CHAPTER SEVEN

Ontic Decision

§ 48. NOMINALISM AND REALISM[1]

One finds or can imagine disagreement on whether there are wombats, unicorns, angels, neutrinos, classes, points, miles, propositions. Philosophy and the special sciences afford infinite scope for disagreement on what there is. One such issue that has traditionally divided philosophers is whether there are abstract objects. *Nominalists* have held that there are not; *realists* (in a special sense of the word), or *Platonists* (as they have been called to avoid the troubles of 'realist'), have held that there are.

General definition of the term 'abstract', or 'universal', and its opposite 'concrete', or 'particular', need not detain us.[2] No matter if there are things whose status under the dichotomy remains enigmatic—"abstract particulars" such as the Equator and the North Pole, for instance; for no capital will be made of the dichotomy as such. It will suffice for now to cite classes, attributes, propositions, numbers, relations, and functions as typical abstract objects, and physical objects as concrete objects *par excellence,* and to consider the ontological issue as it touches such typical cases.

That more confidence should be felt in there being physical objects than in there being classes, attributes, and the like is not to

[1] A penultimate draft of much of Chapter VII was presented under the title "The Assuming of Objects" at the University of California, Berkeley, on May 13, 1959, as the Howison Lecture in Philosophy.

[2] Strawson's ingenious partial formulation of the distinction in "Particular and general," p. 257, presupposes a general notion of analyticity.

be wondered. For one thing, terms for physical objects belong to a more basic stage in our acquisition of language than abstract terms do. Concrete reference is felt as more secure than abstract reference because it is more deeply rooted in our formative past. For another thing, terms for intersubjectively observable physical things are at the focus of the most successful of unprepared communication, as between strangers in the marketplace. Surely such rapport tends to encourage confidence, however unconsciously, that one is making no mistake about his objects. Third, our terms for physical objects are commonly learned through fairly direct conditioning to stimulatory effects of the denoted objects. The empirical evidence for such physical objects, if not immediate, is at any rate less farfetched and so less suspect than that for objects whose terms are learned only in deep context. Note that whereas the first two causes for relative confidence in physical objects were causes only, this third one is a defensible reason.

Defensible but still contestable, on two counts: that it makes no case for physical objects of highly inferential sorts, and that it makes yet more of a case for sense data or sense qualities than for physical objects. Now the former of these two objections might be answered by appeal to continuity. If some physical objects are better attested than any abstract ones, then other and more conjectural physical objects are likewise more to be welcomed than abstract ones, because their acceptance along with those well-attested objects entails less loss of homogeneity, hence less loss of simplicity (*caeteris paribus*), than would acceptance of the abstract objects.

The other objection, insofar as it champions sense data in the sense of concrete sensory events (as against recurrent qualities), is an objection at most to physicalism and not to nominalism. But no matter; the likely rejoinder to the objection is independent of whether the subjective sensory objects envisaged are events or qualities. It is that no sufficient purpose is served by positing subjective sensory objects. This rejoinder would need sustaining on perhaps three counts, as follows, corresponding to three real or fancied purposes of positing such objects. (*a*) It would be argued that we cannot hope to make such objects suffice to the exclusion of physical objects. This point, urged in § 1, seems pretty widely acknowledged nowadays. (*b*) It would be argued (against Roderick Firth, for instance) that we do not need them in addition to physical objects, as means e.g. of reporting illusions and uncertainties. Thus one might claim that such purposes are adequately met by a propositional-attitude construction in which 'seems that' or the like is made to govern a subsidiary sentence about physical objects. One might claim that special objects of illusion are then no more called for than peculiar non-physical objects of quest or desire were called for in § 32. True, this argument is threatened by our high line on propositional attitudes in §§ 45 and 47; but perhaps appearance deserves no better, after all, than the *demimondain* status accorded to propositional attitudes generally. (*c*) It would be argued that we also do not need sensory objects to account for our knowledge or discourse of physical objects themselves. The claim here would be that the relevance of sensory stimulation to sentences about physical objects can as well (and better) be explored and explained in terms directly of the conditioning of such sentences or their parts to physical irritations of the subject's surfaces. Intervening neural activity goes on, but the claim is that nothing is clarified, nothing but excess baggage is added, by positing intermediary subjective objects of apprehension anterior to the physical objects overtly alleged in the spoken sentences themselves. The supposed function of sense-datum reports, in contributing a component of something like certainty to the formulations of empirical knowledge, may more realistically be assigned to observation sentences in the sense of § 10. These enjoy a privileged evidential position, in the directness of their correlation with non-verbal stimulation; yet they are not, typically, about sense data.

Points (*a*), (*b*), and (*c*) reflect my own general attitude. What perhaps basically distinguishes this from the attitude of sense-datum philosophers is that I favor treating cognition from within our own evolving theory of a cognized world, not fancying that firmer ground exists somehow outside all that. However, such cursory remarks as these on the philosophy of sense data can aspire at most to sort out issues and sketch a position; not to persuade.[3]

Let us now locate all this in its immediate context. What had been confronting us was the plea in behalf of sense data that if some physical objects are to be preferred to abstract ones on the score of comparative directness of association with sensory stimulation,

[3] For further representations against sense data, and bibliographical references, see Chisholm, *Perceiving*, pp. 117–125, 151–157, and Pasch, Ch. III. Further see § 54, below, and my note "On mental entities."

then sense data are to be preferred *a fortiori.* The answer proposed was predicated on utility for theory: that sense data neither suffice to the exclusion of physical objects nor are needed in addition. Now here we begin to witness the collision of two standards. Comparative directness of association with sensory stimulation was counted in favor of physical objects, but then we raised against the sense data themselves a second standard: utility for theory. Does one then have simply to weigh opposing considerations? No; on maturer reflection the picture changes. For let us recall the predicament in radical translation, which showed that a full knowledge of the stimulus meaning of an observation sentence is not sufficient for translating or even spotting a term. In our own language, by the same token, the stimulus meaning of an observation sentence in no way settles whether any part of the sentence should be distinguished as a term for sense data, or as a term for physical objects, or as a term at all. How directly the sentence and its words are associated with sensory stimulation, or how confidently the sentence may be affirmed on the strength of a given sensory stimulation, does not settle whether to posit objects of one sort or another for words of the sentence to denote in the capacity of terms.

We may be perceived to have posited the objects only when we have brought the contemplated terms into suitable interplay with the whole distinctively objectificatory apparatus of our language: articles and pronouns and the idioms of identity, plurality, and predication, or, in canonical notation, quantification. Even a superficially termlike occurrence is no proof of termhood, failing systematic interplay with the key idioms generally. Thus we habitually say 'for the sake of', with 'sake' seemingly in term position, and never thereby convict ourselves of positing any such objects as sakes, for we do not bring the rest of the apparatus to bear: we never use 'sake' as antecedent of 'it', nor do we predicate 'sake' of anything. 'Sake' figures in effect as an invariable fragment of a preposition 'for the sake of', or 'for 's sake'.

Let a word, therefore, have occurred as a fragment of ever so many empirically well-attested sentential wholes; even as a rather termlike fragment, by superficial appearances. Still, the question whether to treat it as a term is the question whether to give it general access to positions appropriate to general terms, or perhaps to singular terms, subject to the usual laws of such contexts. Whether to do so may reasonably be decided by considerations of systematic efficacy, utility for theory.

But if nominalism and realism are to be adjudicated on such grounds, nominalism's claims dwindle. The reason for admitting numbers as objects is precisely their efficacy in organizing and expediting the sciences. The reason for admitting classes is much the same. Examples have been noted (§ 43) of the access of power that comes with classes. A further example is Frege's celebrated definition of 'x is ancestor of y':

$$(z)(\text{if all parents of members of } z \text{ belong to } z \text{ and } y \in z \text{ then } x \in z).$$

Simplicity ensues, since we are spared separate, piecemeal provision for the things that classes provide. The efficacy of classes becomes yet more impressive when we find that they can be made to serve the purposes also of a great lot of further abstract objects of undeniable utility: relations, functions, numbers themselves (§§ 53–55).

We gain a perhaps more fundamental insight into the unifying force of the class concept when we observe how classes help us get by with quantifiers as the sole variable-binding operators. Thus think of '$\ldots z \ldots$' as some open sentence. *Concretion*[4] is the transformation that carries '$x \in \hat{z}(\ldots z \ldots)$' into '$\ldots x \ldots$'. Now let '$\Phi_x$' represent some variable-binding operator that builds sentences from sentences. If we suppose merely that 'Φ_x' is such that the substitutivity of concretion holds under it, we can drop 'Φ_x' in favor of a general term 'G'. For, take 'G' as true of just the classes y such that $\Phi_x(x \in y)$; then '$\Phi_x(\ldots x \ldots)$' can be rendered '$G\,\hat{x}(\ldots x \ldots)$'. Finally the operator of class abstraction in '$G\,\hat{x}(\ldots x \ldots)$' can be reduced to description, and description to quantifiers. (Cf. §§ 34, 38. But see also § 55.)

Closeness of association with stimulation has stood up poorly as an argument for giving physical objects preferential status. But something could still perhaps be salvaged from it. For, grant that the question whether to dignify given words as terms is a question whether to admit them freely to all term positions. Then instead of what was said earlier for physical objects, viz. that terms for them are fairly directly associated with sensory stimulation, perhaps we could say this: sentences fairly directly associated with sensory

[4] So called in my dissertation, Harvard, 1932, and in *A System of Logistic.*

stimulation exhibit terms for physical objects in all sorts of term positions, not just in rather special positions. It seems plausible that common terms for physical objects come out better by such a standard than abstract terms do.[5] But I shall not try to establish the point.

The case that emerged meanwhile for classes rested on systematic efficacy. Now it is certainly a case against nominalism's negative claims, but still it is no case against a preferential status for physical objects. In a contest for sheer systematic utility to science, the notion of physical object still leads the field.[6] On this score alone, therefore, one might still put a premium on explanations that appeal to physical objects and not to abstract ones, even if abstract objects be grudgingly admitted too for their efficacy elsewhere in the theory.

Nor let us scorn those two earlier causes for confidence in physical objects—the causes that were not recognized as reasons. One was that terms for such objects are so basic to our language; the other was that they are at the focus of such successful communication. To show why certain terms are felt as comfortable termini of explanation is not, after all, to render them otherwise.

§ 49. FALSE PREDILECTIONS. ONTIC COMMITMENT

We have considered the predilection for concrete objects and the case, despite such predilection, for admitting abstract objects. For symmetry let us now ponder the positive predilection for abstract objects; for it is not unknown.

An apparent reason for favoring physical objects was proximity to stimulation. This seemed all the more reason for favoring sensory objects of some sort, even sense qualities. Then, if attributes generally are held to be broadly analogous to sense qualities (as are the inferential particles of physics to common-sense bodies), the same appeal to continuity can be made in support of attributes as was made in support of the particles (§ 48). Here, I think, is one cause of the predilection that is sometimes manifested for attributes.

Not that I accept the line of reasoning. That argument for

[5] Cf. Alston, note 7.

[6] Cf. Strawson, *Individuals*, pp. 38–58.

sensory objects is offset, as urged in § 48, if we hold that such objects are neither adequate in lieu of physical objects nor helpful in addition to them. Moreover, to project non-sensory attributes purely on the analogy of sense qualities, hence as recurrent characters somehow of a subjective show within the mind, betrays surely a cavalier attitude toward psychological processes and a lack of curiosity about the mechanisms of behavior.

Such is one likely cause of a predilection for attributes (apart from motives of systematic utility). There is also a second. Some of us are carried away by the object-directed pattern of our thinking, to the point of seeking the gist of every sentence in things it is about. When a general term occurs predicatively alongside a name, the sentence thus formed will be seen by such a person as "about" not just the named object, but the named object and an attribute symbolized by the general term.[1] He will feel therefore that any general term for physical objects, such as 'round' or 'dog', simultaneously symbolizes an attribute. But then, he will reason, any argument for physical objects from the utility of such terms must, *ipso facto*, support attributes as well *and even better;* for the terms neatly symbolize one precise attribute apiece, while standing in no such pat correspondence to the indefinitely numerous physical objects that they purport to be true of. (Much the same argument can be used also to support classes instead of attributes, since a general term can as well be said to symbolize its extension as its intension if we appropriately shade the sense of 'symbolize'.)

In this reasoning the mistake is not just the initial one of overdoing the object matter. There is a subsequent fallacy in the idea that the utility of a word counts, of itself, in favor of all associated objects. A word can prove useful in such positions as to favor the assumption of objects for it to be true of, without thereby favoring the assumption of objects related to it in other ways, e.g., as extension or intension. Let us reflect on the mechanism.

Typical of the positions proper to general terms are the post-articular and the predicative. The one position is contained in

[1] Thus for Locke general terms were names of general ideas (Bk. II, Ch. XI, paragraph 9). Again Bergmann: "Who admits a single primitive predicate admits properties among the building stones of his world" ("Two types of linguistic philosophy," p. 430). And see Baylis, where he argues in effect that to understand a general term is to grasp its meaning, and hence that there are such meanings, or attributes. The fallacy of subtraction noted at the beginning of § 43, above, has doubtless encouraged the tendency to overdo 'about'.

singular terms; the other accompanies singular terms (which can be variables). These singular terms in turn are marked as singular terms by their occurrences as subjects of other predicatively occurring general terms, notably '=', and in variable-binding operators. And where do objects come in? The purported objects of whatever sort, concrete or abstract, are just what the singular terms in their several ways name, refer to, take as values.[2] They are what count as cases when, quantifying, we say that everything, or something, is thus and so. So when on grounds of systematic efficacy we decide to allow a word—'glint', say, to take a debatable case—full currency as general term, the effect is only that the glints, not glinthood or glintkind, are made to count as objects.

Actually, the effect is not even quite that much; for a general term in good standing can still, like 'unicorn', be true of nothing. But what typically happens is the following. Already while we debate whether our sentences may best be so analyzed and extended as to count 'glint' a full-fledged general term, we have before us certain incompletely analyzed but useful truths of theory or observation that contain the word; and then our taking 'glint' as a general term settles the analysis of these sentences in such a way that some of them come to affirm or imply '$(\exists x)(x$ is a glint$)$'.

So if 'round' and 'dog' have acquitted themselves to the glory of physical objects, they have done so as general terms true of physical objects and not as singular terms naming attributes or classes. The case for attributes or classes remains open as a separate question, however analogous. The general terms relevant to it are not 'round', 'dog', and the like, but 'trait', 'species', and the like; and the relevant singular terms are not such as 'Sputnik I' and 'Fido', but such as 'roundness', 'caninity', 'dogkind'.

The offenders who have called forth the past few pages are those who, through a confusion that I have just now tried to clear up, take it for granted that everyone in his use of general terms talks directly of attributes (or classes), *ipso facto* and willy nilly. The offenders are not those who make a considered argument for the existence of an attribute or class for every general term. Such an argument, coming under the head of what was tolerantly viewed in § 48, would be predicated on the systematic efficacy of admitting abstract general and perhaps abstract singular terms and using

[2] See § 40, note 1.

them in such a way as to bring attributes or classes into the universe of discourse as values, in effect, of the variables of quantification. The merits of such a course are considered further in §§ 43 and 55.

The offenders may be depended upon to dismiss the distinction between concrete general terms like 'round' and abstract singular terms like 'roundness' as an insignificant quirk of grammar. Now let me not seem to be making capital of any pedantic distinction in word forms. This distinction is only a convenient and dispensable way of marking an underlying difference that can be uncovered anyway in a distinction of functions, as lately outlined. But I venture to say that a failure to appreciate the underlying difference correlates nicely with the dismissal of the verbal distinction.

Along with the offenders last touched on there are others who, likewise making light of the distinction between abstract singular and concrete general terms, decide *against* abstract objects. Apparently these thinkers have appreciated, for whatever reasons, that concrete general terms carry no commitment to attributes or classes, and then have concluded the same for the corresponding abstract singular terms, by dint of drawing no distinction. This line of thought derives wishful vigor from a distaste for abstract objects coupled with a taste for their systematic efficacy. The motivation has proved sufficient to induce remarkable extremes. We find philosophers allowing themselves not only abstract terms but even pretty unmistakable quantifications over abstract objects ("There are concepts with which . . . ," ". . . some of which propositions . . . ," ". . . there is something that he doubts or believes"), and still blandly disavowing, within the paragraph, any claim that there are such objects.[3]

Pressed, they may explain that abstract objects do not exist the way physical ones do. The difference is not, they say, just a difference in two sorts of objects, one in space-time and one not, but a difference in two senses of 'there are'; so that, in the sense in which there are concrete objects, there are no abstract ones. But then there remain two difficulties, a little one and a big one. The little one is that the philosopher who would repudiate abstract objects seems to be left saying that there are such after all, in the sense of

[3] See Church, "Ontological commitment," for discussion of illustrative texts from Ayer and Ryle.

'there are' appropriate to them. The big one is that the distinction between there being one sense of 'there are' for concrete objects and another for abstract ones, and there being just one sense of 'there are' for both, makes no sense.[4]

Such philosophical double talk, which would repudiate an ontology while enjoying its benefits, thrives on vagaries of ordinary language. The trouble is that at best there is no simple correlation between the outward forms of ordinary affirmations and the existences implied. Thus, granted that the construction exemplified by 'Agnes has fleas' can very often be accorded the forthrightly existential sense intended by '$(\exists x)(Fx$ and $Gx)$', there remain abundant cases like 'Tabby eats mice' (§ 28) and 'Ernest hunts lions' (§ 32) that cannot. Reflective persons unswayed by wishful thinking can themselves now and again have cause to wonder what, if anything, they are talking about.

In our canonical notation of quantification, then, we find the restoration of law and order. Insofar as we adhere to this notation, the objects we are to be understood to admit are precisely the objects which we reckon to the universe of values over which the bound variables of quantification are to be considered to range. Such is simply the intended sense of the quantifiers '(x)' and '$(\exists x)$': 'every object x is such that', 'there is an object x such that'. The quantifiers are encapsulations of these specially selected, unequivocally referential idioms of ordinary language. To paraphrase a sentence into the canonical notation of quantification is, first and foremost, to make its ontic content explicit, quantification being a device for talking in general of objects.

The moot or controversial part of the question of the ontic import of a sentence may of course survive in a new guise, as the question how to paraphrase the sentence into canonical notation. But the change of guise conveniently shifts the burden of claims and disavowals. Futile caviling over ontic implications gives way to an invitation to reformulate one's point in canonical notation. We cannot paraphrase our opponent's sentences into canonical notation for him and convict him of the consequences, for there is no synonymy; rather we must ask him what canonical sentences he is pre-

[4] Cf. § 27. But the familiar vague notion that the assumption of abstract entities is somehow a purely formal expedient, as against the more factual character of the assumption of physical objects, may still not be wholly beyond making sense of; see Putnam, "Mathematics and the existence of abstract entities."

pared to offer, consonantly with his own inadequately expressed purposes. If he declines to play this game, the argument terminates. To decline to explain oneself in terms of quantification, or in terms of those special idioms of ordinary language by which quantification is directly explained, is simply to decline to disclose one's referential intent. We saw in our consideration of radical translation that an alien language may well fail to share, by any universal standard, the object-positing pattern of our own; and now our supposititious opponent is simply standing, however legalistically, on his alien rights. We remain free as always to project analytical hypotheses (§§ 15 f.) and translate his sentences into canonical notation as seems most reasonable; but he is no more bound by our conclusions than the native by the field linguist's.[5]

§ 50. ENTIA NON GRATA

The resort to canonical notation as an aid to clarifying ontic commitments is of limited polemical power, as just now explained. But it does help us who are agreeable to the canonical forms to judge what we care to consider there to be. We can face the question squarely as a question what to admit to the universe of values of our variables of quantification.

Economy is a consideration, but economy of theory and not just

[5] For more on quantification as avenue of ontic commitment see my *From a Logical Point of View*, Essays 1 and 6. On pp. 19 and 103 thereof it is stressed that I look to variables and quantification for evidence as to what a theory says that there is, not for evidence as to what there is; but the point can be missed, as by Henderson, pp. 279 f. — A more accountable misapprehension is that I am a nominalist. I must correct it; my best efforts to write clearly about reference, referential position, and ontic commitment will fail of communication to readers who, like Mates ("Synonymity," p. 213) and Braithwaite (review), endeavor in all good will to reconcile my words with a supposed nominalist doctrine. In all books and most papers I have appealed to classes and recognized them as abstract objects. I have indeed inveighed against making and imputing platonistic assumptions gratuitously, but equally against obscuring them. Where I have speculated on what can be got from a nominalistic basis, I have stressed the difficulties and limitations. True, my 1947 paper with Goodman opened on a nominalist declaration; readers cannot be blamed. For consistency with my general attitude early and late, that sentence needs demotion to the status of a mere statement of conditions for the construction in hand; cf. *From a Logical Point of View*, top of p. 174.

of objects. Also some objects may be preferable to others in the way suggested for physical objects late in § 48: representative sentences in which they are treated as objects may be relatively closely associated with sensory stimulation.

We have looked into the benefits of admitting physical objects and classes (§ 48), though there will be more to say of classes (§ 55). We considered also the claims and the difficulties of attributes and propositions (§§ 42 f.), and the weakness of the case for sense data (§§ 1, 48). At the extreme, finally, are the sakes and behalves. No one wants these, but the form of the argument for their exclusion is instructive. It is that 'sake' and 'behalf' have their uses only in the clichés 'for the sake of' and 'in behalf of' and their variants; hence these clichés can be left unanalyzed as simple prepositions. (From the point of view of canonical notation, prepositions in turn ordinarily get bundled off into relative terms; cf. § 22.)

Units of measure turn out somewhat like sakes and behalves. 'Mile', 'minute', 'degree Fahrenheit', and the like resemble 'sake' and 'behalf' in being *defective* nouns: they are normally used only in a limited selection of the usual term positions. Their defectiveness, though less extreme than that of 'sake' and 'behalf', is easily exposed in absurd interrogation. Are miles alike? If so, how can they count as many? And if they cannot, what of the two hundred between Boston and New York?

Questions about identity of attributes or of propositions are in their turn less absurd, on the face of them, than these about identity of miles. Still the lack of a standard of identity for attributes and propositions can be viewed similarly, as a case of defectiveness on the part of 'attribute' and 'proposition'. Philosophers undertook, however unsuccessfully, to supply this defect by devising a standard of identity, because they were persuaded of the advantages, in systematic utility or whatever, of taking 'attribute' and 'proposition' as full-fledged terms and so admitting attributes and propositions to the universe of discourse. This line is debatable on its specific merits, and we have debated it. The case of 'mile', 'degree Fahrenheit', and the like is clearer: no purpose is served by making units of measure accessible to variables of quantification. We can adequately accommodate these nouns as parts of relative terms 'length in miles', 'temperature in degrees Fahrenheit'.[1]

[1] So Carnap, *Physikalische Begriffsbildung.*

Just as the relative term 'author' is true of this and that man relative to this and that book, so 'length in miles' is to be understood as true of this and that number relative to this and that body or region. Thus instead of 'length of Manhattan = 11 miles' we would now say 'length-in-miles of Manhattan = 11' (form 'F of $b = a$') or '11 is length-in-miles-of Manhattan' (form '$F\breve{a}b$').

This leaves us recognizing numbers as objects. For the numeral '11' figures here as a singular term, on a par with 'Manhattan'. If we were to push forward to minimum canonical notation by eliminating singular terms as in Chapter V we would find our quantifiers calling for the number and the island unmistakably enough:

$$(\exists x)(\exists y)(x \text{ is-11 and } y \text{ is-Manhattan and } x \text{ is-length-in-miles-of } y).$$

And indeed we may expect numbers to be very much wanted, not only for this example, as values of our variables; they are nearly as useful as classes.

In *possible* concrete objects, unactualized possibles, we have another category of doubtful objects whose doubtfulness can be laid to defective nouns, with as good reason at least as in the case of attributes and propositions. For here again, and more glaringly than in the case of intensions, there is perplexity over identity; cf. § 8. Even when a position is specified, as in 'the possible new church on that corner', 'the possible hotel on that corner', the identity of position does not make the possible objects identical. Happily we can cut through all this, sometimes by retreating to universals as in § 8, and more usually by just absorbing the 'possible' of 'possible object' appropriately into the context and so not treating 'possible object' as a term. A sentence about possible churches can usually be paraphrased satisfactorily enough into a sentence that treats of churches and is governed, as a whole, by a modal operator of possibility. One may still ask what kind of modality is wanted, how to make sense of it, and how to cope with other problems that the modalities of one or another sort have been known to raise; but talk of possible objects would have been no better off with respect to such questions.

The notion of possible objects has been encouraged by two philosophical quandaries. One of them is raised by the verbs 'hunting', 'wanting', and the like, which cannot in general be looked upon as relating the agent to actual objects (cf. §§ 28, 32). Possible lions, possible unicorns, possible sloops suggest themselves as surrogate objects for such activities. But these matters can be handled more illuminatingly by paraphrase, as seen, into the idioms of propositional attitude. One is left with the problems of propositional attitude, but those, unlike the vagaries of unactualized possibles, are with us anyway.

The other quandary is raised by terms in want of objects: what are we talking about when we say there are no unicorns, or that there is no such thing as Pegasus? Partly this quandary comes of being carried away by the object-directed pattern of our thinking—if not to the extreme talked of in § 49, at least to the point of trying to see every sentence as "about" certain objects. Actually 'unicorn' and 'Pegasus' can be perfectly good terms, well understood in that their contexts are well enough linked to sensory stimulation or to intervening theory, without there being unicorns or Pegasus. Mainly it is on singular terms like 'Pegasus' that the quandary has centered rather than general ones like 'unicorn'; for it is there that under ordinary usage the truth-value gaps set in (§ 37), in a philosophically uncomfortable way. However, the canonical expedient of reparsing singular terms regularizes these matters and so ends, one may hope, any temptation to venture out on the morass of unactualized possibles.[2]

The notions of possible object and proposition are two that were encouraged by philosophical quandaries. A third is that of fact. The word 'fact' is commonplace enough, but where the philosophical motivation enters is in choosing to admit facts as objects rather than fob the word off with the lower-grade sort of treatment accorded 'sake' and 'mile'.

Part of what encouraged admission of propositions was a wish for eternal truth-value vehicles independent of particular languages (§ 40). Part of what encourages admission of facts is perhaps a wish to defer the question what makes a sentence or proposition true: those are true that state facts. Another force that has encouraged both acceptances is, again, the tendency to be carried away by object-directed thinking: a tendency, in this case, to liken sentences to names and then posit objects for them to name. It is perhaps where this force is the dominant one that we encounter readiness, as we occasionally do, to identify facts with propositions (viz., with some or all of the true ones).

[2] See Russell, "On denoting".

An additional connotation that often invests the word 'fact', both in philosophy and in lay usage, is that of unvarnished objectivity plus a certain accessibility to observation. In philosophical usage this connotation is sometimes adopted and widened in such wise that facts come to be posited corresponding to all the "synthetic" truths and withheld only from the "analytic" ones. So here that same analytic-synthetic dichotomy intrudes which we have found so dubious (§ 14); and it intrudes in a most implausibly absolute guise, independent apparently of all choice of language. The disarmingly commonplace ring of the word 'fact' comes even to lend the dichotomy a spurious air of intelligibility: the analytic sentences (or propositions) are the true ones that lack factual content.

There is a tendency—not among those who take facts as propositions—to think of facts as concrete. This is fostered by the commonplace ring of the word and the hint of bruteness, and is of a piece, for that matter, with the basic conception that it is facts that make sentences true. Yet what can they be, and be concrete? The sentences 'Fifth Avenue is six miles long' and 'Fifth Avenue is a hundred feet wide', if we suppose them true, presumably state different facts; yet the only concrete or at any rate physical object involved is Fifth Avenue. I resolved (§ 48) not to cavil over 'concrete', but I suspect that the sense of 'concrete' in which facts are concrete is not one that need endear them to us.

Facts, moreover, are in the same difficulty over a standard of identity as propositions were seen to be. And surely they cannot be seriously supposed to help us explain truth. Our two sentences last quoted are true because of Fifth Avenue, because it is a hundred feet wide and six miles long, because it was planned and made that way, and because of the way we use our words; only indirection results from positing facts, in the image of sentences, as intermediaries. Probably no such temptation would arise if the word were not already there performing an overlapping though unphilosophical function for ordinary discourse.

In ordinary usage 'fact' often occurs where we could without loss say 'true sentence' or (if it is our way) 'true proposition'. But its main utility seems to be rather as a reinforcement of the flimsy 'that' of propositional abstraction (§ 34). It is wanted there merely because of the idiomatic unnaturalness, in many substantival positions, of a pure 'that'-clause. (Yet it is limited in this syntactical service to 'that'-clauses that are taken to be true; for 'fact' persists in imputing truth.) It has a further use still in abbreviated cross-reference: we often manage to avoid repeating a long previous affirmation by saying 'that fact'. Now so far as these uses go there is no call to posit facts, certainly not over and above propositions, nor any difficulty in absorbing or paraphrasing away the word. Nor have the peculiarly philosophical appeals to fact impressed us.

§ 51. LIMIT MYTHS

We were able to abjure sakes, measures, unactualized possibles, and facts without a pang, having satisfied ourselves that to admit them would serve no good purpose. Examples are not far to seek, on the other hand, of supposed objects that are absurd or troublesome and yet such that their banishment from the domain of values of our variables threatens to impair our apparatus. A classic example of such conflict and its resolution is afforded by the *infinitesimal.*

The notion of the infinitesimal emerged from the question how to deal with rates, e.g. instantaneous velocities. What does it mean to say of a particle that at a momentary time t its velocity is ten feet a second? Not precisely that during some actual period of s seconds (say a hundredth of a second), spanning t, the particle traverses the appropriate distance of $10s$ feet (a tenth of a foot); for the velocity may change during that and every poriod. Newton and Leibnitz answered, in their differential calculus, by positing infinitesimals: quantities infinitely close to zero and yet, absurdly enough, distinct from one another. A particle traveling ten feet a second at time t was said to traverse a certain infinitesimal distance d at time t, and a particle traveling twenty feet a second at time t was said to traverse a different infinitesimal distance $2d$ at time t, the elapsed time in both cases being zero. Though the idea of infinitesimals was absurd, the differential calculus, in which infinitesimals were reckoned as values of the variables, gave true and valuable results.

The conflict was resolved by Weierstrass, who showed by his theory of limits how the sentences of the differential calculus could be systematically reconstrued so as to draw only on proper numbers as values of the variables, without impairing the utility of the calculus. On his analysis, to say that the particle is traveling ten feet a second at t is to say that by narrowing the time span s around t you can get the distance as close to $10s$ as you like; i.e.,

(x)(if $x > 0$ then $(\exists s)$(distance in feet traversed during s seconds around t is between $10s - x$ and $10s + x$)).

A case with certain parallels to that of the infinitesimals is presented by the *ideal objects* that seem to be talked of in expositions of mechanics: mass points, frictionless surfaces, isolated systems. Just as infinitesimal numbers were contrary to arithmetic, so a point with mass, a surface without friction, or a system immune to outside forces would be contrary to physical theory. At the same time the elementary laws of mechanics are regularly formulated in terms of these ideal objects, as was once the differential calculus in terms of infinitesimals.

The appeal to ideal objects in mechanics occurs regularly through universal conditionals: thus (x)(if x is a mass point then . . .). The non-existence of ideal objects consequently does not falsify mechanics; it leaves such sentences vacuously true for lack of counter-instances. Thus mechanics may seem better off with respect to ideal objects than the differential calculus had once been with respect to the infinitesimals. But the contrast is superficial. What should worry us about the ideal objects is the following. If by physical laws there are no such objects, and hence by physical laws all universal conditionals concerning them are trivially true, then how is it that some of these conditionals, rather than others, still evidently impart useful scientific theory?

This quandary over ideal objects, like that over infinitesimals, has its solution in the theory of limits. When one asserts that mass points behave thus and so, he can be understood as saying roughly this: that particles of given mass behave the more nearly thus and so the smaller their volumes. When one speaks of an isolated system of particles as behaving thus and so, he can be understood as saying that a system of particles behaves the more nearly thus and so the smaller the proportion of energy transferred from or to the outside world. This, broadly speaking, is how one's succinct talk of ideal objects would presumably be paraphrased when challenged.

The doctrine of ideal objects in physics is "symbolic," as this word is used by literary critics, psychoanalysts, and philosophers of religion. It is a deliberate myth, useful for the vividness, beauty, and substantial correctness with which it portrays certain aspects of nature even while, on a literal reading, it falsifies nature in other respects. It is useful also for the simplicity that it brings to certain computations. Now simplicity, in a theory that squares with observation sentences so far as its contacts with them go, is the best evidence of truth we can ask; no better can be claimed for the doctrines of molecules and electrons. What makes for the mythicalness of the doctrine of ideal objects, as against the literal truth (by today's lights) of the doctrines of molecules and electrons, is that the former works its simplifications in a limited domain of statements at the cost of more seriously complicating a more inclusive domain. When we paraphrase our talk of ideal objects in the Weierstrassian spirit as sketched above, we are merely switching from a theory that is conveniently simple in a short view and complex in a long view to a theory of opposite character. Since the latter, if either, is the one to count as true, the former gets the inferior rating of convenient myth, purely symbolic of that ulterior truth. Definition, meanwhile, or rule of paraphrase, enables us to enjoy the best of both worlds. (Cf. § 39.)

It is once more relevant to remind ourselves that paraphrase makes no synonymy claim. It only coordinates the uses one makes of diverse theories for diverse advantages. Hold, if you will, that the myth of ideal objects is merely convenient and not quite true, and that the paraphrase is what is true; or hold, if you will, that the myth of ideal objects is strictly true by virtue of having the paraphrase as its true meaning. Either of these philosophies is acceptable as long as both are recognized as loose formulations of one and the same situation; that they seem opposed is due to fancying, in 'true meaning', a more than impressionistic manner of speaking.

Much the same relationship that we have been observing between the doctrine of ideal objects and full-dress physical theory may be said also nowadays to obtain between Newtonian physics and relativity theory. Being simpler, Newton's laws are conveniently retained for use where the departures from strict truth thus entailed are slight enough not to obstruct one's purposes. Thus, in the same sense in which we called the doctrine of ideal objects a convenient myth, symbolic of truths other than its manifest context, we might equally call Newtonian physics a convenient myth, symbolic of that ulterior truth (by today's lights) which is relativity theory. The paraphrase of myth into literally accepted theory would proceed, here also, along Weierstrassian lines: each Newtonian sentence, to the effect that bodies behave thus and so, would be treated as saying that bodies behave the more nearly thus and so the smaller their relative velocities.

Such reflections partake of what churchmen call higher criticism. They are directed at reconciling some limitedly useful lesser theory with a more sweeping theory which on a literal-minded reading it contradicts: the infinitesimal calculus with the classical mathematics of number, the mechanics of ideal objects with general physics, and Newton's physics with Einstein's. But meanwhile let us keep in mind also that knowledge normally develops in a multiplicity of theories, each with its limited utility and each, unless it harbors more danger than utility, with its internal consistency.[1] These theories overlap very considerably, in their so-called logical laws and in much else, but that they add up to an integrated and consistent whole is only a worthy ideal and happily not a prerequisite of scientific progress. The continuing utility of the mechanics of ideal objects and of Newtonian mechanics is ample reason for treasuring and teaching these theories, whatever their conflicts with more august ones; and the same was true of the infinitesimal calculus before Weierstrass. This said, let the reconciliations proceed; each such step advances our understanding of the world.

§ 52. GEOMETRICAL OBJECTS

Traditionally geometry was the theory of relative position. For Poincaré and others influenced by the pluralism of non-Euclidean geometries, the geometries were a family rather of uninterpreted theory-forms, called geometries only because of structural resemblances to Euclid's original theory of position. The question of the nature of the objects of geometries in the latter sense need not detain us, secured as it is against an answer. But meanwhile geometry also in something like the traditional sense continues as a handmaiden, by whatever name, of natural science. Its objects would appear to be points, curves, surfaces, and solids, conceived as portions of a real space that bathes and permeates the physical world. They are objects which we are tempted to admit on a par with physical objects as values of our variables of quantification, as

[1] See Conant, pp. 98 ff.

when we say that Boston, Buffalo, and Detroit are cut by a great circle of the earth.

The objects of geometry can for some purposes be adequately explained away in the manner already contemplated for the ideal objects of mechanics; for we may think of points, curves, and geometrical surfaces as ideally small particles, ideally slender wires, and ideally thin sheets. This treatment conforms well enough to the pure universal statements of geometry—statements to the effect merely that any geometrical objects interrelated in such and such ways are interrelated in such and such further ways. But it ill fits the existential statements of geometry, which require there to be points, curves, surfaces, and solids everywhere.

May we then hold to the naïve view? Here we have a dualistic theory of spatiotemporal reality, whose two sorts of objects, physical and geometrical, interpenetrate without conflict. There is no conflict simply because the physical laws are not extended to the geometrical objects.

But if such a plan is tolerable here, why could we not equally in § 51 have admitted the ideal objects of mechanics in a single spatiotemporal universe along with the full-fledged physical objects, simply exempting them from some of the laws? Is it just that these two categories would be intuitively too much alike to make the separation of laws seem natural? No. There is a more substantial reason why mass points and the like, as objects supplementary to the full-fledged bodies, should be less welcome than geometrical objects. No sense has been made of their date and location. Evidently, to judge by what is said of them, mass points and such ideal objects are supposed to be in space-time of some sort, ours or another; but just where is each? And, if we waive location, there supervenes a perplexity of identity: when do mass points (or frictionless surfaces, etc.) count as one and when as two? Talk of ideal objects in mechanics, significantly enough, tends not to turn on such questions. There is in this circumstance strong reason to define the ideal objects away—say along the Weierstrassian lines of § 51—rather than to keep them and try to settle the perplexities of position or identity by multiplying artificialities. On the other hand geometrical objects raise no such evident problems of position or identity; they are positions outright.

But are we prepared to admit absolute positions, and therewith an absolute distinction between rest and motion? Is not motion relative rather, so that what would count from one point of view as an identical position twice over would count from another point of view as two distinct positions? No doubt. However, we can accommodate this relativistic scruple simply by adding a dimension and speaking of positions not in space but in space-time. Distinct point-instants are distinct absolutely, regardless of the relative movement of the point of view.

If motion is relative, then obviously the question whether a given spatiotemporal region (or aggregate of point-instants) is constant in shape through time, or whether its internal distances readjust through time, will depend on the relative movement of the point of view; and so will the question whether its shape at a time is spherical or elongated. But this is to say only that shape-at-a-time is relative to frames of reference; the geometrical objects whose shapes are concerned remain absolute aggregates of point-instants, however shaped and however specified.

Whether it was better to stay within the three spatial dimensions for our geometrical objects, or better to look beyond space into space-time for them, turned on whether it was wise to assume an absolute distinction between rest and motion. This question, in turn, is the question what theory will best systematize the data of physics. Thus we may fairly say that the question of the nature of the geometrical objects is, like the question of the nature of the elementary particles of physics, a question of physical theory. Granted, laboratory data only incline and do not constrain us in our geometrizing; but likewise they only incline and do not constrain us in our invention of a physical theory. Let fashion and terminology not mislead us into viewing geometry too differently from physics.

And the fact is that Einstein's physical theorizing included geometrical decisions also beyond the relativity of motion. Considerations of overall theoretical simplicity of physical theory induced him to settle for a non-Euclidean form of geometry, simpler though the Euclidean is when considered apart. Accepting then this four-dimensional non-Euclidean geometry along with relativity physics as the literal truth (by today's lights), we may view Euclidean geometry on a par with Newtonian physics (cf. § 51) as a convenient myth, simpler for some purposes but symbolic of that ulterior truth. The geometrical objects of Euclidean geometry then take on, relative to the "real" geometrical objects of the non-Euclidean "true" geometry of ideal objects, the status of manners of speaking, limit myths, explicable in principle by paraphrasing our sentences along Weierstrassian lines.

There remain also other geometries, other in various ways. There are the more abstract ones, culminating in topology, which treat of the geometrical objects in decreasingly specific detail. These geometries raise no further ontic problem, for their objects can be taken to be our same old geometrical objects; we can look upon these geometries as merely saying less about them.

And there remain geometries that are not just more abstract than, but actually contrary to, our "true" geometry of relativity physics. Shall we rate these as simply false? or seek ways of reconstruing their words that would make them true after all, whether of our same old geometrical objects or of something else? We need do none of this; an uninterpreted theory-form can be worthy of study for its structure without its talking about anything. When it is brought into connection with the quantifiers of a broader scientific context in such a way as to purport to talk unfeignedly of objects of some sort, then is time enough to wonder what the objects are.

Up to now I have defended geometrical objects not because I think it is best to admit them as part of the furniture of our universe, but only in order to exhibit relevant considerations. Meanwhile there remains, obviously, an objection to geometrical objects, on the score of economy of objects. Let us now consider how we may manage without them.

The only sentences we need try to paraphrase, for elimination of reference to geometrical objects, are those that cannot be facilely dismissed as gibberish of an uninterpreted calculus: those that contribute, rather, like sentences about the Equator or the one about Boston, Buffalo, and Detroit, to discourse about the real world outside geometry. Now sentences about the Equator can all probably be satisfactorily paraphrased into forms in which 'Equator' has the immediate context 'nearer the Equator than'; and these four words can be treated as a simple relative term or even defined away in terms of centrifugal force or mean solar elevation. The more serious cases are sentences which, like that about Boston, Buffalo, and Detroit, ostensibly call for a geometrical object as value of a variable of quantification.

But the reference to geometrical objects in such cases is an auxiliary merely to what we want to say about the movements and spatiotemporal relations of bodies; and we can hope to by-pass the geometrical objects by falling back on a relative term of distance (cf. § 50), or spatiotemporal interval, conceived as relating physical bodies and numbers. This course involves accepting numbers as objects along with bodies, but spares us assuming geometrical objects in addition. The elements are thus simplified. The practical convenience of geometrical objects can still be preserved by reinstating definitionally (cf. § 39) whatever idioms we analyze away.

The elimination can be systematized along the lines of analytical geometry. In its minimum essentials the idea is as follows, for our four-dimensional space-time. We pick five particle-events *a, b, c, d, e,* not quite at random. (The requirement is merely that they mark the vertices of a full-fledged four-dimensional "hypersolid," rather than all lying in a plane or a three-dimensional solid.) We can think of the five as given by proper names, or, what comes to the same (cf. § 37), by general terms true uniquely of each. Now every point (or point-instant) in space-time is uniquely determined once we specify its "distance" (or interval: the analogue of distance in four-dimensional space-time) from each of the five. The position of a body in space-time is therefore determined by the distance of its various extremes from each of the five reference particle-events. The attribution of (four-dimensional) shapes to bodies can be paraphrased as attribution of appropriate arithmetical conditions to the classes of ordered quintuples of numbers that fix the bodies' boundaries. Correspondingly for the attribution of collinearity or other geometrical relations.

We could take the further step, if we wish, of nominally restituting geometrical objects by *identifying* points (actually point-instants) with the appropriate ordered quintuples of numbers, and identifying the rest of the geometrical objects with the classes of their constituent points in that sense. Whether to speak of geometrical objects as by-passed or as reconstrued is a matter of indifference.

The five-point type of coordinate system thus simply described would be prohibitively awkward in practice. The least of its inelegances is that it ill exploits its numerical resources. For instance, distances from *a* and *b* that add up to less than the distance from *a* to *b* would never be wanted in the same quintuple of numbers. The compatible distances from the five points make up a quite special and not quickly recognizable class of quintuples. The more strictly Cartesian scheme, of fixing each point by its distance from each of various mutually perpendicular planes, is far superior: it gets by with quadruples of numbers instead of quintuples, it wastes no quadruples, and, above all, it correlates the important geometrical conditions with far simpler arithmetical conditions than our five-point method would do. Certainly one would want to set up a system of Cartesian coordinates. But its construction, given as starting point only a distance measure and selected reference particles, is a long story. The five-point method is a more readily describable one to the same theoretical effect, and suffices for conveying some concrete sense of what the elimination of geometrical objects means.

To the same end it may be worth while now to turn more specific still: to cut through the whole apparatus of systematic reference points and quadruples or quintuples of real numbers, and consider rather how some very definite geometrical remark about physical bodies, considered simply by itself, might be paraphrased into terms of distance without geometrical objects. Let us take the sentence to the effect that there is a line passing through the bodies A, B, and C, where B is the one in the middle.

A simple paraphrase that does not quite fill the bill is this: there are particles x, y, and z, respectively in A, B, and C, such that the distance from x to z is the sum of those from x to y and from y to z. The trouble with it is that it makes no allowance for gaps between the component particles (or particle-events) of a body. It does not allow for the possibility that every line through A, B, and C that hits particles of both A and C passes between particles of B, hitting none.

There is a way through this difficulty which may most readily be grasped in principle if we suppose that we are working in just two dimensions. So A, B, and C are now clusters of dots on a page; and we want to say in effect that there is a geometrical line through A, B, and C, without actually referring to any objects but the dots and their clusters, nor relating them otherwise than in point of distance. We suppose still that of these three clusters (if a line does cut through them) the middle one is B. What we want in effect to say, then, though within the allotted means, is that there are a dot x of A, a dot z of C, and dots y and y' of B (same or different), such that the geometrical line xz hits y or y' or passes between them. But xz hits y or y' or passes between them if and only if the area of the triangle xyz plus the area of the triangle $xy'z$ equals the area of the triangle xyy' plus the area of the triangle zyy'. But the area of a triangle is a known function f of the lengths of the sides. The following, then, is a formulation to our purpose, where 'dxy' means 'distance from x to y':

> There are a dot x of A, a dot z of C, and dots y and y' of B such that $f(dxy, dyz, dxz) + f(dxy', dy'z, dxz) = f(dxy, dyy', dxy') + f(dzy, dyy', dzy')$.

§ 53. THE ORDERED PAIR AS PHILOSOPHICAL PARADIGM

A pattern repeatedly illustrated in recent sections is that of the defective noun that proves undeserving of objects and is dismissed as an irreferential fragment of a few containing phrases. But sometimes a defective noun fares oppositely: its utility is found to turn on the admission of denoted objects as values of the variables of quantification. In such a case our job is to devise interpretations for it in the term positions where, in its defectiveness, it had not used to occur.

We shall find that a peculiarly clear-cut case of the latter sort is provided by the ordered pair, a device for treating objects two at a time as if we were treating objects of some sort one at a time. A typical use of the device is in assimilating relations to classes, by taking them as classes of ordered pairs.[1] The father relation becomes the class of just those ordered pairs which, like ⟨Abraham, Isaac⟩,[2] have a male and one of his offspring as their respective components.

Now just what is an ordered pair? See Peirce's answer:

> The Dyad is a mental Diagram consisting of two images of two objects, one existentially connected with one member of the pair, the other with the other; the one having attached to it, as representing it, a Symbol whose meaning is "First," and the other a Symbol whose meaning is "Second."[3]

[1] Relations as they concern us here are "relations-in-extension." They are to relations-in-intension (§ 43) as classes are to attributes. Anyone who persists in recognizing intensional objects can take relations-in-intension, analogously, as attributes of ordered pairs.

[2] The traditional notation '$x;y$' of Frege and Peano, for the ordered pair of x and y, is giving way nowadays to '$\langle x, y\rangle$'.

[3] Peirce, vol. 2, paragraph 316.

We do better to face the fact that 'ordered pair' is (pending added conventions) a defective noun, not at home in all the questions and answers in which we are accustomed to imbed terms at their full-fledged best.

For illustrative purposes a special virtue of the notion of ordered pair is that mathematicians pretty deliberately introduced it, subject in effect to the single postulate:

(1) If $\langle x, y\rangle = \langle z, w\rangle$ then $x = z$ and $y = w$.

Pending added conventions, the expressions of the form '$\langle x, y\rangle$' are, like 'ordered pair' itself, defective nouns, their normal occurrences being limited to special sorts of context where (1) can be exploited.

Yet it is central to the purposes of the notion of ordered pair to admit ordered pairs as objects. If relations are to be assimilated to classes as classes of ordered pairs, ordered pairs must be available on a par with other objects as members of classes. The demands of further uses in mathematics of the notion of ordered pair are similar; in every case the very point of the ordered pair is its role of object—of a single object doing the work of two. A notion of ordered pair would fail of all purpose without ordered pairs as values of the variables of quantification.

The problem of suitably eking out the use of these defective nouns can be solved once for all by systematically fixing upon some suitable already-recognized object, for each x and y, with which to identify $\langle x, y\rangle$. The problem is a neat one, for we have in (1) a single explicit standard whereby to judge whether a version is suitable.

There are many solutions. The earliest, put forward by Wiener in 1914, ran (nearly enough) as follows: $\langle x, y\rangle$ is identified with the class $\{\{x\}, \{y, \Lambda\}\}$, whose members are just (*a*) the class $\{x\}$, whose sole member is x, and (*b*) the class $\{y, \Lambda\}$, whose sole members are y and the empty class.

This construction is paradigmatic of what we are most typically up to when in a philosophical spirit we offer an "analysis" or "explication" of some hitherto inadequately formulated "idea" or expression. We do not claim synonymy. We do not claim to make clear and explicit what the users of the unclear expression had unconsciously in mind all along. We do not expose hidden meanings, as the words 'analysis' and 'explication' would suggest; we supply lacks. We fix on the particular functions of the unclear expression that make it worth troubling about, and then devise a substitute, clear and couched in terms to our liking, that fills those functions. Beyond those conditions of partial agreement, dictated by our interests and purposes, any traits of the explicans come under the head of "don't-cares" (§ 38). Under this head we are free to allow the explicans all manner of novel connotations never associated with the explicandum. This point is strikingly illustrated by Wiener's $\{\{x\}, \{y, \Lambda\}\}$. Our example is atypical in just one respect: the demands of partial agreement are preternaturally succinct and explicit, in (1).

Philosophical analysis, explication, has not always been seen in this way.[4] Only the reading of a synonymy claim into analysis could engender the so-called paradox of analysis, which runs thus: how can a correct analysis be informative, since to understand it we must already know the meanings of its terms, and hence already know that the terms which it equates are synonymous?[5] The notion that analysis must consist somehow in the uncovering of hidden meanings underlies also the recent tendency of some of the Oxford philosophers to take as their business an examination of the subtle irregularities of ordinary language. And there is no mistaking the obliviousness of various writers to the point about the don't-cares. If nobody has objected to Wiener's definition as falsifying the ordinary notion of ordered pair, e.g. in that it makes x and y members of members of $\langle x, y\rangle$, the reason is perhaps that here the relevant considerations are so clearly on view; or perhaps only that 'ordered pair' is not ordinary language. Analogous objections to other and more classical philosophical analyses, at any rate, are not wanting. Russell's theory of descriptions has been called wrong because of what it does to the truth-value gaps.[6] Frege's definition of number has been called wrong because it says of numbers that they have classes as members, which was not so before. But I anticipate.

Before we turn to number, there is still a point about ordered pairs worth exploiting: that Wiener's version of ordered pairs is but one among many possible. A later and better known one is Kuratowski's, which identifies $\langle x, y\rangle$ rather with $\{\{x\}, \{x, y\}\}$. If one happens to be working in pure number theory, it can be desirable to construe ordered pairs of numbers in such a way rather as not to carry us outside the domain of natural numbers; and this again can be accom-

[4] By Carnap, yes; see *Meaning and Necessity*, pp. 7 f.

[5] On this issue see Carnap, *op. cit.*, pp. 63 f.; White, "On the Church-Frege solution"; and further references therein.

[6] Strawson, *Introduction to Logical Theory*, pp. 185 ff.

plished in an infinite variety of ways—e.g., by taking $\langle x, y\rangle$ as $2^x \cdot 3^y$, or as $3^x \cdot 2^y$, or as $x + (x + y)^2$. Each of these versions of the ordered pair conflicts with all the others, but each fulfills (1).

Which is right? All are; all fulfill (1), and conflict with one another only out among the don't-cares. Any air of paradox comes only of supposing that there is a unique right analysis—a mistake that is encouraged by the practice, otherwise convenient, of using the term 'ordered pair' for each version. On this and other points, the nature of explication as illustrated by the ordered pair may be made wholly evident by retelling the story of Wiener, Kuratowski, and the ordered pair in a modified terminology. In the beginning there was the notion of the ordered pair, defective and perplexing but serviceable. Then men found that whatever good had been accomplished by talking of an ordered pair $\langle x, y\rangle$ could be accomplished by talking instead of the class $\{\{x\}, \{y, \Lambda\}\}$—or, for that matter, of $\{\{x\}, \{x, y\}\}$.

A similar view can be taken of every case of explication: *explication is elimination.* We have, to begin with, an expression or form of expression that is somehow troublesome. It behaves partly like a term but not enough so, or it is vague in ways that bother us, or it puts kinks in a theory or encourages one or another confusion. But also it serves certain purposes that are not to be abandoned. Then we find a way of accomplishing those same purposes through other channels, using other and less troublesome forms of expression. The old perplexities are resolved.

According to an influential doctrine of Wittgenstein's, the task of philosophy is not to solve problems but to dissolve them by showing that there were really none there. This doctrine has its limitations, but it aptly fits explication. For when explication banishes a problem it does so by showing it to be in an important sense unreal; viz., in the sense of proceeding only from needless usages.[7]

The ordered pair has had illustrative value because of the crispness of requirement (1) and because of the multiplicity and the conspicuous artificiality of the explications. But what it illustrates as to the nature of explication applies very widely. In the case of the ordered pair the initial philosophical problem, summed up in the question 'What is an ordered pair?', is dissolved by showing how we can dispense with ordered pairs in any problematic sense in favor of certain clearer notions. In the case of singular descrip-

[7] See Alston, p. 16; Lazerowitz, pp. 21 f.

tions, the initial problems are the inconvenience of truth-value gaps and the paradoxes of talking of what does not exist; and Russell dissolves them by showing how we can dispense with singular descriptions, in any problematic sense, in favor of certain uses of identity and quantifiers. In the case of the indicative conditional, the initial problems are the inconvenience of truth-value gaps and the obscurity of truth conditions; and they are dissolved by making evident that we can in general dispense with the indicative conditional, in any problematic sense, in favor of a truth function. In the case of 'nothing', 'everything', and 'something', the initial problems (to dignify them) are those that come of handling these words too much like proper names; and they are dissolved by dispensing with the offending words in favor of quantification. In all these cases, problems have been dissolved in the important sense of being shown to be purely verbal, and purely verbal in the important sense of arising from usages that can be avoided in favor of ones that engender no such problems.

It is ironical that those philosophers most influenced by Wittgenstein are largely the ones who most deplore the explications just now enumerated. In steadfast laymanship they deplore them as departures from ordinary usage, failing to appreciate that it is precisely by showing how to circumvent the problematic parts of ordinary usage that we show the problems to be purely verbal.

Explication is elimination but not all elimination is explication. Showing how the useful purposes of some perplexing expression can be accomplished through new channels would seem to count as explication just in case the new channels parallel the old ones sufficiently for there to be a striking if partial parallelism of function between the old troublesome form of expression and some form of expression figuring in the new method. In this case we are likely to view the latter form of expression as an explicans of the old, and, if it is longer, even abbreviate it by the old word. If there was a question of objects, and the partial parallelism which we are now picturing obtains, the corresponding objects of the new scheme will tend to be looked upon as the old mysterious objects minus the mystery. Clearly this is merely a way of phrasing matters, and wrong only as it threatens the immunity of the don't-cares and suggests that one of two divergent explicantia must be wrong.

The contrast drawn at the beginning of this section, between the defective noun whose objects we dispense with and the defective noun whose defectiveness we are at pains to eke out so as to keep the objects, can then be put more simply: it is just a matter of whether the ostensible objects of the defective noun played roles that still want playing by some sort of object.

§ 54. NUMBERS, MIND, AND BODY

But for its greater antiquity and its concern with a more venerable notion, the philosophical question 'What is a number?' is on a par with the corresponding question about ordered pairs. Frege dealt with the one question, as Wiener did with the other, by showing how the work for which the objects in question might be wanted could be done by objects whose nature was presumed to be less in question. He identified—as one says—each natural number n with a certain class N of classes, as follows: 0 with $\{\Lambda\}$, and $n+1$, for each n, with the class of all those classes which come to belong to N when deprived of a member. Thus, to put the matter circularly, each n is identified with the class of all n-member classes.[1]

After § 53, nothing needs be said in rebuttal of those critics, from Peano onward, who have rejected Frege's version because there are things about classes of classes that we have not been prone to say about numbers.[2] Nothing, indeed, is more logical than to say that if numbers and classes of classes have different properties then numbers are not classes of classes; but what is overlooked is the point of explication.

Von Neumann, playing Kuratowski to Frege's Wiener, offered a different identification: 0 with Λ, and $n+1$, for each n, with the class of all the classes identified with $0, 1, \ldots, n$.

The condition upon all acceptable explications of number (that is, of the natural numbers $0, 1, 2, \ldots$) can be put almost as succinctly as (1) of § 53: any *progression*—i.e., any infinite series each of whose members has only finitely many precursors—will do nicely. Russell once held[3] that a further condition had to be met, to the

[1] Frege, *Grundlagen*, § 68. In detail the version I am using comes rather from Russell, *Principles*, Ch. XI.

[2] Peano's phrase: "... *car ces objets ont des propriétés différentes*" (*Formulaire*, p. 70).

[3] *Introduction to Mathematical Philosophy*, p. 10.

effect that there be a way of applying one's would-be numbers to the measurement of multiplicity: a way of saying that

(1) There are n objects x such that Fx.

This, however, was a mistake; any progression can be fitted to that further condition. For, (1) can be paraphrased as saying that the numbers less than n admit of correlation with the objects x such that Fx. This requires that our apparatus include enough of the elementary theory of relations for talk of correlation, or one-one relation; but it requires nothing special about numbers except that they form a progression.

Over and above the strict condition one might still argue for the intuitiveness of Frege's version, as follows. A natural number n serves primarily to measure multiplicity, and may hence be naturally viewed as an attribute of classes, viz., the attribute of having n members; or, if we favor classes over attributes, the class of the n-member classes. One might argue differently for the intuitiveness of von Neumann's version: a number is to count with. When we count the members of an n-member class we pair them with the first n numbers; and n itself is, for von Neumann, the class precisely of those first n numbers. (We have to count from 0 instead of 1 to make this come out right, but this is little to ask.)

Actually there has been no dispute that I know of over the relative intuitiveness of the two versions. One uses Frege's version or von Neumann's or yet another, such as Zermelo's, opportunistically to suit the job in hand, if the job is one that calls for providing a version of number at all. The situation is unlike matrimony. Frege's progression, von Neumann's, and Zermelo's are three progressions of classes, all present in our universe of values of variables (if we accept a usual theory of classes), and available for selective use as convenient. That all are adequate as explications of natural number means that natural numbers, in any distinctive sense, do not need to be reckoned into our universe in addition. Each of the three progressions or any other will do the work of natural numbers, and each happens to be geared also to further jobs to which the others are not.

It is thus borne in on us, as in the case of ordered pairs, that explication is elimination. The step is to be seen as depending, usually, on a compensatory revision of adjacent text. Thus consider again

Frege's explication of number, under which 'x has n members' can be paraphrased as '$x \in n$'. If we picture his explication not as identifying each number n with a class of classes N but as avoiding reference to n by recourse to N, then what is afoot with 'has-...-members' is not an equating of it to '$\in$', but a compensatory revision of it as '$\in$'; a paraphrasing of 'has n members' not as '$\in n$' but as '$\in N$'. There are those whom an appreciation of the role of compensatory revision would have spared the error of objecting to Frege's version of number that 'has-...-members' does not mean '$\in$', or parallel errors elsewhere in philosophy.

I hardly need add that I wholly approve of playing Frege's game straight, with 'n' for 'N' and '$x \in n$' for 'x has n members', when one is not engaged in this particular project of clarification.

That explication is elimination, and hence conversely that elimination can often be allowed the gentler air of explication, is an observation about a philosophical activity that far transcends the philosophy of mathematics, even if the best examples are there. Before we drop the topic, we may do well to note the bearing of that observation on the philosophical issue over mind and body. Let me lead up to the matter with a defense of physicalism.

As illustrated by 'Ouch' (§ 2), any subjective talk of mental events proceeds necessarily in terms that are acquired and understood through their associations, direct or indirect, with the socially observable behavior of physical objects. If there is a case for mental events and mental states, it must be just that the positing of them, like the positing of molecules, has some indirect systematic efficacy in the development of theory. But if a certain organization of theory is achieved by thus positing distinctive mental states and events behind physical behavior, surely as much organization could be achieved by positing merely certain correlative physiological states and events instead. Nor need we spot special centers in the body for these seizures; physical states of the undivided organism will serve, whatever their finer physiology. Lack of a detailed physiological explanation of the states is scarcely an objection to acknowledging them as states of human bodies, when we reflect that those who posit the mental states and events have no details of appropriate mechanisms to offer nor, what with their mind-body problem, prospects of any. The bodily states exist anyway; why add the others? Thus introspection may be seen as a witnessing to one's own bodily condition, as in introspecting an acid stomach, even though the introspector be vague on the medical details. Granted, my words 'vague' and 'witnessing' here are mentalistic. But then my argument is directed to mentalists; physicalists do not need it.

This brief for physicalism adds little to foreshadowings in earlier pages, and nothing to what others have said.[4] But I assemble it here with an eye to the somewhat mitigating consideration afforded by our thoughts on explication and elimination. Is physicalism a repudiation of mental objects after all, or a theory of them? Does it repudiate the mental state of pain or anger in favor of its physical concomitant, or does it identify the mental state with a state of the physical organism (and so a state of the physical organism with the mental state)? The latter version sounds less drastic. Even ordinary language, in its least self-conscious attributions, falls neatly in with physicalism thus mildly conceived; one says 'Jones is in pain', 'Jones is angry', of quite the same object as 'Jones is tall'. What may primarily be said in characterization of physicalism thus mildly conceived is that it declares no unbridgeable differences in kind between the mental and the physical. Some may therefore find comfort in reflecting that the distinction between an eliminative and an explicative physicalism is unreal.[5]

For a further parallel consider the molecular theory. Does it repudiate our familiar solids and declare for swarms of molecules in their stead, or does it keep the solids and explain them as subvisibly swarming with molecules? Eddington took the former line in his opening paragraphs; common sense, with Miss Stebbing as spokeswoman, took the latter.[6] The option, again, is unreal. Nor is this surprising enough to be of much interest, except as a further analogical aid to appreciating the status of physicalism.

Not to discriminate between elimination and explication, there remains an important sense in which the physicalism contemplated above may be said to be less clearly *reductive* than Frege's version of number.[7] When Frege explains numbers as classes of classes, or

[4] See Carnap, *The Unity of Science;* Feigl, "The 'mental' and the 'physical'"; and, in Feigl, hundreds of further references. In particular see Feigl, pp. 417 f., for separation of physicalism from the intentionality issue of § 45.

[5] Perhaps this distinction is basically what is renounced in saying that "Philosophical behaviorism is not a metaphysical theory: it is the denial of a metaphysical theory. Consequently, it asserts nothing" (Ziff, p. 136).

[6] See Urmson.

[7] I am indebted to Davidson in the ensuing remarks, and to Feigl, p. 425.

eliminates them in favor of classes of classes, he paraphrases the standard contexts of numerical expressions into antecedently significant contexts of the corresponding expressions for classes; thus 'has-. . .-members' gives way to 'ϵ', and arithmetical operators such as '$+$' give way to appropriately definable class-theoretic operators. But when we explain mental states as bodily states, or eliminate them in favor of bodily states, in the easy fashion here envisaged, we do not paraphrase the standard contexts of the mental terms into independently explained contexts of physical terms. Thus the 'Jones is in' of 'Jones is in pain', the 'Jones is' of 'Jones is angry', remain unchanged, but merely come to be thought of as taking physicalistic rather than mentalistic complements. The radical reduction that would resolve the mental states into the independently recognized elements of physiological theory is a separate and far more ambitious program.

§ 55. WHITHER CLASSES?

The infinitesimals and the ideal objects were supposed objects whose recognition was *prima facie* useful to theory and, at the same time, troublesome. (Cf. § 51.) Now classes are another example of the same, but they seem to resist similar treatment. Ways of serving the theoretical purposes of infinitesimals and ideal objects were found which did not call for these troublesome objects after all, and the objects were accordingly dropped. On the other hand no similar circumvention of classes suggests itself; one is impelled rather to the opposite course, that of keeping classes and coping with the trouble they make. Let us examine this matter.

Where classes offend is not just on the score, so doubtfully offensive, of their abstractness. Numbers likewise are abstract; but classes, if uncritically accepted, lead to absurdities. There are infinitely many such paradoxes of classes; the simplest is Russell's familiar one of the class $\hat{x}(x \notin x)$, which is a member of itself if it is not and is not if it is.

Yet the admission of classes as values of variables of quantification brings power that is not lightly to be surrendered. Examples of this access of power were noted in §§ 43 and 48, and have been added to since. Classes can do the work of ordered pairs and hence also of relations (§ 53), and they can do the work of natural numbers (§ 54). They can do the work of the richer sorts of numbers too—rational, real, complex; for these can be variously explicated on the basis of natural numbers by suitable constructions of classes and relations.

Numerical functions, in turn, can be explicated as certain relations of numbers. All in all the universe of classes leaves no further objects to be desired for the whole of classical mathematics.

The versatility of classes in thus serving the purposes of widely varied sorts of abstract objects is best seen in mathematics, but it spills over, as illustrated by relations. Again, consider a disease; it can be taken as the class of all the appropriately afflicted temporal segments of its victims. Correspondingly for anger and other states. Intensional objects aside, the abstract objects that it is useful to admit to the universe of discourse at all seem to be adequately explicable in terms of a universe comprising just physical objects and all classes of the objects in the universe (hence classes of physical objects, classes of such classes, etc.). At any rate I think of no persuasive exceptions.

Such is the power of the notion of class to unify our abstract ontology. To surrender this benefit and face the old abstract objects again in all their primeval disorder would be a wrench, worth making if it were all. But we must remember that the utility of classes is not limited to explication of the various other sorts of abstract objects. The power of the notion on other counts, glimpsed in §§ 43 and 48, keeps it in continuing demand in mathematics and elsewhere as a working notion in its own right: not only in the protean guises of number, function, state, and all else that it has served to explicate, but also straight. It confers a power that is not known to be available through less objectionable channels.

Let it not be supposed that attributes, despite the special difficulties enveloping them (cf. § 43), bear consideration as a means of dispensing with classes. For they are obviously involved also in paradoxes exactly parallel to those of classes. Two reasons for making little of this point are that attributes are badly off anyway, and that any remedy for the class paradoxes would presumably work for attributes too.

Thus it is that one resolves to keep classes and somehow excise the paradoxes. Now it is not to be wondered that a self-contradictory notion of class should have proved powerful. No holds are barred. It is too powerful for anyone's good, enabling one as it does to prove truths and falsehoods indiscriminately. The problem, then, is that of weakening it enough but not too much for future service.

Various ways are known. They have their several strengths and weaknesses, and none stands out clearly as the most satisfactory. All of them restrict, in some fashion, the universal applicability of the operator '$\hat{x}$' of class abstraction.[1] There ceases to be the old guarantee that for each open sentence there is a class whose members are just the values of the variable for which the sentence comes out true.[2] Whether classes continue to do all the services claimed for them, e.g. in foregoing pages and chapters, has then to be checked up with an eye to the specific restricted theory adopted. One argument that would call for qualifications is the one in § 48 which eliminated 'Φ_x'. On the whole, however, one manages to salvage most of the utility that the old class theory seemed (in happy ignorance of the paradoxes) to afford, apart from simplicity of governing principles. Naturalness, for whatever it is worth, is of course lost; a multitude of mutually alternative, mutually incompatible systems of class theory arises, each with only the most bleakly pragmatic claims to attention. Insofar as a leaning or tolerance toward classes may have turned on considerations of naturalness, nominalism scores.

Initial reason for favoring physical objects over abstract objects was urged at the end of § 48. Moved further by these latest reflections, one may wistfully survey the chances of getting by as a nominalist. One can afford to sacrifice some surely of the systematic benefits of abstract objects, as offset by a twofold gain: elimination of the less welcome objects, and elimination of a drastic dualism of categories.

In such a program the basic problem is how to say what one wants to say of physical objects without invoking abstract objects as auxiliaries. Thus, interested in whooping cranes and not at all in numbers, one still wants to say there are six whooping cranes. Here in fact there is no difficulty. The form 'There are n objects x such that Fx' can, for each specific n, be paraphrased with help of '$=$' and quantifiers (cf. § 24), and makes no demand upon numbers as values of variables of quantification. Again there is no difficulty in bringing in a time variable when we please, and even quantifying it; for times can be taken as physical objects, according to § 36.

[1] Note that the universally applicable '$\hat{x}$' of my *Mathematical Logic* has a different use: it collects "elements," not objects generally.

[2] Russell gets the same effect by a method that preserves, indeed, the letter if not the spirit of the old guarantee: he excises part of the domain of open sentences. See above, § 47.

'There are just as many husbands as wives' is where difficulties begin. '$(\exists n)$(there are n husbands and there are n wives)' will not do, for it requires numbers as values of quantified variables. Neither will 'There is a correlation between husbands and wives', for it requires relations as values of variables. And problems like that of 'just as many' are raised again by 'more than', 'twice as many', and the like.

Another difficulty is that the nominalist deprives himself of Frege's technique of paraphrasing 'ancestor' in terms of 'parent' and quantification over classes (§ 48). He is still free to accept 'ancestor' and 'parent' each as a relative term, but he loses the theory that links them. Such a law as that ancestors of ancestors are ancestors he has to take as irreducible, rather than seeing it as implicit in Frege's paraphrase. The 'ancestor' example, moreover, is one among countless. For every open sentence in two variables another is wanted that stands to it as 'x is ancestor of y' stands to 'x is parent of y'. The connection is an important one to be able to exploit in general.

In the face of these difficulties the nominalist is not quite helpless. With some loss in naturalness, simplicity, and generality, he can devise alternative paraphrases of 'ancestor', 'just as many', etc. that quantify over physical objects instead of abstract ones.[3] But these are only samples of the difficulties that continue to beset him. He is going to have to accommodate his natural sciences unaided by mathematics; for mathematics, except for some trivial portions such as very elementary arithmetic, is irredeemably committed to quantification over abstract objects.[4]

If a thoroughgoing nominalist doctrine is too much to live up to, there are compromises. The logical paradoxes, which just now seemed to provide at least a last small push toward nominalism, would never have threatened if classes had been held to classes of concrete objects, classes of such, and so on, up to some fixed level and not beyond. This restriction would impair the explication of

[3] See Goodman and Quine.

[4] The nominalist program seems already and painlessly achieved if Whitehead and Russell's elimination of classes by a theory of incomplete symbols is thought to bear. But it does not; it only eliminates classes in favor of attributes. See my "Whitehead and the rise of modern logic." For more on the scope of nominalism see my *From a Logical Point of View,* Essay 6; Goodman, *Structure of Appearance,* Ch. II; Martin, *Truth and Denotation,* Ch. XIII; Stegmüller.

numbers as well as further work in mathematics, but it would be less austere than nominalism; and numbers, in particular, could be thrown in without explication if desired. Most such compromises, of course, are pointless as general philosophies because of the arbitrariness of their stopping places. One is led on to further indulgences as occasions arise.

Scope still remains for nominalism, however, and for various intermediate grades of abnegation of abstract objects, when with Conant we think of science not as one evolving world view but as a multiplicity of working theories. (See end of § 51.) The nominalist can realize his predilection in special branches, and point with pride to a theoretical improvement of those branches. In the same spirit even the mathematician, realist *ex officio,* is always glad to find that some particular mathematical results that had been thought to depend on functions or classes of numbers, for instance, can be proved anew without appealing to objects other than numbers. It is generally conducive to understanding to keep track of our presuppositions, in point of objects and otherwise, project by project; and to welcome ontological economy in connection with one project even if a more lavish ontology is needed for the next. But it is also important to have the less economical and more powerful mathematical theories well in hand as engines of discovery, for swift use in unforeseen places—even though in each such case we take pains afterward to find more economical ways of gaining the same result.

§ 56. SEMANTIC ASCENT

This chapter has been centrally occupied with the question what objects to recognize. Yet it has treated of words as much as its predecessors. Part of our concern here has been with the question what a theory's commitments to objects consist in (§ 49), and of course this second-order question is about words. But what is noteworthy is that we have talked more of words than of objects even when most concerned to decide what there really is: what objects to admit on our own account.

This would not have happened if and insofar as we had lingered over the question whether in particular there are wombats, or whether there are unicorns. Discourse about non-linguistic objects would have been an excellent medium in which to debate those issues. But when the debate shifts to whether there are points, miles, numbers, attributes, propositions, facts, or classes, it takes on an in some sense philosophical cast, and straightway we find ourselves talking of words almost to the exclusion of the non-linguistic objects under debate.

Carnap has long held that the questions of philosophy, when real at all, are questions of language; and the present observation would seem to illustrate his point. He holds that the philosophical questions of what there is are questions of how we may most conveniently fashion our "linguistic framework," and not, as in the case of the wombat or unicorn, questions about extralinguistic reality.[1] He holds that those philosophical questions are only apparently about sorts of objects, and are really pragmatic questions of language policy.

But why should this be true of the philosophical questions and not of theoretical questions generally? Such a distinction of status is of a piece with the notion of analyticity (§ 14), and as little to be trusted. After all, theoretical sentences in general are defensible only pragmatically; we can but assess the structural merits of the theory which embraces them along with sentences directly conditioned to multifarious stimulations. How then can Carnap draw a line across this theoretical part and hold that the sentences this side of the line enjoy non-verbal content or meaning in a way that those beyond the line do not? His own appeal to convenience of linguistic framework allows pragmatic connections across the line. What other sort of connection can be asked anywhere, short of direct conditioning to non-verbal stimulations?

Yet we do recognize a shift from talk of objects to talk of words as debate progresses from existence of wombats and unicorns to existence of points, miles, classes, and the rest. How can we account for this? Amply, I think, by proper account of a useful and much used manoeuvre which I shall call *semantic ascent.*

It is the shift from talk of miles to talk of 'mile'. It is what leads from the material (*inhaltlich*) mode into the formal mode, to invoke an old terminology of Carnap's. It is the shift from talking in certain terms to talking about them. It is precisely the shift that Carnap thinks of as divesting philosophical questions of a deceptive guise and setting them forth in their true colors. But this tenet of Carnap's is the part that I do not accept. Semantic ascent, as I

[1] Carnap, "Empiricism, semantics, and ontology."

speak of it, applies anywhere.[2] 'There are wombats in Tasmania' might be paraphrased as ''Wombat' is true of some creatures in Tasmania', if there were any point in it. But it does happen that semantic ascent is more useful in philosophical connections than in most, and I think I can explain why.

Consider what it would be like to debate over the existence of miles without ascending to talk of 'mile'. "Of course there are miles. Wherever you have 1760 yards you have a mile." "But there are no yards either. Only bodies of various lengths." "Are the earth and moon separated by bodies of various lengths?" The continuation is lost in a jumble of invective and question-begging. When on the other hand we ascend to 'mile' and ask which of its contexts are useful and for what purposes, we can get on; we are no longer caught in the toils of our opposed uses.

The strategy of semantic ascent is that it carries the discussion into a domain where both parties are better agreed on the objects (viz., words) and on the main terms concerning them. Words, or their inscriptions, unlike points, miles, classes, and the rest, are tangible objects of the size so popular in the marketplace, where men of unlike conceptual schemes communicate at their best. The strategy is one of ascending to a common part of two fundamentally disparate conceptual schemes, the better to discuss the disparate foundations. No wonder it helps in philosophy.

But it also figures in the natural sciences. Einstein's theory of relativity was accepted in consequence not just of reflections on time, light, headlong bodies, and the perturbations of Mercury, but of reflections also on the theory itself, as discourse, and its simplicity in comparison with alternative theories. Its departure from classical conceptions of absolute time and length is too radical to be efficiently debated at the level of object talk unaided by semantic ascent. The case was similar, if in lesser degrees, for the disruptions of traditional outlook occasioned by the doctrines of molecules and electrons. These particles come after wombats and unicorns, and before points and miles, in a significant gradation.

The device of semantic ascent has been used much and carefully in axiomatic studies in mathematics, for the avoidance, again, of

[2] In a word, I reject Carnap's doctrine of "quasi-syntactic" or "pseudo-object" sentences, but accept his distinction between the material and the formal mode. See his *Logical Syntax*, §§ 63–64. (It was indeed I, if I may reminisce, who in 1934 proposed 'material mode' to him as translation of his German.)

question-begging. In axiomatizing some already familiar theory, geometry say, one used to be in danger of imagining that he had deduced some familiar truth of the theory purely from his axioms when actually he had made inadvertent use of further geometrical knowledge. As a precaution against this danger, a device other than semantic ascent was at first resorted to: the device of disinterpretation. One feigned to understand only the logical vocabulary and not the distinctive terms of the axiom system concerned. This was an effective way of barring information extraneous to the axioms and thus limiting one's inferences to what the axioms logically implied. The device of disinterpretation had impressive side effects, some good, such as the rise of abstract algebra, and some bad, such as the notion that in pure mathematics "we never know what we are talking about, nor whether what we are saying is true."[3] At any rate, with Frege's achievement of a full formalization of logic, an alternative and more refined precaution against question-begging became available to axiomatic studies; and it is a case, precisely, of what I am calling semantic ascent. Given the deductive apparatus of logic in the form of specified operations on notational forms, the question whether a given formula follows logically from given axioms reduces to the question whether the specified operations on notational forms are capable of leading to that formula from the axioms. An affirmative answer to such a question can be established without disinterpretation, yet without fear of circularity, indeed without using the terms of the theory at all except to talk about them and the operations upon them.

We must also notice a further reason for semantic ascent in philosophy. This further reason holds also, and more strikingly, for logic; so let us look there first. Most truths of elementary logic contain extralogical terms; thus 'If all Greeks are men and all men are mortal . . .'. The main truths of physics, in contrast, contain terms of physics only. Thus whereas we can expound physics in its full generality without semantic ascent, we can expound logic in a general way only by talking of forms of sentences. The generality wanted in physics can be got by quantifying over non-linguistic objects, while the dimension of generality wanted for logic runs

[3] Russell, *Mysticism and Logic and Other Essays,* p. 75. The essay in question dates from 1901, and happily the aphorism expressed no enduring attitude on Russell's part. But the attitude expressed has been widespread.

crosswise to what can be got by such quantification. It is a difference in shape of field and not in content; the above syllogism about the Greeks need owe its truth no more peculiarly to language than other sentences do.

There are characteristic efforts in philosophy, those coping e.g. with perplexities of lion-hunting or believing (§§ 30–32), that resemble logic in their need of semantic ascent as a means of generalizing beyond examples.[4] Not that I would for a moment deny that when the perplexities about lion-hunting or believing and its analogues are cleared up they are cleared up by an improved structuring of discourse; but the same is true of an advance in physics. The same is true even though the latter restructuring be led up to (as often happens) within discourse of objects, and not by semantic ascent.

For it is not as though considerations of systematic efficacy, broadly pragmatic considerations, were operative only when we make a semantic ascent and talk of theory, and factual considerations of the behavior of objects in the world were operative only when we avoid semantic ascent and talk within the theory. Considerations of systematic efficacy are equally essential in both cases; it is just that in the one case we voice them and in the other we are tacitly guided by them. And considerations of the behavior of objects in the world, even behavior affecting our sensory surfaces by contact or radiation, are likewise essential in both cases.

There are two reasons why observation is felt to have no such bearing on logic and philosophy as it has on theoretical physics. One is traceable to misapprehensions about semantic ascent. The other is traceable to curriculum classifications. This latter factor tends likewise to make one feel that observation has no such bearing on mathematics as it has on theoretical physics. Theoretical assertions in physics, being terminologically physics, are generally conceded to owe a certain empirical content to the physical observations which, however indirectly, they help to systematize, whereas laws of so-called logic and mathematics, however useful in systematizing physical observations, are not considered to pick up any empirical substance thereby. A more reasonable attitude is that there are merely variations in degree of centrality to the theoretical

[4] Wittgenstein's characteristic style, in his later period, consisted in avoiding semantic ascent by sticking to the examples.

structure, and in degree of relevance to one or another set of observations.

In § 49 I spoke of dodges whereby philosophers have thought to enjoy the systematic benefits of abstract objects without suffering the objects. There is one more such dodge in what I have been inveighing against in these last pages: the suggestion that the acceptance of such objects is a linguistic convention distinct somehow from serious views about reality.

The question what there is is a shared concern of philosophy and most other non-fiction genres. The descriptive answer has been given only in part, but at some length. A representative assortment of land masses, seas, planets, and stars have been individually described in the geography and astronomy books, and an occasional biped or other middle-sized object in the biographies and art books. Description has been stepped up by mass production in zoology, botany, and mineralogy, where things are grouped by similarities and described collectively. Physics, by more ruthless abstraction from differences in detail, carries mass description farther still. And even pure mathematics belongs to the descriptive answer to the question what there is; for the things about which the question asks do not exclude the numbers, classes, functions, etc., if such there be, whereof pure mathematics treats.

What distinguishes between the ontological philosopher's concern and all this is only breadth of categories. Given physical objects in general, the natural scientist is the man to decide about wombats and unicorns. Given classes, or whatever other broad realm of objects the mathematician needs, it is for the mathematician to say whether in particular there are any even prime numbers or any cubic numbers that are sums of pairs of cubic numbers. On the other hand it is scrutiny of this uncritical acceptance of the realm of physical objects itself, or of classes, etc., that devolves upon ontology. Here is the task of making explicit what had been tacit, and precise what had been vague; of exposing and resolving paradoxes, smoothing kinks, lopping off vestigial growths, clearing ontological slums.

The philosopher's task differs from the others', then, in detail; but in no such drastic way as those suppose who imagine for the philosopher a vantage point outside the conceptual scheme that he takes in charge. There is no such cosmic exile. He cannot study and revise the fundamental conceptual scheme of science and common sense without having some conceptual scheme, whether the same or another no less in need of philosophical scrutiny, in which to work. He can scrutinize and improve the system from within, appealing to coherence and simplicity; but this is the theoretician's method generally. He has recourse to semantic ascent, but so has the scientist. And if the theoretical scientist in his remote way is bound to save the eventual connections with non-verbal stimulation, the philosopher in his remoter way is bound to save them too. True, no experiment may be expected to settle an ontological issue; but this is only because such issues are connected with surface irritations in such multifarious ways, through such a maze of intervening theory.