theory, and, of course, the particular manner in which we choose to translate or embed the one in the other.

Ontology is internally indifferent also, I think, to any theory that is complete and decidable. Where we can always settle truth values mechanically, there is no evident internal reason for interest in the theory of quantifiers nor, therefore, in values of variables. These matters take on significance only as we think of the decidable theory as embedded in a richer background theory in which the variables and their values are serious business.

Ontology may also be said to be internally indifferent even to a theory that is not decidable and does not have a finite universe, if it happens still that each of the infinitely numerous objects of the theory has a name. We can no longer expand quantifications into conjunctions and alternations, barring infinitely long expressions. We can, however, revise our semantical account of the truth conditions of quantification, in such a way as to turn our backs on questions of reference. We can explain universal quantifications as true when true under all substitutions; and correspondingly for existential. Such is the course that has been favored by Leśniewski and by Ruth Marcus.[15] Its nonreferential orientation is seen in the fact that it makes no essential use of namehood. That is, additional quantifications could be explained whose variables are placeholders for words of any syntactical category. *Substitutional* quantification, as I call it, thus brings no way of distinguishing

[15] Ruth B. Marcus, "Modalities and intensional languages," *Synthese* 13 (1961), 303–322. I cannot locate an adequate statement of Stanisław Leśniewski's philosophy of quantification in his writings; I have it from his conversations. E. C. Luschei, in *The Logical Systems of Leśniewski* (Amsterdam: North-Holland, 1962), pp. 108 f, confirms my attribution but still cites no passage. On this version of quantification see further "Existence and Quantification," in this volume.

names from other vocabulary, nor any way of distinguishing between genuinely referential or value-taking variables and other place-holders. Ontology is thus meaningless for a theory whose only quantification is substitutionally construed; meaningless, that is, insofar as the theory is considered in and of itself. The question of its ontology makes sense only relative to some translation of the theory into a background theory in which we use referential quantification. The answer depends on both theories and, again, on the chosen way of translating the one into the other.

A final touch of relativity can in some cases cap this, when we try to distinguish between substitutional and referential quantification. Suppose again a theory with an infinite lot of names, and suppose that, by Gödel numbering or otherwise, we are treating of the theory's notations and proofs within the terms of the theory. If we succeed in showing that every result of substituting a name for the variable in a certain open sentence is true in the theory, but at the same time we disprove the universal quantification of the sentence,[16] then certainly we have shown that the universe of the theory contained some nameless objects. This is a case where an absolute decision can be reached in favor of referential quantification and against substitutional quantification, without ever retreating to a background theory.

But consider now the opposite situation, where there is no such open sentence. Imagine on the contrary that, whenever an open sentence is such that each result of substituting a name in it can be proved, its universal quantification can be

[16] Such is the typical way of a numerically insegregative system, misleadingly called "ω-inconsistent." See my *Selected Logic Papers* (New York: Random House, 1966), pp. 118 f, or *Journal of Symbolic Logic*, 1953, pp. 122 f.

proved in the theory too. Under these circumstances we can construe the universe as devoid of nameless objects and hence reconstrue the quantifications as substitutional, but we need not. We could still construe the universe as containing nameless objects. It could just happen that the nameless ones are *inseparable* from the named ones, in this sense: it could happen that all properties of nameless objects that we can express in the notation of the theory are shared by named objects.

We could construe the universe of the theory as containing, e.g., all real numbers. Some of them are nameless, since the real numbers are indenumerable while the names are denumerable. But it could still happen that the nameless reals are inseparable from the named reals. This would leave us unable within the theory to prove a distinction between referential and substitutional quantification.[17] Every expressible quantification that is true when referentially construed remains true when substitutionally construed, and vice versa.

We might still make the distinction from the vantage point of a background theory. In it we might specify some real number that was nameless in the object theory; for there are always ways of strengthening a theory so as to name more real numbers, though never all. Further, in the background theory, we might construe the universe of the object theory as exhausting the real numbers. In the background theory we could, in this way, clinch the quantifications in the object theory as referential. But this clinching is doubly relative: it is relative to the background theory and to the interpretation or translation imposed on the object theory from within the background theory.

One might hope that this recourse to a background theory

[17] This possibility was suggested by Saul Kripke.

could often be avoided, even when the nameless reals are inseparable from the named reals in the object theory. One might hope by indirect means to show within the object theory that there are nameless reals. For we might prove within the object theory that the reals are indenumerable and that the names are denumerable and hence that there is no function whose arguments are names and whose values exhaust the real numbers. Since the relation of real numbers to their names would be such a function if each real number had a name, we would seem to have proved within the object theory itself that there are nameless reals and hence that quantification must be taken referentially.

However, this is wrong; there is a loophole. This reasoning would prove only that a relation of all real numbers to their names cannot exist as an entity in the universe of the theory. This reasoning denies no number a name in the notation of the theory, as long as the name relation does not belong to the universe of the theory. And anyway we should know better than to expect such a relation, for it is what causes Berry's and Richard's and related paradoxes.

Some theories can attest to their own nameless objects and so claim referential quantification on their own; other theories have to look to background theories for this service. We saw how a theory might attest to its own nameless objects, namely, by showing that some open sentence became true under all constant substitutions but false under universal quantification. Perhaps this is the only way a theory can claim referential import for its own quantifications. Perhaps, when the nameless objects happen to be inseparable from the named, the quantification used in a theory cannot meaningfully be declared referential except through the medium of a background theory. Yet referential quantification is the key idiom of ontology.

Thus ontology can be multiply relative, multiply meaningless apart from a background theory. Besides being unable to say in absolute terms just what the objects are, we are sometimes unable even to distinguish objectively between referential quantification and a substitutional counterfeit. When we do relativize these matters to a background theory, moreover, the relativization itself has two components: relativity to the choice of background theory and relativity to the choice of how to translate the object theory into the background theory. As for the ontology in turn of the background theory, and even the referentiality of its quantification—these matters can call for a background theory in turn.

There is not always a genuine regress. We saw that, if we are merely clarifying the range of the variables of a theory or the denotations of its terms, and are taking the referentiality of quantification itself for granted, we can commonly use the object theory itself as background theory. We found that when we undertake an ontological reduction, we must accept at least the unreduced theory in order to cite the proxy function; but this we were able cheerfully to accept in the spirit of *reductio ad absurdum* arguments. And now in the end we have found further that if we care to question quantification itself, and settle whether it imports a universe of discourse or turns merely on substitution at the linguistic level, we in some cases have genuinely to regress to a background language endowed with additional resources. We seem to have to do this unless the nameless objects are separable from the named in the object theory.

Regress in ontology is reminiscent of the now familiar regress in the semantics of truth and kindred notions—satisfaction, naming. We know from Tarski's work how the semantics, in this sense, of a theory regularly demands an in some way more inclusive theory. This similarity should perhaps not surprise us, since both ontology and satisfaction are matters of reference. In their elusiveness, at any rate—in their emptiness now and again except relative to a broader background—both truth and ontology may in a suddenly rather clear and even tolerant sense be said to belong to transcendental metaphysics.[18]

Note added in proof. Besides such ontological reduction as is provided by proxy functions (cf. pp. 55–60), there is that which consists simply in dropping objects whose absence will not falsify any truths expressible in the notation. Commonly this sort of deflation can be managed by proxy functions, but R. E. Grandy has shown me that sometimes it cannot. Let us by all means recognize it then as a further kind of reduction. In the background language we must, of course, be able to say what class of objects is dropped, just as in other cases we had to be able to specify the proxy function. This requirement seems sufficient still to stem any resurgence of Pythagoreanism on the strength of the Löwenheim-Skolem theorem.

[18] In developing these thoughts I have been helped by discussions with Saul Kripke, Thomas Nagel, and especially Burton Dreben.

3

Epistemology Naturalized

Epistemology is concerned with the foundations of science. Conceived thus broadly, epistemology includes the study of the foundations of mathematics as one of its departments. Specialists at the turn of the century thought that their efforts in this particular department were achieving notable success: mathematics seemed to reduce altogether to logic. In a more recent perspective this reduction is seen to be better describable as a reduction to logic and set theory. This correction is a disappointment epistemologically, since the firmness and obviousness that we associate with logic cannot be claimed for set theory. But still the success achieved in the foundations of mathematics remains exemplary by comparative standards, and we can illuminate the rest of epistemology somewhat by drawing parallels to this department.

Studies in the foundations of mathematics divide symmetrically into two sorts, conceptual and doctrinal. The conceptual studies are concerned with meaning, the doctrinal with truth. The conceptual studies are concerned with clarifying concepts by defining them, some in terms of others. The doctrinal studies are concerned with establishing laws by proving them,