ABSTRACT. W.V.O. Quine's doctrine of referential inscrutability (RI) is the thesis that, first, linguistic reference must always be determined relative to an interpretation of the discourse and, second, that the empirical evidence always underdetermines our choice of interpretation—at least in principle. Although this thesis is a central result of Quine's theory of language, it was long unclear just how much force RI actually carried. At best, Quine's discussions provided localized examples of RI (e.g., 'gavagai'), supplemented merely by arguments for the (in principle) constructability of more general referentially divergent manuals. In defense of Quine, Gerald Massey provides a method for generating large-scale referentially divergent manuals for a complex language. I argue that, while Massey's rival manuals do meet Quine's translational criteria, they are demonstrably inferior to their commonsensical "homophonic" competitor. This result provides a clear indication of seminal deficiencies in Quine's behavioral approach to the theory of language. Next I argue that Quine's acceptance of standard assumptions about the nature of perception strongly influences the shape of his semantical theory. Finally, I suggest how an alternative to the standard account of perception might provide grounds for a more adequate understanding of language.
There can be no difference anywhere that doesn't make a difference elsewhere—no difference in abstract truth that doesn't express itself in a difference in concrete fact and in conduct consequent upon that fact, imposed on somebody, somehow, somewhere, and somewhen.

—William James

Any complete theory of language must be embedded in an adequate theory of mind. The reasons for this follow directly from many of the crucial questions which a theory of language must face: e.g., questions concerning how a natural language can be learned quickly and from relatively sketchy data, how speakers are able to "connect" words and larger linguistic constructions with individuals and classes of objects (and events) in the world, etc. The latter constitutes what I shall call the 'empirical foundation of meaning' question. There are, of course, many formal issues which must be resolved as well, but these have already received the major share of attention from philosophers and linguists. They will not be at the focus of attention in the present paper.

Although many theorists acknowledge deep connections between the philosophy of language and the philosophy of mind, relatively few linguistic programs incorporate anything approaching a developed psychological component. Even such an apparent exception to this rule as Chomsky's work provides us more of an innatist psychological schema than an articulated theory. By contrast, W.V.O. Quine's account of language represents a more significant exception because it builds in a rigorous and reasonably developed behaviorism from the ground up. Quine (1960) begins by presupposing a standard account of speaker-world interaction—i.e., perception, assumes a behavioral learning theory (which is used to forge crucial links throughout his entire program, see Quine 1974, 1975a, and Glotzbach 1979), and proceeds to construct a behavioral semantics. Upon this basis he develops an account of reference intended to be powerful enough to accommodate (at least) scientific and mathematical theories. Quine argues that his reliance upon behaviorism is a consistent strategy aimed, first, at satisfying the demands of rigorous physicalism (Quine 1979) and, second, at avoiding the obscurantism of mentalism (Quine 1975). This self-conscious appeal to psychological theory does much to give Quine's work its preeminent stature. In fact, it is difficult to find alternative (contemporary) theories of language which cover as much territory, while yielding such powerful and unified results—though some of these remain highly counterintuitive.

For such reasons, the literature teems with critical
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discussions of the Quinean program; yet many of these either ignore its psychological dimension altogether (see e.g., Field, 1974) or else proceed as if a general critique of behaviorism is all that is required (see, e.g., Chomsky 1966, 1975; and McCauley 1979). However, Quine takes pains to render his brand of behaviorism less vulnerable than is sometimes realized to standard antibehaviorist objections (see Glotzbach 1979, especially secs. 15, 22). Because of this, a better critical strategy is to investigate the consequences of Quine's philosophy of psychology as they surface in his theory of language. Thus the present paper examines Quine's doctrine of referential inscrutability, as well as an important defense of the Quinean position proposed by Gerald Massey. Specifically, after a brief discussion showing why referential inscrutability is an unavoidable consequence of Quine's program (I), I argue that--Massey's defense to the contrary--any theory of language containing a strong version of this doctrine is ipso facto highly problematic (II-IV). Next I attempt to show that a seldom-considered aspect of Quine's philosophy, viz., the theory of perception which his theory presupposes, bears a good measure of responsibility for his problems (V). Finally, I suggest that the difficulties can best be remedied by adopting a new approach to perception (VI) as part of the psychological groundwork for the philosophy of language.

I. REFERENTIAL DESCRIPTIVISM, BEHAVIORAL SEMANTICS, AND THE INSCRUTABILITY OF REFERENCE.

Referential Inscrutability (RI) comprises two main claims. First, Quine holds that it is impossible to determine the reference of an expression absolutely, i.e., as a reference to one specified object excluding all other possibilities. Second, he maintains that this impossibility is not just epistemic. The indeterminacy goes deeper: there is no such thing as the referent of an expression simpliciter. Instead, there are only interpretations of expressions which assign referents to them.

Quine's reasons for holding RI are quite straightforward. To begin with, he espouses what I shall label a 'descriptivist' view of reference. Referential descriptivism interprets reference as a function of a speaker's use of, or ability to appeal to, a set of descriptions which the referent uniquely satisfies.[1] Two characteristics of descriptivism are salient. First, it denies the existence of an irreducible or linguistically primitive operation (or speech-act) of reference. Descriptivism thus collapses reference into denotation, with the latter defined in terms of predicate satisfaction: a predicate expression denotes those individuals which satisfy it. Second, and closely related to the first point, descriptivist accounts provide (or should provide) clear answers to the question of how a
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referent is to be individuated. This requirement usually translates into an epistemic task. Such accounts often try to show how referring expressions incorporate the semantic resources needed to remove uncertainty—even if this is only a theoretical or methodological uncertainty—as to what the intended referent might be. For Quine et al., this entails the subthesis that the limits of predicative content for a linguistic system L fundamentally restrict the range of objects which can be individuated, identified, and so referred to by speakers of L.

Let us say that an utterance or statement u is referentially scrutable if an auditor (reader) h is able to determine unambiguously to what object the speaker (writer) s is referring. If we assume the worst case, that h and s speak different languages L and L', and if we also assume (for a moment) that there is such an item as the referent of an expression—at least on some occasions, then what is required for u to be referentially scrutable for h? It should be evident that, in general, scrutability for the descriptivist depends upon h's ability to determine accurately and unambiguously the semantic content of s's referring expression u based upon an adequate translation. For Quine in particular, this would require the successful completion of at least the following four tasks.

First (a), h must be able to identify in L' a set of linguistic components functionally analogous to the referential apparatus of L—in English: demonstratives, indexicals, quantifiers, (perhaps most importantly) terms of divided reference, etc. This is tantamount to claiming that the concept of reference itself is always defined in terms of a basic language, and that extensions of the concept to other languages remain parasitic upon the home language analysis. Second (b), the identified referential apparatus of L' must be schematized in some way approximating the Quinean canonical notation with its objectual interpretation of the quantifiers. This requirement follows directly from Quine's contention that questions of reference or ontology can be posed only when discourse has been suitably regimented in terms of the predicate calculus. (See Quine 1974, p. 136; 1975a, p. 79; and, 1979, pp. 159–60.) Next, (c), h must accept a lexicon or translation manual which pairs what are taken to be the terms of L', e.g., 'gavagai' with specific terms or compound expressions of L. And finally (d), h must settle upon the referential force of the home language expressions themselves.[2] From Quine's point of view, therefore, determinations of referential scrutability will always be relative to one or more interpretative commitments (or procedures). And so, despite the fact that reference (denotation) is defined as a relation between words and objects, determinations of referent (denotatum) turn out to consist primarily of interlinguistic and/or intralinguistic correlations. Such determinations concern relations between linguistic and nonlinguistic objects only
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in a secondary or derivative sense. (See Aune 1975, p. 233.) Consequently, the question of referential scrutability never admits of an unqualified affirmative answer in any given case. At most we can say that relative to some interpretation $M$ of $L'$, $s$ is talking about what we talk about (in $L$) as object $a$ under the description $D=\{d_1, d_2, \ldots, d_n\}$.

Combining the preceding with the indeterminacy of translation (TI) effectively drives a wedge between our intuitive demand for referential scrutability and what can actually be achieved. For TI assures us of at least the in principle availability of alternatives to any $M$ we might propose—those alternatives being equally defensible on empirical grounds. This thesis follows from Quine's (1960) now familiar contentions that, first, "words mean only as their use in sentences is conditioned to sensory stimuli, verbal and otherwise" (p. 17), and, second, that such patterns of conditioning are discernible on the basis of behavioral evidence if at all. But behavioral evidence, as Quine understands it, radically underdetermines our choice among possible mappings of one language onto another (or onto itself): hence TI. If Quine is correct to hold that the relevant evidence is limited to just what is behaviorally determinable, then TI does not just deny us the epistemic resources needed to discover the correct translation. Once again, the deeper claim is that given $n$ manuals, each of which accords equally well with all dispositions to linguistic behavior, there is no remaining "fact of the matter" which makes any one of them correct to the exclusion of the others.[3]

TI generates referential inscrutability by contaminating each of the interpretation requirements adduced above. in (a) it forces us to countenance possible alternative large-scale mappings of $L$'s referential apparatus onto $L'$; in (b) we must allow for alternative canonical schematizations of $L'$; in (c) we face alternative translation manuals for terms; and in (d), TI raises the spectre of competing interpretation schemes which map the home language onto itself. This result can be reduced to a slogan: descriptivism plus translational indeterminacy entails inscrutability of reference. RI first reared its head with the renowned gavagai example of Word and Object. But lest this appear too contrived, Quine (1969b) later introduced another case drawn from actual practice: a class of Japanese qualifier expressions which admit of alternative translations into English. In the first way, certain Japanese expressions are mapped onto English mass nouns; alternatively, they are mapped onto terms of divided reference. (E.g., four head of cattle, vs. four cows.) This purports, then, to be a real life example of RI.

But RI cannot be restricted to cases of translation; as already indicated, everything which has been said applies
with equal force to determinations of reference in the home language itself. Generally, Quine admits, we are able to interpret the utterances of co-speakers homophonically; however, he also reminds us that on occasion even this practice breaks down (as with certain colloquialisms, for example). And in fact, if we are so moved,

we can systematically reconstrue our neighbor's apparent references to rabbits as really references to Gödel numbers and vice versa. We can reconcile all this with our neighbor's verbal behavior, by cunningly readjusting our translations of his various connecting predicates so as to compensate for the switch in ontology. In short, we can reproduce the inscrutability of reference at home (Quine 1969b, p. 37).

Our neighbor's protestations to the contrary will be to no avail because we are free to reinterpret them as well. Nor does the indeterminacy stop there. For Quine also says, quite rightly, that

if there really is no fact of the matter, then the inscrutability of reference can be brought even closer to home than the neighbor's case; we can apply it to ourselves. If it is to make sense to say even of oneself that one is referring to rabbits and formulas and not to rabbit stages and Gödel numbers, then it should make sense equally to say it of someone else. After all, as Dewey stressed, there is no private language (p. 47).

Thus do the complementary theses of TI and RI come home to roost. Of course, Quine is neither denying that translation can and should be done, nor that there are better and worse translations, nor that we can distinguish between our (or our neighbor's) references to rabbits and rabbit parts. But he is claiming that questions of interpretation could always be raised, and once raised, could be resolved only by choosing among a range of (in principle) equally defensible interpretation schemes.

II. QUINE'S DILEMMA

Let us now play Moore to Quine's Bradley and raise a forthright, commonsensical question: what prevents Quine's doctrine of RI from constituting a reductio ad absurdum of his entire position? For I know that when I say, "Look at the rabbit eating our zucchini" I am not referring to Gödel numbers or to rabbit-stages, but to a rabbit—except, of course, insofar as a rabbit is a collection of rabbit-stages. Alternatively, if there is referential indeterminacy here, then it is surely trivial and for the purposes of any reasonable theorizing about language can best be ig-
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nored. This question presupposes the standard belief that speakers are able to provide commentaries upon their own linguistic activities which are sufficiently determinate and binding as to rule out anything approximating the kind of indeterminacy Quine claims to have uncovered. To clarify the objection, let me formulate it as a dilemma. On the other hand, if the force of RI is revealed adequately by the extremely limited range of alternatives generated by e.g., the gavagai or Japanese classifier cases, then the thesis amounts to little and is hardly worth the fanfare with which it promulgation has been accompanied. On the other hand, if RI does have any significant content, then it is unacceptable because it applies to one's own discourse. And it is obviously false to say that one does not know to what one is referring—at least in an important range of cases. To determine whether the intuitive objection stands up, I shall first consider several possible responses to the triviality charge.

The claim that TI is too weak to be of interest can be taken to mean that no discernible effects of the indeterminacy show up elsewhere in the system. An immediate reply would be that RI, like its progenitor TI, arises only after all speech dispositions have been taken into account. Therefore, the thesis itself entails that no effect of the inscrutability will turn up elsewhere (pace, James); consequently, the absence of such effects cannot be grounds for criticism. But this still leaves the question of why we should ever concern ourselves with such apparently inconsequential differences in the first place, so a more telling reply would stand upon the issue of ontological commitment. That is, the difference between the referential force of, e.g., 'four cows' and 'four head of cattle' may appear insignificant; however, in a more esoteric context the choice between reading an expression as a mass term or a general term might be of the greatest theoretical importance. Next, even apart from the issue of ontological commitment, Quine's account of language acquisition places mastery of general terms at the crucial boundary between nonreferential and referential discourse. And finally, if one takes descriptivism seriously, it becomes difficult to regard any predicative difference among denoting expressions as insignificant.

The central point, then, is that both the ontological commitments of a language as well as the competence of its speakers would be quite different in the cases where there really were only a mass term such as 'cattle' or only a general term such as 'cows' with its implicit individuation criteria. Such concerns are amplified considerably by Quine's dictum that the ontological commitments of a (suitably regimented) language or theory can be read off the bound variables of its true existentially quantified sentences. Conversely, any move to relax the Quinean standards and determine ontological commitment by appeal to larger segments
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of discourse would tend to lower the ontological stakes riding on RI. Thus in the latter case, Quine's opponent could grant that the situation in Japanese is as imagined, and still argue that either translation nonetheless leaves us (and the Japanese cowboy) with five animals. To put it more precisely: any effect of the referential slack in the case of cows vs. cattle could be contained so long as we attribute to the Japanese (what we take to be) standard beliefs about the cardinal numbers and biology. Quine's method of determining ontological commitment initially recommends itself by its apparent simplicity and directness. But if it makes the cows vs. cattle distinction significant, perhaps the position actually creates more problems than it resolves. Why not say, instead, that we should be more sensitive to the ontological force predicates acquire in the broader contexts of their use? One might compare Quine's (1963) own suggestion that "the unit of empirical significance is the whole of science."[4] If the interior sentences of a linguistic/conceptual system are said to have no meaning "in isolation," perhaps they should not be said to carry ontological commitment "in isolation" either. Interestingly enough, however, Quine (1979) recently seems to have pulled back from previous concerns with ontology since he writes, "Sentences, in their truth or falsity are what run deep; ontology is by the way" (p. 165). Whatever its merits, Quine's new position severely undercuts the attempt to defend RI on ontological grounds.

III. MASSEY'S d-MANUAL

Previous worries aside, the major impediment to establishing the nontriviality of RI has long been the absence of any compelling demonstration that large-scale alternative translation manuals, displaying substantive referential divergence, could actually be constructed for complex languages. Quine has suggested that the task of producing such full-blown alternatives is simply too large to be attempted. And so, he has rested his position on the relatively localized examples already noted, in conjunction with methodological arguments that larger scale deviant manuals are, at least, possible in principle. But RI clearly requires more than this. Thus Massey (1978) criticizes Quine's examples as unilluminating precisely where matters are obscure. They illustrate only local compensatory adjustments, long familiar to students of language. What needs showing is how to effect compensatory adjustments on a global scale, something Quine's examples fail to do. As evidence of translational indeterminacy they fare no better—they simply beg the question (p. 44).

To be interesting, the thesis of the inscrutabili-
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nty of reference must demand much more than occasional or isolated divergence of reference; it must require thoroughgoing referential divergence, divergence that cannot be waved off as a mere quirk of translation (p. 49).

Putnam (1975) constructs an algorithm for generating alternative manuals; nevertheless, the resulting referential differences remain reasonably well contained. By contrast, Massey displays several strategies for constructing full-fledged, large-scale alternative manuals (for a complex language)—thus providing the most vigorous defense of Quine's position to date.[5]

Let me stress two features of Massey's program. First, as will become clear, the referential divergence of his manuals is both significant and beyond dispute. For they do not differ merely by construing certain elements as, e.g., mass terms here and general terms there; instead, they incorporate much more sweeping changes in predication. This alone should rescue RI from any charge of triviality. Second, Massey is convinced that his alternative manuals will meet any methodological or empirical criteria relevant to the translation enterprise.

It is worth pausing to ask what this second claim might entail. Initially, Quine recognized four aspects of the translation enterprise as governed (at least in principle) by the empirical facts of the matter: viz., causal relations between environmental stimuli and dependable dispositions to "verdictive" speech behavior (Berger 1980, p. 261). (i) Observation sentences are translatable; (ii) truth functions are translatable; (iii) stimulus-analytic and stimulus-contradictory sentences can be recognized, though not necessarily translated; and, (iv) intrasubjective stimulus synonymy of occasion sentences can be determined, though the sentences cannot be translated (Quine 1960, p. 68). If tenable, (i)-(iv) would function as empirical objectives for translation, since any manual not achieving this much would fail to exhaust all the empirical resources available. More importantly, they would specify the empirical bounds of translation, since any additional L-L' mappings would depend upon empirically unfounded analytical hypotheses." Unfortunately, Quine was later forced to modify (ii) when he realized that the three-valued, behaviorally scrutable verdict logic of assent/dissent/abstention did not specify a complete interpretation for each truth functional connective (see Quine 1974, pp. 75 ff.; and Berger 1980). More importantly, Quine admits that even the linguist's fundamental classification of certain native locutions as assent and dissent itself depends upon analytical hypotheses (see Quine 1974, pp. 45 ff.; and Massey 1978, forthcoming). But this means, in effect, that all L-L' mappings fall under the control of analytical hypotheses and, hence, that (i)-(iv) cannot function as definitive
empirical constraints on translation. At most, they can play an important heuristic role. Massey (forthcoming) emphasizes this, and points out that his own d-manual (see below) "violates all three conditions [i.e., (i), (ii), (iii)] everywhere" (p. 49).

What empirical and/or methodological considerations survive to constrain our choice of manuals? Presumably, we are left with the relatively weak demand that genuine rivals provide equally successful mappings of speech dispositions onto objectively determinable happenings in the world. ('Successful' would be characterized in terms of comprehensiveness and predictive power.) For Quine, this means that genuine rivals must each fare (roughly) as well in connecting speech dispositions with classes of environment stimuli. (See V, below.) Additional and more general constraints such as simplicity, conformity with relevant (and noncontroversial) psychological laws (if such there be), and so on could also be invoked. However, any such move must be clearly motivated and vigorously defended to ward off obvious objections arising from the heterogeneity of psychological theories, the difficulty of defining 'simplicity', and other familiar problems. Beyond this, little can be laid down in advance.

Thus constrained, Massey (1978) considers three "translation manuals" for mapping a complex language \( \mathbb{L} \) onto itself: a homophonic manual (h-manual), a dualistic manual (d-manual), and a Cretan manual (e-manual: e for Epimenides). \( \mathbb{L} \) is described (a la Church) as a "first-order language with identity and alethic modality but without singular terms" (p. 50). (Individuating descriptions replace singular terms.) Each of the manuals requires its own set of psychological presuppositions about the speakers of \( \mathbb{L} \). For example, the e-manual's proponent holds that the speakers always lie to prowling linguists. As expected, the h-manual just maps the terms and sentences of \( \mathbb{L} \) onto themselves in accordance with a set of standard psychological presuppositions. It also licenses metalinguistic statements such as: "'rabbit' is true of rabbits," etc.

By contrast, the d-manual, with which we shall be primarily concerned, systematically reconstrues the one-place predicates of \( \mathbb{L} \) as the complements of what they mean according to the h-manual:

Where \( \_ \) is a general term, \( d(\_) \) is the complement term of \( \_ \). That is, \( d(\_ \_ \_ ) \) is true (false) of something just in case \( \_ \_ \_ \_ \_ \_ \) is false (true) of it. For example, whereas "rabbit" is true of rabbits according to the homophonic \( \mathbb{L}-\mathbb{L} \) manual \( h \), "rabbit" is true of all and only non-rabbits according to the dualistic manual \( d \) (p. 50).

A corresponding switch is made for the two-place predicates-
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-e.g., '>' is substituted for '<' and so on. Massey concludes:

Quite in general, then, the d reference of a general term is the complement of its h reference. More referentially divergent manuals are scarcely conceivable (p. 50).[6]

To compensate for this semantic inversion, the d-manual must translate all connective operators into their duals (e.g., negation by negation, conjunction by alternation, . . . ); it must replace universal (existential) quantifiers by existential (universal) quantifiers; and, it must substitute possibility (necessity) for necessity (possibility).[7] "Finally, the d-linguist posits assent (dissent) whenever the h-linguist posits dissent (assent)" (p. 50).

The consequences of these maneuvers are readily apparent, but it will be helpful nonetheless to consider a few specific examples. Take (1)-(4) to be sentences of L followed by the results of querying them of native speakers, as reported by the h-linguist and the d-linguist respectively. (Placing a horizontal bar over a predicate transforms it into its complement term.)

(1) All rabbits are vegetarians.
(1') (x) (∼Rx v Vx)? "Yes."
(1") (∃x) (∼Rx & Vx)? "No."

(2) Some firetrucks are nonred.
(2') (∃x) (Fx & ∼Rx)? "Yes."
(2") (x) (Fx v ∼Rx)? "No."

(3) Every book has a hard cover.
(3') (x) (∼Bx v Hx)? "No."
(3") (∃x) (∼Bx & Hx)? "Yes."

(4) Some stoplights are green.
(4') (∃x) (Sx & Gx)? "No."
(4") (x) (∼Sx v Gx)? "Yes."

Massey provides only (1) - (1") (with some added variations we need not consider), but all the others are equally sanctioned by the two manuals.

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At this point, there should be no question that Massey has supplied what we earlier demanded: viz., a set of rival manuals incorporating large-scale, interesting referential divergences. Moreover, as he also demonstrates, additional rivals can be constructed easily as variations upon the dualistic theme. The underlying logical mechanism is quite unexceptionable, and it should be evident that if the h-manual satisfies Quine's behavioral and semantic criteria, then the d-manual does so as well. That is, any behavioral evidence for h, suitably reconstrued, becomes equally persuasive evidence for d. Moreover, as mentioned above, Massey holds that d would satisfy any additional criteria which might reasonably be invoked. Thus we are told:

Whatever the linguists ask and however the natives respond, the h-linguist and the d-linguist will be equally convinced, on equally good grounds, [emphasis mine] that their respective manuals correctly translate L. And yet the d-translation of an \( L \)-sentence will be true or false (at a world) just in the case its h-translation is respectively false or true (at that world). Systematic interchange within the pairs truth-falsehood and assert-dissent guarantees that the d-manual will enjoy whatever success in the field that the h-manual meets with, and to exactly the same degree. And since each manual is as simple, parsimonious, etc., as the other, they qualify in every respect [emphasis mine] as rival manuals (p. 51).

If this is true, then much of Quine's point with regard to RI is indeed won. Certainly the charge that RI is trivial must immediately fall.

Does Massey's algorithm live up to his claims? I think not, and I shall argue that if Quine's theory of language entails the equivalence of the h- and d-manuals, as I believe it does, then it is in severe trouble. To see this we must turn to the other horn of the dilemma (II): the charge that RI establishes too much.

IV. THE WAGES OF INSCRUTABILITY

A translation manual \( M \) for language \( L \) can be regarded as a partial theory of meaning for \( L \)--relative to some other language \( L' \) or, as in the present case, relative to itself. \( M \) thus constitutes a partial theory of how \( L \) works, how speakers use it. As Massey (forthcoming) points out in response to criticism by Kirk (forthcoming), it is perfectly appropriate that \( M \) contain two "stages": the first comprising the expected lexicon and procedures for establishing sentence-sentence semantic equivalence; the second, identifying "mechanisms of illocutionary force," to enable the
linguist to "make appropriate adjustments" such as the d-manual's permutation of its counterpart's assent/dissent (p. 4).

Davidson (1965, 1967) has urged that any theory of meaning must show how a given language is (or how languages in general are) learnable. Specifically, a semantic theory must show how it is possible for a speaker to master only a finite set of rules and a finite vocabulary in a finite (and relatively short) length of time, and yet be able to construct the potentially infinite number of sentences expressible in the language. Davidson is concerned primarily with formal recursive rules of sentence formation and the like, but it is legitimate to generalize his underlying insight, and apply it with equal force, to other more empirical dimensions of linguistic theory. Thus Moravcsik (1975) specifies that

a theory of language . . . must be under the constraint that it cannot postulate semantic structures that would be impossible to learn by a human within the normal contexts (p. 56)

Such considerations are so basic to the understanding of natural languages that it is reasonable to reject any account of meaning (or reference) which can be shown to violate them. However, even a cursory inspection reveals L-as-represented-in-the-d-manual (L-d) to be unlearnable "within the normal contexts." Consider the case of observation sentences and observation terms. The former take on extreme importance in Quine's system—figuring prominently in his account of the relation between the evidential (peripheral) and the more "theoretical" (interior) components of a scientific theory, as well as in his account of language learning itself. He writes:

Observation sentences are crucial in two enterprises: in the conveying of evidence and in the learning of a language. Such sentences are necessarily our entering wedge into our first language; for clearly we can begin only by connecting heard utterances with concurrent stimulation and by being confirmed in our utterances by speakers who share the concurrent stimulation (Quine 1970, p. 4).

And it is this also, intersubjective observability at the time, that qualifies observation sentences as check points for scientific theory. Observation sentences state the evidence to which all witnesses must accede (Quine 1975a, p. 79).

Quine provides essentially the same behavioral account of the learning which occurs in each of the indicated contexts. And indeed, one even finds the (rather startling) claim that
although some observational expressions . . . are learned indi-
rectly by some of us, through explanations in oth-
er words[,] . . . all could be learned in the di-
rect way, [emphasis mine] such is their observa-
tional character (Quine 1979, p. 157).

Observational terms are more recondite, much less accessible
to behavioral criteria. So mastery over them depends upon
the ability to gain command over the corresponding sentenc-
es. In theory, therefore, one first learns when to assent
to the sentence 'Rabbit' or 'Position', and only later mas-
ters the full identity criteria associated with 'rabbit' or
'position' qua term.

Now what of the L-\textsuperscript{d} sentence 'Not rabbit'? What are
its "stimulus conditions," and more importantly, what pos-
tulated conditioning mechanism(s) could ever give a speaker
control over it? How could it be "learned in the direct
way?" Quine himself writes (1969c) that "learning to use a
word depends on . . . a resemblance between the present cir-
cumstances and past circumstances in which the word was
used" (p. 116-17). However, it is possible to specify (or
even to imagine ) sufficiently salient similarity relations
obtaining among circumstances in which it would be correct
to assent to or dissent from 'Not-rabbit'--without appeal-
ing, of course, to the truth conditions for 'Rabbit'? Quine
places great stress upon prior, hard-wired propensi-
ties which enable language learners to respond differentially
to different classes of stimuli. Such "innate quality
spaces" play a fundamental role in his behavioral account of
language acquisition, and they are presumed to be the prod-
uct of evolution. (See Quine 1969c, 1979.)

But what conceivable evolutionary process could gener-
ate the "quality spaces" needed to learn L-\textsuperscript{d}? We can
countenance learning 'Rabbit', then the term 'rabbit', and
finally the complement function separately. (Presumably,
some such sequence is a precondition for understanding Mas-
sey's paper.) But we can afford speakers of L-\textsuperscript{d} no such
advantages, unless we are willing, perversely, to make com-
petence in L-\textsuperscript{d} parasitic upon competence in L-h. So far
this objection is internal to Quine's system: it is just
not clear how a human speaker could master the observation
sentences (and eventually the observation predicates) of L-\textsuperscript{d}
by relying solely upon the restricted conditioning mechanism
posited by Quine. But if we allow the d-linguist to appeal
to some enhanced psychological theory T capable of doing the
work, then we must also ask what is to prevent T's being so
monstrously complex that it could be rejected on grounds of
simplicity alone--never mind its probable lack of fit with
other currently accepted psychological theories.[8] It goes
without saying that the main difficulties here stem from the
unmanageable diversity marking the extension of the L-\textsuperscript{d}
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observation predicates.

Davidson (1975) proposes a second constraint upon theories of meaning (a condition which Quine (1975b) has accepted implicitly), and yet one which the d-manual also violates. Davidson requires that a semantic theory \( M \) provide a method for deciding the truth conditions of any sentence \( L \) from the arrangement of its component parts: \( M \) must show that \( L \) is (semantically) "scrutable" in his terms. Once again, though Davidson is concerned primarily with formal rules of sentence formation, we should have no compunctions about extrapolating the underlying insight, viz., that a theory of meaning--and so, a translation manual--must explain how speakers are able to assign truth values to their sentences. In a purely formal sense, of course, the d-linguist will be able to construct Tarski-type truth definitions for \( L \) (more or less) as easily as will her h-counterpart. But consider a closely related notion which, following a suggestion by Massey (1981) I shall term 'serviceability': "a language \( L \) is serviceable if and only if \( L \)-speakers can ascertain the truth values of many of its short sentences" (Massey 1981, p. 8). I believe that the d-linguist will find this difficult to establish for an important class of sentences.

In (1"), for example, the d-linguist takes the speaker to be dissenting from an existentially quantified sentence. What could be the (rational) basis of this dissent? In most cases, a sufficiently "general"[9] negative existential statement can be justified only by appeal to a general theory which holds either that no such entities exist (e.g., witches, solitary quarks, carnivorous rabbits, ...) or, at least, that there is none in the immediate vicinity (e.g., penguins). Thus the h-linguist tacitly attributes to the \( L \)-speaker a theory about rabbits with the appropriate dietary component. By contrast, the \( L \)-speaker must be supposed to hold a theory about nonrabbits. This generates insuperable problems of projectability. For as Quine (1969c) himself points out, the complement of a projectable predicate is not necessarily projectable itself and, in fact, all such complements may be unprojectable. If the occurrence of projectable predicates in natural languages is closely bound up with the recognition of natural kinds (whatever analysis one wants to give of this)--as Quine says it is, and if the ability to recognize appropriate similarity classes as natural kinds is closely bound up with survival--as Quine also says it is (and I suspect that he is right on both counts), then the d-linguist will have a doubly difficult time explaining how \( L \)-speakers have ever managed to survive in a hostile world. Questions concerning the generating, testing, and confirmation of the theories which would have to be attributed to \( L \)-speakers could be multiplied with ease.

Apparently with similar concerns in mind, Kirk (forth-
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coming) contends that because an L-d sentence will have "links with non-verbal stimulation" which are "very much less direct than those of the given [L-h] sentence," the d-manual "is thereby deprived of all chance of empirical adequacy" (pp. 13-14). Massey (forthcoming) responds that, after all, the h- and d-linguists end up attributing the same beliefs about the world to L speakers. For example, (1') and (1'') project "the same dietary belief about rabbits" (p. 2). This brief foray into the realm of "intuitive semantics" is meant to capture the (intuitive) claim that the linguists and natives "share a mass of beliefs . . . because [they] inhabit a common world" (p. 2). Following a similar tack, Massey could reply that the h- and d-linguists would assign similar causal etiologies to the natives' empirical beliefs--and (for now) let linguistic reinterpretation be hanged.

But such a reply remains quite puzzling. The original purpose of constructing the d-manual was to defend Quine, and the latter explicitly eschews any appeal to propositional attitudes which cannot be cashed out in terms of dispositions to affirm/deny sentences. (See Quine 1960, Ch. VI.) Massey might reply that this is all we find here: wherever the h-linguist uncovers a disposition to affirm (1'), the d-linguist discloses a disposition to deny (1''). The projected beliefs are identical only because of the extensional equivalence of the states of affairs which would confirm the native verdicts. As attractive as this way out might appear, however, it runs afoul of familiar difficulties associated with attempts to reduce intensional to extensional equivalence. Secondly, and more germane to the present investigation, to attribute the same beliefs to the natives is to forfeit the very referential divergence which the h- and d-manuals were supposed to display. This divergence is preserved only if we maintain, regardless of extensional considerations, that the L-h speakers refer to--and therefore, one presumes, must have beliefs about--rabbits; while L-d speakers refer to--and therefore have beliefs about--nonrabbits. To deny this is to deny the relevance of the d-manual as a defense of RI. Finally, even if we were to grant Massey's belief equivalence claim, we still would lack a solution to the problem of how the L-d speakers are supposed to acquire their beliefs in the first place--their language being of so little help. The point is that even if the d-manual passes formal tests for (pure) scrutability, it will surely fail subsequent tests for serviceability.

To return briefly to the other sentences we see that the L-d speaker's task of assigning truth-values to (2'') and (3'') goes more smoothly, but only because each case can be settled by affirming an existentially quantified sentence. Thus (2'') could be rejected because an L-d speaker is presumed to know that

\[(5) (\exists x) (\neg Fx \land \neg R_x).\]
But why not just allow that (2") is false because

\[(6) \ (\exists x) \ (Fx \ & \ \sim Rx)\]?

The problem is that if we were to credit the L-d speaker with knowledge of the simplification rules which get one from (5) to (6), there would be a clear and present danger of L-d's collapsing into L-h. (In Massey's terms, this would be equivalent to imagining that L-d speakers could discover the relevant, i.e., truth-functionally significant, relations between the lexical and illocutionary force components of the d-manual.) Similar remarks apply to (3"). Once again, therefore, simplicity considerations appear to favor the h-manual. Taken on their own, such considerations would do little for Quine's critic, but in the context of the other worries already raised they add additional warrant to the h-manual.

Taken together, the preceding criticisms cast serious doubt upon Massey's contention that d and h "qualify in every respect as rival manuals." In fact, for Massey's defense of RI to prevail, we must attend primarily to the logical transformations which generate the d-manual, and ignore the empirical constraints on linguistic theory which awareness of its psychological dimension should bring to the fore. Precisely this attitude is fostered by Quine's perennial insistence, first, that the structure of natural languages is (essentially) the same as that of scientific theory systems and, second, that the "basic structure of the language of science" is provided by the predicate calculus (Quine 1979, p. 160). It is a truism that any (consistent) uninterpreted formal system will admit of indefinitely many models. But natural languages are not uninterpreted formal systems: their semantics are built-in from the start, and they resist large-scale reinterpretation. To realize this is to see why pressing the (admittedly significant) analogy between natural languages and formal systems to the extent demanded by Quine leads to the indicated untenable results which surface when we shift our attention away from the logic.

One final point should be made. Even if we were to discount the preceding criticisms and so regard the d- and h-manuals as genuine rivals after all, we could still rank their comparative referential force. RI holds that we will be unable to determine whether L-users are referring to L-h or L-d objects. But consider, again on purely Quinean grounds, whether the L-d speakers could refer to anything at all. For the latter's predicates could not perform the individuative work which Quine (or indeed, any descriptivist) requires of general terms. How many nonrabbits, nonfire-trucks, or nonanythings, are in the room right now? How are we to reidentify them? It is a truism that such questions are unanswerable, pending further specification of just what
is being queried. Thus, if Quine's theory really does entail the equivalence of the d- and h-manuals, then it is unable to distinguish what—on its own terms—should count as referential and nonreferential languages. But a theory of reference owes us at least this much.

To summarize: First, the problems of the d-manual make it an important anomaly for Quine's theory. For if the preceding arguments are on target, they do not show merely that the h-manual is preferable to, or more serviceable than the d-manual; they show that the d-manual must be wrong. Second, a recapitulation of the characteristics attributed to L-h and L-d underscores the significance of any theory's failure to distinguish between them. L-h is learnable; L-d is not. L-h is semantically scrutable and serviceable in that we are able to understand how speakers could ascribe truth values to various types of empirical sentences; it is not at all clear how L-d speakers could do so in an equally broad range of cases. Many of the predicates of L-h have clear individuative force; those of L-d (generally) have none. These are hardly unimportant differences, and they challenge the adequacy of any theory of language insensitive to them. These results indicate that sufficiently interesting referential divergences between competing manuals can indeed generate meaningful and detectable differences which will turn up elsewhere, and which provide genuine grounds for rank ordering the candidates. Nothing which has been said counts as an argument for absolute referential scrutability, but the analysis does reveal the severe problems which must be faced by any theory of language which generates a strong version of referential inscrutability.

Finally, Quine maintains that alternative manuals which are equally successful at attributing behaviorally scrutable linguistic dispositions to speakers are thereby empirically indistinguishable. Let us grant that the h- and d-manuals do pass this test, in accordance with Quinean methods of attributing such dispositions. But this only indicates the extent to which said "Quinean methods" sidestep a range of absolutely crucial questions with undeniable empirical implications. For, as we have seen, the manuals appear indistinguishable only so long as we ignore questions of psychological constraints on learnability, ecological considerations relating to the evolutionary fitness of a linguistic community speaking a referentially deviant language such as L-d, the functional characteristics of natural kind predicates in L-d and L-h, empirical constraints on languages able to support theories capable of assigning truth values to the full range of sentences exemplified in (1)-(4), and so on. To recognize these difficulties is to realize the weakness of the empirical constraints on translation, and semantic interpretation in general, which are acknowledged by Quine. They are simply incapable of grounding an adequate analysis of natural language.
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V. QUINEAN LINGUISTIC SCEPTICISM, PERCEPTION, AND OUR MUNDANE ONTOLOGY.

If it has fulfilled its primary purpose, the preceding critique of RI should also motivate the search for additional constraints on translation and semantic interpretation to insure that any manuals admitted by a given theory of language can pass, at least, the tests which the d-manual failed. Quine (1969b) also considers, and rejects, the possibility of such additional constraints. He writes:

"An actual field linguist would of course be sensible enough to equate "gavagai" with "rabbit," dismissing such perverse alternatives as "undetached rabbit part" and "rabbit stage" out of hand. This sensible choice and others like it would help in turn to determine his subsequent hypotheses as to what native locutions should answer to the English apparatus of individuation, and thus everything would come out all right. The implicit maxim guiding his choice of "rabbit," and similar choices for other native words, is that an enduring and relatively homogeneous object, moving as a whole against a contrasting background, is a likely reference for a short expression. If he were to become conscious of this maxim, he might celebrate it as one of the linguistic universals, or traits of all languages, and he would have no trouble pointing out its psychological plausibility. But he would be wrong; the maxim is his own imposition, tending settling what is objectively indeterminate. It is a very sensible imposition, and I would recommend no other. But I am making a philosophical point (p. 34)."

Quine's sceptical, "philosophical" position requires us to theorize from uncertainty, and so to demand compelling evidence (which turns out never to be forthcoming) before committing ourselves to any claim of substantive semantic agreement. (See Rorty 1972, pp. 437 ff; Moravcsik 1975, pp. 53; and Glotzbach 1979 sec. 32.) To see why Quine prefers this to the "sensible" approach of the linguist, we must trace the origins of RI back through his theory of language to the philosophy of psychology in which it is embedded.

Early on (I), I maintained that RI results from referential descriptivism coupled with TI; it does not follow from descriptivism alone. A descriptivist could avoid RI merely by embracing a semantic theory which assigned determinate interpretations to key predicate expressions.[10] What then of TI? Competing analyses abound. Quine (1970) once proposed that TI be understood as an extended result of the thesis that physical theories are underdetermined by data—though semantic indeterminacy is supposed to be "addi-
tional" in the sense that it would remain even if all empirically determinable issues in science could be decided. However, this analysis is problematic on two counts. First, it rests on a number of controversial theses about the relation(s) between scientific theories and natural languages. [11] Second, and more importantly, this defense fails to make clear just why the semantic determination which Quine finds on a language's periphery cannot be "transmitted" into its interior. In more recent writings Quine (1974-1975b) responds directly to the latter question, and so provides a more complete account of TI. There he stresses the behaviorist learning theory which he invokes to carry the language learner beyond the level of first-order, stimulus meaning semantic competence. TI falls out of this aspect of Quine's program almost without effort (see Glotzbach 1979, Ch. IV). To oppose Quine here, one can reject his learning theory proper (as, e.g., Chomsky et al. are wont to do), or one can reexamine his claim that the empirical foundation of language is captured adequately in his concept of stimulus meaning. Let us pursue the latter issue, since Quine's impoverished behavioral semantics defines the task which his learning theory must complete. Why does Quine insist so strenuously that stimulus meaning provides the only scientifically acceptable empirical foundation for semantic theory?

It is hardly obvious that semantics must begin by connecting (i) dispositions to assent to/dissent from sentences and (ii) classes of environmental stimuli. Theorizing naively, we might seek an alternative empirical foundation by noting, with Massey, that human beings do "inhabit a common world." Taking this seriously could motivate an appeal to fundamental features of human perceptual competence as the basis for additional empirical constraints on semantic theory. For if we grant that our perceptual systems have evolved to enable us to deal successfully with our environment, we might argue that the optimal strategy has turned out to be a species-wide, hard-wired propensity to perceive rabbits and not rabbit-stages or other such exotica (e.g., of the Whorfian variety). Nor would this require postulating innate "rabbit spaces." It would only demand, first, a zoology which acknowledges the existence of rabbits (etc.) as an environmental matter of fact and, second, a perceptual theory capable of explaining how we, in fact, perceive them. Such a perceptual theory would provide the psychological foundation for a more robust semantics and, ultimately, a more powerful theory of reference than are available to Quine. The envisioned theory of language could begin with shared propensities to perceive the sorts of mundane objects which occur in the extension of, e.g., English observation terms of divided reference (standardly interpreted). Such propensities would be ascribed to language learners generally (no matter what their linguistic communities), and would supersede Quine's vastly weaker notion of shared quality spaces. On such an account, it would become reasonable to
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suppose that all natural languages are appropriately con­ strained, at least in their "lower level," mundane observa­ tion vocabularies—i.e., in the linguistic subregions which children are first expected to master. On this conception, the structure of the environment itself would play a force­ ful role in determining the semantic and referential fea­ tures of (at least) the "low level" components of natural languages. In addition to supporting the linguist's "sensi­ ble position," these results would help to explain the defi­ ciencies of deviant alternatives such as the d-manual and, more generally, the doctrine of RI itself.

Quine, on the other hand, sees things as much more in­ determine. He has long pressed the aforementioned analogy between natural languages and scientific theories to argue that all objects are, in effect, theoretical entities. Thus even the medium-sized things in the extension of natural language observation predicates, the items constituting our mundane ontology, are to be regarded as theory-bound "pos­ its." This is not to disparage them as unreal, but it does mean that the question of what individuals constitute the ontology of a given discourse can be settled only by decid­ ing what object concepts can be constructed in it. Fur­ thermore, as Quine has stressed, interpretations of a suffi­ ciently complex, highly theoretical language appear to admit of significant variation in this area:

Statements about bodies, common-sense or recon­ dite, thus commonly make little or no empirical sense except as bits of a collectively significant containing system. Various statements can surely be supplanted by their negations, without conflict with any possible sensory contingency, provided that we revise other portions of our science in compensatory ways. Science is empirically under­ determined: there is slack. What can be said about the hypothetical particles of physics is underdetermined by what can be said about sensible bodies, and what can be said about these is under­ determined by the stimulation of our surfaces (Quine 1976b, p. 254).

It might appear that this position immediately grants the theorist or translator the extreme measure of ontological self-determination claimed by Quine (1976a) when he speci­ fies as his own

broad concept of a physical object . . . the ma­ terial content of any portion of space-time, how­ ever scattered and discontinuous. Equivalently: any sum or aggregate of point-events. The world's water is for me a physical object, comprising all the molecules of H2O anywhere ever (p. 839).

Even if correct, however, Quine's claim that all objects are
"posits," taken by itself, actually warrants neither his ontological expansionism nor his linguistic scepticism. For he still must show that the process of "posing" mundane objects in response to "the stimulation of our surfaces" is just as empirically underdetermined as is the posing of theoretical entities in, e.g., high energy physics. This is an important claim, and it cannot be established by fiat. It is at least possible that the kind of perceptual considerations previously entertained constrain our low-level semantic "theorizing," whereas no comparable restrictions apply to the esoteric sciences. In short, Quine's theory of language must be grounded in its own theory of perception, and the latter must be quite different from the one imagined above. This is, in fact, the case.

Qua behaviorist, Quine (1974) eschews any talk of high-level perceptual experience (regarding this as mentalistic) in favor of the triadic notion of perceptual similarity: "episode a is more similar to b than to c" (p. 16). This relation is behaviorally accessible through conditioning experiments. Perceptual similarity is definable, in turn, via the lower level notion of receptual similarity—another triadic relation comparing patterns of sensory receptor firings, as repeated across perceptual events. His behaviorist superstructure aside, however, Quine's emphasis upon sensory reception places him squarely in the larger tradition of classical sensation-based theories of perception which has informed empiricist thinking since the 17th century, and which has dominated experimental psychology (with very few exceptions) since the 19th century work of Muller and Helmholtz.

At its base, the received view of perception places a physics-level analysis of perceptual stimulation and a psychophysics-level description of events occurring at the sensory receptors. Or, as Quine (1974) puts it:

Science itself teaches that there is no clairvoyance; that the only information that can reach our sensory surfaces from external objects must be limited to two-dimensional optical projections and various impacts of air waves on the eardrums and some gaseous reactions in the nasal passages and a few kindred odds and ends (p. 2; see also Quine 1960, p. 2).

This is not to deny the activation of receptors in groups. Thus Quine (1960) also characterizes visual stimulation as "a pattern of chromatic irradiation of the eye," and so urges that we take as the relevant stimulations not momentary irradiation patterns, but evolving irradiation patterns of all durations up to some convenient limit or modulus (p. 32).
Nevertheless, the effective structure of the stimulus ultimately remains isomorphic with the pattern of receptors it causes to fire. Pressing this claim to its limit yields a Quinean psychophysical atomism:

Once we recognize the datum thus single-mindedly as what is closest to the physical stimulation, we find a strangely faithful realization of sensory atomism in the discreteness of sensory nerve endings. Fire one afferent nerve and chalk up one sensory atom, and awareness be hanged (Quine 1970, p. 3).

Such commitments to a psychophysics-level description of perceptual stimuli may appear innocuous. But they impose fundamental constraints upon perception theory by placing severe limits upon the structure of the input—and hence, severe limits upon the information which is "directly" available to perceivers.

Among the many reasons for beginning perceptual theory at this level of analysis, two stand out as important—both historically and for Quine. First, perceptual illusions demand explanations. On the received view, illusions result when patterns of proximal stimuli are so structured as to admit of possible correlations with diverse distal objects, or perhaps with no object at all. It is thus argued that we must distinguish between the sensory input itself—qua objective fact of the matter—and any interpretation or enrichment of the input due to subsequent processing—the latter having only a psychological or subjective status. This analysis reflects the two-stage configuration typical of sensation-based perception theories. Attending to similar concerns, Quine (1960) begins his account of stimulus meaning, his fundamental semantic concept, by writing:

It is important to think of what prompts the native's assent to 'Gavagai?' as stimulations and not rabbits. Stimulation can remain the same though the rabbit be supplanted by a counterfeit. Conversely, stimulation can vary in its power to prompt assent to 'Gavagai' because of variations in angle, lighting, and color contrast, though the rabbit remain the same. In experimentally equating the uses of 'Gavagai' and 'Rabbit' it is stimulations that must be made to match, not animals (p. 31).

Thus it is no accident that Quine goes on to construct a two-stage semantic theory. Here too, objective or empirical status is accorded only to the first, or stimulus-meaning level. By contrast, of course, he finds that the second level (encompassing all higher-order semantic structures and including reference) is governed by empirically underdeter-
Second, Quine (1979) believes that his physicalism commits him to an in principle reductionism. Thus he writes that there can be "no difference in matters of fact without a difference in the fulfillment of the physical state predicates [of an idealized physics] by space-time regions." Nevertheless, he is careful to reject the "presupposition that anyone [could ever] be in the position to come up with the appropriate state predicates for the pertinent regions in any particular case" (p. 166). Despite this caution, and like adherents to the received view generally, Quine seeks to make perception a partial exception to the rule by beginning with the indicated reductionistic analysis of sensory input. When he moves beyond that starting point, Quine substitutes his behaviorist analysis of perceptual similarity for the accounts of higher level processing found in most other psychological theories. Thus he appeals not to processes intended to enrich the impoverished input, but to learning mechanisms leading to dispositions to discriminate among classes of stimuli by grouping them in socially sanctioned ways. (Needless to say, language learning plays a major role in bending the individual's initial quality-space propensities toward a specific end.) But neither Quine's perceptual behaviorism, nor the more standard accounts of perceptual enrichment diminishes the reductionism built into the classical position's foundation.

Though severely abbreviated, the preceding characterization indicates how the dominant sensation-based approach to perception gives aid and comfort to Quine's linguistic scepticism. First of all, it clearly precludes any appeal to perception theory to ground our mundane ontology in the face of Quine's ontological relativity. For even though Quine (1974) grants that from a phenomenological or "introspective" point of view, we experience "not primarily . . . sensory elements, but significantly structured wholes" (p. 1), he (quite correctly) concludes that the received view of perception bestows "evidential priority" upon sensations (Quine 1976b, p. 252). For the latter represent our only direct or immediate link with the environment. But unfortunately, as generations of epistemologists have realized, when taken at this level of analysis, immediate experience simply will not, of itself, cohere as an autonomous domain. References to physical things are largely what held it together. These references are not just inessential vestiges of the initially intersubjective character of language, capable of being weeded out by devising an artificially subjective language for sense data. Rather, they give us our main continuing access to past sense data themselves; for past sense data are mostly gone for good except as commemorated in physical posits. All we would have apart from
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posits and speculation are present sense data and present memories of past ones; and a memory trace of a sense datum is too meagre an affair to do much good. Actual memories mostly are traces not of past sensations but of past conceptualizations or verbalization (Quine 1960 pp. 2-3; emphasis mine).

Therein lies the problem. Though objects are "conceptually fundamental" (Quine 1976b, p. 252), they are epistemically derivative, and reference to them is derivative from the standpoint of the Quinean philosophy of language. So even the most rudimentary ontological content, either in language or in the objects of perceptual experience, represents an empirically unsupported imposition upon fundamentally ambiguous sensory data.

Quine's attempt to ground semantics in stimulus meaning now emerges as much more than merely a commitment to behaviorist S-R learning theory. It is equally an attempt to show how language builds upon the perceptual facts of the matter. This is why he finds "every reason to inquire into the sensory or stimulatory background of ordinary physical things [emphasis mine]," but regards it as a mistake . . . [to seek] an implicit sub-basement of conceptualization, or language. Conceptualization on any considerable scale is inseparable from language, and our ordinary language of physical things is about as basic as language gets (Quine 1960, p. 3).

On Quine's theory, reference and higher-order semantic structures are basic to language use; without them we would be able to do little more than to "volunteer [an occasion sentence] from time to time by way of laconic comment on the passing show" (p. 5). But just as sensation-based theories find that perception takes the perceiver beyond the immediate sensory data, so too does Quine find that reference and higher-order meaning are underdetermined by the linguistic facts of the matter. My claim is not merely to have uncovered an interesting parallel here. I am urging, instead, that both Quine's stimulus meaning semantics and its consequences follow straightforwardly from the indicated perceptual presuppositions. Thus the complementary sceptical theses of TI and RI emerge as the linguistic apotheosis of the classical perceptual thesis that reception systematically underdetermines perception. And Quine believes that the latter is forced upon us by the demands of empiricism itself.

However, if Quine's linguistic scepticism is placed in question by the earlier critique of RI, then so too are its foundations. Thus if I have succeeded in linking RI (and TI) with the received view of perception, then the problems
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of the former give us reason to rethink the latter. But has it not also been shown that a sensation-based approach to perception is just good science?[13] If so, then our options are either to abandon the scientific investigation of perception and, indeed, of language, or else to accept R1 and T1 regardless of their unpalatability. For the past two decades, Quine has tried to force upon us precisely this choice. Fortunately, it is a false dilemma. For despite its continued dominance, the received view of perception finds itself increasingly challenged by an alternative which offers a new perspective upon perception, and—if there is anything to the present analysis—upon the philosophy of language as well. This is the ecological approach to perception, primarily attributable to the psychologist J.J. Gibson (1966, 1979; see also Neisser 1976).

VI. THE ECOLOGICAL APPROACH TO PERCEPTION AND THE EMPIRICAL FOUNDATION OF MEANING

In opposition to the pre-Darwinian stance of sensation-based theories, the ecological approach to perception proceeds from an evolutionary point of view. It regards perception, first of all, as an interaction between an animal and its environment occurring at the terrestrial scale of the natural objects and events to which the species has adapted, and to which the animal must adjust in the course of its ongoing activities. An animal’s perceptual systems have evolved to enable it to keep in touch with those stable and changing conditions in its environmental niche which have functional significance for it—i.e., which bear upon its survival. So, instead of beginning with a physics-level description of events at an animal’s sensory boundaries, ecological theories start with a description of the environment to be perceived, using time- and size-scales appropriate to the animal in question. Secondly, ecological theories depart from most classical accounts by insisting that a proper analysis of the stimulus for perception, again: at an appropriate level of complexity, reveals information sufficient to specify environmental characteristics relevant to the perceiving organism. In the case of vision, for example, unequivocal information about the structure of the environment is carried in the ambient light which has been reflected off the surfaces of objects. Since this light fills the medium, the structure of the environment can be determined effectively at any point in that medium by a suitably equipped perceiver. That is to say, since the available information unequivocally specifies the environmental layout, an animal which is sensitive to the appropriate structures in the ambient light (as a consequence of its phylogenetic and ontogenetic history) can pick-up this information directly.

Quine and other adherents of the received view are entirely correct to hold that sensations, in and of them-
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selves, cannot "cohere as an autonomous domain"—and so cannot determine perception unequivocally; however, according to the ecological approach, sensations are not the stimuli to which the perceiving organism reacts. The actual stimuli for perception (at least in the case of higher organisms) are higher level variables of environmental structure carried in proximal stimuli. This is why the reductionism so deeply entrenched in the received view leads to inadequate theories. It is impossible either to represent the crucial (higher level) variables at the level of physics, or to decompose them into structures characterizable at that level. Consequently, it is impossible to construct an adequate account of perception when working under the constraints imposed by a psychophysical description of the stimulus. This is not to deny the relevance of psychophysics to the overall task of understanding perception. The latter is a paradigm case of a multi-leveled explanandum calling for an "inter-level" response (see Darden and Maull 1977; and Wimsatt 1976), and so it must be investigated on all the different levels of analysis which come into play. However, if conceptual priority is to be placed anywhere, then it must go to the ecological level. Reduction of the latter to any lower level analysis (in the classical sense in which upper-level theories are reduced wholesale to lower-level theories, and in which reduction is tantamount to elimination) is out of the question. [14]

The ecological position remains controversial, and its challenge to the received view is not likely to be quickly resolved. [15] However, proponents of the ecological approach can point to a number of specific advantages of their view. For example, activities of the perceiver (e.g., head and eye movements) which are part of the problem for the received view (since they produce differing "presentations" which subsequently must be either "synthesized" or "filtered out") play a crucial positive role in ecological theories. This is because such movements induce transformations in the ambient array which actually facilitate the perceiver’s detection of the invariant environmental structures which are, after all, the ultimate objects of perception. Other benefits could be mentioned, but it is more important in the present context to suggest how this alternative account of perception might relate to the philosophy of language.

The ecological approach to perception promises to provide a psychological foundation upon which an extremely powerful semantic theory could be built. First, it would make sense of the claim that even the prelinguistic child already inhabits a world of public objects and other "robust" [16] structures with which it interacts. Indeed, the child must do so in order to avoid calamity as it goes about its daily business, as must adults. (Though, of course, this is not at all to say that the child’s perception of such objects and structures is as developed as an adult’s.) Second, therefore, the ecological approach to perception could

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ground a primitive definition of linguistic meaning by appealing to relations between expressions and classes of objects—not just stimulus classes.[17] It would also follow, thirdly, that we need not wait until considerable contextual language learning (e.g., of qualifiers such as 'same', 'different', and so on) has occurred—as is the case with Quine's theory—before being able to ascribe genuine reference to a speaker. The projected theory would predicate reference to speakers almost from the beginning of language acquisition. And finally, such a theory of language could explain just why Massey's d-manual encounters so many difficulties.

The latter's systematic ontological inversion generates a language which could not possibly conform to the perceptual experience of higher animals fitted by evolution, as we are, to deal with both the gross and fine-grained features of our world (any world?) at the terrestrial level.

None of this should be taken as denying the vital and pervasive role of cognition and, hence, of language in human perceptual activities. First, it is beyond question, for example, that a great deal of perceptual learning is "language based" in a sense exploited by Quine (1974). But neither cognition nor language is required to make up for fundamental deficiencies in the stimulus array which forms the basis of perception; therefore, they are not required to explain the basic structures of our mundane ontology. A theory of language has more than enough work to do in its own domain; it should not have to do the work of a theory of perception as well. Secondly, I am making no implicit claims about what a theory of language must do to account for higher-order linguistic competence. Thus these remarks must not be taken as a prelude to an attempted reintroduction of any variety of classical epistemological foundationalism. It remains an open question how the suggested theory of language would deal with higher-order linguistic structures such as, e.g., scientific theories. But there is no reason to think (or to hope) that the indicated semantic theory entails classical epistemological reductionism. The point to be made here is more modest: despite the overwhelming significance of language in human existence, it remains a social artifact of relatively recent origin—at least on an evolutionary time scale. As such, it stands upon much earlier (evolutionary) accomplishments. Many of the latter are perceptual. Because of them, we do not require language in order to "posit" the medium-sized objects encountered in our day-to-day existence; in fact, we do not need to "posit" them at all in any theoretically interesting sense of the term. And this remains true despite the undeniable fact that we learn to perceive objects—via language—that we could never have perceived without it; that we can use language to learn more about our mundane objects than would otherwise have been possible; and that we can extend our concept of an object, once it has been acquired, to cover even such esoteric particulars as "all the molecules of H2O anywhere ever."
These brief remarks do not pretend to replace the detailed defense which this position obviously requires. Instead, their purpose has been, first, to urge a thorough reexamination of the psychological theses upon which Quine's theory of language so heavily depends, and, second, to draw attention to a promising but largely unexplored area for further research. Quine's account of language remains a brilliant and powerful exploration of the linguistic possibilities presented by the received view of perception combined with a rigorous behavioral theory of learning. He remains steadfastly within the confines of his chosen conceptual resources, while nevertheless tackling many of the most important questions in the philosophy of language. Moreover, he realizes the ramifications of those questions far better than do many other philosophers. Finally, Quine cheerfully embraces the highly counterintuitive conclusions to which his program inexorably leads. In doing all this he has performed an invaluable service to our discipline. But once we appreciate the profound difficulties which Quine's conclusions actually entail, we should become eager to discover a new psychological standpoint from which we might better undertake the investigation of language.[18]

FOOTNOTES

1. The alternative position has been labeled 'Haecceitism', and it denies that reference requires either tacit or explicit recourse to such descriptions. Its adherents maintain that reference can go through (via "causal," "historical," or other unspecified kinds intentional connections) even in cases where a speaker cannot supply anything approximating an identifying description of the intended referent. In short, Haecceitism attempts to drive a wedge between our ability to refer to an object and both (i) our ways of coming to know what sort of thing it is, and (ii) our actual possession of such knowledge. See Kaplan 1975. Kaplan credits R.M. Adams with the expression 'Haecceitism'; the latter was thinking of Duns Scotus' concept of haecceitas: thisness. See Adams 1979. Ingber 1979 argues that Haecceitist critics of descriptivism (he calls it 'descriptionalism'), have focused primarily on one formulation of that position. He suggests that alternative formulations may be able to escape some of the standard objections. This remains to be seen, but his paper provides important clarifications of many aspects of descriptivism. See also Donnellan 1966; and Glotzbach 1979, Sec. 28.

2. Quine (1969b) writes:

It is meaningless to ask whether, in general, our terms "rabbits," "rabbit part," "number," etc., really refer respectively to rabbits, rabbit parts, number, etc., rather than to some ingeniously permuted denotations. It is meaningless to ask this absolutely; we can meaningfully ask it only relative to some background language. When we ask, "Does 'rabbit' really refer to rabbits?" someone can counter
with the question: "Refer to rabbits in what sense of 'rabbits'?" thus launching a regress; and we need the background language to regress into. The background language gives the query sense, if only relative sense; sense relative in turn to it, this background language (pp. 48-9).

For an argument that Quine's "relative" sense of reference presupposes reference in some nonrelative sense, see Field 1974.

3. For a more complete analysis of translational indeterminacy, see Glotzbach 1979, Ch. IV, and especially Sec. 23. See also (V) below. This move from a claimed underdetermination of translation manuals to the indeterminacy of translation simpliciter is a most controversial step in Quine's program. I accept his position here without argument merely to get the present discussion going. If successful, some of my later arguments will count against both aspects of Quine's position: viz., the claim that choice among manuals is radically underdetermined by empirical data, and hence, the claim that translation must be radically indeterminate.


5. This paper was first presented at the International Philosophy Conference held in New York from 28 March to 4 April 1976. There Quine commented on Massey's paper and accepted it as a powerful defense of his position. In fact, Quine indicated that he had been using similar examples for some time. More recently, at the Western Division of the American Philosophical Association 1980 Meeting (in Detroit), Quine cited the possibility of constructing a manual of the kind to be discussed below (the d-manual) as a response to a criticism of his position. I mention all this because I intend the conclusions to be drawn about Massey's position to apply with equal force to Quine.

6. Note that Massey too, like Quine, blurs the distinction between reference and denotation.

7. In his willingness to treat these modal concepts as operators, Massey diverges from Quine, but this does not affect the primary issue at hand.

8. Psychological research indicates that subjects facing sorting tasks perform more efficiently and show higher rates of learning when they employ affirmative conjunctive hypotheses, as opposed to negative disjunctive hypotheses—even when the alternative hypotheses are logically equivalent. See Fodor 1975, pp. 40-1, and references cited therein.

9. It is necessary to speak of "general" existential statements to rule out cases such as dissent from 'There is presently a nonvegetarian rabbit in my suitcase', and the like. The interesting cases which present problems from the d-linguist concern sentences whose truth values cannot be decided conclusively merely by looking about in some limited area for a brief time. The difficult cases contain substantial
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inductive components.

10. On the other hand, I have argued elsewhere that accepting a stronger account of reference—particularly, any version of Haecceitism—is tantamount to accepting a stronger or more determinate semantics than Quine can construct out of his behaviorist conceptual resources. Such are the ties between reference and sense. See Glotzbach 1979, Sec. 34.

11. See Quine 1970 for a more complete rendering. For opposing arguments, see Rorty 1972; and especially Bechtel 1978.

12. See Quine 1974 Secs. 5-7. The discussion there expands upon points made in the earlier Quine 1969b. For a more complete behavioral theory of perception, see Taylor 1962. I discuss Quine's behavioral analysis and its role in his theory of language in Glotzbach 1979, Secs. 17-19.

13. For arguments in the affirmative, see Jackson 1977; and Maxwell 1970.

14. It is arguable that the issues of reductionistic theory construction and research strategies stand at the heart of the current debate over perception. Oddly, despite the high level of interest in these topics recently shown by philosophers of science (see McCauley 1979 for an excellent critical review of recent work), very few reflections of the current disaffection with reductionism have made their way into the perception literature.

15. Because it is attaining increased attention, the ecological position is also beginning to generate a number of penetrating and detailed critiques. See e.g., Fodor and Pylyshyn 1981, Ullman 1980, and Heil 1979. But in reply see Turvey and Shaw 1979, Heft 1980, and Glotzbach and Heft 1982.

16. I take this term from Wimsatt who uses it to characterize an entity or property which is detectable via a "variety of independent processes" or which is "invariant or identical in the results of those processes" (p. 3). Wimsatt is building upon the work of Richard Levins and D.T. Campbell. See Wimsatt 1980 for a more complete analysis of the concept of robustness and references to other relevant sources.

17. There are resonances between these remarks and comments made by Moravcsik 1981; see especially pp. 16-17. However, Moravcsik seems to have more conventional psychological programs in mind and not the ecological program under consideration here.

18. My thinking on these issues has benefitted considerably from discussions with Richard L. Fern, Ruth Barcan Marcus, Robert Fogelin, Harry Heft, Gerald Massey, William C. Wimsatt, Robert Kraut, and Robert A. Jaeger. The last of these, especially, provided many invaluable comments on earlier versions of some main arguments. This is not to say, however, that any of the preceding necessarily share the views expressed above. Earlier versions of this paper were presented at The Ohio State University and the APA Eastern Division Meeting in 1981. Sita Ranchod
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