

VI. Semantics, Ontology, and Epi-ontology.

We can no longer evade present need for a hard look at ontology, with side glances at the associated semantics. I would gladly forego that examination here, for the issues are so recondite that even to explain why they matter is itself a task of considerable difficulty that requires me to stake contentious philosophic claims. But there can be no systems theory, nor indeed any explanatory science, without ontological foundations that are perforce implicit if not made explicit; and the commonsense ontological intuitions that support ordinary science decently enough too quickly crumble under the sometimes-intense localized pressures of deep analysis of "structure."

If the notion of "existence" were sufficiently foundational for human affairs, ontology could be defined as the theory of our usage--as it is and as it should be--of sentence schema 'x exists'. (This singular form, illustrated by 'Satan exists', must not be confused with general existence schema 'ϕs exist', e.g. 'Demons exist'. Although ontologists often slight singular existence in favor of general existence, we shall see that the former has primacy over the latter.¹) However, the label 'exists'

¹'ϕs exist' is ambiguous between (i) 'Something (i.e. something that exists) is a ϕ' and (ii) 'Everything that is a ϕ exists'. Most ontologists have presumed reading (i); and this is indeed the one to favor, except ^{that} to do so without argument evades the basic question of "existence." It is entirely sound to contend that (ii) is tautological and that (i)'s i.e.-parenthesis is otiose; but establishing that this is so is precisely what a theory of ontology needs to accomplish.

is not where ontology lives. The real action concerns which expressions in our language should be conceded a certain elite semantic status. If we think that expression 'a' is suitably privileged, the most emphatically explicit way for us to acknowledge this is by asserting 'a exists'. But we reveal our commitment in that respect through our ordinary use of 'a' regardless of what, if anything, we

vocalize about a's "existence."

The semantical essence of ontology is reference: What there is is just what can in principle be referred to, and the ontological presuppositions of my/your language lie in the locutions by which I/you profess to talk about (i.e. refer to) something. But even provisionally granting that this is so (though I have not yet argued its case), ascription of the label 'reference' is still not what confers ontic status. The assertion 'a refers' (equivalently, 'a has a referent') is just the metalinguistic counterpart of 'a exists': Both are post-hoc devices to endorse our acceptance of expression 'a' in a special linguistic role; and that role, together with its ontic import, is independent of whatever wise or foolish philosophic views we may profess on reference and existence. (Ontological realism in one version or another is a philosophical doctrine that one lives.)

What is this seminal linguistic role? It is simply nominality, or "noun-ness," i.e. the job done by names/nouns/noun-phrases. Accordingly, the thesis to be developed here is that roughly speaking, my commitment to what there is, i.e. to what "exists," is demarked by just the expressions I use as nominals. But so simple an account of practical ontology needs shoring against a number of complications, one of which is in fact the main reason why ontology intrudes into systems theory in the first place.

To begin, what is a "nominal" anyway? In part, this initial problem is the obscure generic nature of the entities presumed by even the most familiar of our linguistic categories. Nouns, adjectives, verbs, articles, and other expressions variously classified by grammarians are not just external stimulus patterns (even were the notion of "stimulus pattern" itself at all clear); they also involve the internal effects of these stimuli on persons who "know the language" in which these are expressions. I shall here make no attempt whatever to clarify what are these things, "expressions," such that the same stimulus pattern can correspond to different expressions in different languages or even in different contexts of usage within the same language. I simply want to put on record at the outset that a nominal (or any other word or phrase) is not just a symbol-sequence; it is a symbol-sequence cum

meaning, so that two occurrences of the same external language device may or may not be instances of the same word or phrase. (Thus if I complain, 'Damn; I left my left glove in my bag, and my bag's in the car', the second word in this utterance is not the same expression as its fourth word. Yet the two occurrences here of letter-string 'my#bag' do demark the same expression, else I could not properly be taken to imply that I left a glove in something that is in the car.) Having acknowledged this, I shall revert to conventional treatment of words/phrases/expressions as something that we can identify simply by enclosing in quotes the symbol-sequences through which, bereft of quotes, we endeavor communication. Even so, it is important to appreciate that what we designate in this familiar way is context dependent and rather mysterious in nature.

Given, then, that it makes sense to identify some entity 'a' as a particular word or phrase in my language, what is it for 'a' to be, or not to be, a "nominal" (i.e. name/noun/noun-phrase) for me, and why should I think that any such grammatical category can be distinguished in the first place? I have already appealed to this notion in Section I, but said little there to justify its assumption and neither will I now succeed in establishing it as conclusively as I would like. I could just claim--as seems true enough--that to anyone capable of reflecting upon his own language usage it is intuitively evident not only that such a linguistic role exists, but also that it and predication jointly constitute language's most basic syntactic articulation. But if so, there should be tests that can diagnose whether a given occurrence of an expression 'a' is functioning as a nominal. And even more importantly, it should also be possible to say in what way it matters whether 'a' is a nominal in a given context. Such tests and identifiable import can indeed be found. But because they are vastly complicated by ambiguity, context dependency, idiom, and the surface grammar of markers for tense, animacy, number, etc., my present treatment of this matter must remain superficial and fragmentary.

Basically, a particular occurrence of 'a' in a sentence 'a ...' (alternatively annotated, in a sentence 'P(a)' or 'Pa') functions as a nominal there just in case this sentence is analytically equivalent to 'a is such that ... it ...' or

to some idiomatic paraphrase thereof that adjusts the copula and demonstrative for an appropriate marking of tense, gender, and number. (By "sentence" I shall mean any meaningful declarative sentence, though I hesitate to make that clarification explicit for fear of agitating problems in the attitudinal aspects of mental acts --assertion, query, command, etc.--that I shall flagrantly avoid here despite the need for any serious account of language usage to deal extensively with them.) Present notation 'a ... a ...' for a sentence containing expression 'a' intends the marker 'a' therein to represent just one occurrence of the expression abbreviated by 'a', so that 'a' may occur elsewhere in sentence schema ' ' as well. But multiple occurrences of 'a' can also be tested for simultaneous nominality. Thus if 'a ... a ...' is equivalent to 'a is such that ... it ... it ...', 'a' functions as a nominal, and moreover as the same nominal, in both of its indicated occurrences in 'a ... a ...'. When later I write 'P(a)' (or 'Pa') for a sentence containing an expression 'a' declared to function nominally therein, we may allow that the sentence schema abbreviated by 'P()' perhaps contains several blanks which are simultaneously filled in 'P(a)' by 'a' functioning in these multiple occurrences as the same nominal.

To illustrate the "such-that" test's application to everyday English, observe that

[6.1] John is bald

is equivalent to

John is such that it (i.e. he) is bald,

but is not equivalent to any of

Is is such that John it bald,

Bald is such that John is it,

Is bald is such that John it.

All three of these latter word-strings are in fact intuitively meaningless, though the middle one's similarity to 'Baldness is such that John has it' may give an

illusion of meaning. On the other hand, [6.1]'s prima facie paraphrase,

[6.2] John exemplifies baldness,

is equivalent to

Baldness is such that John exemplifies it,

but not to

Bald is such that John exemplifies it-ness.

Thus the "such-that" test shows that 'baldness' is a nominal in [6.2] (as is also 'John'), and thereby makes it exceedingly difficult to regard [6.2] as strictly equivalent to [6.1]. (More on this distinction later.) This test also diagnoses complexly phrased and nested nominals. Thus in

[6.3] The man in the torn shirt swore at his children,

'the man in the torn shirt', 'the torn shirt', and 'his children' all qualify as nominals by "such-that" paraphrasing (e.g., 'His children are such that the man in the torn shirt swore at them'), whereas 'man', 'shirt', and 'children', inter alia, do not.

The results of this test are by no means always decisive (e.g. when applied to phrases beginning with an indefinite article), though presumably that largely reflects ambiguities in the locution tested. Thus to decide whether 'the man' functions as a nominal in [6.3], we must first judge whether our understanding of [6.3] would remain unaltered by segregating 'in the torn shirt' from 'the man' by commas. (This also nicely illustrates the profound context dependency of what we denote by putting letter-string tee-aich-ee-gap-em-ay-en in linguistic quotes.) Neither is "such-that" paraphrasing the only way to diagnose whether 'a' functions nominally in '... a ...'. An even better test in some respects is whether 'a' can be meaningfully replaced therein by some demonstrative ('this', 'here', etc.) or demonstrative phrase (e.g., 'those big ones over there'). For 'a' to be demarked as functionally nominal in '... a ...' by the demonstrative test it is not necessary that 'a' be synonymous with its demonstrative substitute, but only that the latter

can meaningfully go proxy for 'a' in this context. (Thus, 'John loaned his records to Mary last night' can be meaningfully transformed by demonstratives into 'He loaned those to her then'. In what sense these demonstratives "go proxy" for the expressions they respectively replace is a tricky question that need not be addressed here. More generally, the grammar of nominals engages our usage of demonstratives and pronouns with much greater intimacy than can be done justice on this occasion.)

A further complication for any test of nominality, when the symbol sequence 'a' expresses different nominal concepts in different contexts, is to judge which sense of 'a' occurs nominally in '... a ...'. We can do this without explicit talk of meanings by asking whether the 'a' that occurs nominally in some previously appraised context 'P(a)' functions as the same nominal in '... a ...', and test for this by judging whether these two sentences are jointly equivalent to 'a is such that both P(it) and ... it ...'. But some ambiguities in 'a' or the sentence schema that embeds it are so subtle that none of the tests cited so far are completely reliable. A further class of relevant tests is to find yoked predicates 'P₁(__)' and 'P₂(__)' such that 'a' occurs nominally with the same sense in both 'P₁(a)' and 'P₂(a)' if it occurs nominally in either, and then to test the occurrence of 'a' in '... a ...' against its occurrences in both 'P₁(a)' and 'P₂(a)' separately. An example of such an appraisal will be given in fn. 7, below.

When one attempts to discern linguistic principles that govern whether 'a' occurs nominally in a particular context '... a ...', it becomes quickly evident that '... __ ...' determines this fully as much as does 'a'. In the main, if 'a' functions nominally in '... a ...' and 'b' is substituted for 'a' therein, 'b' also functions nominally in '... b ...' if the latter is meaningful at all; whereas if '... a ...' is meaningful but does not use 'a' nominally, then neither does any substitution for 'a' therein give its replacement a nominal function in that context. (For example, the letter-string 'John's dog' readily expresses a nominal if given half a chance; but it does not do so in 'John's dogged persistence enabled him to succeed', and neither can any other expression when put in its place.²) Accordingly,

²The brackets in '[John's dog] died recently' and '[John's dog]ged persistence ...' demark two occurrences of the same letter-string, but not of the same expression, i.e. vehicle-cum-meaning. The essential point here is that no letter-string can express a nominal in the second bracketed context, whereas whatever is put into the first one must function nominally there if the result is to be a meaningful sentence.

when 'a ...' is a sentence formed by embedding expression 'a' in sentence schema '___ ...' (equivalently, schema 'x ...'), if 'a' functions nominally in 'a ...' I shall characterize '___ ...' (or 'x ...') as a predicate and imply thereby that for any expression 'b', if 'b ...' is a meaningful sentence then 'b' occurs as a nominal therein. Thus in particular, when I stipulate that 'P()', or 'P(x)', or more briefly 'P' is to abbreviate some "predicate" whose completion by 'a' is a sentence 'P(a)', it will be redundant to add that 'a' abbreviates an expression that functions nominally in 'P(a)'.

One virtue of the "such-that" test for nominality is its close tie to what is perhaps the most significant consequence of nominal function, namely, its grounding of quantification and generalization. If 'a' is demarked as nominal in 'a ...' by the latter's equivalence to 'a is such that ... it ...', then both of these sentences are also equivalent to 'a is something such that ... it ...', which in turn evidently entails 'Something is such that ... it ...'. More formally, if 'a ...' is a (declarative) sentence in my language, then for every place-marker (logical variable) 'x' not already in predicate '___ ...', 'a ...' logically entails '(∃x)... x ...' just in case 'a' functions as a nominal in 'a ...'. Also, if 'a ...' and 'Everything that is P is also Q' are both sentences in my language, they jointly entail 'If a is P then a is Q' just in case 'a' functions nominally in 'a ...'. (This holds even when predicate '___ ...' is totally unrelated to 'P' and 'Q'.) Moreover, this inference remains essentially sound albeit with weakened conclusion if the strict universal conditional is replaced by a

probabilistic generality such as 'Ps are usually Qs'. I further suggest--though the logic of ampliative inference is still too obscure to allow any solid proof or even formalization of this claim--that we can make rational inductive extrapolations from data statements only by generalization on the expressions that occur as nominals therein. The role of nominals in inference, especially in ampliation, is far too vast and unexplored a topic for serious discussion here; but I submit as a thesis for future elaboration that nominals are essential to any rational inference whose relevant form cannot be expressed within just the propositional calculus, and that the practical import of which particular expressions function as nominals for me lies most indispensably in how these affect the distribution of credibility within my belief system.

What I have offered so far is fragments of an account of the nominal linguistic role. But what is it for me to accept, or not to accept, a particular expression 'a' in this role? This is really a two-stage question: First is whether 'a' is nominal for me in meaning, i.e. whether this concept complex as I understand it is one that I could use nominally. If it is, as identified by a test to be described in a moment, let us say that 'a' is linguistically nominal for me. By saying that 'a' is a nominal in my language, without reference to any particular sentence in which it occurs, I mean just that 'a' is linguistically nominal for me. But secondly, even if 'a' is linguistically nominal for me, I may still not accept it fully in this role. If I do so accept it, let us say that 'a' is objectively nominal for me, or better, that the degree to which I accept 'a' as a nominal, is the degree, Obj[a], to which 'a' is objectively nominal for me. Symbolizing the latter by 'Obj[a]' without enclosing 'a' in quotes is akin to symbolizing the credibility I give to a sentence 'P(a)' by 'Cr[P(a)]'. There is an important connection between Cr and Obj that will emerge as I try to clarify what it is for me to accept a linguistic nominal.

There are several (related) ways to test whether a given expression 'a' is linguistically nominal for me. The simplest is that this is so just in case 'a'

occurs as a nominal in some sentence 'a ...' in my language. But that may raise the question whether 'a' can function nominally in any sentence for me if I do not fully accept 'a' as objectively nominal. In fact, my previously offered tests for nominal function do not require that the nominal in question be accorded full objectivity; but since it still remains to clarify what is at issue in objective nominality, I cannot yet show that the latter is not a factor in, e.g., "such-that" paraphrasings. Accordingly, we may alternatively say that if 'b ...' is a sentence in which 'b' occurs nominally for me (or, if degrees of objective nominality are relevant, is accepted therein to the highest degree of nominality by me), then 'a' is linguistically nominal for me just in case 'a ...' is a (meaningful) sentence in my language. Better yet, the concepts of "predicate" and "linguistic nominal" can be defined simultaneously, without appeal to any other nominals, by saying that '__ ...' is a predicate and 'a' is a linguistic nominal for me just in case 'There is something such that ... it ...' and 'a ...' are (meaningful) sentences in my language. Henceforth, I shall suppress explicit recognition of linguistic relativity by speaking of an expression as being or not being a linguistic nominal, or a sentence, or a predicate, etc., without reference to whom, or in what language, these expressions do or do not have this status.

When 'a' is linguistically nominal (for me, or whomever), let us also say that any sentence 'a ...', or more precisely the sentence schema '__ ...' in which sentence 'a ...' embeds 'a', is an objective or "referentially transparent" context for 'a' just in case 'a' functions as a nominal in 'a ...'. (We may understand (i) "Nominal 'a' occurs objectively in sentence 'P(a)'," (ii) "'P(a)' is an objective context of nominal 'a'," and (iii) "'P(__)' is a predicate in sentence 'P(a)', as all equivalent to "'a' functions as a nominal in 'P(a)'.") Not all sentences containing a nominal 'a' are prima facie objective contexts for 'a', however; not even when 'a' is wholly contained in a clause which by itself would be an objective context for 'a'. For example, 'the next Canadian Prime Minister' occurs objectively in 'The next Canadian Prime Minister will be unmarried' but not in

either 'It is unlikely that the next Canadian Prime Minister will be unmarried' or in 'John hopes that the next Canadian Prime Minister will be unmarried'. And in 'John's given name is 'John'', the first but not the second occurrence of 'John' is objective. Indeed, sentential contexts for nominals that are non-objective, or "referentially opaque," have received intense discussion in modern philosophy of language.^{1,2} Traditionally, nominal 'a' is referentially transparent or referentially

² Considering that ^{two} occurrences of the same letter-string need not demark instances of the same linguistic expression, it is in fact a very nice question whether it is logically possible for the very same expression to occur as a nominal in one context but not in another. But there is evidently more in common, semantically, to the two occurrences of 'John' in 'John's name is 'John'' than there is to the two occurrences of 'left' in 'I left my left glove ...'; and I shall provisionally concur with the prevailing presumption that whatever is common to the first instance-pair but not the second is what we seek to capture by the notion of "same expression." That this assumption could well be wrong does not matter here, inasmuch as nothing I say in this essay requires it to be the case that nominals do not always function nominally.

opaque in sentence 'a ...' according to whether, for any nominal 'b', 'a ...' and 'a = b' jointly entail 'b ...'. My present criterion for "objective occurrence" of nominals, which I equate with referential transparency, does not dispute this tradition but proposes an additional demarkation of opacity, namely, that if nominal 'a' is referentially opaque in sentence 'a ...', the latter is not equivalent to 'a is such that ... it ...' and does not entail 'Something is such that ... it ...'.³

³ I avoid exhibiting specifics here because the matter is not particularly germane to present concerns and my present criterion for non-objective contexts (referential opacity)--namely, that the nominal in that position not function as a nominal therein--can be disputed (cf. fn. 2). Paradigmatically, 'John fears

that Mary will be late' is an opaque context of 'Mary'; and perhaps most will agree with me that this sentence is not equivalent to 'Mary is such that John fears that she will be late'. But that it does not entail 'Something is such that John fears that it will be late' is perhaps not so clear. Indeed, while I deny this entailment, I want also to claim that the former does imply something that everyday English can scarcely distinguish from the latter. (Cf., 'There is someone who John fears will be late' and 'There is a concept in terms of which John fears someone's prospective lateness', neither of which is an entirely correct consequence of 'John fears that Mary will be late' but which jointly point to one that is if we can just work out an acceptable wording for it.) It can be argued--as did Frege (1892) long ago and I have endorsed more recently (Rozeboom, 1961)--that a sentence 'a ...' in which nominal 'a' occurs opaquely can often be explicated as something like ' $\mu(\underline{a})$...' in which ' $\mu(\underline{a})$ ' designates the meaning of 'a'. However, for this insertion to make idiomatic sense some not-entirely-commonsensical adjustments must also be made in ' ...'.

Even if 'a' is linguistically a nominal for me, however, it does not follow that I necessarily accept 'a' in this role, anymore than I believe every expression that is a sentence for me. Consider, for example, my varied acceptance of the nominals 'Aristotle', 'Helen of Troy', and 'Zeus'. I regard 'Zeus' as unquestionably deficient, and 'Helen of Troy' likely so, in a way that 'Aristotle' is probably not. I can express this deficiency metalinguistically by saying that whereas I am reasonably confident that 'Aristotle' has a referent, I am certain that 'Zeus' does not and am unsure whether 'Helen of Troy' does. Or speaking ontologically, whereas I have no reason to doubt that Aristotle existed (i.e. that he "exists" in a tense-free sense of the verb), I am convinced that Zeus never did and have considerable doubt about Helen. (Since 'a exists' and 'a does not exist' are not objective contexts for 'a'--see Rozeboom, 1961; also below--the latter does not entail 'a is something that does not exist'. Neither does 'a' occur objectively in 'Expression

'a' has a referent'.) But the differentially problematic status of these names for me does not consist in my talking in one of these special ways. Rather, it lies most directly in how they affect the credibility for me of sentences in which they have their primary function as nominals. Specifically, if '... a ...' is a sentence in which nominal 'a' occurs objectively, the degree to which I believe '... a ...' is determined in part by the degree to which I accept 'a' as objectively nominal. For example, if anything can truthfully be predicated of 'Zeus', it is 'is a Greek god'. Indeed, 'Zeus is a Greek god' is definitive of 'Zeus'. Yet for me to believe 'Zeus is (was) a Greek god' would be inconsistent with my conviction that there are no gods, Greek or otherwise. But neither do I believe 'Zeus is (was) not a Greek god'. Both of these sentences are made deficient in beliefworthiness to me by their containing in objective context a nominal that I do not accept objectively.

Despite the considerable philosophic attention that nominals and their contexts, objective and otherwise, have received in recent times, virtually nothing has yet been done* (with the partial exception of my own past writings on the semantics of theoretical concepts) to broaden classical semantics, deductive logic, or credibility theory to encompass sentences containing objective occurrences of nominals whose degrees of objective nominality are less than perfect. A fragment of the

*In each of these theories, a further distinction is needed between the degree to which 'a' is in fact objectively nominal for me and the degree to which, in one normative sense or another specifically appropriate to semantics, deduction, or credibility, respectively, it should be accepted objectively, comparable to the distinction between defacto and de jure degrees of belief. All of that is far beyond the present scope.

needed extension in credibility theory is that for any universally quantified sentence ' $(\forall x)Px$ ' and nominal 'a', the joint credibility for me of ' $(\forall x)Px$ ' and its prima facie deductive consequence 'Pa' is an increasing function jointly of my belief in ' $(\forall x)Px$ ' and the degree to which I accept 'a' as objectively nominal.

Specifically, for an idealized credibility system in which Obj is a scale for objective nominality ranging from zero for complete rejection to unity for complete acceptance, and Cr is a scaling of sentence credibility that obeys the usual probability axioms when all nominals have perfect objectivity, $\text{Cr}[(\forall x)Px \cdot Pa] = \text{Cr}[(\forall x)Px] \times \text{Obj}[a]$. More generally in this idealized model, if nominal 'a' occurs objectively in 'Pa', $\text{Cr}[Pa] = \text{Cr}_a[Pa] \times \text{Obj}[a]$, where Cr_a is the credibility function I would have were I to accept 'a' objectively without question.⁵ Moreover, if 'P' is analytically predicable of 'a', as 'is a Greek god' is of 'Zeus', then $\text{Cr}[Pa] = \text{Obj}[a]$ and $\text{Cr}[\text{Not-Pa}] = 0$.⁶ Similarly, if $\text{Cr}[(\forall x)Px] = 1$, then $\text{Cr}[Pa] = \text{Obj}[a]$

⁵This model follows by embedding the object language in a metalanguage to which Cr is extended, and then exploiting certain special relations between objective and non-objective contexts for the same nominal, notably, that sentence 'Pa' is true only if Pa, and that if 'a' occurs objectively in 'Pa', then 'Pa' is true only if 'a' refers (equivalently, only if a exists).

⁶This assumes that 'Not-Pa' (i.e., 'a is not P' or 'a is non-P') is an objective context for 'a'. If that is so, 'Not-Pa' must be sharply distinguished from 'It is not the case that Pa', since when properly understood the latter is not an objective context for 'a'. Thus, since $\text{Obj}[\text{Zeus}] = 0$ (for me), $\text{Cr}[\text{Zeus was not a Greek god}] = 0$ but $\text{Cr}[\text{It is not the case that Zeus was a Greek god}] = 1$ --which seems paradoxical only when we fail to appreciate that for any sentence 'p', 'It is the case that p' is much closer in meaning to 'Sentence 'p' is true' than it is to 'p'.

and $\text{Cr}[\text{Not-Pa}] = 0$. None of this requires me to have an explicit "existence" concept; but if I do, and if I use it properly, then $\text{Cr}[(\forall x)(x \text{ exists})] = 1$ and $\text{Cr}[a \text{ exists}] = \text{Obj}[a]$, just as would obtain were 'a exists' an objective context for 'a'. However, since 'a' is not objective in 'a exists',⁷ $\text{Cr}[a \text{ does not exist}]$ can and does generally

⁷That 'a' is not objective in 'a exists' is not effectively diagnosed by "such-

that" paraphrasing, since it is not at all evident that 'a exists' diverges from 'a is such that it exists'. Rather, we can argue for this from (i) the assumed principle that 'a' occurs objectively in 'Pa' only if it is also objective in 'Not-Pa', and (ii) the observation that 'a does not exist' is far from equivalent to 'a is something that does not exist.'

exceed zero; in fact, $Cr[\underline{a} \text{ exists}] + Cr[\underline{a} \text{ does not exist}] = 1$ in the idealized model, so that $Cr[\text{Zeus does not exist}] = 1$ for me.

Probably few philosophers of language will look favorably on this account of nominal objectivity without vastly more supporting argument than I have provided here; but that will have to await another occasion. For present purposes it suffices just to insist that my epistemic economy does in fact budget for varying degrees of warranted distrust of particular nominals in objective contexts, and that this scepticism's pragmatic force is to attenuate my belief in sentences that use these nominals objectively. In order to propositionalize my objectivity suspicions about a given nominal 'a', I adopt the ontic predicate 'exists' and mirror the degree to which I accept 'a' objectively by the strength of my belief in 'a exists'. But the existence claim is parasitical upon my objective usage of 'a' and readily becomes vacuous when detached from this.

A similar but more complicated story applies to my semantical concept of "reference." When I inquire into how my language is coupled with the world, I often find that my degree of belief in a metalinguistic sentence 'S('a')', in which a linguistic quotation of nominal 'a' functions nominally but which contains no objective occurrence of 'a' itself, derives in part from my belief in some sentence 'P(a)' containing 'a' only objectively. That is, to exploit a familiar and valuable distinction, I use 'a' (objectively) in 'P(a)' but mention it in 'S('a')'. For example, my belief in

The garbage truck didn't come today

is the main determinant of how strongly I believe

The sentence formed by adjoining 'didn't come today' to 'The garbage truck' is true.

But when I attempt to generalize the inferential force of 'P(a)' for 'S('a)'—that is, when I seek the principle according to which 'P(x); therefore S(w)' is a plausible inference schema under the constraint that any nominal substituted for place-marker 'x' must also be substituted within linguistic quotes for 'w'—and especially when I try to extract what is essentially supportive of 'S('a)' in 'P(a)' without using nominal 'a' objectively (since I may well distrust the objectivity of 'a'), I find it necessary to hypothesize a theoretical predicate 'D(w,x)', in which 'w' and 'x' are place-markers for nominals used objectively, that has the following credibility properties: (1) For any nominal 'a', I believe each of 'D('a',a)' and '(∃x)D('a',x)' just as strongly as I accept 'a' as objectively nominal. (2) When 'P(a); therefore S('a)' is an inference whose plausibility depends (up to synonymy-preserving replacements of parts) on the use/mention connection, then sentences 'For any x, if D('a',x) and P(x), then S('a)' (e.g., 'If 'a' is D-related to any x such that P(x), then 'P(a)' is true') and 'For any w and x, if D(w,x) and P(x), then S(w)' both have essentially the same credibility for me as does 'If P(a), then S('a)'. My verbal label for this D-predicate is 'designates' or 'refers to'; and if I also entertain ontological beliefs, Cr['a' refers to a] = Cr[a exists] for me.

The essential semantic point here is that when I both use and mention a nominal 'a', what I do by means of this juxtaposition usually exploits the special meaning relation between the concepts on opposed sides of the use/mention contrast that I can justify and generalize only by appeal to a predicate 'D(x,y)' such that in any act of reasoning that exploits joint use and mention of 'a', I can replace all objective occurrences of 'a' by some other nominal 'b' not tied to 'a' in this special way and still preserve the argument's force by adding the premise 'D('a',b). Similar needs—to justify and generalize—arise when I jointly use and mention some expression other than a nominal; but constructing the appropriate aboutness coupling is more complicated in the latter case, inasmuch as it cannot be characterized by a simple predicate whose arguments are nominals. (More on this later.)

The Practical Import of Ontological Uncertainty.

We come at last to the existence perplexity that systems theory must face openly at some point. By rights, which nominals I accept objectively should be of major epistemic importance to me, since roughly speaking I can hold no belief with confidence that contains, or is based on an argument that includes, objective occurrence of any nominal whose objectivity I mistrust. Yet practical reasoning teems with objective use of nominals that well merit scepticism, not merely in everyday life but in science and other technical disciplines as well. Does that not make futility of our cognitive efforts?

Not necessarily. Often, what we say is not required to be literally correct, because it can be translated into a conjunctive complex in which the parts that err are not germane to the matter at hand. "Convenient fictions" can indeed be convenient if what is fictitious in them can be expunged without loss of anything but convenience. The ultimate challenge of problematic nominals is to distinguish the ones for which we have no real objective need from those for which we do; to make clear just how the former are dispensable despite naive usage to the contrary; and to become equally aware of the limits beyond which we cannot suppress all the nominals of certain broad ontic types without intolerable depletion of our cognitive resources.

As a final preliminary, I must explain what I shall mean by "ontic type." Any linguistic nominal 'a' is related conceptually to one or more predicates 'P' in such fashion that $Cr[Pa] = Obj[a]$, i.e. if a exists then necessarily P(a). When 'P' is such a predicate for nominal 'a', let us say that expression 'a' (not thing a) is of ontic type 'P'. All nominals belong to ontic type 'is an entity'; while at a much lower level of specificity, 'Zeus' is of ontic type 'is a Greek god'. (Eventually, I would prefer to restrict the concept of "ontic type" to predicates much more abstractly general than this latter example; but we are not yet positioned to draw the needed distinctions and such refinements will in any case not be needed here.) To the extent that I objectively accept some nominal 'a' of ontic type 'P',

I perforce also believe ' $(\exists x)Px$ '. And conversely, although I can believe ' $(\exists x)Px$ ' without believing ' Pa ' for any particular nominal ' a ', to the extent that I disbelieve ' $(\exists x)Px$ ' I must also reject objective use of all nominals of ontic type ' P '. No general theory of ontology, whether philosophical or scientific, can hope to deal individually with many of the nominals whose objectivity seems problematic. But it can usefully inquire of certain select ontic types whether these are true of anything. Hence if our objectivity qualms about specific nominals of ontic type ' P ' derive in large measure from unease about whether it is or even could be the case that $(\exists x)Px$, it makes good sense for ontological theory to concern itself with the general existence question, 'Are there Ps ?' Philosophers have for centuries skirmished inconclusively with the existence of "abstract entities" in various esoteric categories; and unhappily, these still-unresolved issues have far more practical import than even professional philosophers often seem to appreciate. Abstractly general queries whether properties/propositions/classes/facts/events/numbers/minds/etc. "exist" can easily appear to be--and in the journeyman philosophical literature usually are--little more than empty word games. But if taken seriously, they challenge the coherence of our thinking; specifically, they question whether we should allow ourselves objective use of certain prevalent types of complex nominals, and if not, how we can get along without that usage--really get along without it, not just profess to do so.

It is time for details. Ordinary language contains a surprisingly rich array of operators that convert any expression of the proper grammatical sort into a corresponding nominal. Among those that operate upon predicates are (1) definite description ('The x such that Px '), (2) class designation ('The totality of things that are P ', 'The set of all Ps ', etc.), and (3) predicate nominalization (' P -ness, ' P -hood', ' P -ity', ' P -ence', and others). Although there exists a vast philosophic literature on definite descriptions, these are relatively unproblematic ontologically. For while definite descriptions often fail, adjudicating whether 'The x such that Px ' successfully picks out a referent raises no issues de re beyond how frequently the

particular received predicate 'P' is satisfied. But class designation and predicate nominalization are a very different matter, since neither P-hood nor the class (set, collection, group, ensemble) of all Ps, if extant, is itself a P. What ontic-type predicates apply to 'P-hood' and 'the class of Ps', and how we determine the extensions of these^{types}, is poorly if at all illuminated by our understanding of predicate 'P'. I shall largely ignore the ontology of classes on this occasion. But the existence of properties--i.e. the alleged referents of nominalized predicates--is a large part of present concern.

Consider the difference between

- [6.4] John is tall,
- and
- [6.5a] John exemplifies Tallness,
- [6.5b] John has Tallness,
- [6.5c] Tallness is a property of John,
- [6.5d] Tallness characterizes John.

I intuit little difference among [6.5a-d] beyond paraphrase (though one might raise an eyebrow at emergence of the label 'property' in [6.5c]), and list these alternatives only to note in passing the abundance of idiom at our disposal for coupling nominalized predicates with their erstwhile arguments.⁸ In contrast, [6.4] differs

⁸To be sure, it is also worth noting that while nominalizers '-ness', '-hood', '-ity', etc. seem clearly equivalent in meaning, they are seldom idiomatically interchangeable. Even so, the grammar that allows us to speak of Tallness but not of Tallhood or Tallity appears keyed to surface features having only dubious philosophic significance. Thus, 'P-ity' is good English just in case 'P' ends in '-ar' (e.g. 'circular'). The latter is not entirely without ontic provocation, inasmuch as it demarks adjectives derived from certain common nouns. Yet '-like' converts these same common nouns into adjectives whose nominalization calls for the '-ness' operator. Enough said.

importantly from [6.5] in that the latter all contain an existence commitment that [6.4] prima facie lacks. What shall we make of this difference? One thesis, which I shall call reifism, is that the difference is largely illusory because [6.4] is elliptical for some variant of [6.5]. Extreme reifism—or more precisely extreme predicate reifism, since eventually it will be useful to let "reifism" comprise a variety of nominalization ontologies—claims that every sentence of surface form 'P(a)' is just idiom for what is most perspicuously written as 'a has P-ness' with 'a has ___' an objective context for the nominalized predicate. In contrast, anti-reifism holds that nominalized predicates never have referents, not just because all predicates created by fallible mortals are flawed by semantic imperfections (e.g. vagueness) that thwart success at reference, but because every nominalized predicate is of ontic type 'is a property' and there are no properties.⁹ In

⁹This "reifism vs. anti-reifism" opposition is more traditionally known as "realism vs. nominalism". However, philosophers have often used 'realism' to mark other contrasts as well, while the term 'nominalism' is singularly ill-suited to identify a particular constrictive view of which nominals refer.

anti-reifist eyes, nominalized predicates are tolerable in objective contexts only as figures of speech that can be set right on demand, and are never to be quantified over or otherwise taken as substitution instances of logical variables. So viewed, [6.5a] is just a fancy way to say [6.4], and does not, contrary to its misleading surface form, entail 'John exemplifies something.'

But unhappily for ontological simplicity, neither of these extreme positions seems at all plausible. To become disenchanted with extreme reifism, one needs only to consider its harvest from complex predicates. For example, from just two monadic predicates, say '___ is tall' and '___ smokes a lot', we can construct not only monadic molecular predicates like

x is both tall and smokes a lot,
x is either tall or smokes a lot,

but also relational predicates of arbitrary polyadicity such as

Either x or y is tall,

Unless x is tall, y and z both smoke a lot,

etc. ad infinitum. If John is tall and smokes a lot, even if we have no qualms about including Tallness and Smokes-a-lot-hood among John's properties, do we really feel that Tall-and-smokes-a-lot-hood and Either-tall-or-smokes-a-lot-hood belong on the list as well? Do the relations in which tall John stands to Mary include (One-or-both-being-tall)-ness'?¹⁰ Even more boggling are nominalizations of

¹⁰Everyday idiom does badly at nominalizing polyadic predicates; but we can manage the simplest cases after a fashion by the '-ness' operator--e.g., converting 'x is taller than y' into 'Taller-than-ness'--while for more complicated cases like 'x gave y to z' we can fall back on 'The relation of one thing giving another to a third'. Awkwardness of idiom is as such no mark against reifism, however; for idiom has trouble with complexly structured sentences even when reifism is not at issue. The concept of predicate nominalization is perfectly clear in full generality, and in formalized languages can be applied with equal ease to predicates of any complexity by use of the familiar lambda-operator.

of predicates containing a coordinate clause that is fully sentential. Can we really stomach, e.g., a property Tall-and-Mary-smokes-a-lot-ness corresponding to the predicate 'x is tall and Mary smokes a lot'?

The mere counterintuitiveness of such examples is not in itself strong evidence against extreme reifism. Commonsense intuition is a valuable exploratory guide but a notoriously unreliable one, especially in technical matters. But technical peculiarities, too, arise from wanton predicate nominalization. The logical paradoxes are a classic example, of which the well-known Heterology paradox is most directly germane here. We may take it as definitive of predicates 'property' and 'exemplify' that

[PPN] Principle of Predicate Nominalization: If ' $(\lambda x)Px$ ' is the nominalization of predicate ' Px ', and ' a ' is any nominal, then: (1) ' $(\lambda x)Px$ ' is of ontic type 'is a property'. (2) ' a exemplifies $(\lambda x)Px$ ' entails ' Pa '. (3) ' Pa ' and ' $(\lambda x)Px$ is a property' jointly entail ' a exemplifies $(\lambda x)Px$ '.

Let ' x is heterological' abbreviate ' x is a property that does not exemplify itself'. Then from PPN we find that 'Heterologicality is heterological' entails 'Heterologicality is not heterological' and conversely--a paradox that can be blocked short of abandoning PPN or Excluded Middle only by denying the objectivity of 'Heterologicality'.

Less paradoxical, perhaps, but scarcely less perplexing are cases like the following: Let 'Talltallness' and 'Selftallness' be the nominalizations of relational predicate ' x is tall or y is tall' and monadic predicate ' x is tall or x is tall', respectively. If predicate nominalization is always objectively successful, and John is tall, John exemplifies both Tallness and Selftallness, and also stands in Talltallness to himself. Clearly Talltallness is not the same as Selftallness, and for reasons having to do with the order of explanation it is not even comfortable to identify Selftallness with Tallness if both exist; yet it seems most strange to explain John's status in these respects by appeal to distinct realities. (This example will take on additional force when we consider sentence nominalization and its relation to predicate nominalization.)

Neither these nor any other test cases I know of conclusively refute extreme reifism, not even the Heterology paradox. (Rejecting nominalized predicates does not suffice to block all the important paradoxes that philosophers of language have discovered--certain versions of the Liar are particularly recalcitrant--and whatever solutions may eventually be forthcoming for the others may well defuse Heterologicality as well.) In fact, I doubt that extreme reifism can ever be seriously undermined just by exhibiting local distresses. Its main adjudication

must lie in its co-tenability or conflict with other putative principles of language/logic/reality that to one degree or another seem attractive even if far from apodictic.¹¹ We still have much to learn in that respect, most crucially concerning

¹¹ Many such principles take the form that if two predicates satisfy certain conditions, then their respective nominalizations cannot have the same referent. But unhappily for ontic simplicity, the initial plausibility of such principles usually deteriorates under close examination.

the inferential force of bringing the logical machinery of quantification to bear, through nominalization, on expressions that are not prima facie nominal at the outset. But one conclusion seems reasonable enough, namely, that the plausibility of extreme reifism at this stage in our mastery of ontology is far from overwhelming. If so, it is entirely rational to demur occasionally at objective use of particular nominalized predicates. But how strong a demurrer, and how pervasive?

Rejection of extreme reifism leaves two main alternatives. One is (extreme) anti-reifism, already described here. The other is modest reifism, which accepts that properties do exist and can in principle be designated by nominalized predicates, but denies that every nominalized predicate necessarily corresponds to one specific property, anymore than every definite description succeeds in picking out a unique referent. It is conceivable in this view that no nominalized predicate in our language actually refers, simply because none may have achieved the ideal character required for this. But modest reifism insists on the right to quantify over properties as needs be, and to justify this by the in-principle legitimacy of using nominalized predicates objectively.

Do not mistake modest realism's maybe-yes-maybe-no posture for the easy way out. For unlike either extreme reifism or anti-reifism, modest reifism stands responsible for identifying principles that discriminate when predicate nominalization succeeds, even while also undertaking to explain the semantic functioning of predicates that are not linked by simple correspondence to de re properties.

Anti-reifism shares the second but not the first of these concerns, while neither is a problem for extreme reifism.

The Practical Necessity of Predicate Nominalization: Three Arguments.

So why not avoid the discrimination problem by opting for anti-reifism? Simply because quantification over properties is epistemically indispensable to me. I can neither avoid nor find good reason to attempt avoiding belief in ' $(\exists x)Qx$ ' for certain predicates 'Q' that can be satisfied only by entities so intimately connected with certain other predicates $\{P_1\}$ in my language, or what would be predicates for me if my language were sufficiently complete, that it would be perverse not to conclude that Q-things are the referents of the nominalizations of $\{P_1\}$. I shall give three examples. The first is just heuristically illustrative, but the other two are both foundational for systems theory in their own right and will be examined in some detail.

1) Consider the statements, 'John is tall'. 'Mary is not tall', and 'John is taller than Mary'. All three of these are intuitively entailed by 'John is 78 inches tall and Mary is 65 inches tall'--but why? Not to make much of vagueness here, let us assume that 'x is tall' means the same as 'x is over 72 inches tall'. Then part of our problem is to say why, for every positive numeral 'N', 'x is (exactly) N inches tall' entails either 'x is over 72 inches tall' or 'x is not over 72 inches tall'. A simple answer would be that 'x is over 72 inches tall' is analytically equivalent to the disjunction of all predicates of form 'x is N inches tall' that entail 'x is over 72 inches tall'. But that won't work; first of all because our language contains infinitely many such predicates but no infinitely long disjunctions, and secondly, because it is far from certain that every individual over 72 inches tall satisfies a predicate of form 'x is (exactly) N inches tall' for one of the numerals 'N' actually available in our language. (Whereas height is presumably a continuum, our numerals are only countably infinite.)

The same problem arises if we seek to explicate so basic a thesis about height as 'Every human has a height' by equating this with 'Every human x is such

that x is N_1 inches tall or x is N_2 inches tall or ...'. We cannot actually complete this disjunction, and even if we could, we have no assurance that the height predicates actually in our language exhaust all the physical possibilities.

On the other hand, if we accept that properties exist which (a) satisfy the predicate ' is a height', (b) are designated (inter alia) by nominalized predicates of form ' x is N inches tall', and (c) are linearly ordered by the binary predicate ' exceeds ', we can analyze ' x is over 72 inches tall' as equivalent to 'There exists a height h such that x has h and h exceeds Being-72-inches-tall'. Then the entailment of ' x is over 72 inches tall' by ' x is 78 inches tall', and of ' x is not over 72 inches tall' by ' x is 65 inches tall', follows from the prima facie analyticity of 'Being-78-inches-tall exceeds Being-72-inches-tall', 'Being-65-inches-tall does not exceed Being-72-inches-tall', and 'For any object x and heights h_1 and h_2 , x has both h_1 and h_2 only if $h_1 = h_2$ '. (In what sense these height relations may be "analytic" is an important question of which an adequate account is still wanting.) Similarly, ' x is taller than y ' may be analyzed as 'There are heights h_1 and h_2 such that x has h_1 , y has h_2 , and h_1 exceeds h_2 '. Then 'John is 78 inches tall and Mary is 65 inches tall' entails 'John is taller than Mary' in whatever grade of necessity is enjoyed by 'Being-78-inches-tall exceeds Being-65-inches-tall'.

Many obscurities remain in this height example, including whether our predicates of form ' x is N inches tall' are really more basic than those of form ' x is over N inches tall' or even than ' x is taller than y '. But whatever may be the correct story, we have got to quantify over something to bring out the meaning relations among these predicates. Instead of quantifying over heights, one can try to make do with numbers (e.g., perhaps ' x is over N inches tall' can be read as 'There is a number n , larger than N , such that x is n inches tall') or even just with ordinary objects (notably, by equating ' x is over N inches tall' with ' x is taller than a_N ' for some distinguished individual a_N). So long as such options remain viable, the present example is not decisive against anti-reifism. But it

does raise a fundamental point: We often need conceptual resources having the force of what in an ideal language could be expressed as the disjunction of a set of predicates--except that this set has infinite or unknown cardinality, and its subset available in our extant language is in all likelihood incomplete. The only known way to fill that need by the linguistic resources actually at our command is through existential quantification over an appropriate domain, and the onus is on anti-reifism to show how we can effectively manage this without accepting properties as targets of reference.

2) Height predicates, or any other everyday examples, cannot sustain a convincing argument against anti-reifism because a plausible case can always be made that careful analysis reveals 'x is N inches tall' and other commonsense predicates to have such complexity that only the most extreme reifist can take their nominalizations seriously. Yet when formulating prospective principles of causal regularity in Sections II-IV, above, I quantified extensively over properties. That this is inescapable is shown by a two-staged argument: In the first place, quantification over what are prima facie properties is essential to assertions of scientific generality; and secondly, the order of explanation makes exceedingly implausible any prospect for eliminating these prima facie properties from our ontology.

Manifest need to ground assertions of causal regularity on sets of referencable properties stands forth most starkly in the very concept of "scientific variable," or more specifically "causal variable." Whatever variant of this notion one finds most convenient for a given technical purpose (see Rozeboom, 1966, or more briefly Sect. II above), its most essential ingredient is conception of a set $\{P_i\}$ of causal properties that are disjoint and disjunctively exhaustive over some domain D of loci or locus tuples. If we felt sure that all causal variables correspond to sets of predicates already in our language we could perhaps aschew this notion, but not otherwise. I shall try to make clear why.

Suppose that ' P_1 ', ..., ' P_n ' and ' Q_1 ', ..., ' Q_n ' are two finite sets of event

predicates such that ' P_1 ', ..., ' P_n ' are disjoint over domain D , that some of the ' Q_i ' may be the same for different values of index ' i ' but are otherwise disjoint over D , and that we feel sure that all the generalities

$$[6.6] \quad \{ (\forall x)[D(x) \cdot P_i(x) \supset Q_i(x)] \} \quad (i = 1, \dots, n)$$

are not merely true but also explanatory, i.e. that for any a in D such that $P_i(a)$, $Q_i(a)$ because $P_i(a)$. But suppose too that we are uncertain whether

$$(\forall x)[D(x) \supset [P_1(x) \vee P_2(x) \vee \dots \vee P_n(x)]]$$

is true even though the only predicates disjoint with ' P_1 ', ..., ' P_n ' over D are logical constructions in which all the ' P_i ' are negated, notably

$$\bar{P}_{1n}(x) =_{\text{def}} \sim [P_1(x) \vee \dots \vee P_n(x)] .$$

(I let n be finite here to make it easy for us to consider whether ' P_1 ', ..., ' P_n ' are exhaustive over D . We have already noted anti-reifism's difficulties in that respect when n is infinite.) By what conceptual devices can we speculate--not necessarily to believe, but just to entertain the possibility--that generalities [6.6] are incomplete in a way that is partly captured by saying, metalinguistically, that a sufficiently complete extension of our language would contain one or more additional predicates ' P_{n+1} ', disjoint with ' P_1 ', ..., ' P_n ' and not equivalent to any predicate now in our language, such that for some ' Q_{n+1} ' either disjoint with or one of ' Q_1 ', ..., ' Q_n ',

$$[6.7] \quad (\forall x)[D(x) \cdot P_{n+1}(x) \supset Q_{n+1}(x)]$$

is both true and on an explanatory par with the generalities in [6.6]? (Note that even if n is finite, so that ' \bar{P}_{1n} ' is constructable and can be added to ' P_1 ', ..., ' P_n ' to form a set that is exhaustive as well as disjoint over D , it does not follow that ' \bar{P}_{1n} ' has the same explanatory force as the other P -predicates even if substitution of ' \bar{P}_{1n} ' for ' P_{n+1} ' in [6.7] yields a true generality.) It would be irrational not to admit the possibility that [6.6] is incomplete in this way; yet

to do so articulately we must hypothesize something like: 'There exists a causal variable P over \underline{D} whose values include but are not exhausted by $\underline{P}_1, \dots, \underline{P}_n$, and at least one value \underline{P} of P distinct from $\underline{P}_1, \dots, \underline{P}_n$, such that'. (For simplicity, here and henceforth I use the same symbol for a predicate and its nominalization. Context easily tells which is which.) The only evident alternative is to quantify in my metalanguage over predicates, e.g. by conjecturing 'There exists a predicate ϕ , not now in my object language but which could be added to it, such that'. But that will not do at all, if only because the causal efficacy to be included in the existential hypothesis cannot be properly attributed to predicates in this case. To claim that $\underline{Q}_1(\underline{a})$ because $\underline{P}_1(\underline{a})$ is not at all the same as claiming that ' $\underline{Q}_1(\underline{a})$ ' because ' $\underline{P}_1(\underline{a})$ ' (which is grammatically ill-formed) or even that ' $\underline{Q}_1(\underline{a})$ ' is true because ' $\underline{P}_1(\underline{a})$ ' is true. When explaining through generalities [6.6] why \underline{D} -things are \underline{Q} -wise the way they are, we use predicates $\{\underline{P}_i\}$ rather than refer to them, and hence do not thereby meta-predicate anything of the ' \underline{P}_i ' that can be included in an existential hypothesis about additional object-language predicates by which the extent ' \underline{P} '-set might be extended. (In any case, quantifying over predicates, or over any other parts of speech, would be cold comfort for anti-reifism; for surely, whatever predicates are, they are themselves features--i.e, properties--of speech events and hence win the day for modest reifism.)

Let me push essentially the same point with a modified example. Suppose that we take ' $\underline{P}_1, \dots, \underline{P}_n$ ' to be exhaustive over \underline{D} , but find generalities [6.6] empirically acceptable only if their strict universality is replaced by a conditional that is only probabilistic. How can we speculate that there exist hidden factors that work jointly with the array of conditions characterized by predicates $\{\underline{P}_i\}$ to bring about conditions described by predicates $\{\underline{Q}_i\}$? This is entirely straightforward if we can use the conceptual apparatus of scientific variables and their values: We define variables P and Q to be the appropriate functions from \underline{D} into property-sets $\dot{P} = \{\underline{P}_i\}$ and $\dot{Q} = \{\underline{Q}_i\}$, respectively, and hypothesize that there is another variable or set of variables V over \underline{D} , and a function \underline{f} from

$\dot{V}x\dot{P}$ into \dot{Q} such that

$$(\forall x)[D(x) \supset Qx = f(Vx, Px)].$$

(Note that my very definition of ' Px ' in Sect. II, namely, that Px is the value of variable P exemplified by object x , presupposes that values of variables are entities to which definite descriptions can refer.) But if we don't exploit this apparatus, how otherwise might we hypothesize (i) that for every pair $\langle 'P_i', 'Q_j' \rangle$ of P -predicates and Q -predicates co-satisfied by some object in domain D , there is some way to fill the blank in

$$(\forall x)[D(x) \cdot _ (x) \cdot P_i(x) \supset Q_j(x)]$$

to yield a true statement of causal regularity, (ii) that the set of all such replacements for co-satisfied ' P_i ' and ' Q_j ' determine a partition of D , and (iii) that the replacements are not limited just to predicates already in our language. I cannot prove that this is possible only by quantification in the object language over entities other than the loci of causal events; but I can see no remotely plausible way to do so and submit that the burden of proof lies on the opposition.

There is simply no way for modern science to run its conceptual business without appeal to causal variables and their values as entities we can refer to and quantify over. (The preceding two paragraphs exhibit only a fragment of that need.) But it still requires arguing that values of scientific variables are indeed properties. When ' a is converted by predicate nominalization into ' a has (exemplifies) P -ness', the latter statement, unlike the former, has relational form ' $\dot{E}^2(a, P\text{-ness})$ ' in which ' $\dot{E}^2(x, y)$ ' is the dyadic predicate ' x exemplifies y '.¹²

¹²For extreme realism, this precipitates Bradley's regress. Specifically, nominalization of 'exemplifies' converts ' a exemplifies P -ness' into ' a is related by Exemplification to P -ness', the form of which is ' $\dot{E}^3(a, P\text{-ness}, \dot{E}^2\text{-hood})$ ', and so on for endlessly increasing polyadicities of Exemplification. It is not evident that this regress is vicious; but vicious or benign, modest reifism can block it simply by denying that 'Exemplification' has a referent.

But even if this form is essential, its interpretation may be negotiable. That is, given that ' $P(a)$ ' needs analysis as ' $R(a,b)$ ' for some dyadic predicate ' $R(x,y)$ ' and nominal ' b ' additional to ' a ', it may or may not be further necessary for ' R ' to be 'exemplifies' and ' b ' to be of ontic type 'is a property'. We have already touched upon three other alternatives, namely, that ' b ' might be a predicate with ' $R(x,y)$ ' read as ' x satisfies y ', that ' $R(x,y)$ ' might express some aspect of resemblance between objects of the same kind, and that b might be a number. To be sure, I have already argued against the first of these, and will shortly impugn the other two. But there is little hope that all possible readings of ' $R(a,b)$ ' can be adjudicated individually, simply because any relation R with domain D and range B is isomorphic to some relation R^* with domain D and range B^* for any set B^* with the same cardinality as B . Never mind the problematic ontology behind set-theoretical conceptions of isomorphism; the essential point is that even when a set $\{P_i(a_j)\}$ of sentences requires analysis into form $\{R(a_j, b_i)\}$ for some shared ' R ', unlimitedly many interpretations for ' R ' are possible. So why insist that the range of the R implicated by predicate nominalization comprise properties?

In the main, the answer is really quite simple. If we agree that ' $P_i(a_j)$ ' should best be written ' $R(a_j, b_i)$ ' in which ' b_i ' functions as a nominal whose objectivity we accept, then surely the overwhelmingly favored candidate for what nominalization of ' P_i ' refers to, the only one for which any decent case can be made, is b_i . If so, it is a matter of definition (cf. PPN, above) that b_i is a property. One cannot escape modest reifism by acknowledging that some nominal is implicit in predicate ' $P_i(x)$ ' but denying that it refers to a property. "Properties" are whatever bases for the satisfaction of predicates are picked out by these predicates' nominalizations. This does not mean, however, that analyzing ' $P_i(_)$ ' as ' $R(_, b_i)$ ' urges unconditional acceptance that ' P_i -ness' designates b_i ; for unless one's reifism is extreme, the option remains to deny that ' P_i -ness' refers at all. Thus for modest reifism, it is far more plausible to assign zero objectivity to the nominalization of '__ is taller than Mary' than it is to accept that

Is-taller-than-Mary-hood exists and is identical with Mary. Nominalized predicates designate properties only when their nominalizations are objectively successful.

To be sure, isomorphism still presents a problem: Of the presumably many objects $\{b_i\}$ such that $(\forall x)[P_1(x) \equiv R(x, b_i)]$ for some relational predicate 'R', how do we decide which one is P_1 -ness given that this exists? But for nominalization of ' P_1 ' to single out just one referent from this multiplicity of aspirants, it is not required that its selection principle be insightfully verbalizable with our present semantic-theoretical resources. We do know that nominalized ' P_1 ' refers to P_1 -ness if it refers at all; and while it would also be nice to learn something about the nature of P_1 -ness in terms analytically independent of ' P_1 ', it is presumptuous to expect much in that regard until our understanding of empirical semantics--i.e. how language relates to environment in the psychonomic functioning of real persons--develops far beyond its present infancy.

On the other hand, one good reason for doubting the success of predicate nominalization in particular instances may well be that their contexts of usage do not sufficiently restrict their eligible referents. But often--whether always is not so clear--we nominalize the predicate in ' $P_1(a)$ ' specifically to talk about why a satisfies ' $P_1(x)$ ', or to speculate about the causes or effects of a's P_1 ing. In such cases, we are envisioning for P_1 -ness a role in the causal order of events that in all likelihood is true of at most one of the entities that might otherwise contend for the identity of P_1 -ness. I have already argued on grounds of causal order against the propriety of taking the values of scientific variables to be predicates or other linguistic entities;¹³ and the nonreflexiveness of explanation

¹³This is not to deny that predicates, or something like them, can be values of variables in the causal flow of thought processes. The point is that the predicates we use to characterize a particular causal regularity are presumably not themselves participants in that regularity.

similarly forbids P_1 -ness to be ' P_1 ' if a's having P_1 is why a satisfies predicate

' $P_1(x)$ '. Again, we shoulder an enormous structure-of-explanation burden if we construe the nominalized ' P_1 ' to designate a prototypic particular a_1 such that ' $P_1(x)$ ' is coextensive with ' $R_p(x, a_1)$ ' for some dyadic P-wise resemblance predicate ' $R_p(x, y)$ '. (Ordinarily, we would explain a 's P-wise resemblance to some $b \neq a$ in terms of a and b both having P_1 ; but does it make any sense to consider $R_p(a, a_1)$ and $R_p(b, a_1)$ explanatorily prior to $R_p(a, b)$? And how might we then explain $R_p(a, a_1)$ by any principle that applies equally when $a = a_1$ and $a \neq a_1$?) As for the possibility that the nominalized ' $P_1(x)$ ' might designate a number, e.g. that Being-72-inches-tall is identical with seventy-two, the predicate ' x is 72 inches tall' is analytically equivalent to ' x is 6.0 feet tall'; yet surely not $72 = \text{Being-72-inches-tall} = \text{Being-6.0-feet-tall} = 6.0$. To be sure, perhaps it is not nominalization of ' x is 72 inches tall' that should be conceded reference here, but only that of ' x is y inches tall'. In this latter view, there is not just one Height variable whose value for John is designated both by 'Being 72 inches tall' and 'Being 6.0 inches tall', but uncountably many as illustrated by Height-in-inches, whose value for John is the number 72, and, what is not the same, Height-in-feet whose value for John is 6.0. (But how, then, do we explain the perfect correlation between Height-in-inches and Height-in-feet if these are distinct yet neither is explanatorily prior to the other?) More generally, the coordination of numbers with the values of scientific variables is far more complicated and poorly understood than the pervasive use of numerals in technical science would prompt one to suspect. (See Rozeboom, 1966, especially p. 219ff.) Predicates in which numerals occur either adjectivally or as prima facie nominals cry for deeper analysis and, when analyzed, appear high in the order of explanatory derivativeness, far removed from the causal floor on which we presumably find the values of causal variables.

I tentatively conclude that so long as "property P_1 " is understood more explicitly as "causal property P_1 ", there may well be at most one b_1 and ' R ' such that ' $P_1(x)$ ' is coextensive with ' $R(x, b_1)$ ' with b_1 playing the causal role attributed to it by this usage. Whether we often attempt predicate nominalization without

concomitant ascription of causal (or in other cases becausal) responsibility, and if so, whether these non-explanatory nominals deserve to be taken seriously, I cannot say. But the possibility does remain that our de facto usage of predicate nominalization is not all of a piece, that these are not semantically well-formed until our context of usage implicitly ascribes additional individuating attributes to the nominalization's referent, and that these reference-fixing attributions may be of more than one kind. If so, our ontology should envision correspondingly multiple kinds of properties or, what is essentially the same, should distinguish causal (or becausal) properties, designated by nominalized predicates accompanied by ascription of causal/becausal functions, from other kinds of entities to which nominalized predicates may possibly refer and which we may not care to think of as "properties" at all. Evidently, much work remains to clarify these prospects; but until the matter is better understood, I am prepared to stipulate that by "property," I mean "causal (or becausal) property."

3) There is another basic reason for insisting on modest reifism, one to which I have already alluded by speaking (p. 6.27) of conditions described by predicates, and of explaining why an object a satisfies predicate 'P_i'. Evidently, there must be more to reality than just particulars, else how can it be that some particulars satisfy 'P_i' while others do not. Yet so long as 'P_i' is a predicate about whose usage we have no qualms, we can evade explicit ontic commitments when accounting for its satisfactions by exploiting use/mention duality, i.e. by claiming that if a satisfies or does not satisfy 'P_i', this is because P_i(a) or not-P_i(a), respectively. (Cf. 'P_i(a)' is true or false according to whether or not P_i(a).) This "correspondence principle" is neither tautological nor otherwise trivial; for it entails--importantly--that semantic evaluations (at least of predicates) are dependent in the order of explanation on matters de re. Even so, the principle remains un insightfully incomplete until we can convert it into an open generality which does not require use of any predicate in the semantic space at issue. Specifically, we want semantic generalities of form

[6.8] For any n -adic predicate ϕ of kind K , any object n -tuple $\langle x_1, \dots, x_n \rangle$ satisfies ϕ just in case $S_K(\phi, x_1, \dots, x_n)$,

in which neither ' $S_K(_, _, \dots, _)$ ' nor any of its components is a predicate whose semantics is at issue, and where in ultimate versions of [6.8] the biconditional's righthand side is definitive of its lefthand side. It can be argued that unless the number of extensionally distinct predicates of kind K is necessarily finite, ' $S_K(_, _, \dots, _)$ ' in schema [6.8] must include quantifiers, possibly universal but certainly existential. Some of these quantifiers help articulate the logical structure required (in most cases) for a predicate to be of kind K ; but still others are needed to provide for something independent of language (or at least not requiring reference by linguistic quotation) that mediates between the predicate or its constituents on one hand and the objects of predication on the other. That is, ' $S_K(_, _, \dots, _)$ ' needs to quantify over whatever we are talking about, additional to a_1, \dots, a_n , when we attribute a predicate of kind K to an n -tuple $\langle a_1, \dots, a_n \rangle$.

To be specific, consider a fragment of one plausible form-[6.8] approach to predicate semantics. Let us stipulate that a predicate is basic just in case its nominalization has a referent. Then

[6.9] For any basic n -adic predicate ϕ , any object n -tuple $\langle x_1, \dots, x_n \rangle$ satisfies ϕ just in case, by definition, there is something that is designated by the nominalization of ϕ and is exemplified by $\langle x_1, \dots, x_n \rangle$.

Principle [6.9] neither implies that any basic predicates exist nor suggests how to test for that status. But if any predicates are basic, [6.9] characterizes their semantics without requiring any of them to be themselves used in the definiens. Moreover, recursive extension of [6.9] can define satisfaction for any predicate that is a logical construction out of basic predicates. A particularly simple example is

[6.10] For any monadic predicate ϕ that is analytically equivalent to the disjunction of two basic monadic predicates, any object x satisfies ϕ just in case there exist monadic predicates ψ_1 and ψ_2 such that ϕ is equivalent to the disjunction of ψ_1 and ψ_2 while x satisfies either ψ_1 or ψ_2 .

(More generally, [6.10] can let ψ_1 and ψ_2 be monadic predicates of any kinds for which satisfaction has been defined previously.) When appeal to satisfaction of basic predicates is replaced by [6.9]'s definition of this, [6.10] becomes

[6.10a] For any monadic predicate ϕ that is analytically equivalent to the disjunction of two basic monadic predicates, any object x satisfies ϕ just in case there exist monadic predicates ψ_1 and ψ_2 , and a property P , such that ϕ is equivalent to the disjunction of ψ_1 and ψ_2 while P is exemplified by x and is designated either by the nominalization of ψ_1 or by the nominalization of ψ_2 .

To extend the development illustrated by [6.10/6.10a] to all predicates analyzable as logical complexes of basic predicates requires a modicum of technical care. But this style of recursive construction has been routine since the work of Tarski, and needs not be detailed here. For easy reference, let us call the set of form-[6.8] semantical generalities so derived from [6.9] basal predicate semantics. Basal predicate semantics does not entirely avoid use/mention interchange, inasmuch as some of the expressions (or their equivalent) that are cited when characterizing the logical structure of kind- K predicates (e.g. that the predicates covered by [6.10] are disjunctions) are also used to state the conditions of those predicates' satisfaction; but this duality is required only for our logical connectives and quantifiers.¹⁴ Basal predicate semantics presumes a reifist ontology, but only a

¹⁴My present loose definition of "basal predicate semantics" permits this also to exploit a finite number of descriptive concepts by use/mention duality.

But no descriptive concepts have to be so used. To be sure, the line between "logical" and "descriptive" terms has never been drawn with uncontroversial precision. One possible demarcation (though probably not the most basic one) is that logical concepts are just the ones whose use cannot be avoided (up to alternative ways to span their total semantic space) by any version of basal predicate semantics.

modest one since logically complex predicates are not required to nominalize successfully (though neither is that possibility precluded). Thus if Tallness and Smokes-a-lot-hood exist, [6.10a] explains why tall John satisfies 'x is either tall or smokes a lot' without including Tall-or-smokes-a-lot-hood among John's properties. It is also important to note, though, that schema [6.8] and its instances [6.9,6.10] quantify flagrantly over predicates, thereby acknowledging the existence of things in every ontic category \mathcal{T} for which we consider 'Some predicates are also γ s' to be true. No theory of language that does not quantify over such types of "expressions" can hope to formulate any significant principles of language; and I have already suggested (without the argument which, however, is readily available) that this admits abstract entities sufficiently property-like to establish modest reifism's case. But it is beneficial to acknowledge this quantification openly.

Clearly, basal predicate semantics is a defensible view of how predicates relate to reality; but whether its account is either necessary or sufficient remains for debate. Sufficiency is an especially crucial issue with which this Section will conclude. But our immediate undertaking--support for modest reifism--concerns its necessity.

The "necessity" I see here is not so much for basal predicate semantics in exactly the form sketched above (though what alternatives are possible is not clear to me) as it is for some theory of predicate semantics in which principles of form [6.8] quantify in 'S_K(__, __, ..., __)' over nonlinguistic sources of predicate satisfaction. But my comments on form [6.8] and its illustrations have not yet

provided any real motive for seeking such generalities. Why should we care about metalinguistic predicate-satisfaction principles at all, much less feel discontent with grounding their formulation on unrestricted use/mention duality?

I give you a posit, one that extrudes from my long effort as a philosopher, scientist, and survivor of daily living to monitor the mechanism of my own thinking and shape this into a more effective instrument for pursuit of understanding and creditable belief: There is no aspect of one's mental functioning, linguistically coded or otherwise, that is not defeasible in multiple respects. "Rationality" is above all dedication to discerning and correcting defects in all such respects to the fullest extent that one can manage. I posit this not as a verbal thesis for debate, but as a nonnegotiable operational directive that governs not only my own intellectual life but also, I have reason to believe, that of most other persons whose thinking I most respect. Applied to our use of language, it raises first of all the generic question, What are the assorted ways in which specific linguistic expressions, or particular uses of them, may be improvably defective?, and then calls for specific study of each facet of imperfection.

From this pragmatic perspective, "semantics" in its most fundamental sense is the defeasibility appraisal of language and, more generally, of cognitive acts. "True/false" and "probable/uncertain/unlikely" are traditional dimensions of sentence evaluation, as are "valid/invalid" and "plausible/improbable" for arguments, "coherent/incoherent" for texts of many sorts, and "precise/vague" and "meaningful/meaningless" for (inter alia) subsentential expressions. But there are others as well, including some that extant philosophy of language has not yet clearly recognized. The objectivity of nominals is an important case in point; and I now submit that predicates, too, are to varying degrees problematic in their acceptability for use in standard contexts of predication. I trust each predicate 'P' in my language to a certain degree Tr[P] (scaled, say, to range from 0 for complete distrust to 1 for unquestioned acceptance) such that the degree to which Tr[P] is less than perfect for me attenuates the degree to which I can believe any

sentence 'P ...', no matter how tautological formally, in which 'P' functions predicatively. In a credibility system with standard quantitative properties, $\text{Tr}[\text{P}]$ equals the credibility of any logically valid sentence containing no non-logical constituents other than 'P', so that e.g.

$$\text{Tr}[\text{P}] = \text{Cr}[(\forall x)(\text{If } \text{Px} \text{ then } \text{Px})].$$

More generally, my trust in predicates works pragmatically for me very much like the objectivity I concede to nominals, and can largely be equated with the latter for predicates I consider basic. A strongly idealized credibility model that includes imperfect degrees of both nominal obj(ectivity) and predicate tr(ust) might be constructed as follows: (1) For each predicative occurrence of each logically primitive predicate 'P' in the set of sentences whose joint credibility is at issue, replace 'P(__)' by '__ exemplifies P-ness'. (2) Jointly distribute credibility over the sentences and objectivity over their nominals, including the nominalized predicates, under appropriate constraints on synchronic coherence. (3) Return each occurrence of '__ exemplifies P-ness' introduced by step (1) to its original 'P(__)' form while replacing Obj[P-ness] by numerically equivalent Tr[P]. This construction initially assigns trust values only to primitive predicates, but presumably tr can also be extended to logically complex predicates as a function of the trusts of their primitive constituents. (This extension's specifics are a technical challenge that I have not yet attempted fully to meet, even though for reasons mentioned later the matter has some importance.) I am not sure just how seriously this particular quantitative model should be taken, but in any case it illustrates qualitatively how a proposition's credibility is degraded for me by my distrust of its constituent predicates.

Precisely what it is that bothers me when I distrust use of particular predicates is obscurely multidimensional (as is likewise my objectivity appraisal of nominals). Consider the following prima facie logical truths:

- [6.11a] $(\forall x)(\text{If } x \text{ is a snark, then } x \text{ is a snark}).$
[6.11b] $(\forall x)(\text{If } x \text{ is a klutz, then } x \text{ is a klutz}).$
[6.11c] $(\forall x)(\text{If } x \text{ is a quark, then } x \text{ is a quark}).$
[6.11d] $(\forall x)(\text{If } x \text{ is an octuplet, then } x \text{ is an octuplet}).$

For me, these all suffer in believability from my unease about their respective embedded predicates, but to very different degrees in discernably different ways. Most conspicuously defective is [6.11a], since 'snark' is a nonsense word. And if 'is a snark' is meaningless, then not only do I not believe [6.11a], I cannot entertain it in any degree of belief/disbelief at all, anymore than I can believe/disbelieve the sentence fragment ' $(\forall x)(\text{If } x \text{ is } _, \text{ then } x \text{ is } _)$ '. One could say that 'is a snark' is not really an expression at all, and hence no more a genuine predicate than [6.11a] is a meaningful sentence.

And yet--is 'snark' really so empty of meaning that it totally lacks all semantic character? After all, Lewis Carroll's introduction of this term has left subsequent generations not totally unenlightened about the habitat and demeanor of snarks. Why should we not consider 'snark' to be a theoretical predicate defined by the theory cataloged in libraries under title, 'The Hunting of the Snark'? Admittedly, Carroll did not intend his poem to be so taken, nor is this theory well motivated by empirical data; but then it is not clear how seriously a theory must be believed, or how convincingly evidenced, before it confers sufficient meaning on whatever new terms it introduces to bring these within the purview of semantic concern. And if we do grant that 'is a snark' may truly be a predicate in our language even if of minimal semantic quality, by what metalinguistic resources can we then conjecture that it nonetheless lacks some of what is required for

$(\forall x)(x \text{ satisfies ' } _ \text{ is a snark' just in case } x \text{ is a snark})$

to be unobjectionable?

Whatever is wrong with 'is a snark', it is not just its lack of satisfiers.

In all likelihood, the predicate in [6.11d] is also unsatisfied; yet I find 'is an octuplet' nearly as trustworthy, and [6.11d] nearly as believable, as any predicate and sentence in my language. For 'x is an octuplet' means the same as 'x is a human whose mother bore exactly seven other children at the same time as x'. Due to the fuzziness I sense in every one of this predicate's descriptive terms, I do not trust it completely, anymore than I do its contraries defined with some other numeral in place of 'seven'. But neither do I trust it any less than I do its counterpart in which 'seven' is replaced, say, by 'two'. I consent to use 'is a triplet' simply because were I to talk at this level of tr I would be left with virtually no predicates at all for conducting my everyday affairs, and have no reason to think less of 'is an octuplet'. Accordingly, I do believe [6.11d]—not perfectly, but nearly as high as this well-practiced scientist ever goes. (Even so, I still want conceptual means to appraise the semantics of 'is a triplet' and 'is an octuplet' without being required to use these predicates or their cognates on the de re side of the appraisal.)

The defect in [6.11b]'s embedded predicate is so familiar that it takes some effort to appreciate that its semantical import has never been properly fathomed. 'Is a klutz' is vague, so outlandishly vague that few self-respecting semanticists would have the audacity to assert baldly that John satisfies ' is a klutz' just in case John is a klutz. (Even if 'x is a klutz' analyzes adequately as 'x is habitually and pervasively clumsy', the latter's vagueness is amply distressing for the purpose at hand.) And how can one feel genuine conviction in [6.11b], not just voice assent to it, when its content is so shapeless? Yet 'is a klutz' is far from meaningless: we do use this, and quite effectively too, in real-life linguistic communication. Whatever its defects, these do not seem to be entirely the same as the problem with 'is a snark'. But what can we say about the semantics of 'is a klutz'? It will clearly not do to claim that although this predicate is rich in "emotive" or "evaluative" meaning, its vagueness precludes its having any "descriptive" content and hence any truthful applications. The vagueness in 'klutz' differs not in kind but only in degree from the fuzz on every predicate of our language. To dismiss vague predicates as perforce having no satisfiers is tantamount to holding

that satisfiable predicates do not exist. If our theory of predication is not to be vacuous, we must allow that some things probably do in fact satisfy 'is a klutz', and other conspicuously vague predicates, in some fashion or degree. But if so, we can expect to gain little insight into those predicates' less-than-ideal semantics by deploying them in use/mention duality.

And what about 'quark' in [6.11g]? This is a construct recently introduced into particle physics by a theory of how subatomic particles are constituted--which is to say that 'is a quark' is not equivalent to any logical complex of older predicates, but is "implicitly" defined by its role in quark theory. The positivistic thesis that such terms lack cognitive meaning is two decades dead, nor do a theory's new terms seem any more "vague" than the antecedently meaningful ones it exploits. (Problematic, yes; but not in the manner of vagueness.) Rather, modern realistic construals of scientific theories would hold that quark theory may well be as true as any physical theory couched wholly in observational terms (even if the "observational/theoretical" distinction is cogent at all); and that if quark theory is true, 'is a quark' is as good a predicate as any in our language, surely of higher semantic quality than the crudities in our street talk. If so, I should be willing to believe both [6.11g] and

[6.12] $(\forall x)(x \text{ satisfies '___ is a quark' just in case } x \text{ is a quark})$

to at least the degree of my belief in quark theory.

Even so, one should feel acute discomfort in [6.11g] and [6.12] in light of findings that discredit quark theory. (Cf., 'Something satisfies '___ contains phlogiston' just in case it contains phlogiston'.) When a predicate 'P' is implicitly defined by a scientific theory that is factually incorrect, regardless of its internal sophistication or initial plausibility, there is surely something more defective about the relation of 'P' to reality than just failure of 'P' to have satisfiers. Thus, '___ is a 3,000 lb. quark' remains unsatisfied whether quark theory is true or false; but if the theory is false, it is not appropriate for me to believe even that there are no 3,000 lb. quarks. What most distinguishes my imperfect trust in

predicates like 'is a quark' is my marked uncertainty about the proper tr levels to allocate them. Unlike my management of predicates that seem merely vague, or merely low in meaningfulness, my acceptance-for-use of ones that are conspicuously "theoretical" can be shifted anywhere from near-perfect trust to total rejection by evidence whose linguistic expression neither uses nor refers to the predicates in question.

How many different dimensions of predicate defeasibility are illustrated by [6.11a-d] does not really matter here, since my distrust of a given predicate can arise from any or all of them. What does matter is (1) that when seeking to appraise how problematic predicates work for me, I cannot use these as I would predicates I highly trust without begging the questions I hope to answer, or at least thwarting any insights I might otherwise develop into their semantics; and (2) in at least some cases, how much trust seems appropriate for a given predicate 'P' depends somehow on the way the extralinguistic world is beyond just whether or not there are objects to which 'P' applies. It is pragmatically important for me that, from premises of form '... 'P' ...' and 'Q(a)' in which predicate 'P' is mentioned, nominal 'a' is used, and no expressions save perhaps logical terms are both used and mentioned, I be able to reach a metalinguistic appraisal of kind 'It is correct/incorrect for me to believe 'P(a)' by an argument whose conclusion is significantly dependent on what I predicate of 'a' in 'Q(a)' together with what is premised about 'P'. Any such argument must discern in its premises some stated or implied connection between 'P' and something that is implicated by my use of predicate 'Q()'—which is to say that I have to get one or more nominals, or their quantificational equivalent, out of 'Q()' somehow.

In summary, basal predicate semantics appears to be an inescapable first approximation to explaining the connection between the predicates we use and whatever it is that we are talking about (i.e. "extralinguistic reality" when we use them. I call this a "first approximation" for two specific reasons. In the first place, the recursion base envisioned by basal predicate semantics may not be

fully available to us. For even if basal predicate semantics adequately characterizes the aboutness of predicates in a sufficiently complete language of which my actual linguistic repertoire at any given time can be viewed as a fragment, my extant fragment may contain few if any of the basic predicates in terms of which the ones I do have are to be analyzed. (More on this later.) And secondly, our classic construal of semantic properties as binary contrasts--word y is or is not meaningful, sentence q is or is not true, nominal n does or does not refer to thing a which in turn does or does not satisfy predicate p, etc.--are simplistic reductions of what in semantic reality are undoubtedly multidimensional continua. It is understandable that these binary idealizations should prevail so long as our semantical theories remain grounded on use/mention duality. But future advances in the psychology of real-world cognition, guided by a philosophy of language sophisticated enough to appraise the semantics of less-than-ideal expressions, will almost certainly recognize spectra of alternatives in how words apply to objects, and will urge in particular that binary satisfaction and reference in my present formulation of basal predicate semantics give way to something more subtly complex. Given this more technically adequate account of what a theory of semantics seeks to explain, it may no longer be necessary to envision a sharp distinction between predicates that are basic and ones that are not.

On the Existence of Structured Complexes.

We are far from done with the ontology of predicates. But other complex expressions are also part of the puzzle. Especially foundational for the theory of system structure are complex nominals whose embedded terms seem to identify constituents of the former's alleged referent. Indeed, unless such nominalizations can sometimes succeed, there are no such things as "structures" (see Section VIII, below).

The states of affairs purportedly designated by nominalized sentences are a primary case in point. We must first acknowledge that there are many varieties of sentence nominalization. Thus, 'John loves Mary' is converted by gerundization into

John's loving Mary

(as in 'John's loving Mary grieved Jane'), but also by 'that'-prefixing into

that John loves Mary

(as in 'Jane fears that John loves Mary'), and by numerous operators of form

'the C that ___' into nominals of ontic type 'C', such as

the proposition that John loves Mary,
the possibility that John loves Mary,
the probability that John loves Mary,
the fear that John loves Mary,
the speculation that John loves Mary,
the fact that John loves Mary,

and many more. There is some overlap among these operators, but for the most part they convert a given sentence into referentially divergent nominals. I have argued elsewhere at some length (Rozeboom, 1975) that for any (declarative) sentence 'p', the referent of 'that p' in standard contexts is the propositional meaning expressed by 'p', indeed that our concept of "proposition" derives from our use of sentences nominalized by 'that'-prefixing. It can also be argued, though the matter is digressive at this point, that for almost all choices of 'C', 'the C that p' refers (if successful) to the value for that-p of some function on propositions, most often (as in 'the fear that John loves Mary') to a certain kind of mental act of which that-p is the propositional content (cf. Rozeboom, 1972, p. 38f.) At times, these type-specified nominalizations are ambiguous, most importantly the treacherous 'the fact that p', which is often evocative of 'the true proposition that p' even while being most fundamentally equivalent to 'Gp', where 'G' is the gerundization operator. (See Rozeboom, 1975, p. 116.)

Finally, no survey of sentence nominalization can afford to overlook the enigmatic context 'q because p' (e.g., 'John loves Mary because she looks like his

mother'), in which 'p' and 'q' function very like nominals even though their surface grammar as sentences is left unmodified. When 'p' and 'q' are atomic, all of the following seem essentially equivalent: 'q because p', 'Cq is due to Cp', 'Cq is why q', 'p, which is why q', 'Cp explains Cq', 'p, which accounts for Cq', and still other variations on explanatory contexts in which a sentence and its gerundization are virtually interchangeable. Thus, 'John loves Mary because she looks like his mother' can just as well be paraphrased,

John's loving Mary is due to her looking like his Mother.

Mary's looking like John's mother is why he loves her.

Mary looks like John's mother, which is why he loves her.

Mary's looking like John's mother explains his loving her.

Mary looks like John's mother, which accounts for his loving her.

Yet in more complex explanations, 'q because P_1 and P_2 ', say, is not evidently closer to 'Cq is due to G(P_1 and P_2)' than it is to 'Cq is due to Cp₁ and Cp₂'. And 'q because P_1 or P_2 ' gerundizes more happily into 'Cq is due either to Cp₁ or to Cp₂' than into 'Cq is due to G(P_1 or P_2)'. Accordingly, I suggest that when a sentence occurs as a sentence in explanatory contexts, it is best explicated as an ambiguous gr-nominal inviting whatever gerundized replacement is least ontologically offensive in that context. Or more roughly, translate 'q because p' as 'Whatever makes that-p the case explains whatever makes it the case that q'.

Although there is thus a multiplicity of sentence ontologies, corresponding to the different styles of sentence nominalization (albeit these seem to me to be at root only two), our present concern is just with gerundizations and whatever locutions are equivalent to these. Let us agree--what is really too evident to require such argument--that gerundized sentences do generally function as nominals. We are then positioned to stipulate that anything which is or at least can be designated by a gerundized sentence is a "state of affairs" or, more briefly, a "fact".¹⁶ Correlatively, we may understand 'the state of affairs that p' and

¹⁶I do not insist that facts always be equated with states of affairs, especially after having acknowledged that we sometimes endorse our confidence in the truth of certain propositions by calling them "facts". But this term's primary sense is de re, and I can discern no distinction between this and "states of affairs" unless it be an occasional suggestion that facts are atomic states of affairs.

'the fact that p' to mean the same as the gerundization, 'Gp', of 'p'. (Unlike literal gerundization, which is maladroit at idiom in transforming complex sentences, 'the fact that p' is idiomatically impeccable for any idiomatic sentence 'p'.) Given, then, that our use of gerundization commits us to a prima facie ontology of facts (states of affairs), how seriously should this be taken? We can dismiss at the outset the ultra-reifist possibility that every sentence refers when gerundized; for clearly if 'Gp' is to succeed, 'p' must at least be true. That is, Gp exists only if p. (If John doesn't love Mary. John's-loving-Mary inhabits the same philosophic fairy-land as Pegasus, phlogiston, and the Golden Mountain.) So by "extreme sentence reifism" let us understand the thesis that 'Gp' refers whenever 'p' is true. In contrast, "modest sentence reifism holds that states of affairs do exist, but acknowledges that 'Gp' may not designate one even when 'p' is true. And of course anti-reifism in this context never concedes reference to gerundized sentences.

I have argued previously (Sections I-V) that the relata of causal connection --events--are far too intimately associated with certain sentences of our language, or what would be such sentences were our language to contain the needed conceptual resources, to tolerate denial that these can be referenced by those sentences' nominalizations. The explanatory predicates '__ is a cause of __' and, more broadly, '__ is due to __' take gerundized sentences or paraphrastic/elliptic equivalents thereof for their arguments; and while the possibility always lingers that these predicates do not really apply to anything, or that their arguments' prima facie nominal function can be analyzed away, that is not a prospect I know how to take seriously. Accordingly, I judge modest sentence reifism to be the ontology of

choice for at least a domain of causal events. If so, reference to causes and effects anchors a sentence-realist thesis that may be called "basic sentence realism". Let us stipulate

Definition. Any sentence is basic just in case it is equivalent to some sentence ' $\underline{P}(a_1, \dots, a_n)$ ' ($n \geq 1$) in which each ' a_i ' is a referentially successful nominal and the nominalization of predicate ' \underline{P} ' also has a referent (i.e., ' \underline{P} ' is a basic predicate).

Definition. If the gerundization of sentence ' \underline{p} ' has a referent, sentence ' \underline{p} ' signifies state of affairs \underline{Gp} .

Then,

Basic Sentence Realism: For any n -tuple $\langle x_1, \dots, x_n \rangle$, any n -adic property \underline{P} exemplified by $\langle x_1, \dots, x_n \rangle$, and any sentence ' $\underline{S}(a_1, \dots, a_n)$ ' in which ' a_1 ', ..., ' a_n ' respectively designate x_1, \dots, x_n and the nominalization of ' \underline{S} ' designates \underline{P} , ' $\underline{S}(a_1, \dots, a_n)$ ' signifies $\langle x_1, \dots, x_n \rangle$'s having \underline{P} . That is, if ' $\underline{S}(a_1, \dots, a_n)$ ' is a true basic sentence, ' $\underline{G}(\underline{S}(a_1, \dots, a_n))$ ' refers to the fact that $\underline{S}(a_1, \dots, a_n)$.

The briefer statement of this thesis presumes a correspondence theory of "truth" for basic sentences sufficiently straightforward to require no comment at this point.

Acceptance of basic sentence realism really incurs very little risk so long as we also tolerate the ontology of set theory. For here, as for nominals of any problematic sort, one can accept the nominalization while seeking its referent among entities already accessible in more comfortable terms. Thus if sentence ' $\underline{P}(a)$ ' is basic, perhaps ' $\underline{G}(\underline{P}(a))$ ' designates either the 2-tuple $\langle \underline{P}, a \rangle$ or nothing according to whether it is the case that $\underline{P}(a)$. (More generally, the conjecture is that a successful ' $\underline{G}(\underline{P}(a_1, \dots, a_n))$ ' refers to the $(n+1)$ -tuple $\langle \underline{P}, a_1, \dots, a_n \rangle$.) I am not at all happy with this construal of a 's-having- \underline{P} , since intuitively the fact has an integrity that the tuple comprising the property and its satisfier surely lacks. But to make a convincing case that true basic sentence ' $\underline{P}(a)$ '

signifies something other than $\langle P, a \rangle$, we need to find a predicate ' $T(_)$ ' for which we are confident that ' $T(G(P(a)))$ ' is true but ' $T(\langle P, a \rangle)$ ' is false. Possibly ' $_$ is a cause of $_$ ' is such a predicate, since when a 's having P is a cause of g 's having Q it seems nevertheless implausible that $\langle P, a \rangle$ is a cause of $\langle Q, g \rangle$, especially when one looks to the law under which this determination is subsumed and worries how a 's being or not being P can make a difference for whether $\langle P, a \rangle$ causes $\langle Q, g \rangle$. (Note that the existence of pairs $\langle P, a \rangle$ and $\langle Q, g \rangle$ does not depend on whether $P(a)$.) But if we can bring ourselves to swallow that some property/object pairs are events while others are not, the chaser that causation relates just property/object pairs that are events goes down with only wild gagging. In any case, pending disproof that a 's having P could possibly be $\langle P, a \rangle$, I would sooner accept $\langle P, a \rangle$ as ' $G(P(a))$ ''s referent than to deny it any reference at all.

Granting, then, that there do exist states of affairs signified by true basic sentences, are any signified by sentences that are not basic? It is tempting to presume that basic sentence reifism envisions not just sufficient but also necessary conditions for a gerundized sentence to refer, i.e., that Gp exists just in case ' p ' is a true basic sentence. Can we, for example, balk at disjunctive or negative properties and yet accept disjunctive or negative states of affairs? But basal predicate semantics, for which I have expressed sympathy, does not preclude logically complex properties; it merely shows how semantic appraisal of complex predicates can get by without them. So if the gerundizations of certain sentences formed from complex predicates proves indispensable, that may be good reason to accept those predicates' nominalizations despite their complexity.

Even so, ruzzles quickly arise from complex predicates and corresponding states of affairs if one also seeks principles of their individuation, such as

If P and Q are different properties, then for any x having both P and Q , x 's having P is not the same fact as x 's having Q .

Consider again the predicates ' x is tall', ' x is tall or y is tall' and ' x is tall or x is tall', and assume that their nominalizations, 'tallness', 'talltallness',

and 'selftallness', respectively, are all referentially successful. Then if John is tall, John's-being-tall, John's-standing-in-talltallness-to-John, and John's-being-selftall are all facts. But the latter two are the same fact (since they are signified by the same sentence) even though Talltallness \neq Selftallness. And how does John's-being-selftall differ from John's-being-tall? One possibility is to equate these on grounds that Tallness = Selftallness. But to undermine that move, consider also the relation Cotallness, designated by the nominalization of 'x is tall and y is tall'. If John's-being-tall = John's-being-selftall = John's-standing-in-talltallness-to-John, then surely also John's-being-tall = John's-standing-in-cotallness-to-John. Yet since a's-standing-in-talltallness-to-b is not generally the same as a's-standing-in-cotallness-to-b for all tall a and b, how can it become so when a = b?¹⁷

¹⁷Still another prima facie difficulty is illustrated by John's-being-taller-than-Mary seeming to be the same fact as Mary's-being-shorter-than-John even though surely Tallerness \neq Shorterness. But one can cogently deny that Tallerness differs from Shorterness, on grounds that 'x is taller than y' and 'y is shorter than x' are just acoustically/graphically different expressions for the very same predicate concept, essentially of a kind with the variation of type-faces and voice timbres by which this can be expressed. In this view, when a relational predicate 'P(x₁, ..., x_n)' is ascribed to nominals 'a₁', ..., 'a_n' in sentence 'P(a₁, ..., a_n)', the predicate's argument is ordered not by successive integers, or by locations in a spatiotemporal sequence, but by the distinctive positions in the relational concept expressed by the spatiotemporally organized sign complex.

Complications like these show not so much that logically complex properties and correspondingly complex facts are untenable as that, for ontological peace of mind, it would be nice to avoid them. Unhappily, that may not be possible. The problem's root has already been noted, namely, that explanation relates states of affairs. Thus if 'p' is a sentence to be offered in explanation of something,

we must accept that 'p' (or, if 'p' is molecular, at least one of its atomic constituents) signifies some state of affairs.¹⁹ If so, it is important to recall

¹⁹Our only apparent out is the possibility that the prima facie nominals in 'Cp is a cause of Cq', 'Cp explains Cq', 'Cq is due to Cp', etc., are really just corruptions of 'q because p', while the latter does not implicitly gerundize either 'p' or 'q' or any of their sentential constituents. That prospect seems hopeless to me, but if anyone can initiate a halfway-decent argument for it I am prepared to reconsider.

from Sections III and IV that the noncausal preconditions of causal regularities generally need to specify the distribution of loci appropriately related to the locus of the dependent event.

Consider again (pp. 37, 43) the simple case wherein the value of causal variable Q is determined for any locus z by the values of causal variable P for all loci standing in exclusive relation \mathcal{R} to z. And let 'S₂(z₁, z₂, z)' abbreviate ' $\mathcal{R}(z_1, z) \cdot \mathcal{R}(z_2, z) \cdot z_1 \neq z_2 \cdot (\forall y)[\mathcal{R}(y, z) \equiv (y = z_1) \vee (y = z_2)]$ ', which is one way to say that z₁ and z₂ are the only distinct things \mathcal{R} -related to z. Then if there are just two loci, z₁ and z₂, \mathcal{R} -related to locus z, event $\dot{Q}(z)$ is determined by events $\dot{P}(z_1)$ and $\dot{P}(z_2)$ under some causal law

$$(\forall z_1, z_2, z)[S_2(z_1, z_2, z) \supset Qz = f_2(Pz_1, Pz_2)] .$$

In this case, z's having property $Qz (= f_2(Pz_1, Pz_2))$ is caused jointly by z₁'s having Pz_1 and z₂'s having Pz_2 . Yet that is not the complete explanation of $\dot{Q}(z)$, for the existence of additional loci \mathcal{R} -related to z would have created a situation in which $\dot{P}(z_1)$ and $\dot{P}(z_2)$ would not have causally sufficed for $\dot{Q}(z)$. Attempting to express this becauseal residual, we could say that $\dot{Q}(z)$ is due not only to events $\dot{P}(z_1)$ and $\dot{P}(z_2)$, but also to non-event state of affairs $\underline{C}(S_2(z_1, z_2, z))$. But that is not our only option. In the first place, we lose nothing by replacing the gerundization of 'S₂(z₁, z₂, z)' as a whole by the conjunction of the gerundizations of its conjuncts.

Secondly, there are various sets of sentences whose conjunctions have the same force as ' $S_2(a_1, a_2, g)$ ', some of which may seem preferable to the latter's conjuncts. (E.g., the part of ' $S_2(a_1, a_2, g)$ ' saying that a_1 and a_2 are the only things γ -related to g can be replaced by the weaker assertion that only two things are γ -related to g .) And most importantly, that a_1, a_2, g satisfies ' $S_2(x_1, x_2, g)$ ' may be a highly indirect or even "accidental reflection of what is genuinely the becausal basis for $\dot{Q}(g)$'s causation by just $\dot{P}(a_1)$ and $\dot{P}(a_2)$. Pending a later probe of this last prospect, this much seems clear: Whatever may be our ultimate theory of becausal antecedents in causal laws, we shall have to admit states of affairs that singly or in combination have the consequence that one rather than another number of loci are excursively connected in certain relevant ways to the loci of particular dependent events. It seems most unlikely that we can find a way to characterize all of these without use of quantification, negation, identity, and other logical constructions that may well evoke unease when included in the scope of objectively accepted generalizations. In short, there is good reason to fear that we cannot adequately explain events by appeal solely to facts signified by basic sentences.

Aggregates.

There is yet another important class of putative structured complexes that raises serious existence problems, namely, classes themselves and their kin. Let me acknowledge at the outset that quantification over classes occurs so pervasively in the foundations of technical science that the operative question is not whether classes exist, but how far we should trust what current set theory says about them. Yet of all the abstract entities that have worried philosophers, classes are the most obscure in nature. In other cases, notably properties, propositions, and facts, we can derive the category from a plethora of instances conceived by us prior to the category itself. But where in our language do we find nominals that designate classes without drawing upon an antecedently given concept of "class" or one of its cognates? How, that is, can we identify a class by means other than a locution of form 'the class (set, group, etc.) such that ...'?

I can think of just two expression-forms that come close to functioning in the manner wanted. One is 'All the things such that $P(x)$ ', and its briefer variants such 'All P_s ', 'The P_s ' or just ' P_s ', 'The Q_s that are P ' (where ' Q ' is restrictive), and also constructions using plural demonstratives that presumably go proxy for percentual predications, as in 'Those P_s over there' or even simply 'These'. The other form is ' a_1 and a_2 and ... and a_n ', in which the ' a_i ' are nominals. However, it is far from clear that these prima facie plural nouns are really nominals at all or, even if they sometimes are, whether they do the work of class names for us.

Consider some examples:

- [6.12a] John and Mary loathe each other.
[6.12b] John and Mary are impetuous.
[6.12c] Jim, Peter, and Michael are triplets.
[6.12d] All Ms. Jones' pupils are the same age.

Testing the italicized phrases for nominality quickly shows the inadequacy of my previous discussion of this regarding markers for number. If we allow the 'such-that' test to pluralize its copula and pronoun, all these expressions pass; otherwise, they do not. (Thus, [6.12a] and [6.12d] respectively accept paraphrase as 'John and Mary are such that they loathe each other' and 'All Ms. Jones' pupils are such that they are the same age', but not as 'John and Mary is such that it loathe each other' and 'All Ms. Jones' pupils is such that it is all the same age'.) Other tests for nominality here similarly pivot on pluralization. Thus, 'Something loathe each other' makes no sense; but 'Some things loathe each other' is a perfectly good consequence of [6.12a], and so is 'Some things are such that they are all the same age' of [6.12d]. Only by paraphrasing [6.12a] as 'John loathes Mary and Mary loathes John', which contains no cognate of 'John and Mary', do we find hard evidence that 'John and Mary' is not in fact a nominal in [6.12a]. (The implicit premise here is that if two sentences are paraphrastically equivalent, any nominal in one must be preserved up to analytic equivalence in the other.) Similarly, 'John and Mary'

is disqualified as nominal in [6.12b] if we read the latter as 'John is impetuous and so is Mary'. (On the other hand, might we disallow some of these proffered paraphrases precisely on the grounds that they do not preserve the nominals?)

Examples [6.12] leave obscure both whether there are plural nominals at all (i.e. whether passing the tests using plural copula and pronoun is in principle just as nouny as passing in the singular), and whether 'All P_s ' and ' a_1 and ... and a_n ' are ever clearly nominal. What we need are sentences of form ' $Q(a_1$ and ... and $a_n)$ ' (and similarly for 'All P_s ') that are not equivalent to a logical compound of sentences no one of which contains all of nominals ' a_1 ', ..., ' a_n '. Such are

[6.13a] Cabbages are more numerous than kings,

[6.13b] Ms. Jones' pupils are seven in number,

and perhaps

[6.13c] John and his most ardent admirer are just one in number.

To be sure, the prima facie plural nominals vanish from these if the Frege-Russell analysis of number^{*} perfectly paraphrases them. Yet in

[6.14] The number of Ms. Jones' pupils is seven,

the prima facie nominal is the argument of a function and, it can be argued, must therefore be accorded genuine noun status. Of course, it might be that [6.14] is only a corruption of [6.13b], just as we sometimes say 'a exemplifies P-ness' when 'P(a)' is all that we intend. But to protest [6.14] for that reason is to acknowledge that it is not entirely equivalent to [6.13b]. If so, [6.14] may be a genuine nominal occurrence of 'Ms. Jones' pupils'.

Can we amplify this prospect by other examples? Consider

[6.15a] The weights of Ms. Jones' pupils vary considerably.

[6.15b] The (combined) weight of Ms. Jones' pupils is 432 lbs.

[6.15c] Ms. Jones' pupils weigh 432 lbs.

[6.15d] Ms.-Jones'-pupils weighs 432 lbs.

Since [6.15a] is intuitively just an expansive version of 'John's weight differs appreciably from Mary's', one is tempted to seek an analysis of this in which 'the weights of Ms. Jones' pupils' splinters into multiple occurrences of 'x weighs y' or 'the weight of x'. Yet that analysis proves to be extraordinarily elusive, due above all to lack of any conceptual bound on how many pupils Ms. Jones' has. I will not claim that none can be found, but until then [6.15a] is best parsed as having form 'The y of x is P' in which 'x' is a nominal, in this case a plural one, embedded in another nominal, 'the y of x', which here is again plural. Example [6.15b] adds little to the prima facie role of 'Ms. Jones' pupils' in [6.14] as the argument of a function, but establishes a bridge to [6.15c,d]. One possible reading of [6.15b] is 'The sum of the weights-in-lbs. of Ms. Jones' pupils is 432'—which leaves unclear how to de-nominalize 'Ms. Jones' pupils' in [6.15b], but does suggest how one might attempt this if we knew how many pupils Ms. Jones' has. However, [6.15b] seems essentially the same as [6.15c] save for complexly nominalizing the predicate that occurs as a predicate in [6.15c]; and this predicate, '__ weigh(s) 432 lbs.', is not synonymous with some predicate asserting that several numbers sum to 432. (Addition is defined for numbers; it is not a function on weights, albeit it has an analog in the physics of extended bodies.) English conventions regarding subject/verb agreement in number seem to have no deep significance here, for

[6.15e] Ms. Jones' pupils weigh the same as Big Jim, namely, 432 lbs.
is clearly equivalent to

[6.15f] Big Jim weighs the same as Ms. Jones' pupils, namely, 432 lbs.

even though the verb is singular in one and plural in the other. Indeed, one way to resolve the potential ambiguity in [6.15c] (wherein the verb can be read either as 'all weigh' or as 'together weigh') is to singularize the verb and achieve matching singularity of subject by hyphenating the noun phrase as in [6.15d], albeit it is an important moot question whether such hyphenation is good English. Meanwhile, examples [6.15] do seem to provide solid evidence that prima facie

plural nouns are sometimes genuine nominals.

This point is important enough to warrant confirmation. Let 'x outweighs y' be a predicate defined operationally by placing objects on the ends of an appropriately large beam balance, say a well-constructed teeter-totter, and observing which side goes down. Then while

[6.16a] Big Jim outweighs John and Mary

could be understood to say that Big Jim outweighs John and also outweighs Mary, it also has a perfectly good alternative reading that can be made clear as

[6.16b] Big Jim outweighs John and Mary combined,

or

[6.16c] Big Jim outweighs John-and-Mary.

In the [6.16b,c] sense of [6.16a], it would be mischievous to contend that 'x outweighs y and z' is really a three-place predicate, as is 'x outweighs y and x outweighs z'. The verb 'outweighs' is binary in conception, and has exactly the same meaning in [6.16b,c] as it does in 'Big Jim outweighs John'. Similarly, Big Jim can just balance Ms. Jones' pupils (all together) without any hint that this is really a state of affairs in which some number assigned to Big Jim equals the sum of numbers similarly assigned to Ms. Jones' individual pupils. It should be further appreciated that this type of example--a predicate whose argument place or places can accept plural nominals just as meaningfully and even truthfully as it does singular ones--is not at all limited to weight, albeit totally convincing ones are harder to come by than one might anticipate.

Two additional examples are also worth considering. In

[6.17a] John has misplaced his Liberty Dime collection,

[6.17b] John has misplaced his Liberty dimes,

it is not entirely clear whether 'his Liberty Dime collection' and 'his Liberty

dimes' have exactly the same putative referent even given that John has no Liberty dimes other than the ones in his collection. But if John's-Liberty-Dime-collection = John's-Liberty-dimes, then clearly a prima facie plural noun can have the force of a singular nominal save for surface grammar governing markers for number. (Collections are flagrantly unitary in their linguistic demeanor.) And finally, an especially provocative example is

[6.18_a] The wind scattered John's papers,

[6.18_b] The wind scattered some of John's papers.

By itself, [6.18_a] has about the same illustrative import as [6.15_a]: We judge the claim's truth by appraising how each object satisfying the plural noun's embedded predicate stands to every other in a relation implicated by the sentence's main verb phrase (in [6.15_a] a similarity of weight, in [6.18_a] a wind-induced change in spatial separation); but cannot replace the plural noun by quantification over its embedded predicate unless this entails a finite upper bound on the number of objects that can satisfy it. But [6.18_b] is even more revealing. It is evidently an existential quantification--but over what? Certainly not over '___ is one of John's papers', as in 'There is a paper of John's such that the wind scattered it.' As a first approximation to [6.18_b] we might try

[6.18_c] There exists an x and a y such that x and y are both papers of John's and the wind scattered x and y.

But this is not quite right, unless it can be established that 'scatter' is so conceived that three of John's papers cannot be scattered unless two of them are. For that matter, even if [6.18_c] did correctly analyze [6.18_b], one can still argue that '___ scattered ___ and ___' is best viewed as the composition of a descriptor, '___ and ___', into the righthand place of binary predicate '___ scattered ___'. What the quantifier in [6.18_b] aspires to range over is a domain each entity in which could be designated in a suitably complete language by an expression of form

' a_1 and ... and a_n ', where each ' a_i ' names one of John's papers. How best to predicate membership in that domain is still obscure; but if '___ is a portion of John's papers' will do the job without begging too much of the question, [6.18b] can be explicated as

[6.18d] There is some portion of John's papers such that the wind scattered it.

Alternatively, we can avoid using 'John's papers' nominally while also retaining the plural idiom by converting [6.18b] to

[6.18e] There are papers of John's such that the wind scattered them;

but the domain of quantification in [6.18e] remains the same as in [6.18d]: A satisfier of ' x are papers of John's such that the wind scattered x ' must have a particulate texture described at least roughly by the predicates '___ is/are a collection' and '___ is/are an aggregate'.

These examples seem reasonably conclusive that category-free plurals of form 'the P_s ' and ' a_1 and ... and a_n ' do indeed function in some contexts as genuine linguistic nominals (which of course does not vouchsafe their referential success), even while those very same words in other contexts do not function that same way and should not be viewed as the "same expression" as the former. Taking 'the A_s ' to represent both forms ' P_s ' and ' a_1 and ... and a_n ' (note that we can always subsume predicate ' $x = a_1$ or ... or $x = a_n$ ' under the formalism ' x is an A '), let us provisionally say that 'the A_s ' functions collectively when it is truly a nominal and distributively when its function is best analyzed as a predicate under quantification.¹⁹ When henceforth I characterize some plural expression as being

¹⁹This ambiguity between collective and distributive function also applies to plural expressions of less-than-perfect universality, such as 'most A_s ', 'some A_s ', 'a few A_s ', etc. (Cf. [6.18b].) However, these do not evidently implicate an ontology any different from that of the 'all A_s ' form.

a "plural noun" in a given context, not just as prima facie so, I imply that its function in that context is collective.

Although my previously suggested tests for nouniness do not discern whether a given occurrence of some prima facie plural noun has in fact a collective rather than distributive function, it turns out that this can be revealed rather effectively by finding a paraphrase for the sentence, 'Q(the As)', at issue in which the verb associated with 'the As' is singularized. If this verb-singularization can be accomplished by converting 'the As' into 'each A' or 'every one of the As', 'the As' is distributive in 'Q(the As)'. Whereas if the singularization can be brought off just by treating 'the As' as singular, say by hyphenation as in [6.16c] or—reluctantly, since this introduces a category—by expanding 'the As' into 'the aggregate of As' or 'the collection of As' or their like, then 'the As' is collective in 'Q(the As)'. (Observe, however, that while 'the aggregate of As' and 'the collection of As' take singular verbs, these are clearly equivalent to 'the As in aggregate' and 'the As, collectively', both of which are still plural and still want plural verbs. This is particularly strong evidence that the grammar of number agreement has only dubious ontological significance.)

For example, the prima facie plural nouns in [6.15] can be revealed (or disambiguated) as distributive by rewriting these as

- [6.12a*] Each of John and Mary loathes the other.
- [6.12b*] Each of John and Mary is impetuous.
- [6.12c*] Each of Jim, Peter, and Michael is a triplet.
- [6.12d*] Every one of Ms. Jones' pupils is the same age as every other.

It is illuminating, however, that [6.12c*] is not what one would ordinarily understand by [6.12c] even though this turns out to be a perfectly good reading of it. There is no high English disambiguation of [6.12c] in its strong sense that requires Jim et al. to have been co-gestated; but 'Jim-and-Peter-and-Michael is triplets', or more simply 'They is triplets', crisply does the job by emphasizing that the

subject phrase is a single nominal. In this particular case, the ungrammatical 'They is ...' makes good idiom precisely because it demarks the plural noun as collective.

In contrast, none of [6.13]-[6.18] can be paraphrased to replace their instance of 'the As' by 'each of the As'. But each does accept singularization of its plural noun, if only by insertion of a category. Thus [6.13a], to which hyphenation is inapplicable, can be paraphrased as 'The aggregate of cabbages is more numerous than that of kings'. But one can also rephrase this simply as 'Cabbages is more numerous than kings' and hear the plural nouns as singular collectives. Indeed, that [6.13a]'s plural verb is dictated by surface grammar rather than by logic is demonstrated by its having existential consequence 'Something is more numerous than kings' rather than 'Some things are more numerous than kings'.

But now a deeper problem arises: Do all collective occurrences of plural nouns belong to a common ontological type more restrictive than 'is/are an entity'? If not, among what basic types do they divide, and how do these types differ? What motivates this question is that of the many descriptors that take plural nouns for arguments (i.e. nominals of form 'the K of the As', instanced by 'the ages of John and Mary', 'the first date of John and Mary', etc.), some have a special sense that can be expressed as 'the K comprising the As' or, equivalently, 'the K consisting of the As' or 'the K composed of the As'. Thus from 'John and Mary', we get inter alia

The combination of (i.e. comprising) John and Mary,

The group consisting of John and Mary,

The aggregate of (i.e. composed of) John and Mary

The collection comprising John and Mary,

The herd/swarm/cluster/pack/etc. comprising John and Mary,

and above all

The class/set comprising John and Mary.

Are any or all of these identical to one another? And which if any are identical simply to John-and-Mary?

To begin, I submit that it would be bootless to presume that when 'the As' occurs collectively, it usually differs in reference from 'the K comprising the As' for all familiar 'K'. Surely we have evolved these 'K'-concepts precisely as our way to disambiguate occurrences of plural nouns, serving first and foremost to demark them as collective rather than distributive. Indeed, what does it mean to say that x consists of (just) a₁ and ... and a_n unless it be that x is something that can be designated by 'a₁ and ... and a_n' in an appropriate context? (I cite 'a₁ and ... and a_n' here, rather than the less definite 'the As', to emphasize the role of 'and' as a nominal connective, distinct from its function as sentential conjunction. The latter may well be a special application of the former.) I shall provisionally assume, therefore, that whenever 'the As' occurs as a genuine nominal, it is equivalent to 'the K comprising the As' for one 'K' or another, at least to the extent that one should accept the latter as explication of the former. Any such category-label 'K' may correspondingly be called a "collective type", while an occurrence of plural noun 'the As' is of collective type 'K' just in case it can be replaced in that context by 'the K comprising the As' (with adjustments of surrounding idiom as appropriate) without change of referent. We may also consider any singular nominal to be of collective type 'K' just in case it is of ontic type 'K', as true in particular of 'the K comprising the As'. Finally, an entity k is a "K-collective" just in case it is, or in a suitably complete language could be, designated by a plural noun of collective type 'K'.

But what difference for the reference of 'the K comprising the As' does choice of 'K' make, anyway? Having raised this question, I can feel only dismay at the obscurity in which I shall have to leave it. For many such 'K', it seems intuitively clear that there is, or by rights ought to be, some 'K*' in our repertoire of collective types such that

$$(\forall x)(x \text{ is a } \underline{K} \text{ just in case } x \text{ is a } \underline{K^*} \text{ for which } P(x))$$

is true for some predicate 'P'. Thus, a "herd" must comprise animals; a "cluster" must comprise entities whose distances (in perhaps an abstract sense of "distance") from one another must be on the whole smaller than their overall distance from similar individuals not in this cluster; a "crowd" must be rather numerous as well as clustered, with a "mob" even more so; an "array" must in some sense be patterned; and so on. Not only are the conditions P that differentiate collective K from other species under its genus K* seldom conceived other than vaguely, the genus too is virtually always obscure. Presumably, herds, clusters, crowds, mobs, and arrays are all "aggregates"; but are they also groups and/or collections and/or classes? Yet if we are unsure, how are we to decide?

There may well be more to the force of 'K' in 'the K comprising the As' than just optional specification. If context suffices for a given occurrence of 'the As' to have a unique referent k, then its expansion into 'the K comprising the As' should refer either to k or to nothing at all according to whether k has the properties required of a K-collective. But an alternative possibility is that this occurrence of 'the As' has an ambiguity of reference that is resolved by selection of 'K' in 'the K comprising the As'. That is, perhaps several different collectives all comprise the very same As. Plural nouns do not feel ambiguous to me in this way when I use them, but that intuition could easily be in error. Yet if it is in error, how can I learn of this? Further, it may be rash to presume that for every meaningful choice of 'K', the referent (if any) of 'the K comprising the As' is also designated, if only ambiguously, by 'the As'. Possibly there is such a diversity of things all composed of the very same As that my only referential access to some of them is through nominals that stipulate their collective types. But once again, if that is so, how can I tell?

Unhappily, I see no present way to make headway on this and related problems of collectives more convincing than the murmurings of intuition. If it were entirely clear for at least some occurrences of some plural nouns what collective types they belong to, we could adjudicate such questions straightforwardly enough. But that is

just not so, at least not for me; I can think of no collective type 'K' and plural noun 'the As' for which I am quite certain that the As are collectively the same as the K comprising the As. I suggest, however, that this is in part because there is not really any right or wrong to the matter. Our collective types are nascent theoretical constructs whose defining theories have, with one notable exception, remained so primordially inarticulate that for a given collective type 'K', we are still largely free to choose what is or is not to be true of K-collectives. The exception is, of course, classes (or, almost equivalently, sets), whose governing generalities have been stipulated in fine detail by modern set theory. But like all axiomatic mathematics, the official theory says nothing about what, if anything, it applies to. Thus, while the class comprising the As is of course a class if it exists at all, set theory is mute whether the As themselves are collectively a class. More generally, for any nominal 'a' that is not of ontic type 'is a class', the only way to judge whether a is a class is to determine whether a and things like it behave the way set theory says classes behave—except that this behavior is described by set theory in terms of class-membership and class-inclusion, both of which are theoretical relations that must be assigned counterparts conceived outside of set theory before we can begin to appraise whether a is at all class-like.

We shall need to reconsider aggregates more carefully when seeking, in Section VIII, to identify the senses in which something can be said to be "structured." Meanwhile, it is useful to make explicit the assumption behind my treating the definite and indefinite forms of plural nouns as of the same collective type, and acknowledge the seemingly-evident connection between plural nouns and class-concepts that leads to an unexpectedly worrisome consequence.

Principle of 'and'-connection. There is a sense of conjunction under which plural nouns 'the Ps' and 'a₁ and ... and a_n' are co-referential just in case the satisfiers of predicate 'P' are exactly the referents of one or another of 'a₁', ..., 'a_n'. Moreover (the Principle's transfinite generalization), if predicates 'P' and 'Q' have exactly the same satisfiers,

'the Ps' is co-referential with 'the Qs'.

Class Ontology Prospect 1 [COP-1]: If there exists a class k whose members are just the satisfiers of predicate 'P', then 'the Ps' designates k. In particular, if the members of k are just a₁, ..., a_n, k is identical with a₁-and-...-and-a_n.

It is hard to imagine what 'and'-aggregates could possibly be if not classes. Yet it follows from COP-1 and the axioms of set theory that in the sense of conjunction by which class-names are generated from the names of their members;

Corollary. For any nominals 'a', 'b', and 'c', the referents (if any) of 'a and b and c', 'a and (b and c)', and '(a and b) and c' are all distinct. Similarly, for any predicates 'P' and 'Q' and nominal 'a', 'the Ps and the Qs' (i.e. '(the Ps) and (the Qs)') is not co-referential with 'the (P-or-Q)s', nor is 'the Ps and a' with 'the things that are either P or are identical with a', nor is 'a and a' with 'a'.

For otherwise, a class of classes would not differ from the union of its members—a distinction which is of the utmost centrality for set theory. I find this corollary disturbing, not so much in its being flagrantly counterintuitive as in my linguistic intuition's failure to find it evident. So far as I can discern, my nominal connective does not come equipped with grouping brackets; I just can't feel any de re difference between Peter-and-John-and-Mary and Peter-and-(John-and-Mary), nor between Tom-and-his-brothers and Tom-and-Dick-and-Harry given that Tom's brothers are just Dick and Harry. Perhaps that is only because, whenever need for collective grouping arises in my thinking, I turn immediately to the vocabulary of set theory instead of making do with uncategorized plural nouns. Or perhaps if I examine my use of nominal connection closely enough, I will find that not only does this in fact exploit groupings, it does so unavoidably, committing me to the entailed ontology regardless of my preference in the matter.

But delighted as I will be to receive such evidence--which had better be forthcoming if COP-1 is to retain its initial plausibility--I cannot myself yet produce it.

There are property-constructed alternatives to COP-1 that may appeal to a set-theorist who doesn't care where his classes come from. Perhaps most elegant is

Class Ontology Prospect 2 [COP-2]: For any predicate 'P', 'the class of all Ps' is co-referential with plural noun 'the properties that are exemplified by any object x just in case P(x)'. More generally, class k is identical with the-properties-whose-exemplars-are-exactly-the-members-of-k.

Since a₁-and-...-and-a_n must be distinguished from the-properties-exemplified-by-exactly-a₁,...,a_n, COP-2 denies COP-1. Under COP-2, the class of all Ps is a collective of all the properties co-extensive with P-hood, but is not a class of properties inasmuch as its aggregation is by the mechanism of plural noun.

With a few discomforts, notably, requiring us to accept at least one property wherever we want a class, COP-2 provides all the ontology that set theory needs for real-world applications. But for systems theory, it does not seem to be the right ontology. Regardless of details, we can appreciate that if "system structure" concerns the properties of complex objects, there must first of all be a sense in which such objects are compounded out of constituents, the fashion of their compounding being also an issue if more than one is possible. If the difference between loci and their attributes is fundamental for causal regulation of single events, with systems theory seeking to conceive of system objects vs. their holistic properties as something like this elemental contrast writ large, then system objects had better be locus complexes having an ontic character similar to that of their constituents. The K-collective comprising loci a₁,...,a_n that seems most ontologically similar to the individual a₁ is just a₁-and-...-and-a_n itself--which is to say that if COP-2 is correct to imply that classes are not the same as their members' aggregation by plural noun, then set theory is a dubious

framework (contrary to my formalism in Section V) within which to develop systems theory.

In short, while the ontology of classes remains an important problem, that of the referents of plural nouns is even more basic if the two cannot be equated. I would still hope that COP-1 can be sustained, despite its problematic Corollary; but meanwhile we must worry:

Does 'and' have more than one sense when used as a nominal connective? I.e., is 'a₁ and ... and a_n', or 'the Ps and a', or 'the Ps and the Qs' ambiguous?

and

For each sense of 'and' (if more than one), if nominals 'a₁', ..., 'a_n' all have referents, does collective noun 'a₁ and ... and a_n' always/sometimes/ever refer? If only sometimes, what determines when it does and when it does not? *

From there, we move to a host of more intricate questions, starting with the relation between definite and indefinite plural nouns (e.g., is the "Principle of 'and'-connection" proposed above entirely satisfactory?), and moving to combinatorial principles that may or may not find representation within axiomatic set theory if COP-1 fails. But these all presume that collections do exist. Believing otherwise is not genuinely an intellectual option for me, anymore than is an anti-refist stand on properties and facts; yet they do perplex. Of all complex nominals, plural nouns seem intuitively the most innocuous—but why? If they are really so ontologically secure, why is not the truth or falsity of COP-1's Corollary more intuitively evident to me?

The ontology of aggregates is far more important than it might at first appear. For a good case can be made (see Section VIII) that virtually every commonsense nominal, possibly excluding nominalized predicates and sentences, refers to some K-collective if it refers at all. If so, ordinary language is essentially incapable of truth unless collectives exist. Specifically, no matter how credible I find a given subject/predicate sentence 'P(a)' conditional on the existence of a, the unconditional plausibility of 'P(a)' for me is attenuated by my suspecting, for any collective type 'K', (i) that any referent of 'a' must be a K-collective, and (ii) that there are no K-collectives. (Even if I accept that some K-collectives exist, believing that 'a' can designate only a K-collective still cripples my confidence in 'P(a)' if I think that K-collectives exist only frugally, e.g. that there may well be no K-collective comprising a₁ and ... and a_n even when each of a₁, ..., a_n exists individually.) Conversely, however, probably the strongest argument we could mount for the existence of K-collectives would be that we have no practical way to conduct our cognitive affairs without accepting in principle the objectivity of nominals that are of ontic type 'is a K-collective'.

The existence of collectives is also closely tied to "emergentism," the obscure but perennial thesis that some properties of wholes are not reducible to the properties of their parts. Precisely what that might mean is unclear. But one reading is that not every true sentence about some K-collective k is equivalent to some sentence, or set of sentences, about the individual constituents of k. If that is to be denied, we must argue that whenever a prima facie plural noun occurs in a truthful objective context, its function is really distributive. And that in turn is essentially to say that prima facie plural nouns are never genuine nominals, or at least never objectively successful ones--which is surely indefensible unless one rejects the existence of collectives altogether. Conversely, to the extent that we give credence to at least one sentence containing a plural noun whose function is ineliminably collective, we perforce grant weak emergentism. Even so, that concession is merely prelude to a stronger and much more important version of

emergentism to be disapproved but not conclusively refuted in Section VIII.

Other part/whole issues are far more basic than emergentism, however, starting with the overlap among "parts," "constituents," "components," and still more like-minded notions (e.g. "ingredients"). In linking emergentism to the objectivity of plural nouns, I have already presumed that each A is a constituent of the As, and that constituents are parts. Even so,

- Q1. Are '__ is a part of __', '__ is a constituent of __', and '__ is a component of __' synonymous or at least co-extensive with one another? If not, is the extension of any one included in the extension of another?
- Q2. Can '__ is a part of __', or '__ is a constituent of __', or '__ is a component of __' be explicitly defined from predicates of form '__ is the K-collective comprising __'? For a given collective type 'K', if k is a K-collective comprising the As, is each A a part, or a constituent, or a component of k?
- Q3. For a given collective type 'K', if k is the K-collective comprising the As, is each part, or each constituent, of k an A?
- Q4. If every part, or every constituent, of k is also a part, or a constituent, of k', how is k related to k'? In particular, is k then "included" in k', or is k a part of k'?
- Q5. Is the relation '__ is a part of __' transitive? Reflexive?
- Q6. Do there exist entities that have parts, yet are not K-collectives for any collective type 'K'? Are "complexes," or "structures," examples of such?
- Q7. For a given collective type 'K', is a K-collective k always/sometimes/never such that k is the K comprising some, or all, of its parts?
- Q8. For a given 'K' and entities a₁, ..., a_n (n ≥ 1), is there exactly one K-collective comprising just a₁ and ... and a_n? If k is a K-collective comprising just a, is a identical with k?
- Q9. For a given 'K' and predicates 'A' and 'B', can the K-collective comprising the As be identical with the K-collective comprising the Bs even when some A is not a B? (I.e., does each K-collective have a unique particulation with determinate cardinality?)

Working out answers to these questions will be to an important degree a matter of how we choose to explicate the terms 'part', 'constituent', 'include', etc., our ordinary use of which is severely ambiguous or, more precisely, inchoate. But that certainly does not trivialize Q1-Q9; for these apply independently to each reading of the terms at issue. Different patterns of answers to such questions sharply distinguish one part/whole relation from another, including possible differentiation among fundamentally different kinds of "wholes," not all of which may be collectives (cf. Q6). Which Q-patterns can be realized by constructions developed from concepts already basic in our natural thinking, rather than introduced de novo by arbitrary axiomatizations that have no assurance of real-world applicability, is a fundamental issue that will be our first concern in Section VIII. In particular, we must seek realizations of the Q-patterns for set-theoretical membership and class-inclusion if we are not to remain apprehensive that classes may not really exist.

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