W. V. QUINE

REPLIES

TO SMART

In the first half of his paper Smart describes my position clearly, correctly, and approvingly. It is a pleasure to be thus understood and agreed with.

A misunderstanding seems to emerge at the middle of his paper, where he finds me ambivalent on the paradigm-case argument. In fact my attitude toward the paradigm case is univalent but intermediate. What I meant in the misunderstood pages (*W. & O.*, pp. 3f.) was that the paradigm case is not a permanent stopping place, but a point of departure. The expressions 'real', 'exist', 'there is', first come to make sense to us through our observing their commonest uses. So do pronouns, the prototypes of bound variables. The paradigmatic objects of reference of all these devices are, I suggest, visible, tangible bodies. If certain speakers have learned these expressions only from such applications, and then someone proceeds forthwith to deny the reality or existence of bodies, those speakers will find the denial puzzling or absurd. Someone can, on the other hand, intelligibly shift his attributions of existence a little at a time. First he adds some bodies which are invisible and intangible only because absent; then some more which are invisible and intangible only because we are not sensitive enough. At length a systematic usage of the existential idioms thus develops which we find manageable by dint more of system and analogy than of visibility and tangibility. When we have reached that point, we can begin even to understand the denial of existence of visible, tangible things. We understand it as a systematic extrusion of such objects from the range of reference of pronouns, or of values of variables, in some proposed regimentation of scientific theory.

In the beginning we needed the paradigmatic bodies, in order to begin to get the knack of the pronouns themselves and of kindred terms and devices. Paradigm cases confer intelligibility, but continuity of change suffices to preserve intelligibility. Paradigm cases launch our ship, but afterward, in Neurath's figure, we can stay afloat while we rebuild it plank by plank.

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Apropos of the example 'demonic possession', Smart speaks of "theory-laden" expressions and goes on to suggest that existence in a theory-laden sense could intelligibly be denied of visible, tangible objects. This is very much my view. In a superstitious community one could first learn 'demonic possession' as an observation term, by simple holophrastic conditioning to epileptic fits as paradigm cases. Later, learning an articulate theory about demons and possession, and learning that the theory is false, one could warp the term away from the paradigm cases that originally gave it what meaning it had for him. So it is with 'there is'. Growing up in a community of believers in stones and rabbits, we first learn 'there is' in connection with stony and rabbity sorts of stimulation. Eventually, after mastering the logic of quantifiers and identity or their vernacular equivalents, we invest 'there is' with a theoretical quality and are prepared, in an extremity, to warp it away from its paradigm cases. This is why I have urged the inscrutability of reference[1]; existence in its final estate is theoretical. For convenient communication between persons with unlike ontologies there arises, even, a double usage: the sophisticate who has dismissed rabbits or perhaps numbers as values of variables will still assent to 'There are rabbits' and 'There are large prime numbers' holophrastically, while reserving the right to paraphrase if anyone wants to make ontological capital of the internal constitution of these sentences.[2]

Smart contrasts my "pragmatism and instrumentalism" in *From a Logical Point of View* with the dominant realism of *Word and Object*. Also he senses traces of the earlier attitude lingering in *Word and Object*. Now this appearance of vacillation is a misunderstanding, and one which I was trying to ward off when I wrote this (*W.&O.*, p. 22):

> To call a posit a posit is not to patronize it.... Everything to which we concede existence is a posit from the standpoint of a description of the theory-building process and simultaneously real from the standpoint of the theory that is being built. Nor let us look down on the standpoint of the theory as make-believe, for we can never do better than occupy the standpoint of some theory or other, the best we can muster at the time.

The key consideration is rejection of the ideal of a first philosophy, somehow prior to science. Epistemology, for me, is only science self-applied; Smart describes my view of it very well at the end of his § 2. Science tells us that our data regarding the external world are limited to the irritations of our bodily surfaces, and then science asks how it is that people manage from those data to project their story about the external world – true though the story is. 'Posit' is a term to this methodological facet of science. To apply the term proper to molecules and wombats is not to deny that these are real; but declaring them real is left to other facets of science, namely physics and zoology.

REFERENCES

[1] *W.&O.*, p. 54. See also 'Ontological Relativity', *Journal of Philosophy* **65** (1968) 185–212.
[2] See my 'Existence and Quantification', in *Fact and Existence* (ed. by J. Margolis), Blackwell, Oxford, 1968 (at press).

REPLIES

TO HARMAN

In 'Two Dogmas of Empiricism' I reflected that interchangeability *salva veritate* is a sufficient condition for synonymy if the language contains, besides standard equipment, a necessity operator that is fulfilled by just the analytic sentences. But I added that a definition of synonymy thus based would offer small comfort, being "not flatly circular, but something like it. It has the form, figuratively speaking, of a closed curve in space".

Kirk recently made an analogous point about translation: there is no indeterminacy if the home language is well equipped for indirect quotation.[1] For my indeterminacy thesis was that two translators could disagree on a translation and still agree in all speech dispositions, in both languages, except translation. Kirk's reflection, to the contrary, is that the conflicting translations would entail conflicting speech dispositions also within the home language, at the level of indirect quotation. This reflection brings as little comfort, regarding determinacy of translation, as my previous reflection brought regarding definability of synonymy. Both situations involve the same quasi-circularity.

I grant Kirk his critical point: the phrase "except translation" in my statement of indeterminacy of translation needs to be elaborated so as to except also indirect quotation and related idioms of propositional attitude. All these devices reflect interlinguistic correlations intralinguistically.

Niceties of formulation aside, however, Kirk's observation can be seen as challenging not the indeterminacy of translation but the determinacy of indirect quotation. Harman makes the point: we can apply indirect quotation and other idioms of propositional attitude to foreign speakers only subject to the same parameter that underlies translation itself, namely, the choice of a scheme of translation.

Harman alluded to this point already earlier.[2] In the present essay he rounds it out into this equation: the doctrine of indeterminacy of translation is equivalent to saying that the so-called propositional attitudes must be seen as not really propositional but sentential attitudes. For, if behind the sentences there were linguistically neutral propositions, translation would of course be determinate. Conversely, if translation were determinate, we could reasonably posit the propositions; we could even define them, somewhat arbitrarily, as the equivalence classes of intertranslatable sentences.

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Harman ably defends my indeterminacy thesis. By way of illustrating the point he makes good use of the contrasting explications of number in set theory. The limitation of this illustration is an interesting point too, and a point made by Harman: the sentences about numbers that take on opposite truth values under different explications are sentences that have no clear truth values before explication.

It is a strength of Harman's defense of the indeterminacy thesis that he shows, in the course of it, a tolerant concern for the status of mental entities. His schema for psychology gives beliefs and other mental states the status of hypothetical states of the nervous system. This is just the sort of status I think they should have. To take an easy example, acceptance of a sentence is for me, as Harman remarks, the disposition to assent to it; and for me a disposition, in turn, is a hypothetical state of the internal mechanism.

I am not sure whether my agreement with Harman over mental entities suffices to clear me of the suspicion of philosophical behaviorism, nor whether I want to be cleared. I am not sure what philosophical behaviorism involves, but I do consider myself as behavioristic as anyone in his right mind could be. Writers have sometimes used the word 'behaviorism' pejoratively to denote some doctrine too absurd to admit to; and perhaps the qualifier 'philosophical' serves to identify that usage. But in that sense nobody is a philosophical behaviorist.

Harman quotes from the third paragraph of Chapter II of *Word and Object* with just disapproval. My intuitive idea was that a permutation of the sentence meanings could go forever unreflected in dispositions to use or respond to the sentences. I wanted to say something to that effect without, of course, positing sentence meanings; and I see now that I failed. Harman's suggested remedy is simple and seems adequate: I should appeal at this point not to dispositions to use the sentences in question, but only to dispositions to assent to them or dissent from them.

That passage was lame also in another respect, and of this I was aware at the time: the appeal to "any plausible sense of equivalence however loose". Substantially the same phrase recurs in statements of the indeterminacy of translation where I say that the two translators assign to the jungle sentence English translations that are not equivalent English sentences in any plausible sense of equivalence however loose. I disliked having to appeal thus to equivalence, however apologetically, in the very formulation of a thesis that casts doubt on notions of translation or synonymy or equivalence. But I did better on pages 73f, in a passage that Harman also quotes:

> ... rival systems of analytical hypotheses can conform to all speech dispositions within each of the languages concerned and yet dictate ... translations each of which would be excluded by the other system.

Here there is no appeal to equivalence. As Harman recently put it to me in conversation, it is just that the one translator would reject the other's translation. I have already put this statement of the matter to good use in the second paragraph of the present reply to Harman. As noted in that connection, the formulation does still need hedging against indirect quotation and related idioms. I do not see that it needs also an emendation that Harman suggests, namely, "dispositions to accept sentences" in place of "speech dispositions", though we saw that such an emendation was invaluable elsewhere.

REFERENCES

[1] Robert Kirk, 'Translation and Indeterminacy', *Mind* (forthcoming).

[2] Gilbert Harman, 'Quine on Meaning and Existence', *Review of Metaphysics* **21** (1967) 124–151, 343–367, specifically pp. 142ff.

W. V. QUINE

TO STENIUS

Of his feeling that the things I say are more or less inconsistent with one another, Stenius says that it may be founded on misunderstandings. This proves to be the case. Misunderstanding begins with his reading of the first sentence of *Word and Object*: "This familiar desk manifests its presence by resisting my pressures and by deflecting light to my eyes." "He starts", writes Stenius, "with the familiar Russellian desk and the sense data we get of it."

Having thus willed a sense-datum ontology into my physicalistic opening sentence, Stenius is bound to find things "more or less inconsistent" within my same opening paragraph:

> Our common-sense talk of physical things goes forward without benefit of explanations in more intimately sensory terms. Entification begins at arm's length.... The things in sharpest focus are the things that are public enough to be talked of publicly.... It is to these that words apply first and foremost.

The most explicit writing is not proof against stalwart reading; at most it creates a sense of strain, expressed in such comments as "Quine is immediately aware of a difficulty in his outlook" or again "he explains the deviation as the effect of a special 'objective pull'".

Stenius's leading principle, that I posit sense data along with the stimulations, accounts for his notion that I am close epistemologically to Russell. It accounts also for his heading 'In the Beginning was Subjectivity'. The fact is rather that I give linguistic and conceptual primacy to ordinary things, not only on page 1 but steadfastly. The burden that Russell placed on sense data, I place on neural input – adopting thus a black-box model with no awareness presumptions. I am able to take this stance because of my naturalism, my repudiation of any first philosophy logically prior to science. My affinity here is not to Russell but to Neurath. See my adjoining reply to Smart.

Stenius's leading principle aforementioned causes a misunderstanding of what I mean by 'simpler to learn'. The sphere is *simpler* to learn than the objective square, not in being *easier* or less effortful, but as involving less processing of information: all its retinal projections are geometrically similar. What I am comparing in respect of simplicity are unconscious processings of information in the black box which is the human nervous system. And note that 'similar' here is a technical term of geometry.

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The gentle but persistent patter of my physicalism does have this much effect: what Stenius had seen as my phenomenal world of sense data, he comes at length to see rather as "a world of surface irritations". "Of course," he concedes, "Quine does not believe that our language really refers to surface stimulations." Of course not; but why the "really"?

I wonder if Stenius was confusing meaning with reference. I use stimulations in meanings – and in meanings primarily of observation sentences. Reference, even on the part of observation terms, is in my view theory-enveloped and thus subject to the indeterminacy of translation. Middle-sized bodies are objects of reference *par excellence*, as urged in the quotation from p. 1. Surface stimulations are seldom referred to except by psychologists, dermatologists, and an occasional philosopher of language.

Misunderstanding comes also of supposing that I intend a one-to-one correspondence between stimulations and meanings. This is behind his objection that "surface irritations are often not internally observable". It was to ward off this misunderstanding that I wrote the paragraph which began:

> In taking the visual stimulations as irradiation patterns we invest them with a fineness of detail beyond anything that our linguist can be called upon to check for. But this is all right. He can reasonably conjecture... (*W.&O.*, p. 31).

On the other hand Stenius's accompanying objection that "surface irritations are not socially observable in any relevant sense" is a point on which I disagree, as witness that same page:

> We are after his socially inculcated linguistic usage, hence his responses to conditions normally subject to social assessment. Ocular irradiation *is* intersubjectively checked to some degree by society and linguist alike, by making allowances for the speaker's orientation and the relative disposition of objects.

Stenius suggests that the linguist should learn the jungle language from within, and not by translation into English. This suggests to me that he reads Chapter II of *Word and Object* as instructions for field linguists – a sufficiently embarrassing misinterpretation to make me wish I had italicized p. 27, which tells my purpose.

Quoting me thus:

> A rabbit scurries by, the native says 'Gavagai' and the linguist notes down the sentence 'Rabbit' as a tentative translation, subject to testing,

Stenius goes on to say that this would be an amazingly good guess. I take issue; it would be an easy guess. It *could* be wrong; hence the further testing. What is worrying Stenius here may be traceable in part to a failure to distinguish between the theoretical or definitional role of stimulus meaning and the linguist's method of discovery. See then my adjoining reply to Hintikka.

In questioning why I call 'Gavagai' a sentence, Stenius overlooks the fact that I speak both of a sentence 'Gavagai' and of a term 'gavagai', and that the latter is enmeshed in the problem of indeterminacy of translation. Evidence for this oversight mounts when, proceeding to the analogical construction of composite sentences, he states some reasonable points which he thinks are at variance with my views. The trouble seems to be that he thinks that I think that 'hurts' and 'my foot' are always sentences and never terms.

Stenius makes a plea for facts, as what make sentences true or false. He seems to agree with me in not wanting to quantify over them, and yet he feels that he is for facts in some sense in which I am against them. "What would be the inconvenience arising from speaking about facts?" If variables are not in point, this issue is not clear.

Of my strictures on intension he writes: "Quine seems to be rather unhesitant about this. To me it seems to be a kind of prejudice." On the contrary, my hesitancy rivaled Hamlet's in its ostentation. I used intensions explicitly in §§ 34, 35, 38, 41, and 42 of *Word and Object*, and introduced special symbols to depict them. When at last I repudiated them in § 43, it was for two strong and explicit reasons unrelated to prejudice. One reason was obscurity of individuation – a point which is bound up with my critique of analyticity and with my doctrine of the indeterminacy of translation. The other reason was referential opacity.

Stenius defends intensions by citing psychological observations on the apprehension of qualities. But such observations have no obvious bearing on the question whether intensional objects, conceived in some sense that would be inimical to extensional substitutivity, should be admitted as values of bound variables. I think it is clear that these, only these, are what I so hesitantly ended up by repudiating in § 43 under the head of intensions.

Of the elimination of names in favor of predicates and bound variables, Stenius writes, "I dispute its claim to be of essential importance for the understanding of how language works." I think I would join him in disputing such a claim, if I were to encounter it. At any rate I see the elimination as independent of one's feeling for English. I make the step for stated reasons and only in the regimentation phase of *Word and Object,* where the identificatory force of singular terms has already lapsed along with the truth-value gaps. See Strawson's paper, adjoining, and the early part of my reply to it. In connection with what Stenius says about singular descriptions in modal contexts see also my reply to Sellars.

W. V. QUINE

TO CHOMSKY

Chomsky's remarks leave me with feelings at once of reassurance and frustration. What I find reassuring is that he nowhere clearly disagrees with my position. What I find frustrating is that he expresses much disagreement with what he thinks to be my position.

1. *Indeterminacy of Translation*

I have stressed, he notes, a contrast between ordinary inductive uncertainty, such as attaches to the identifying of stimulus meanings, and the deeper matter which is indeterminacy of translation. He explains the contrast thus:

> Quine has in mind a distinction between "normal induction" ... and "hypothesis formation" or "theory construction" ... What distinguishes the case of physics from the case of language is that we are, for some reason, not permitted to have a "tentative theory" in the case of language (except for the "normal inductive cases" mentioned above).

This misinterpretation of my position was already familiar to me, in the classroom and in discussions with colleagues, before *Word and Object* went to press. Consequently I took special precautions against it, in *Word and Object*. It was in order to obviate this misunderstanding that I wrote the paragraph (pp. 75f.) which began:

> May we conclude that translational synonymy at its worst is no worse off than physics? To be thus reassured is to misjudge the parallel.

Yet I cannot charge Chomsky with overlooking this precautionary paragraph of mine. On the contrary, he quoted the rest of it almost in full in the middle of that very paragraph of his own which I dolefully excerpted above. So I must face the fact that the point of my paragraph escaped him, and that it will have escaped others. Let me try again.

In respect of being under-determined by all possible data, translational synonymy and theoretical physics are indeed alike. The totality of possible observations of nature, made and unmade, is compatible with physical theories that are incompatible with one another. Correspondingly the totality of possible observations of verbal behavior, made and unmade, is compatible with systems of analytical hypotheses of translation that are

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incompatible with one another. Thus far the parallel holds. If you ask a physicist a theoretical question, well out beyond the observation sentences, his answer will be predicated on his theory and not on some unknown and incompatible theory which would have fitted all possible data just as well. Again the parallel holds: if you ask a linguist 'What did the native say?', where the native's remark was far from the category of observation sentences, the linguist's answer will be predicated on his manual of translation and not on some unknown and incompatible manual which would have fitted all possible linguistic behavior just as well. Where then does the parallel fail?

Essentially in this: theory in physics is an ultimate parameter. There is no legitimate first philosophy, higher or firmer than physics, to which to appeal over physicists' heads. Even our appreciation of the partial arbitrariness or under-determination of our overall theory of nature is not a higher-level intuition; it is integral to our under-determined theory of nature itself, and of ourselves as natural objects. So we go on reasoning and affirming as best we can within our ever under-determined and evolving theory of nature, the best one that we can muster at any one time; and it is usually redundant to cite the theory as parameter of our assertions, since no higher standard offers. It ceases to be redundant only when we are contrasting alternative theories at a deep level, e.g. with a view to a change.

Though linguistics is of course a part of the theory of nature, the indeterminacy of translation is not just inherited as a special case of the under-determination of our theory of nature. It is parallel but additional. Thus, adopt for now my fully realistic attitude toward electrons and muons and curved space-time, thus falling in with the current theory of the world despite knowing that it is in principle methodologically under-determined. Consider, from this realistic point of view, the totality of truths of nature, known and unknown, observable and unobservable, past and future. The point about indeterminacy of translation is that it withstands even all this truth, the whole truth about nature. This is what I mean by saying that, where indeterminacy of translation applies, there is no real question of right choice; there is no fact of the matter even to *within* the acknowledged under-determination of a theory of nature.

When someone asks the linguist 'What did the native say?', he thinks the question has a right English answer which is unique up to equivalence transformations of English sentences. He expects this even when the native's remark was far from the category of observation sentences. He expects this insofar as we agree, with him, to neglect the omnipresent under-determination of natural knowledge generally. But in this expectation, even as hedged by this last proviso, he is mistaken.

An unconvincing rebuttal is that everybody who is anybody knows better than to expect even this much factuality of translation. Chomsky hints such a rebuttal in his final sentence: "But why should all of this occasion any surprise or concern?"

Translation is fine and should go on. "All of this" occasions no crisis in linguistics such as the antinomies occasioned in set theory. What "all of this" does occasion, if grasped, is a change in prevalent attitudes toward meaning, idea, proposition. And in the main the sad fact is, conversely, that "all of this" escapes recognition precisely because of the uncritical persistence of old notions of meaning, idea, proposition. A conviction persists, often unacknowledged, that our sentences express ideas, and express these ideas rather than those, even when behavioral criteria can never say which. There is the stubborn notion that we can tell intuitively which idea someone's sentence expresses, our sentence anyway, even when the intuition is irreducible to behavioral criteria. This is why one thinks that one's question 'What did the native say?' has a right answer independent of choices among mutually incompatible manuals of translation. In asking "But why should all of this occasion any surprise or concern?" Chomsky did not dismiss my point. He missed it.

2. *Learning Sentences*

The more absurd the doctrine attributed to someone, *caeteris paribus*, the less the likelihood that we have well construed his words. In *Word and Object* I urged this precept in connection with the notion of a pre-logical people and other examples, and I remarked that it applies not only in radical translation but also at home. I wish Chomsky had considered this precept before attributing to me the absurd belief that the sentences in a man's repertoire are finite in number and generally learned as wholes. For surely it is generally appreciated that generative grammar is what mainly distinguishes language from subhuman communication systems. In a 1951 essay from which Chomsky even quotes, moreover – the one in *From a Logical Point of View* – I had written:

Our grammarian's attempted recursive specification ... will follow the orthodox line, we may suppose, of listing 'morphemes' and describing constructions.

Then I had gone on with further particulars. Also in *Word and Object* there are such passages as "the infinite totality of sentences of any given speaker's language" (p. 27) which might have been expected to preclude Chomsky's strange attribution, though I had not sensed that any such safeguard could be needed. He even notices one such passage (p. 71) himself, but unaccountably refuses to be swerved by it from his systematic misinterpretations.

The nature of his misunderstanding is hinted here:

It ... is clear that when we learn a language we are not "learning sentences".... Rather, we somehow develop certain principles ... that determine the form and meaning of indefinitely many sentences. A description of knowledge of language ... as an associative net constructed by conditioned response is in sharp conflict with whatever evidence we have about these matters.

This sense of conflict is wrong. It comes of taking 'learning sentences' narrowly to mean 'learning sentences outright as unstructured wholes', and taking 'associative net' and 'conditioned response' to refer narrowly to the association of sentences with sentences as unstructured wholes. No wonder he writes "As far as 'learning of sentences' is concerned, the entire notion seems almost unintelligible"; at any rate he has not understood it, or he would have seen in it no conflict with the old familiar doctrine which he sets over against it in the quoted passage. Perhaps my phrases "learning of sentences" and "association of sentences" were obscure, but there were clarificatory passages that should have helped if noted. Thus, in a passage of *Word and Object* (p. 9) which he even cites, I speak of

our learning of sentences ... [in] two modes: (1) learning sentences as wholes by a direct conditioning of them to appropriate non-verbal stimulations, and (2) producing further sentences ... by analogical substitution.

I add that these *two* modes are only a beginning. A page later I write that

mode (2) above is already, in a way, an associating of sentences with sentences; but only in too restrained a way.

Such passages as these cannot be reconciled with the idea that I intended my phrases "learning of sentences" and "association of sentences" to relate to sentences only as unstructured wholes.

3. *Innate Ideas*

Chomsky rightly notes my *penchant* for innate ideas. Rightly, anyway, if we construe 'innate ideas' in terms of innate dispositions to overt behavior. As stressed in *Word and Object*, this *penchant* is one which I share with behaviorists generally. The contrary doctrine in Hobbes, Gassendi, and Locke hinges on the dominance in their day of the idea idea. In an idea-oriented empiricism, the empiricist's premium on external sense would be unfavorable to innate ideas. With Tooke and Bentham, however, there began the serious externalization of empiricism: the shift of focus from ideas, which are subjective, to language, which is an intersubjective and social institution. Language aptitude is innate; language learning, on the other hand, in which that aptitude is put to work, turns on intersubjectively observable features of human behavior and its environing circumstances, there being no innate language and no telepathy. The linguist has little choice but to be a behaviorist at least *qua* linguist; and, like any behaviorist, he is bound to lay great weight upon innate endowments.

There could be no induction, no habit formation, no conditioning, without prior dispositions on the subject's part to treat one stimulation as more nearly similar to a second than to a third. The subject's 'quality space', in this sense, can even be explored and plotted by behavioral tests in the differential conditioning and extinction of his responses. Also there are experimental ways of separating, to some degree, the innate features of his quality space from the acquired ones. I stressed all this in *Word and Object* (pp. 83f.), and cited old experiments (1923–37) by behavioral psychologists.

Chomsky says I "postulate a pre-linguistic (and presumably innate) 'quality space' with a built-in distance measure". But 'postulate' is an odd word for it, since a quality space is so obviously a prerequisite of learning, and since distances in a quality space can be compared experimentally.

He goes on rather as if my idea were nebulous or obscure.

> The handful of examples and references that Quine gives suggests that he has something much narrower in mind, however; perhaps, a restriction to dimensions which have some simple physical correlate such as hue or brightness, with distance defined in terms of these physical correlates.

In fact the denizens of the quality spaces are expressly stimulations

(p. 84), any and all, with no prior imposition of dimensions. Any irrelevant features of the stimulations will in principle disappear of themselves in the course of the experimental determination of the quality space. A little advance guessing of relevant dimensions could be handy in practice to economize on experiments, but this need not concern us. In principle the final dimensionality of someone's quality space, if wanted, would be settled only after all the simply ordinal comparisons of distance had been got by the differential conditioning and extinction tests. It would be settled by considerations of neatest accommodation – the sort of thing that Chomsky will have seen in his student days under Goodman. Thus, though Chomsky has a good deal to say about the want of remarks on my part with respect to dimensions of quality spaces, I see no evidence of a problem in this quarter.

In view of the alarming narrowness of the communication margin, I may do well to add here an explicit word of welcome toward any innate mechanisms of language aptitude, however elaborate, that Chomsky can make intelligible and plausible. Innate mechanism, after all, is the heart and sinew of behavior. See Putnam, on the other hand, for remarks on how hypothetical innate mechanism can prove wanting in intelligibility when specified in less scrupulously experimental terms than was the concept of quality space.[1]

4. *Arbitrariness disowned*

Referring to my "definition of 'language' as a 'complex of dispositions to verbal behavior'", Chomsky writes:

> Presumably, a complex of dispositions is a structure that can be represented as a set of probabilities for utterances in certain definable "circumstances" or "situations". But it must be recognized that the notion "probability of a sentence" is an entirely useless one .... On empirical grounds, the probability of my producing some given sentence of English ... is indistinguishable from the probability of my producing a given sentence of Japanese.

Later he writes:

> Actually, Quine avoids these problems, in his exposition, by shifting his ground from "totality of speech dispositions" to "stimulus meanings", that is, dispositions to "assent or dissent" in a situation determined by one ... arbitrarily selected experiment.

Why does he write "shifting his ground", since "dispositions to 'assent or dissent'" are surely within the "totality of speech dispositions"? I am free to pick, from that totality, whatever dispositions are most favorable to my purpose of distinguishing ostensive meanings. And, this being the case, why does he say "avoids these problems" and not "solves these problems"? The purported equiprobability of his producing a Japanese sentence bears none upon my "arbitrarily selected experiment" in which sentences are queried for native assent and dissent. I venture to suggest that this shows my selection of the experiment to have been less arbitrary than judicious.

Speaking of arbitrariness, I gave also another reason, in *Word and Object* (p. 29), why the linguist must resort thus to query and assent. Passive observation cannot give reasonable evidence even of stimulus meanings of observation sentences, because of an overlap problem.

The main trend of Chomsky's criticism has been to impute to me various hidden, narrow, and arbitrary empirical assumptions. I have tried in foregoing pages to explain how some of these imputations rest on misinterpretation. We find here a further instance of the same:

> It is ... not at all obvious that the potential concepts of ordinary language are characterizable in terms of simple physical dimensions of the kind Quine appears to presuppose. ... It is a question of fact whether the concept "house" is characterized ... as a "region" in a space of physical dimensions, or ... in terms of ... function. ... The same is true of many other concepts ... a knife....

Clearly this criticism is related to the remarks about dimensionality which I answered above at the end of § 3. But one sees also that the generality and studied neutrality of my method of stimulus classes has escaped Chomsky. The method is designed to capture all sensory input and all differences of sensory input, however irrelevant. Whether the stimulus meaning of a given observation sentence has a unifying theme of a spatial sort, or a chromatic sort, or a functional sort, or whatever, is not prejudged; in principle it would be determined afterward, if at all, by sizing up the discovered stimulus meaning. Of course all this is theoretical formulation. In practice we would direct and shortcut our inductions of stimulus meanings by guessing at the unifying theme in advance. And there is no reason not to guess a functional one; there is no bias toward spatial or chromatic traits.

5. *Theories*

So as to close on a more serious theme, I have left the first of Chomsky's criticisms to the end. He remarks my "tendency to use the terms 'language' and 'theory' interchangeably". This tendency is related to my rejection of the traditional distinction between analytic and synthetic statements; or, what comes to the same thing, the distinction between meaning and widely shared collateral information; or, what comes in the end to much the same thing again, the notion that the sentences of a theory have their several and separable empirical contents.

The term 'theory' has a technical sense, as in Tarski, which is not in point here. A set of sentences is a theory, in that sense, if and only if it consists of some subset S of sentences together with all the further sentences that are logically implied by S and do not exceed the vocabulary of S. This concept has its uses when, in proof theory or model theory, we work within a preassigned logical framework – ordinarily the apparatus of quantifiers and truth functions. But it has little evident bearing on general questions of translation and language learning, where we are given no specific logical apparatus nor even any distinction between logical apparatus and other apparatus.

In *Word and Object* and related writings my use of the term 'theory' is not technical. For these purposes a man's theory on a given subject may be conceived, nearly enough, as the class of all those sentences, within some limited vocabulary appropriate to the desired subject matter, that he believes to be true. Next we may picture a theory, more generally, as an imaginary man's theory, even if held by nobody. Theory in this intuitive and somewhat figurative sense is what lies behind Tarski's technical notion; the one goes over into the other when we allow the imaginary man full logical acumen.

One contrast which common sense makes between theory and language is that the same theory can be stated in different languages. I am setting no store by such a translation-invariant notion of theory, because of indeterminacy of translation. Even limiting our consideration to theory within a language, however, we see a contrast of a converse kind: many theories, even conflicting theories, can be couched in one language. Language settles the sentences and what they mean; a theory adds, selectively, the assertive quality or the simulation of selective belief. A language has its grammar and semantics; a theory goes farther and asserts some of the sentences.

But, common sense or anyway traditional philosophy goes on to say, some sentences also are fixed as true already by the semantics of the language, true by virtue purely of meanings, without help of any theory. These, the analytic sentences, could be said to comprise the null theory. Other theories differ from this null one in containing further sentences, and even perhaps in omitting some analytic ones through limitation of vocabulary.

Once I reject the distinction between analytic sentences and other community-wide beliefs, however, my nearest approximation to a null theory is the class of all community-wide beliefs. Still, even from my point of view, theory continues to contrast with language in that many theories are couched in one language. What then of my "tendency to use the terms 'language' and 'theory' interchangeably"? Clearly they are not interchangeable in all contexts, and they are pretty sure to be interchangeable in some. The contexts where Chomsky notices my interchangeable use of these terms are contexts where I speak of language or theory as a fabric or "network of sentences associated to one another and to external stimuli by the mechanism of conditioned response".[2] Such contexts are insensitive to a distinction between language and theory. Such, after all, is the semantic learning of language, once we get beyond observation sentences: we learn truth conditions of some sentences relative to other sentences. We learn thus to use the component words to form new sentences whose relative truth conditions are derivable. Which of these dependencies of truth value are due to meaning, or language, and which belong rather to a substantive theory that is widely shared, is in my view a wholly unclear question. It is no mere vagueness of terminology that makes language and theory indistinguishable in this connection.

Chomsky is right in protesting that I am "surely not proposing that two monolingual speakers cannot disagree on questions of belief". It is only when a belief is shared by the whole linguistic community that a distinction between language and theory runs into trouble.

Even at that point the effect of the distinction is not wholly to be despaired of. The useful effect of a distinction between matters of terminology and matters of fact can still be gained by talking of community-wide acceptance but manipulating the parameter, namely, community width. Thus take the case where, rather than charge someone with an altogether absurd belief, we conclude that his use of a crucial word differs from ours. This is, on the face of it, to conclude that our disagreement with him is verbal rather than factual. Still, our conclusion is no more than a trimming of our speech community to exclude our well-meaning but ill-spoken friend. The negation of the absurd sentence in question is made to count as a community-wide belief, by cutting the community down to size; and our friend's utterance counts then only as a foreign homophone of the absurd sentence. This is all very natural: we demarcate our practical speech community, for particular given purposes, as the community in which all dialogue that is concerned with those purposes runs smoothly and effectively.

One criterion for blaming a disagreement thus on aberrant usage, instead of aberrant belief, is that the tension of disagreement can be relieved by talking in other words. Another basis for imputing aberrant usage instead of aberrant belief is the psychology of learning, intuitively applied: a likelier cause of our friend's seemingly absurd assertion may be found in some phonetic or etymological mechanism of word-switching, say, than in any sufficiently gross misassessment of evidence relating to the subject matter of his sentence.

This same contrast between language and theory, or meaning and belief, dominates radical translation as soon as the linguist's field work reaches the point where he feels he can stop taking every native assertion as true. Instead of further complicating his growing system of analytical hypothesis of translation in order to make a surprising new native assertion come out true, he decides to call the statement false. In so doing he estimates, however undeliberately, which of two psychological processes is likelier to have happened. One is the process whereby this and other natives could have learned a language subject to the new hypothetical kink of syntax or lexicon by which the linguist might hope to accommodate the native's new assertion as true. The other is the process whereby the native might, through faulty observation or false hearsay, have erred about the subject matter of his assertion.

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[1] Hilary Putnam, 'The "Innateness Hypothesis" and Explanatory Models in Linguistics', *Synthese* **17** (1967) 12–22.

[2] When Chomsky finds "this factual assumption far from obvious", he is assuming that the mechanism of conditioned response has to apply simply to each of the innumerable sentences as an unstructured whole. I discussed this misunderstanding in § 2 of the present reply.

W. V. QUINE

TO HINTIKKA

Preparatory to discussing Hintikka's suggestions, let me clarify the intent of those pages of *Word and Object* where I considered the field linguist's initial situation and plausible first moves toward radical translation. I represented him as arriving early, if tentatively, at an identification of the native's ways of expressing assent and dissent. Hintikka suggests at several points that this identification might not be easy, and that we might "find a tribe which did not have any standard expression for assent and dissent". I agree, and can cite three such tribes: the Germans, the French, and the Japanese. 'Yes' goes into 'ja' and 'oui' after affirmative questions but into 'doch' and 'si' after negative questions; 'hai' goes into 'yes' after affirmative questions but into 'no' after negative questions.

There is no reason for the native's sign of assent not to be disjunctive – a 'ja, ja' here, a 'doch, doch' there – and no reason for it not to be elusive. I suggested bases for guessing. "However inconclusive these methods", I continued, "they generate a working hypothesis. If extraordinary difficulties attend all his subsequent steps, the linguist may decide to discard that hypothesis and guess again" (*W. & O.*, p. 30). The linguist's decision as to what to treat as native signs of assent and dissent is on a par with the analytical hypotheses of translation that he adopts at later stages of his enterprise; they differ from those later ones only in coming first, needed as they are in defining stimulus meaning. This initial indeterminacy, then, carries over into the identification of the stimulus meanings. In addition there is in the identification of stimulus meanings the normal uncertainty of induction, though, as stressed in my reply to Chomsky, this is not what the indeterminacy thesis is about. And finally there are the linguist's later adoptions of analytical hypotheses, undetermined still by what he takes to be the native's signs of assent and dissent, and undetermined still by all the stimulus meanings. As Dreben has well remarked, the indeterminacy of translation comes in degrees.

Thus I do not view the recognition of assent and dissent as different in kind from the subsequent higher-level translations, as if the one were firm and good and the other discredited. On the contrary, they are very much of a kind, and anyway I am in favor also of translation, even radical translation. I am concerned only to show what goes into it, and to what degree our behavioral data should be viewed as guides to a creative

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decision rather than to an awaiting reality. Hintikka seems to have misunderstood me here.

Also he over-estimates the role intended for stimulus meanings. These are quite special bundles of dispositions to verbal behavior, and are meant to reflect ostensive learning. Hintikka is wrong if he supposes that I want to ban behavioral cues that do not figure in stimulus meanings. The expressions of assent, dissent, and greeting are learned, he reminds us, from other behavioral cues. I have been stressing that the expressions of assent and dissent are not fully determined by behavior, and I would say the same of greeting; but still these remain good examples, since behavioral evidence does go into them, and it is not the same behavior that goes into stimulus meaning.

For that matter, even the linguist's evidence regarding a native observation sentence will rest on behavior other than what goes into stimulus meaning. The linguist sees the native looking toward a rabbit and shifting his gaze concomitantly with the rabbit's movement, and he hears him report 'Gavagai'. This behavior is evidence that the stimulus meaning of 'Gavagai' is that of 'Rabbit', but it is very different from the assent-dissent sort of behavior that defines stimulus meaning. Discovering where stimulus synonymy holds is one thing; defining what it is that one thus discovers is another.

My definition of stimulus meaning and stimulus synonymy was meant to individuate what can be learned in ostension. That is, though the ostensive learning of an observation sentence turns upon behavior that is not mentioned in my definition of stimulus meaning, I hold that the particularities of such behavior are indifferent to future usage of the observation sentence thus learned as long as the stimulus meaning stays the same. I was not advising linguists to adhere to assent-dissent tests in learning the jungle observation sentences. Without other behavioral cues they could not even guess what stimulations to test; and they could never test them all.

Various behavioral evidence other than what goes into stimulus meanings will give the linguist clues not only to stimulus meanings, but also to analytical hypotheses. According to my thesis of indeterminacy of translation, many alternative systems of analytical hypotheses will conform equally to all the facts of stimulus meaning and stimulus synonymy; but this does not mean that choice among these alternatives is impossible, nor capricious. Supplementary suggestions, helpful in pointing toward natural choices among alternatives, may well arise from observations of behavior, including perhaps rites and taboos. But I expect that all such further aids, if codified, would still leave a lot of slack, and also that the codification would itself come to look rather arbitrary along the outer edge. Above all, in such a codification of available behavioral aids, every care would need to be taken not to relax behavioristic standards and inadvertently admit any intuitive semantics.

Enough of generalities. I turn now to Hintikka's specific proposal. He proposes a language game as a behavioral criterion for translating quantification. But his game hinges on substitution instances and so is insensitive to the difference between substitutional quantification and objectual quantification.

The difference is that in the substitutional sense an existential quantification is true only if there is a specifiable object fulfilling the given open sentence, whereas in the objectual sense it is true so long as there is any object at all fulfilling the open sentence; and correspondingly for universal quantification. The difference is a real one whenever, as for instance in a theory of real numbers, there are objects in the universe of discourse that are not individually specifiable in the language.

Hintikka wants determinacy of translation for quantification in order to make interlinguistic sense of ontology. But, as I have argued elsewhere, substitutional quantification has no bearing on ontology.[1,2] Anyway, substitutional quantification lends itself to translation by the methods of *Word and Object* quite as determinately as the truth functions do; so it is not clear that his language game adds anything.

Since writing *Word and Object* I have observed[2], by the way, that the determinacy of translation even of the truth functions is less than complete. In the case of conjunction the gap is due to the fact that a speaker may dissent from a conjunction without dissenting from either component. Alternation has a similar gap, dually situated; and substitutional quantification is similarly affected. Still, all these notions enjoy much more determinacy of translation than objectual quantification does.

The gaps in the case of substitutional quantification prove to be gaps for Hintikka's quantification game as well as for my approach. Thus take the case of existential substitutional quantification: a man may be prepared to say that there is a spy on the staff, yet forever unprepared to specify any. A similar remark applies to Hintikka's truth-function game. I grant that it is reasonable and natural to extrapolate across the gap and end up by translating the native idioms into our truth functions and quantification, substitutional or even objectual; but on this score Hintikka's games offer no evident gain over my approach.

My remaining remark aims at clearing up a not unusual misunderstanding of my use of the term 'ontic commitment'. The trouble comes of viewing it as my key ontological term, and therefore identifying the ontology of a theory with the class of all things to which the theory is ontically committed. This is not my intention. The ontology is the range of the variables. Each of various reinterpretations of the range (while keeping the interpretations of predicates fixed) might be compatible with the theory. But the theory is ontically *committed* to an object only if that object is common to all those ranges. And the theory is ontically committed to 'objects of such and such kind', say dogs, just in case each of those ranges contains some dog or other.[2]

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[1] 'Reply to Professor Marcus', in *The Ways of Paradox And Other Essays*, p. 181.
[2] 'Existence and Quantification', in *Fact and Existence* (ed. by J. Margolis), Blackwell's, Oxford (at press).

W. V. QUINE

TO STROUD

Stroud's early pages show a gratifyingly sympathetic grasp of thoughts I have tried to convey early and late regarding convention, analyticity, and indeterminacy. Later portions of his paper show a similar appreciation of my case for gradualism and Neurath's plank-by-plank methodology. It is amusing that Neurath, politically so identified with Marxism and Moscow, should emerge as a mainstay of epistemological conservatism. Politics are one thing, epistemology another.

Also there are places where Stroud has missed my intent, and there are points that invite further development also apart from any evident question of right or wrong interpretation. I shall take up these various points indiscriminately, guided only by how they relate to one another.

Between 'Two Dogmas of Empiricism' and *Word and Object* there is in one respect an opposition in emphasis and feeling, though no conflict, I believe, in doctrine. In 'Two Dogmas', concerned to stress general revisibility, I wrote as Stroud quotes me:

> Even a statement very close to the periphery can be held true in the face of recalcitrant experience by pleading hallucination.... Conversely, by the same token, no statement is immune to revision.

In *Word and Object*, concerned to stress sensory evidence, I wrote of systems withering when their predictions fail. The sustaining force is observation. The more observational a sentence, the more fully it can be sustained by concurrent observation; the more observational, therefore, the less susceptible to revision. But this does not contradict the 'Two Dogmas' passage, because there is still no claim that the limit, utter insusceptibility to revision, can be reached. On the contrary, the passage from 'Two Dogmas' is even echoed in *Word and Object* (pp. 18f.) thus:

> In an extreme case, the theory may consist in such firmly conditioned connections between sentences that it withstands the failure of a prediction or two. We find ourselves excusing the failure of prediction as a mistake in observation or a result of unexplained interference. The tail thus comes, in an extremity, to wag the dog.

The epistemology of *Word and Object* is rather an elaboration than a revision of the view sketched at the end of 'Two Dogmas'. What I alluded to metaphorically as periphery in 'Two Dogmas' reappears as stimulus in

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Word and Object, and what were sentences near the periphery reappear as sentences strong in observationality.

Other sentences to think about under the head of immunity to revision are the logically true sentences, or say more specifically the truth-functional tautologies. Stroud connects the question of their immunity interestingly with my rigid semantic criteria for translating truth functions. Now I should say to begin with that the determinacy of translation afforded by those criteria is subject to two limitations, both of which are remarked on in my adjoining reply to Hintikka. One of these limitations, which was noted also in *Word and Object*, is the groping quality of the linguist's early decision as to what to take as the native's signs of assent and dissent. This decision has much the quality of analytical hypothesis, even though it underlies stimulus meaning. The other limitation is a gap in those semantic criteria for translating truth functions; the criteria fail to cover certain cases.

This gap does not need to affect Stroud's problem. He appeals to my semantic translation criteria for truth functions in order to show that I expect translation of truth functions to preserve logical laws. But he could rest assured of that point anyway, quite apart from those semantic criteria and even without confinement to the truth-functional part of logic; for I have insisted unconditionally that translation not conflict with any logical truths (*W. & O.*, pp. 58ff.). Insofar, then, I sustain Stroud's interpretation. And certainly I am prepared to pass over whatever traces of underlying indeterminacy there may be in the signs of assent and dissent themselves.

What is interesting to ponder is the connection between this rigidity of logic in translation and the question of the immunity of logic to revision. For no fixity of dispositions to verbal behavior is assumed; Stroud seems to misunderstand me here. A phoneme sequence which is a logically true English sentence today could sometime cease to be logically true. We would call this change a change not in logic but in English; and what would we mean by so calling it? Simply that in a manual for translating the one phase of English into the other we would provide for translating that logically true string of phonemes into some different string of phonemes, still to be counted logically true. We would do this because of our convention 'Save logical truth'. This convention of translation safeguards logical truth, nominally, against or through all behavioral vicissitudes. In this curious sense logical truth may even be said after all to be true by convention. Yet it is not a sense that gives logic a distinctive epistemological basis.

'Save logical truth' is conventional in character because of the indeterminacy of translation. It is a rule which, compatibly with all stimulus meanings and other verbal dispositions, could be obeyed or flouted. But it is not capricious. The very want of determinacy puts a premium on adhering to this strong and simple rule as a partial determinant.

More generally, we are well advised in translation to choose among our indeterminates in such a way, when we can, that sentences which natives assent to as a matter of course become translated into English sentences that likewise go without saying. This policy is regularly reflected in domestic communication: when our compatriot denies something that would seem to go without saying, we are apt to decide that his idiolect of English deviates on some word. This conclusion in the domestic case contains, as noted in my reply to Chomsky, some amateur psychology. When we carry the same policy over to radical translation, as we would most naturally do anyway, we are in effect assuming general psychological similarities also across the language barrier; and this again is good strategy, where no specific reasons arise to the contrary. Any such happy conformity to native psychological patterns is bound to help us get on more smoothly with the language.

This general policy of translating the obvious (that is, what is assented to as a matter of course) into the obvious is a policy that comes to a head in the logical truths, because of a combination of two circumstances. One circumstance is that the logical truths are all either obvious in the above sense or else potentially obvious, in the sense of being derivable from the obvious by individually obvious steps.[1] The other circumstance is that the translator can deal with them wholesale by abstracting shared skeletal forms. We see, then, how it is that 'Save logical truth' is both a convention and a wise one. And we see also that it gives logical truths no epistemological status distinct from that of any obvious truths of a so-called factual kind.

Proof-theorists and set-theorists, accustomed to contrasting strengths of systems, will point out that a language might turn out to be too poor in logical structure to afford any translations at all of some logical truths. This seems fanciful if we take logic in the strict and narrow sense; less so if we move out to set theory or other mathematics. In linguistics this problem of contrasting strengths of languages tends not to arise, partly because of the margin of vagueness allowable in practical translation and partly because of vagueness as to the boundaries of the languages themselves. Anyway, I may just say for the benefit of those proof-theorists and set-theorists that the convention 'Save logical truth' would have, in the imagined extremity, to be taken in this weak sense: refrain from translating logical truths into falsehoods.

REFERENCE

[1] Cf. 'Carnap and Logical Truth', in *The Ways of Paradox and Other Essays*, pp. 104f.

W. V. QUINE

TO STRAWSON

As Strawson remarks, the schema of predication 'Fx' and the distinction between general and singular are for me intimately connected. The distinction between general and singular is, at bottom, the distinction between the role of 'F' and the role of 'x' in 'Fx'. This connection of course explains either matter, as Strawson rightly protests, only in terms of the other.

There is also quantification. The crucial thing about the position of 'x' in 'Fx' is that it is accessible to variables of quantification. Still we have just this little circle of interrelated devices; and Strawson wants to tie them down. What wish could be more reasonable, considering that by my own account the variable of quantification is ultimately to carry full responsibility for objective reference?

I do tie this little circle of devices down to natural language, ours, after a fashion. Pronominal cross-reference is the prototype of quantification. Occurrence after 'is a' signalizes general terms. "But", Strawson writes, "it is the distinction of role thus signalized, and not the form of signaling, that is important for logical theory."

I am not the one to urge Strawson to settle for ordinary language and to scuttle logical theory. However, I argued in *Word and Object* that objective reference is subject to the indeterminacy of translation. This indeterminacy invests the whole peculiarly referential apparatus of quantification, pronouns, identity, predication, and the distinction between singular and general. This whole apparatus, and with it the ontological question itself, is in this sense parochial: it is identifiable in other languages only relative to analytical hypotheses of translation which could as well have taken other lines.

In a sense, thus, Strawson is right in saying that I explain not the distinction between general and singular, but only the form of signaling it. He would be wrong in supposing that I thought I had or should have done more.

Strawson has an interesting further suggestion of how to recognize a singular term, in its identificatory capacity: use the fact that failure in this capacity engenders a truth-value gap. An attractive thing about this suggestion is that a native's acquiescence in a truth-value gap can be reflected behaviorally in the enterprise of radical translation, by his refusal to assent

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or dissent. There will of course be the problem of deciding which word of the truth-valueless sentence to blame the truth-value gap on, and there will be other technical problems. If they can be met, we may have here a supplementary behavioral consideration to help govern our choice among analytical hypotheses for translating singular terms.

It could be objected that in radical translation we have no way of knowing whether the jungle words that serve as singular terms in an identificatory way, as checked by appeal somehow to truth-value gaps, are the same words that serve as singular terms in the referential or ontological way that is relevant to quantification. I do not so object, for I consider this question unreal. I hold that in construing terms at all we are working within the indeterminacy of translation. The appeal to truth-value gaps, if it helps us spot singular terms, does so only as a voluntarily added maxim for relieving our indecision among otherwise equally eligible systems of analytical hypotheses.

At any rate, quite apart from any questions of radical translation, the identificatory work of singular terms must be seen as separable from their referential or ontological work. In *Word and Object* a conspicuous effect of regimentation is that a predication of the form 'Fa', with identificatory singular term in the 'a' place, goes over into the symmetrical form '$(\exists x)\,(Fx.\,Ax)$'. A uniqueness clause regarding 'A' may still be added, but the identificatory work of singular terms has lapsed. A language of this kind can still have indicator words, but they will be general terms: 'here', 'now', 'there', 'then'.

I represented predication, the distinction between general and singular, and even ontology itself, as in a sense parochial. I see the identificatory role of singular terms as parochial too, and independent. Under regimentation according to *Word and Object* it lapses, as do the truth-value gaps.

A notion scarcely separable from the identificatory use of terms is that of aboutness: what thing or things is some sentence about? Under the regimentation this lapses likewise, and good riddance. Sentences quantify over everything, and they fall into one or another special field depending on what general terms occur essentially in them; but the idea of their being about certain things and not others seems dispensable.

I return now, for a further remark, to the distinction between general and singular. These were two of a tight circle of kindred notions which were variously interdefinable, but, as Strawson protested, I showed no way of breaking out of the circle. In this there is something ironically reminiscent of my own old critique of the analytic and synthetic, along with their kinship circle. When Strawson objects that I do not really explain the distinction between general and singular, but tell "only the form of signaling it", he reminds me of my own protest against Carnap: that he did not really explain the distinction between analytic and synthetic, but told only how to spot it in specific languages of his making. What then have I to say for myself?

The distinction between general and singular is clear within our own language, or its regimentations. Equally, Carnap's distinction between analytic and synthetic is clear within some artificial diminutive language L_0 of his own making, for he tells us the analytic sentences of L_0 by an outright recursion. My complaint was that his clearly defined class of sentences called analytic-in-L_0 might as well have just been called K; it threw no light on analyticity as applied to our own language, nor yet to any full-size substitute language adequate to science.[1] On the other hand the distinction between general and singular is expressly tailored to a full-size language adequate to science. What the distinction between general and singular does lack is another quality (lacked also, of course, by the distinction between analytic and synthetic): the quality of applicability to all languages, a quality enjoyed by stimulus synonymy and stimulus analyticity.

A third distinction which in these respects is like the distinction between general and singular is the distinction between logical truth and other truth. Logical truth, it will be recalled, resembles analyticity in holding of 'No man not married is married', but differs from analyticity in not holding of 'No bachelor is married'. Now the notion of logical truth is evidently on a par with that of general *vs.* singular, and superior to the notion of analyticity, in that we can make clear sense of it for a full language adequate to science.[2] For, we can just list an adequate vocabulary of logical particles, and then define a logical truth as a true sentence in which no words other than logical particles occur essentially. At the same time this notion is also like that of general *vs.* singular in its lack of direct applicability to languages generally. The obstacle is that we have no clear notion of logical particle applicable to languages generally.

The word 'evidently', occurring at an inconspicuous point in the above paragraph, is a hedge against an earlier paper of Strawson's to which in conclusion I should like to turn.[3] He there argues that the notion of essential occurrence, which I used just now in defining logical truth, depends in a hidden way on a notion of meaning or synonymy. For, we want to say of some word other than a logical particle that its occurrences in a logical truth are not essential: that they could be supplanted by occurrences of any other one expression without falsifying the whole. The hidden dependence on meaning is this: we have to suppose that the supplanted word, which could be ambiguous, was used in the same sense at all its occurrences in our logical truth, and similarly for the supplanting expression.

Leaning heavily on regimentation, we can assure that all supplanting and supplanted expressions will be general terms. Then we can speak of extensions instead of meanings. We can simply stipulate, it would seem, that the expression to be supplanted in our logical truth have the same extension at all its occurrences therein, and similarly for the supplanting expression. However, Strawson saw this, and more. He saw that to speak of the extension of a term *at* an occurrence is itself not intelligible without appeal to a speaker's changing meaning or intent. To talk of the extension of a term is one thing; the extension of 'table' is simply the class of all tables. But to talk distinctively of the extension of an occurrence of a term is another thing.

Leaning yet more heavily on regimentation, we might content ourselves with the definability of logical truth for language regimentations in which this difficulty does not arise: *univocal* regimentations, in which the extension of a term stays the same from one occurrence to another. But wait: how can I even state this univocality law, without intensionalism? I could say simply and extensionally that '$(x)(Fx \equiv Fx)$' is to hold true for every one-place general term in the 'F' positions, and similarly for many-place general terms; but this is not enough, for it does not preclude shifts of extension in contexts of other forms than '$(x)(Fx \equiv Fx)$'.

There is a long way around. Start out with one of the known complete proof procedures for logic – the logic, specifically, of truth functions, quantification, and identity. It can be fashioned to prove sentences directly – all the sentences that are instances of valid logical schemata – so we may omit any talk of schemata as intermediate devices. By just setting forth this general proof procedure we can define what it is for a sentence to be, as we may say, *logically demonstrable*. Thus far no talk of logical truth, nor validity, nor truth. Indeed some of the sentences that are logically demonstrable in this sense may be false, because (to speak crypto-intensionally) of changes in the extension of a term from one occurrence to another. But now we are in a position to state extensionally an adequate univocality condition: *a regimentation of our language is univocal if all the logically demonstrable sentences are true*. For a regimented language that is in this sense univocal, finally, logical demonstrability is logical truth.

I have not defined 'univocal' in an all-purpose way; a qualification would be prudent, 'weakly univocal'. It seems clearly to serve its specific technical purpose of getting us through to an extensional definition of logical truth. It is remarkable how heavily this definition depends on regimentation, and how heavily also on logical theory, exploiting as it does the completeness theorem itself.

Can the definition be extended afterward to logical truth in some broader sense, or say mathematical truth, so as to cover even a domain that resists a complete proof procedure? I think it can, as follows. Begin tentatively with the old method of definition in terms of essential occurrence; that is, list a mathematical vocabulary, and define a mathematical truth *tentatively* as a true sentence in which no words outside the mathematical vocabulary occur essentially. Then say that, for a weakly univocal language, mathematical truth in this tentative sense is indeed mathematical truth. Weak univocity remains defined as before – hence in terms of a complete proof procedure for mere logic. My conjecture is that this logical modicum of univocity suffices to shield mathematical truth generally from the Strawson effect.

It should be clear that my ventures at defining logical and mathematical truth are and have been epistemologically neutral. I am concerned to demarcate the class of logical or mathematical truths, as I might the class of chemical truths; not to show how or why the evidence for truths in the class differs from the evidence for other truths. Each of my proposed definitions makes use in one way or another of the general notion of truth, and seeks to mark out the appropriate subclass. The general notion of truth thus presupposed is meant in Tarski's way. The question could be raised whether Tarski's truth definition is itself threatened by the Strawson effect; but surely, with our construction limited as it is to a weakly univocal language, we are safe on that score.

There is a final point to notice regarding our dependence upon the completeness theorem. We used it to avoid defining logical truth along the old semantical lines. But the completeness theorem is itself intelligible only as equating demonstrability with logical truth or validity semantically defined. The theorem is deprived of sense, in short, by the very use we make of it. This, however, is a tenable situation. It is a case, in Wittgenstein's figure, of kicking away the ladder by which we have climbed.

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[1] 'Two Dogmas of Empiricism', in *From a Logical Point of View*, p. 33.
[2] I made a point of this superiority in 'Carnap and Logical Truth', in *The Ways of Paradox and Other Essays*, p. 123.
[3] P. F. Strawson, 'Propositions, Concepts, and Logical Truth', *Philosophical Quarterly* 7 (1957) 15–25.

W. V. QUINE

TO GEACH

Preparatory to taking up the first of two main points of interest in Geach's paper, I want to write a three-page essay on the enterprise of syntax.

Roughly speaking, the task of the syntactician of a given language is to demarcate formally the class of all phoneme sequences belonging to that language. His data, in the observed behavior of his chosen community, are sample members of the class; and what he in the fullness of time produces is a demarcation of the class in formal terms.

The range of the syntactician's data is indeterminate, *prima facie*, in two ways. First, there is the question how wide to take the linguistic community. In practice he will want to include as many people as he can without sacrificing a practical degree of uniformity. In principle, however, he can view his concern as the idiolect of some single native, and regard then his observations of other natives simply as indirect evidence on his paradigmatic individual. Second, there is the question what utterances to disregard as due to inadvertence or playfulness or the effort to communicate with a foreigner. The syntactician may be expected to adjust such decisions in ways conducive to simplicity on the part of his eventual syntax, guarding, however, against procrustean excesses.

By constructing his eventual syntax to fit such samples as he accepts, the syntactician extrapolates to an infinite class of phoneme sequences which he represents as belonging to the language. Simplicity considerations have vast scope here, since an infinite variety of infinite classes all fit the finite samples. There are controls, since the syntactician continues to gather samples and to check his system against them. He produces cases himself, and tries them on natives for bizarreness reactions. But even so he preserves much scope for simplicity considerations, by calling some bizarre cases grammatical, such as Carnap's example "This stone is thinking about Vienna", and others not.

The syntactician's product is, I said, a formal demarcation. By this I mean that it can be couched in a notation consisting only of names of phonemes, a sign of concatenation, and the notations of logic. (Suprasegmental phonemes can be accommodated by an uninteresting adjustment.)

This formal demarcation can be accomplished more particularly, at least in substantial part, by the classical method of substitution classes and

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constructions. Let us at first ignore the reason for the name 'substitution class', and view these simply as certain classes each of which the syntactician is about to specify recursively. He begins, then, by consigning each single word or morpheme to one or another of these classes. Next he specifies various syntactical constructions, by saying how they are written and what substitution classes they draw their operands from and what substitution classes to consign their products to. Finally he finishes his job of demarcation by saying that the phoneme sequences belonging to the language are simply all the members of substitution classes – or, better perhaps, all segments of such members.

Chomsky holds that the method of substitution classes and constructions is not enough; we need also transformations. Happily this means a departure, not from formality, but merely from the particular mode of formal specification last described. The reason his transformations require no departure from formality is that he does not need to say in general or in principle what it means to be an admissible transformation, any more than we needed to say in principle what it meant to be an admissible construction or a substitution class; it is enough just to specify each specific transformation wanted, if all one wants to do in the end is to demarcate the class of phoneme sequences of the particular chosen language.

If one is interested rather in comparative syntax, then it does become relevant to ask what it means to be an admissible transformation. One may care to rule (unlike Chomsky) that the transformation must leave meanings unchanged; however, I think the only bit of semantics really needed even here is the notion of assent. A transformation is admissible if it always preserves assent; that is, if no sentence commands a speaker's assent but loses it under the transformation. I believe that this condition is adequate because, if a given transformation could ever plausibly be said to 'change a meaning' when applied to some one sentence, I expect that another sentence could be devised which would simply lose assent under that transformation.

Parenthetically it might be remarked that the appeal to meaning in the familiar definition of phoneme can likewise be by-passed in favor of assent, if we can be confident that for every two phonemes there is some sentence that commands a speaker's assent but loses it when the one phoneme is put for the other. But anyway there is another way of getting phonemes without semantics, if with Harris and Wedberg we can believe that for every two phonemes there is some phoneme sequence that belongs to the language and ceases to belong when the one phoneme is put for the other.[1]

An interest in comparative syntax was what gave relevance to the question what it means in general to be an admissible transformation. Equally an interest in comparative syntax gives relevance to the question what it means in general to be a substitution class; a recursive specification of the substitution classes of a specific language ceases to be the whole story. The classical answer to the general question is this: a substitution class is any maximum class of phoneme sequences such that, whenever any member of the class is substituted for another where it occurs as a segment of a phoneme sequence belonging to the language, the result still belongs to the language. In Geach's learned phrase, it is a maximum class whose members are interchangeable *salva congruitate*. Hence the phrase 'substitution class'.

Now the first major point in Geach's paper is that the notion of substitution class, so construed, ill serves syntax. His reason is that terms interchangeable *salva congruitate* can still differ in syntactical role; and his example is the pair 'Copernicus' and 'some astronomer'. How do these differ in syntactical role? In that two constructions can apply indifferently to 'Copernicus' and yet differ from each other in their effects when applied to 'some astronomer'. In Geach's illustration the two constructions give verbally identical sentences, but the sentence is unambiguous in the case of 'Copernicus' and ambiguous in the case of 'some astronomer'. The point I want to make can be expedited by setting ambiguity aside and switching to this example:

(1) Copernicus was Polish and wrote Latin.
(2) Copernicus was Polish and Copernicus wrote Latin.

(3) Some astronomer was Polish and wrote Latin.
(4) Some astronomer was Polish and some astronomer wrote Latin.

Intuitively the difference in syntactical role between 'Copernicus' and 'some astronomer' is brought out by the equivalence of (1) and (2) as against the inequivalence of (3) and (4). How then ought the concept of substitution class be refined so as to accommodate such differences? With help, I think, of Chomsky's transformation concept. There is an admissible transformation of (1) into (2) but not of (3) into (4). The partitioning of a language into substitution classes could be seen as relative always to some prior listing of transformations; and then a substitution class can be explained as a maximum class whose members are interchangeable *salva congruitate ac transformatione*. Geach's applicatival phrases presumably comprise a substitution class in this corrected sense.

Chomsky argued the syntactical inadequacy of the method of construction and substitution class. Geach shows the old notion of substitution class inadequate also in another respect. And now, if my above suggestion is right, Chomsky's transformation device offers a remedy to the second shortcoming as well as to the first.

In passing I would touch on a second and lesser point of Geach's, where he deplores my policy of eliminating singular terms other than descriptions. He is right insofar as one's purpose is analysis of English; the contrast depicted just now in (1)–(4) bears him out. For that matter, insofar as one's purpose is analysis of English, there is something to be said also for truth gaps rather than falsity in the cases where singular terms lack designata. On both points, my deviant course is defensible only insofar as one's objective is a medium having certain advantages over English.

There remains still a second major point in Geach's paper: his well defended analysis of relative clauses. He shows that when a relative clause is appended to a noun, the combination is ordinarily not a coherent whole. He shows that we must not view the relative clause, as I and others have done, as a complex adjective attachable attributively to a noun to form a noun phrase.

Geach's point is of considerable logical interest because relative clauses are what give us abstraction of complex predicates. They are the clauses that so usefully package a complex sentence about x into a single complex adjective attributable to x. This is what they are, that is, as long as we place the cleavages where Geach now shows we must not. He shows that relative clauses are not members of a substitution class; there are greater cleavages within them than at their termini.

What Geach says of relative or 'which' clauses applies equally, as he says, to 'such that' clauses, insofar again as these are in attributive position. But 'such that' clauses, unlike 'which' clauses, can occur also after the copula, in predicative position; and in this position, so Geach has lately written me, a 'such that' clause does cohere as an adjective. This is coolish comfort, since it is rather in attributive position that predicate abstraction is luxurious; 'such that' in predicative position serves no evident purpose beyond the occasional settling of ambiguities of scope.[2]

Geach's analysis leaves predicate abstraction in somewhat the status of Russell's incomplete symbols, or of the differential operators ('d^2/dx^2' and the like) which suggested them to Russell.[3] Knowing that predicate abstraction qualifies as an English construction only by false cleavages, we can continue to prize it for its logical utility. We remain free also of course to develop coherent notations, not English, in which predicate abstractions qualify as coherent wholes.

It remains interesting, precisely because of the logical importance of predicate abstraction, that no such construction is strictly traditional to English. Logic, by trial and error and other expedients, has come a long way. Already within English the 'such that' clause may be seen as a way station; for it is not the most natural English, and moreover it seems to be straining for adjectival status. The 'which' clause, after all, is seen only in attributive position even when wrongly viewed as a coherent adjectival whole. The 'such that' clause, on the other hand, crowds also into predicative position like a full-fledged adjective, and even qualifies, in that position, as truly adjectival.

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[1] See Anders Wedberg, 'On the Principles of Phonemic Analysis', *Ajatus* **26** (1964) 235–253.

[2] See *Word and Object*, pp. 111, 140f.

[3] See *Principia Mathematica*, 2nd ed., I, p. 24.

REPLIES

TO DAVIDSON

What goes by the name of semantics falls into two domains, the theory of reference and the theory of meaning. Truth is on one side of the boundary, meaning on the Other. The two domains are conspicuously distinct, but still there is this fundamental connection between them: you have given all the meanings when you have given the truth conditions of all the sentences. Davidson took the connection to heart and drew this conclusion: the way to develop a systematic account of meanings for a language is to develop Tarski's recursive definition of truth for that language.

To the notoriously flimsy theory of meaning, this idea offers new hope: the discipline of Tarski's theory of truth. Incidentally it clarifies the semantic role of the sentence; we have appreciated since Bentham that sentences were somehow semantically basic, but a truth-directed semantics drives the point home.

What is more impressive, Davidson's idea gives the logical regimentation of language a clear and central role in the theory of meaning. We regimentalists had already been operating under an unswerving conviction that logical regimentation, especially along truth-functional and quantificational lines, was of the essence of the clarification of meaning; but the conviction carried by our excuses (as for instance in a section of *Word and Object* entitled 'Aims and Claims of Regimentation') was, in contrast, swerving at best. In Davidson's picture the urgency of the regimentation becomes clear. The regimentation implements the recursions in a Tarskian truth definition. What we have already been doing becomes imbued with a new sense of purpose and direction.

This effect is striking in connection with extensionalism. The substitutivity of identity, at least as concerns variables, was a clear-cut imperative anyway; to flout it were to play fast and loose with the word and symbol for identity. But extensionalism calls for more: for the substitutivity of coextensiveness. One thing we have long been saying in defense of this demand is that extension is clearer than intension, but this invites retorts about one man's clarity. We said more, too, in defense of extensionalism: the intensional contexts that anybody was wedded to turned out to make trouble even for the substitutivity of identity and to raise problems about quantification. But now from Davidson's idea there issues a powerful further objection against those intensional contexts: they obstruct the

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recursion of a Tarskian truth definition. If we follow Davidson in equating clarification of meaning with definition of truth, then our old charge that intensions are unclear gains a certain objectivity. So do our scruples against mental entities.

A defense of plain talkers against regimentalists, and so of intensionalists against extensionalists and of mentalists against behaviorists, has been to equate clarity with familiarity and so to declare ordinary language clear *ex officio*. What better can we equate clarity to? A central importance of Davidson's idea is that it offers an answer, thus telling us what is wrong with ordinary language: you cannot launch it into a truth definition.

Does this illumination of the theory of meaning by the theory of truth resolve the indeterminacy of translation? Davidson appreciates that it does not. The reason is that truth itself is immanent to the conceptual scheme: 'Snow is white' is true if and only if snow is white.

What of the well-known dependence of Tarski's truth definition upon a stronger metalanguage? Does this doom the theory of meaning to an infinite regress? No; for the demand for a stronger metalanguage arises, in general, only when we undertake to transform the recursive definition of truth into a direct definition.[1] This we need not insist on doing.

I conclude with a few remarks on Davidson's present special topic, indirect discourse. He agrees with Scheffler[2] that I underestimated the cost of my "final alternative", that of depriving the propositional attitudes of their objects altogether. I also felt that Scheffler made a strong case. Davidson accordingly restores sentences as objects of the propositional attitudes. Choosing indirect discourse as paradigmatic of the idioms of propositional attitude, then, he proceeds to see how far it can be reconciled with definition of truth.

Part of the problem of indirect discourse is the failure of extensionality, but part of it also is the question how far and in what way the content sentence may be allowed to deviate from direct quotation. This is where, as noted in Harman's paper and my reply, the perplexities of translation obtrude on indirect discourse. Davidson does, however, contrive to separate these ills from the other, the failure of extensionality. This is the point of his samesaying relation. It is not supposed to be intelligible, except as indirect discourse in ordinary language is intelligible. It merely packages the problems of indirect discourse that we are not worrying about when we worry about resistance to the truth definition – which is where the failure of extensionality comes in.

Accommodation of indirect discourse to the truth definition is the purpose of Davidson's demonstrative 'that'. This strange device enables him to keep the indirect quoter's quotation in the clear, as the quoter's own pronouncement, however insincere. It can thereupon be construed under the truth definition. And the three-word companion sentence 'Galileo said that' can be construed under the truth definition too, granted that the demonstrative 'that', like 'Galileo', is available in the language in which the truth definition is formulated.

I have thought of the idioms of propositional attitude, like indicator words, as Grade B idiom (*W. & O.*, pp. 218ff.). Now Davidson actually connects them, making indirect discourse accessible to a truth definition precisely by invoking an indicator 'that'.

He does not go into the question of transparent construction in indirect discourse, or the related question of quantifying into indirect discourse. However, my treatment submits directly to his approach. My way of according 'the earth' referential position in the Galileo story would be to say:

Galileo said of the earth that it moves.

This clearly becomes:

Galileo said of the earth that. It moves.

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[1] See 'On an Application of Tarski's Theory of Truth', in my *Selected Logic Papers*, pp. 144f.

[2] *Anatomy of Inquiry*, pp. 108ff.

W. V. QUINE

TO FØLLESDAL

Føllesdal has explained, succinctly but clearly and accurately, my strictures on modalities and how they grew. His account is unusually understanding and sympathetic. And now, turning to set down my comments, I sense an unfortunate disproportion between the satisfaction taken in reading a paper and the length of one's comments upon it. For, whereas disagreements have to be expounded and defended, agreements go almost without saying.

He makes the following point which had not occurred to me before. After showing that extensional transparency implies referential transparency, but not conversely, he points out that it is precisely this failure of the converse that makes quantified modal logic possible at all. He agrees with me that quantified modal logic is possible only at the cost of essentialism, but what he notes further is that it would not be possible even at this price if extensional and referential transparency coincided.

At the end of his paper Føllesdal suggests that the logical modalities are even worse than the propositional attitudes, because of their link to the dubious notion of analyticity. I agree with the somber side of this remark, but am somewhat doubtful about its brighter side: that the propositional attitudes are less obscure. A discouraging vagueness invests all the propositional attitudes, even indirect quotation itself; namely, vagueness as to the manner and degree of variation that is allowable to the subordinate sentence. How far may we go in revising a man's utterance and still be entitled to attribute it to him in indirect quotation? I am not sure that this matter is in better shape than the notion of analyticity on which logical modality depends. What makes me take the propositional attitudes more seriously than logical modality is a different reason: not that they are clearer, but that they are less clearly dispensable. We cannot easily forswear daily reference to belief, pending some substitute idiom as yet unforeseen. We can much more easily do without reference to necessity.

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REPLIES

TO SELLARS

Sellars shares my misgivings about quantifying into positions that resist the substitutivity of identity. But he wants still to allow quantification into belief contexts, even of the opaque kind. To this end he adopts Frege's device of reconstruing singular terms as referring, in such contexts, to their senses instead of to their normal designata; hence to individual concepts rather than to individuals. This intensionalizing of objects is meant to restore substitutivity and so to permit the desired quantification.

The move differs from Frege's in applying only to singular terms. But it carries with it a systematic ambiguity of predicates; e.g. 'is wise' ceases to be a predicate of persons and becomes, in belief contexts, a predicate of individual concepts. Sellars shows how his move avoids drawbacks of moves suggested by Hintikka and Chisholm.

Sellars makes this move not only for belief in the opaque sense, but for belief in the transparent sense as well. This is in order to be able to define the transparent sense in terms of the opaque. Here the reader must watch carefully the changing distinctions. There had been the two senses of belief: one was transparent, in the sense of not resisting substitutivity, and the other was opaque, in the sense of resisting it. The point of Sellars's intensionalizing of objects is to render both senses of belief transparent in the sense of not resisting substitutivity; still they continue to be two senses, so he retains the old contrasting terms 'transparent' and 'opaque' to distinguish them; and it is one of the thus adjusted senses of belief, then, that he defines in terms of the other.

His definition of the one in terms of the other has the effect that $^{t}Bf\mathbf{a}$ whenever $\mathbf{a}$ exists and $^{o}Bf\mathbf{a}$. In 1956, I thought the same[1]: that if

(1) Ralph believes that Ortcutt is a spy

then, assuming that Ortcutt exists,

(2) Ralph believes of Ortcutt that he is a spy.

Lately, however, Sleigh raised a difficulty that bears on the point.[2] Jones, like all of us, believes there are spies, though, unlike Ralph, he has nobody in particular under suspicion. Also he believes, not unreasonably, that no two births are quite simultaneous. Consequently he believes the youngest

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spy is a spy. Then, if the inference from (1) to (2) was right, Jones believes of the youngest spy that he is a spy. But then, by existential generalization from this transparent construction, we can infer after all that there is someone whom Jones believes to be a spy. Kripke pointed out to me that this paradox of Sleigh's can be resolved by ceasing to recognize the form of inference that led from (1) to (2).

I have a somewhat similar comment to make on Sellars's stratagem of intensionalizing objects to restore substitutivity. Such a move seemed at one time to solve the substitutivity problem for modal logic, but I more recently offered an argument to show that it does not.[3] The argument applies equally to Sellars's suggestion, as follows. Suppose that $^{o}Bf\mathbf{a}$ but neither fa nor $^{o}Bf\mathbf{b}$. Since $\sim fa$,

$$(3) \qquad \mathbf{a} = (\iota i)(i = \mathbf{a} \,.\, \sim fa . \vee . i = \mathbf{b} . fa).$$

However, if Jones has his wits about him,

$$\sim {}^{o}Bf(\iota i)(i = \mathbf{a} \,.\, \sim fa . \vee . i = \mathbf{b} . fa).$$

Yet $^{o}Bf\mathbf{a}$; so the substitutivity of the identity (3) has failed.

My objection to quantifying into non-substitutive positions dates from 1942. In response Arthur Smullyan invoked Russell's distinction of scopes of descriptions to show that the failure of substitutivity on the part of descriptions is no valid objection to quantification.[4] He would respond similarly in the present instance. But Sellars would not, for he accepts the substitutivity condition of quantification and has explicitly sought to make his logic of belief safe for substitutivity.

Still, what answer is there to Smullyan? Notice to begin with that if we are to bring out Russell's distinction of scopes we must make two contrasting applications of Russell's contextual definition of description. But, when the description is in a non-substitutive position, one of the two contrasting applications of the contextual definition is going to require quantifying into a non-substitutive position. So the appeal to scopes of descriptions does not justify such quantification, it just begs the question.

Anyway my objection to quantifying into non-substitutive positions can be made without use of descriptions. It can be made using no singular terms except variables. My old example of failure of substitutivity was, nearly enough, this:

$9 =$ the number of planets,
necessarily 9 is odd,
$\sim$ necessarily the number of planets is odd.

Since one and the same object x then evidently fulfills the condition 'necessarily x is odd' or not depending on whether we specify it as 9 or as the number of planets, there is really no sense in the quantification:

(4) $(\exists x)$ necessarily x is odd.

Such was my old argument, using the singular terms '9' and 'the number of planets'. But now let us ban singular terms other than variables. We can still specify things; instead of specifying them by designation we specify them by conditions that uniquely determine them. On this approach we can still challenge the coherence of (4), by asking that such an object x be specified. One answer is that

(5) $(\exists y)(y \neq x = yy = y + y + y)$.

But that same number x is uniquely determined also by this different condition: there are x planets. Yet (5) entails 'x is odd' and thus evidently sustains 'necessarily x is odd', while 'there are x planets' does not.

The point I have just now tried to make is this: (i) *If a position of quantification can be objected to on the score of failures of substitutivity of identity involving descriptions, it remains equally objectionable when no singular terms but variables are available.* My previous point, made in connection with the equation (3), may be put thus: (ii) *Substitutivity of identity for descriptions is not restored by intensionalizing the objects.* I illustrated the one point in terms of necessity and the other in terms of belief, but both apply in both quarters.

What can one do, then, who wants to quantify into contexts of belief or necessity? I say his proper strategy is to reject the hypothesis in (i), and so abandon the objective envisioned in (ii). His proper strategy is to oppose my own stand by condoning quantification into positions that resist substitutivity of identity for descriptions. This does not mean violating substitutivity of identity for variables, which would simply be a wanton misuse of the identity sign; what it does mean is *essentialism*, or the adoption of an asymmetrical attitude toward different ways of specifying the same object. The essentialist's answer to my old objection against (4) would not be Smullyan's appeal to scopes; it would be that '9' designates the number essentially and so is germane to (4) whereas 'the number of planets' designates it accidentally and has no bearing on (4). Or, adapted to my rephrased objection against (4), the essentialist's answer would be that the condition (5) specifies the number essentially and so is germane to (4) whereas the condition 'there are x planets' specifies it accidentally and has no bearing on (4).

What to count as essential specifications would depend on whether one is concerned with necessity or with belief. For belief one requirement would be, vaguely speaking, that the specification hinge on traits by which the object in question is known to Jones. Formally, in systems retaining descriptions, this essentialism would be implemented by not allowing the instantiation of quantifications by terms which designate their objects only accidentally. This restriction is of course needed only for quantifications into opaque contexts. For the logic of belief Føllesdal has worked out this approach in some detail.[5]

My point (ii) does not eliminate all motive for intensionalizing the objects of quantification in modal logic. If a modal logician finds essentialism more congenial in a domain of intensions than elsewhere, then he has reason, when quantifying into modal contexts, to quantify over intensions only. A parallel situation would arise in the logic of belief, if the best version of essentiality for belief purposes turned out to apply rather to individual concepts than to individuals. On the other hand there are strong reasons, connected with doubts about the synonymy relation, for preferring not to admit individual concepts or other intensional objects. Sellars hints a certain sympathy with this attitude himself when he writes, "The mechanics, if not the metaphysics, of the move is comparatively straightforward."

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[1] 'Quantifiers and Propositional Attitudes', in *The Ways of Paradox and Other Essays*, p. 188.

[2] Robert Sleigh, 'On Quantifying into Epistemic Contexts', *Noûs* **1** (1967) 1–31, p. 28. See also Hintikka, *Knowledge and Belief*, pp. 141–144.

[3] *From a Logical Point of View*, 2nd ed., 1961, pp. 152f.

[4] A. F. Smullyan, 'Modality and Description', *Journal of Symbolic Logic* **13** (1948) 31–37.

[5] Dagfinn Føllesdal, 'Knowledge, Identity, and Existence', *Theoria* **33** (1967) 1–27.

REPLIES

TO KAPLAN

This masterly essay is visibly the product of years of ever more subtle thought on referential opacity. It deepens our understanding of these matters in both technical and philosophical ways. It does this, moreover, without enunciating a finished theory; on the contrary, it opens unexpected prospects of future progress. This open-ended character of the work is due to the breadth of Kaplan's philosophical perspective.

I shall begin by remarking on his technical contribution. He analyzes the statement of relative or triadic belief:

(1) z believes 'x is a spy' of y

into terms of absolute or dyadic belief, plus designation, thus:

(2) z believes $\ulcorner\alpha$ is a spy$\urcorner$ for some α which is a standard name of y for z.

Kaplan mischievously leaves it to the reader to recognize, in those "Frege quotes" of his, the "quasi-quotes" or corners that I used in *Mathematical Logic* and earlier writings; the meaning is the same and the shape comes close. For me the device had been the merest practical convenience; and I am pleased now to see it so neatly assimilated to a Fregean philosophy.

That is by the way. What I want to dwell on is the importance of Kaplan's analysis of (1), here, into (2). One great benefit of this analysis is just that it does reduce triadic belief, or belief-of, to dyadic. But also it throws other light. It opens the distinction between Kaplan's (46) and (47), which my formulations left undistinguished. It accomplishes these things while at the same time meshing nicely with prior theory in other respects. Thus it meshes with Føllesdal's plan of treating some of the names of a thing as standard and others not – the standard ones being admissible as instances of variables in opaque constructions.[1] This plan of Føllesdal's is the formal implementation of the essentialism which, I have held, is the price of quantifying into opaque constructions.

The little matter of exportation which I mentioned with tentative approval in 'Quantifiers and propositional attitudes', but luckily made no use of, has now taken on sizeable dimensions. We now see that such exportation is not generally permissible. In my adjoining reply to Sellars, which went to press before I saw Kaplan's paper, I credited Sleigh and

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Kripke with showing this. Now we find Kaplan making the same point independently with help even of substantially the same example. And Furth carries the matter further, by Kaplan's account: he sees names as differing from one another in point of exportability. The exportable ones are precisely the standard names, the names that can instantiate variables within opaque constructions. Clarification and unification are going forward hand in hand.

The question just which of the names of a thing to count as standard, for a believer z, is the open end of Kaplan's theory. He is rightly in no hurry to close it, for it is just here that philosophical significance proceeds to ramify. Already his work on this problem suggests in a sketchy way the foundations of an imposing theory of names, along lines no less relevant to ontology and the philosophy of mind than to logic. A preliminary part of his problem is Neil Wilson's question how wrong a man can be about something and still be said to refer to it.[2] The central part of his problem is, given all the man's names for a thing, to separate the standard ones from the others. I feel that Kaplan's appeal to a "vividness threshold" for this purpose is, for all its vagueness, much the right line, and I find his analogy of names to pictures suggestive.

Toward the end of his reflections on these matters, he argues convincingly that linguistic forms are inadequate as objects of the propositional attitudes and that images or other mental entities must be admitted for the purpose. Since philosophical clarity is so largely a result of avoiding mental entities, we must take care lest this conclusion abet obscurantism. There is some comfort, however, in that images are what are primarily relevant to Kaplan's picture theory of reference, and images are a comparatively innocuous lot as mental entities go.

One place where their comparative innocuousness may be seen is in connection with the problem of meaning. For, recall the rigors of my concept of stimulus meaning, which was my refuge from mental entities. Stimulus meaning gave a satisfying account of meaning over only a limited domain – largely observation sentences. Now an index of the comparative innocuousness of images is that if we were to let them in – if we were to lift the ban on mental entities just that far – we would thereby get no relief from the rigors of stimulus meaning. The terms or sentences for which we can conjure up sensory images are the ones that are already well served by stimulus meaning.

I detect a hint in Kaplan's paper even of how we might hope to legitimize images behaviorally, as dispositions to overt behavior; a man's possession of an image of a thing is his ability to recognize the thing. We may reasonably venture to dabble in mental entities as long as we keep one foot planted on the comparatively firm ground of dispositions to behavior. The way to a full and satisfactory theory of meaning is, I begin to suspect, a phenomenology of act and intension, but one in which all concepts are defined finally in behavioral terms. Such a program, however, is incomparably more visionary than a mere behaviorizing of images. Images promise well as objects of beliefs only of a fairly observational kind, and for these I expect stimulus meanings would serve as well.[3]

Kaplan writes here mainly of belief, but occasionally carries his observations over to the modal logic of necessity. This is where essentialism comes literally into its own; the standard names, for purposes of modal logic, are the names that connote essential peculiarities of the named object. Now I have felt that the unreasonableness of essentialism is most obtrusive when the objects are extensional, and hence that persons bent on quantifying into necessity contexts are apt to be on firmest ground when the values of the variables are intensions.[4] Kaplan expresses a related but somewhat divergent intuition: essentialism is unreasonable for particulars, reasonable for universals. Going intuition one better, he marshals a reason: the universals that enjoy essential traits are universals that admit of standard names of a structural-descriptive kind. This idea of structural-descriptive names as Kaplan sketches it seems to depend on the dubious notion of analyticity; still it is a suggestive idea, and the dependence may prove avoidable.

In any event Kaplan and I see eye to eye, negatively, on essentialism as applied to particulars. The result is that we can make little sense of identification of particulars across possible worlds. And the result of that is that we can make little sense of quantifying into necessity contexts when the values of the variables are particulars. (I keep saying 'little sense' rather than 'no sense' because Kaplan does point to the occasional possibility, in branching worlds, of identifying particulars from branch to branch by continuity of change. But surely this odd case is cold comfort for the quantifying modal logician.)

Kaplan wonders at an asymmetry between my attitude toward belief and my attitude toward modal logic. In my treatment of belief I distinguished between an opaque and a transparent version, but in modal logic I got no further than the opaque. I agree with Kaplan that my treatment was thus asymmetric and that the fact of the matter is symmetric. The distinction between opaque and transparent on the modal side is the distinction between what Chisholm, reviving scholastic terminology, calls *necessitas de dicto* and *necessitas de re*.[5] But I had a reason, as noted in my reply to Føllesdal, for treating belief more fully than necessity. It was that the notion of belief, for all its obscurity, is more useful than the notion of necessity. For this reason my treatment of modal logic was brief and negative; I was content to outline the opacity troubles. Kaplan's charge of "inconsistent skepticism" is off the point; the point is that some obscure notions are, on grounds of utility, more worth trying to salvage than others.

Kaplan suggests twice that I have left what he calls intermediate contexts unanalyzed. I should stress that I have not meant to represent them as without logical or grammatical structure. This would be intolerable, for it would represent us, absurdly, as acquiring an infinite vocabulary. On the contrary, I attributed a logical grammar to the intermediate contexts. I construed 'that' as an operator that attaches to a sentence to produce a name of a proposition.[6] Then, switching to an alternative approach which shunned propositions, I construed 'believes that' rather as an *attitudinative*: a part of speech that applies to a singular term and a sentence to produce a sentence.[7] More complex operators came into play in the analysis of polyadic belief.

In some less obvious sense Kaplan's charge does still seem just, but in what sense? Is it that I do not show how the meanings of intermediate contexts are generated from the meanings of the parts? Or can I protest that I do show just that, by explaining the attitudinatives and other operators? The notion of meaning is so vague that one is at a loss to say what counts here. But I think now that Davidson has hit upon the essential point: we want to be able to carry a Tarskian truth definition recursively through the complex contexts.[8] By this standard Kaplan's treatment of the intermediate contexts could qualify as analysis and mine not.

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[2] N. L. Wilson, 'Substances without Substrata', *Review of Metaphysics* **12** (1959) 521–539.
[3] In a 1965 lecture 'Propositional Objects', forthcoming in *Critica*, I explored this possibility somewhat.
[4] See the last paragraph of my reply to Sellars.
[5] R. M. Chisholm, 'Identity through Possible Worlds', *Noûs* **1** (1967) 1–8.
[6] *Word and Object*, pp. 164, 168, 192, 194.
[7] *Word and Object*, p. 216. The term 'attitudinative' is a classroom addition.
[8] See my adjoining reply to Davidson, and see his 'Truth and Meaning', *Synthese* **17** (1967) 304–323.

W. V. QUINE

TO BERRY

I agree in general with Berry's admirable survey of the ontological options in set theory. I have nothing useful to add to that aspect of his paper. Instead I shall limit myself to a discussion of the specific system NNF of set theory which he proposes.

Berry partially revives my old pair of primitive ideas, namely inclusion and abstraction, but uses exclusion instead of inclusion. I liked that old starting-point both for the paucity of primitive ideas and for the compactness of my two axiom schemata and three rules of inference; but I had two reasons for turning away from it. One reason was that when I turned from the theory of types to 'New foundations' I ceased to assume a class for every membership condition; and on these terms a primitive notation for class abstraction seemed less suitable than a contextually defined notation for class abstraction. Now Berry meets this objection by letting his class abstracts name the null class when they name nothing else. For that matter, when I got to *Mathematical Logic* I likewise had all class abstracts naming again. But I still had another reason not to revert to my old pair of primitives.

The main advantage of starting rather with membership and a quantifier and a truth function is that the three departments stand separate. You get truth-function theory in all its simplicity and decidability before touching a quantifier; then you get quantification theory in all its classical clarity and completeness without yet having ventured upon the unsettled and forever incompletable domain of set theory. In my system based on inclusion and abstraction, in contrast, the axiom schemata and rules were a tight package of all these things; the three departments had to be disengaged in the course of the deductions.

True, Berry does separate the three departments somewhat in his axioms and rules. This is because, though starting with exclusion and abstraction, he promptly defines membership and quantification and the truth functions and then states his axioms and rules largely in terms of these. His two primitive ideas do not set the tone of his deduction; what they were matters less, therefore, than it otherwise might.

There are strong practical reasons for wanting to maintain, by whatever method, the separation of the three departments. One benefit, of course, is the pedagogical benefit of proceeding by easy stages. Another

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benefit is simply that of capital, or of machine tools: the machinery of truth functions and quantification can be used in other set theories besides the one at hand, and in other theories besides set theory. And a third benefit is the facility that standardization affords for the comparison of systems, set-theoretic and otherwise.

Thus take, for instance, von Neumann's set theory. Its primitive ideas included functional application and identity. If we want to compare the strength or other virtues of his system with those of another set theory, say Zermelo's, we are well advised first to translate von Neumann's idiosyncratic primitives (as Bernays did) into terms of the epsilon used by Zermelo. Set theory teems with systems that clamor for comparison; and a generally adequate and convenient medium for this purpose is the standard logic of truth functions, one-sorted quantification, and a single two-place predicate, epsilon.

Let us try thus standardizing Berry's NNF. We start, then, with epsilon and quantifiers and a sufficient truth function as primitive notation, and define Berry's 'χ' in the obvious way:

(i) $$(f\chi g) =_{\mathrm{df}} (a)(a\varepsilon f. | .a\varepsilon g).$$

We have also to define his '$\hat{a}p$', in this sense: $\hat{a}p$ is the x such that $(a)(a\varepsilon x. \equiv p)$ if such there be, and otherwise Λ. Succinctly, $\hat{a}p$ is the union of all classes x such that $(a)(a\varepsilon x. \equiv p)$. Thus

(ii) $$\hat{a}p =_{\mathrm{df}} (\imath y)(z)(z\varepsilon y. \equiv (\exists x)(z\varepsilon x.(a)(a\varepsilon x. \equiv p))).$$

Antecedently we may define description contextually as I did in NF (following Russell). Or, as another avenue to the same end, we may bypass description and define abstraction itself contextually. Half of this definition is:

(iii) $$b\,\varepsilon\,\hat{a}p. =_{\mathrm{df}} (\exists c)(b\varepsilon c.(a)(a\varepsilon c. \equiv p)),$$

which is equivalent to Berry's (48). It remains only to define '$\hat{a}p\,\varepsilon\,\zeta$' where '$\zeta$' stands for a variable or an abstract. A suitable definition can be cribbed from (ii), thus:

(iv) $$\hat{a}p\,\varepsilon\,\zeta. =_{\mathrm{df}} (\exists y)(y\varepsilon\zeta.(z)(z\varepsilon y. \equiv (\exists x)(z\varepsilon x.(a)(a\varepsilon x. \equiv p)))).$$

The existential quantifier and the various truth-function signs in (iii) and (iv) are of course, as in Berry's paper, to be supposed defined in terms of '|' and universal quantification in familiar ways. Berry's definitions (41) and (42) of identity and unit class may likewise be supposed carried over.

To the axiom schemata of NNF, thus standardized, we must reckon not only the primitive expansions of Berry's (44)–(49) according to our adjusted definitions; we must also include, or derive, the biconditionals:

(v) $p|q. \equiv .\hat{a}p\chi\hat{a}q$, ($p$, q lacking a)

(vi) $(a)p \equiv .\hat{a} \sim (a\chi a)\ \chi\ \hat{a}(a\chi\hat{a}p)$.

(vii) $f\varepsilon g. \equiv \sim([f]\ \chi g)$,

which correspond to his superseded definitions (39), (40), and (43).

We may now compare the thus standardized NNF with NF. Briefly stated, NF comprises the logic of truth functions and quantification and in addition this axiom and axiom schema:

(viii) $(b)(c)(d)((a)(a\varepsilon b. \equiv .a\varepsilon c).b\varepsilon d. \supset .c\varepsilon d)$,

(ix) $(\exists c)(a)(a\varepsilon c. \equiv p)$ (p stratified and lacking c).

I think Berry both knows and intends these to be forthcoming in NNF. But what I suspect is that the converse also holds: that NNF as standardized is forthcoming in NF, and therefore differs only in formulation.

I shall leave this as a conjecture, for the proof would be long and laborious. It would require proving (v)–(vii) above and Berry's schemata (47)–(49) all in NF; not under the definitions that were originally in NF, of course, but under the above definitions (i), (iii), and (iv) and Berry's definitions (41)–(42). In addition it would require proving that abstracts, when contextually defined as in (iii)–(iv), can always be substituted for variables; for note that the 'instances' mentioned in connection with Berry's (46) and (51) may use abstracts. The proof of this metatheorem of substitution would be analogous to § 31 of *Mathematical Logic*.

TO JENSEN

NF is, as Jensen says, Ext+Abst. Ext divides in turn into two independent parts. One part is Jensen's Ext′, which says that things that have members are identical when their members are identical. The other part says that only one thing lacks members:

$$\text{C} \qquad (z)(z \notin x \,.\, z \notin y) \supset .\, x = y.$$

I am calling this axiom C because it can also be read as saying that everything is a class; that nothing is memberless but the null class. Jensen's NFU, then, is Ext′+Abst, and NF is NFU+C.

Jensen shows that C bears an astonishing burden. He brings out the following contrasts between the strength of NF and that of NFU. The consistency of NF is unknown; the consistency of NFU is provable in elementary number theory. The axiom of choice is incompatible with NF (Specker); it is compatible with NFU. The axiom of infinity follows from NF (Specker); it is independent of NFU.

Early in his paper, Jensen quotes my deviant doctrine of individuals: my trick of taking individuals as their own unit classes, rather than as memberless, and so reconciling C with the existence of individuals. An unwary reader might infer that the perilous excess of strength in NF comes of this. It does not. For, NF does not assume there are any individuals in this sense. Nor, for that matter, does NFU assume there are any individuals in the old sense of multiple memberless objects. NF and NFU differ only in that NF excludes individuals in the latter sense, through C.

Scott showed that NF, if consistent, is independent of there being individuals in my sense; it implies neither that $(\exists x)(x = \{x\})$ nor that $(x)(x \neq \{x\})$.[1] Thus my concept '$x = \{x\}$' of individuals, however bizarre, is harmless; and thus the footnote which Jensen quotes from me is sustained. What is so surprising about Jensen's findings is rather that the great difference in strength between the two systems all comes from humdrum old C, which is simply the extrusion of *Urelemente*, and a commonplace of Zermelo-Fraenkel set theory.

There has indeed already been evidence, in the latter connection, that C is deceptively strong. Fraenkel was able to prove that the Zermelo-Fraenkel system without C is independent of the axiom of choice (if consistent);[2] but his proof used a model containing *Urelemente*, and so

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could not be extended to include C. It remained to Paul Cohen to prove the more inclusive result, by a very different method. Despite this history, however, one is unprepared for the magnitude of the role of C in NF.

Jensen's brilliant paper contains also other impressive results that I have not mentioned. There are theorems of relative consistency of NFU supplemented by one or more of the axiom of infinity, the axiom of choice, and the schema of mathematical induction. I have not understood all steps. Toward the end of the proof of Lemma 4, for instance, a certain hierarchy is said to be cumulative; yet the inequality stipulated in the immediately preceding sentence seemed designed to obstruct cumulativity. I am doubtless missing something here, as elsewhere. Development of the full details of Jensen's arguments would be a strategic research project for someone, partly because of the remarkable depth and variety of prior theory which his arguments use and relate. Ramsey's theorem, Gödel's constructibility theory, Specker's constructions, Hailperin's finite axiomatization, and the work by Kreisel and Wang on finite axiomatizability, all figure in Jensen's reasoning. I am much gratified to see NF investigated so profoundly.

What of NFU as a working set theory? The assurance of consistency which recommends it also counts against it, since a set theory that can be proved consistent in elementary number theory is too weak to rest with. And indeed Jensen shows that it is too weak for the axiom of infinity, as well as for unstratified cases of mathematical induction. We can of course add these two desiderata; or just induction, since the axiom of infinity then follows.[3] Such an addition is unattractive, however, because of its *ad hoc* character – a character much at odds with the motivation of NF.

NF itself is likewise inadequate to unstratified mathematical induction, even though the axiom of infinity is provable in NF in Specker's diabolically devious way. One may therefore be moved still to supplement even NF with an *ad hoc* axiom schema of mathematical induction. To gain this same effect in a systematic rather than *ad hoc* fashion was a main motive for my supplementing the universe of NF with a domain of ultimate classes in *Mathematical Logic*. This move renders mathematical induction demonstrable independently of stratification. (Incidentally, it undoes the violation by NF of the axiom of choice.[4]) Accordingly the idea suggests itself of a system MLU, related to NFU as ML is related to NF.[5] MLU comes of ML, as did NFU from NF, by dropping C.

Wang proved that ML is consistent if NF is[6]; and his proof seems clearly to carry over, *mutatis mutandis,* to show that MLU is consistent since NFU is. This availability of a consistency proof, this time not just relative to another abstract set theory but outright, is a reason to expect poverty still on the part of MLU, but it is not a reason to expect MLU to be as poor as NFU. The difference is that Wang's proof of the consistency of ML relative to NF used more than elementary number theory; it assumed the consistency of classical analysis. When we carry his argument over to MLU, what we get is a consistency proof of MLU resting on classical analysis; whereas that of NFU needed only elementary number theory.

Both NFU and MLU raise the irksome technical question what to do about the null class. If either system is used where there is occasion to assume individuals, there is no way of saying which of the memberless things is the null class. Zermelo's system and others have faced the same problem, and the usual way of meeting it has been in effect to assume, inelegantly, a primitive name for the null class or a primitive predicate for individuality or for classitude. Fraenkel avoided this inelegance by sacrificing the individuals and imposing axiom C. I did likewise, in NF and ML, but made up the loss by allowing self-unit-classes to serve as individuals when desired. In NFU and MLU, however, C is unavailable. When individuals are wanted in these systems, have we no recourse more elegant than to assume a primitive name 'Λ' and an axiom '$x \notin \Lambda$' to govern it? There is, at least, a sort of way of explaining these additions away contextually. We can explain 'Λ' as an existentially quantified variable whose scope is '$(x)(x \notin \Lambda)$' in conjunction with the totality of our discourse, however extensive. This will be recognized as an application of an idea of Ramsey's. It is unattractive practically in depending upon our setting finite limits to our proposed discourse. But it does enable us to show, at any rate, that 'Λ' and '$x \notin \Lambda$' added no real strength to NFU and MLU.

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[3] See my *Set Theory and Its Logic*, § 41.
[4] See *op. cit.*, § 42.
[5] By ML, of course, I mean the system of the revised edition of *Mathematical Logic*, which incorporates Wang's repair of an earlier inconsistency.
[6] Hao Wang, 'A Formal System of Logic', *Journal of Symbolic Logic* **15** (1950) 25–32. Or see *Set Theory and Its Logic*, § 44.

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