NOTE TO USERS

FROM A PHONO-LOGICAL POINT OF VIEW

by

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Abstract

FROM A PHONO-LOGICAL POINT OF VIEW

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This dissertation work is premised upon the observation that semantic information is required in order to group phonetically distinct word-tokens into phonemically equivalent word-types. For philosophers, like W. V. Quine, who have a dim view of meaning, this claim regarding the semantic basis of natural language phonology, if true, is problematic. This is why in a series of publications, Quine (1953a, 1960, 1981, 1990) has attempted to avoid any appeal to semantics in his efforts to reconstruct phonemic word-type equivalence. Consistent with his rejection of meaning-based analytic equivalence, Quine rejects a meaning-based account of basic phonological equivalence. However, given the empirical failures to "reduce the phoneme" to acoustic, phonetic, and/or distributional data within phonology—combined with the philosophical short-comings of a behavioral reduction—a consistent disavowal of meaning prohibits Quine from acknowledging the legitimacy of basic phonological equivalence anymore than he wishes to acknowledge the legitimacy of analytic equivalence. In this way, a consistent Quinean rejection of meaning entails not only a skeptical dismissal of analytic equivalence, but also a dismissal of basic phonological equivalence.

Yet the observation that semantic information is required in order to group word-tokens into phonemic word-types also has a Fregean application. This is because the

semantic information required to reconstruct basic phonology is not entirely intensional in character. In addition to typing word-tokens of various predicates in terms of properties, word-tokens of names (and common nouns) must also be phonologically typed. So whereas Quineans reject synonymous substitutions because they are based upon meaning, Fregeans claim reference is insufficient to guarantee the substitution of co-referring names *salva veritate*. But if basic phonological substitutions among word-tokens are premised upon co-reference (rather than the apparatus of Fregean co-sense), a consistent rejection of co-naming substitutions within belief contexts also threatens the substitution of allophonically related word-tokens in belief contexts. The semantic basis of natural language phonemic categorization has a number of implications vis-à-vis contemporary philosophy of language.

FROM A PHONO-LOGICAL POINT OF VIEW

Perhaps the root of this misunderstanding is the fact that my interest in the philosophy of language includes an amateur interest in philology and phonetics, to which Miss MacDonald seems to pay no attention. . . . The beginning of our dispute was her assertion that the type-token distinction involves no reference to meaning. If this were so, then it could be recognized that two tokens were or were not of the same type even by someone who did not know how they were used to express meaning—that is, in what language they were, or even whether they were in a language at all. This seemed to me false, for reasons that are familiar to every philologist.

A. MacIver (1937c, 28)

FROM A PHONO-LOGICAL POINT OF VIEW

FROM A PHONO-LOGICAL POINT OF VIEW

Chapter 1
Introduction

What does Phonology have to do with Philosophy?

The present work addresses the issue of grouping different linguistic expressions into truth-preserving substitutionally equivalent categories by exploring the way this problem has been addressed within phonology and philosophy. Within analytic philosophy of language, questions of substitutional equivalence, synonymy, meaning, and reference more or less define our field of inquiry. In phonology, issues of substitutional equivalence revolve around the notion of the phoneme: two linguistic specimen are substitutionally equivalent—indeed the same (type of) word—if they are composed of the same phonemes. These two sorts of substitutions—one basic to phonology and the other basic to philosophy—are *prima facie* different. One concerns the substitution of phonemically equivalent word-tokens. The other concerns the substitution of phonemically distinct word-tokens: tokens of co-referring names which refer to the same object and tokens of synonymous predicates which express the same property. In short, one concerns the substitution of phonemically equivalent word-tokens and the other the substitution of phonemically distinct word-tokens.

Though premised upon on a comparison between phonemically equivalent substitutions and phonemically distinct substitutions, the present dissertation does not challenge or neglect the distinction between word-tokens and word-types, a distinction important for both linguists and philosophers. (In fact, in addition to word-tokens and word-types, a third kind of entity—a "dot quote"-type, adapted from Wilfrid Sellars—is

introduced.[1]) Rather, the dissertation focuses on what the philosophical significance of this basic distinction between phonemically equivalent substitutions and phonemically distinct substitutions is vis-à-vis traditional issues in the philosophy of language regarding the analytic status of synonyms on the one hand, and the substitutional status of co-referring names in belief contexts on the other.

Generally speaking, co-referring names (and perhaps synonyms too) are traditionally held to provoke failures of substitutivity in belief contexts. Moreover, Quineans are skeptical about the analytic substitution of synonyms. By contrast, the substitutional replacement of distinct word-tokens of the same word-type, say, one written in capital letters and the same word written in lower case letters, is typically not considered to be sufficient to genuinely provoke comparable Fregean-like substitutional failures, nor raise Quinean objections. Unlike substitutions involving word-tokens of two different word-types, substitutions involving word-tokens of the same phonological or orthographic word-type do not affect any changes in meaning, reference or truth. This is at least part of what it means to say that two word-tokens—though token distinct—are phonologically or orthographically word-type equivalent. They are actually one and the same (type of) word, and hence have the same semantic value (or values in the case of ambiguous homonyms).

On the one hand, then, there are the kind of token-to-token equivalencies which issue merely from characterizing distinct word-tokens as one and the same type of word. At the level of word-types, distinct word-tokens of the same word-type are strictly equivalent—they are the same word. We can call this kind of substitution among word-tokens of the same word-type "token-substitutions," or "TK-substitutions." On the other

hand, there are the kind of token-to-token equivalencies which arise from trying to characterize various word-tokens of distinct word-types, that is, co-referring names and synonyms, as somehow substitutionally equivalent. We can call these kinds of word-token substitutions among distinct word-types "type-substitutions," or "TP-substitutions." So while both TK-substitutions and TP-substitutions involve word-tokens (indeed, from this perspective, language itself consists of tokens, see below), it is clear that the former typically engage only phonologists and the latter typically philosophers. Moreover, when addressing issues of substitutivity, philosophers usually do not speak in terms of word-token substitutions at all. Instead, philosophers regard substitutions among synonyms and substitutions among co-referring names as substitutions among distinct word-types—without any mention of word-tokens. But whether TP-substitutions are considered substitutions relating distinct word-tokens of distinct word-types or more simply as substitutions relating distinct word-types, TP-substitutions cross a phonemic boundary. Unlike TK-substitutions, TP-substitutions are not phonologically or orthographically licensed.

The usual philosophical question—whether co-referring names like 'Cicero' and 'Tully,' or co-meaning predicates like 'attorney' and 'lawyer'—are intersubstitutable *salva veritate* is often viewed as ambiguous. It is not clear whether such substitutions are intended to operate over word-tokens or word-types. Resolving this ambiguity requires a distinction between substitutions relating word-tokens of the same word-type (TK-substitutions) and substitutions relating word-tokens of different word-types (TP-substitutions). Substitutions between co-referring names and synonyms are substitutions relating word-tokens of two different word-types. The more verbose formulation—

"substitutions among word-tokens of distinct words" rather than just "substitutions among distinct words"—serves as a reminder that language generally and linguistic substitutions in particular are conducted via tokens not types. Words, that is word-types, are language-specific abstract constructions reflecting a classificatory order among various word-tokens. Even TP-substitutions among co-referring names and synonyms relate word-tokens—word-tokens instantiating distinct word-types. In this way, it is tokens which are used and tokens which are substituted, whether one is doing phonology or philosophy. (This issue is discussed more fully below. Chapter 2 introduces the linguistic resources phonologists have developed to address these concerns.)

TK-substitutions and TP-substitutions relate word-tokens—word-tokens of the same word-type or word-tokens of different word-types. And while the present dissertation seeks to relate these two kinds of substitutions, TK-substitutions and TP-substitutions are different. For this reason, any comparison between them must be constructed slowly and carefully. This first introductory section is intended to clarify such a comparison. Following this, another section is intended to outline the philosophical significance of relating TP-substitutions to TK-substitutions. Finally, the chapter ends by briefly introducing the concept of "phonemic reductionism," explaining its linguistic background and its philosophical appeal.

Several problems immediately complicate a comparison between TK-substitutions and TP-substitutions. One problem concerns the intuitive recognition and availability of abstract word-types reflecting the natural language phonological or orthographic equivalence among distinct word-tokens. Another problem concerns the apparent dubitability of TP-substitutions among co-referring names (and perhaps

synonyms) in contrast to the language-specific indubitability of TK-substitutions among tokens of the same word-type. A third problem concerns the semantic basis of TP-substitutions (reference for co-referring names and meaning for co-meaning predicates) versus the strictly phonological or orthographic basis of TK-substitutions. These three discontinuities are briefly introduced and discussed below but are thematically integral to the dissertation as a whole. Their introductory treatment here is intended to initially clear some of the ground surrounding TK- and TP-substitutions and suggest the scaffolding of a bridge relating the phonology of TK-substitutions to the philosophy of TP-substitutions.

The first problem that arises when attempting to relate TK-substitutions and TP-substitutions is the commonsense observation that there is no comparable notion of word-tokens of co-referring names or synonyms instantiating "one and the same" abstract linguistic entity in the way that various word-tokens are said to instantiate one and the same abstract word-type. There exists no standard terminology in linguistics or philosophy by which to say that word-tokens of co-referring names or synonymous word-types are themselves "mere variants" of some abstract linguistic type in the way various word-tokens are said to be mere incidental variants of one and the same word-type. So while the same abstract word-type may be realized, say, in italics or not, this kind of token variation is unlike the kind of token variation found among tokens of co-referring names or synonymous word-types. There is simply no intuitive notion that tokens of co-referring names or synonyms word-types are themselves exemplars of some more abstract linguistic type. This is one reason why substitutions among word-tokens from distinct word-types are, in a sense, intrinsically more problematic and philosophically more engaging than substitutions among word-tokens of the same word-type. TK-

substitutions among distinct word-tokens can be trivially accounted for by invoking the fact that they are just token variants of one and the same word. TP-substitutions among word-tokens from distinct word-types cannot be so easily accounted for. Of course, one may artificially introduce an abstract linguistic entity (and call it a "dot quote"-type following Wilfrid Sellars, see chapter 9) to notationally subsume a pair of synonyms terms (and by extension, co-referring names[2]) as substitutionally equivalent "at the level of dot quote-types." But this does little to philosophically substantiate a move to treat TP-substitutions in a way analogous to TK-substitutions. A closer look at the actual basis and justification, in particular the semantic basis of our intuitive confidence in everyday abstract word-types, however, reveals that they too are stranger than might otherwise be supposed.

A second but related problem confronts a comparison between TP-substitutions and TK-substitutions. Unlike TP-substitutions, language-specific TK-substitutions cannot be coherently denied or perhaps even doubted. In virtue of instantiating the same word-type, TK-substitutions are in some sense logically immune to doubt. One cannot, for instance, genuinely doubt whether one word tokened with capital letters means or refers to the same thing as the same word tokened in all lower case letters. Instead, such doubts tend to be dismissed as symptomatic of some sort of linguistic deficiency or deviancy. Doubt regarding TK-substitutions is indicative of less than full linguistic competence with the basic orthographic or phonological contours of the language. Word-tokens variants of the same word-type are just notational variants of one and the same word, and in virtue of being word-token variants of one and the same word-type, such doubt is not a genuine possibility. So while one may always question word-token

equivalencies in languages one does not speak fluently, one cannot question word-token equivalencies in languages one claims to already be competent in. Phonologists, for instance, routinely confront languages whose phonemic structure they are unfamiliar with. Phonologists, however, cannot genuinely question the phonemic structure of their own native tongue without thereby undermining their native language competence. (Everyone one is free, and phonologists can be employed, to explore the *basis* and phonotactic patterning of their intuitive phonemic awareness, but empirical investigations do not serve to challenge—or substantiate—our phonemic intuitions.)

By contrast, TP-substitutions among co-referring names (and perhaps synonyms too) appear to have no such immunity from doubt. TP-substitutional doubt is not generally regarded as symptomatic of any sort of linguistic confusion or deviance, nor, for this reason, can it be so lightly dismissed. Rather, the possibility of doubting TP-substitutions among co-referring names is the foundation for a Fregean theory of *sense*. (There may be other reasons to postulate Fregean senses, the apparent meaningfulness of denotationless names for one. But it is the test of substitutivity in epistemic contexts which is the *sine quo non* of Fregean reasoning, see Fodor 1990.[3]) Moreover, Quinean opposition toward analyticity is guided by the observation that unlike straightforward tautologies equating, say, 'bachelor' with '*bachelor*', analytic equivalencies relating 'bachelor' to 'unmarried man' require an interpretive equivalence of predicate synonymy. So whether they involve tokens of the same name or tokens of the same predicate, basic TK-substitutions or equivalencies are presumably not subject to Fregean doubts or Quinean objections. Instead Fregean doubts and Quinean misgivings regarding TK-substitutions are more likely to be written off as philosophically misplaced rather than

embraced as philosophically instructive. Unlike TP-substitutions, TK-substitutions between word-tokens of the same name and TK-substitutions between word-tokens of the same predicate appear to be resistant to Fregean and Quinean arguments. Merely in virtue of instantiating the same abstract word-type, TK-substitutions appear to sheltered from Fregean substitutional doubt and Quinean analytic skepticism.

However, it would appear, or at least I shall try to argue, that a parallel dynamic obtains when the TP-substitution of co-referring names is doubted or synonymy challenged. As for co-referring names, insofar as co-referring names refer to the same thing, any discrepancy or potential discrepancy between the use of one name versus its co-naming pair will be revealed in their differential application. But if this is indeed the case, then it would seem—just as with TK-substitutions—one cannot be considered unquestionably competent with the full use of co-referring names when their use diverges so that one member of the pair is either over- or under-extended relative to the other member of the pair. In particular, withholding the use of a name from covering its full denotation is at least some *prima facie* evidence that the term is not being correctly used. This is some indication that co-naming doubt cannot be so easily engaged in with complete linguistic impunity. As for synonymous predicates, an inability to distinguish the co-meaning predication of 'bachelor' and 'unmarried man' from the co-extensive predication of say, 'renate' and 'cordate' is likewise arguably a *prima facie* indication of less than full linguistic competence.

Though unwelcomed by Quine, considerations of "linguistic competence" are not entirely alien to philosophers of language. According to Kripke (1979), if two words are synonymous "then a sufficiently reflective speaker subject to no linguistic inadequacies

or conceptual confusions who sincerely assents to a simple sentence containing one will also (sincerely) assent to the corresponding sentence with the other in its place" (140). No such comparable linguistic requirement is expected—or linguistically enforced—for co-extensive predicates like 'renate' and 'cordate'.[4] Unlike the failure of co-referring names to substitute freely in belief contexts, the possibility of synonymous substitutional failure is not so much explained, but explained away by Kripke (1979) in terms of some sort of "linguistic error" or "semantic confusion."

Though not specifically directed at Mates's (1950) "Synonymy" paper, Kripke's (1979) "A Puzzle About Belief" does address the possibility of synonymous doubt—but only to dismiss it. Recall that Mates's article famously raises an objection against any possible standard for substitutivity—whether based upon traditional notions of analyticity and synonymity, or any variety of other more technical standards including Carnap's "intensional isomorphism" or Church's "synonymous isomorphism (and arguably even Taschek's 1995, 1998 more recent proposals). Mates suggests that no standard is possible because it is always at least conceivable that someone might balk at a substitution even when the pair is stipulated to be strictly synonymous:

> Let "D" and "D'" be abbreviations for two intensionally isomorphic sentences. Then the following sentences are also intensionally isomorphic:
>
> Whoever believes that D, believes that D.
> Whoever believes that D, believes that D'.
>
> But nobody doubts that whoever believes that D believes that D. Therefore nobody doubts that whoever believes that D believes that D'. This seems to suggest that, for any pair of intensionally isomorphic sentences—let them be abbreviated by "D" and "D'"—if anybody even doubts that whoever believes that D believes that D', then Carnap's explication is incorrect. What is more, *any* adequate explication of synonymity will have this result, for the validity of the argument is not affected if we replace the words 'intensionally isomorphic' by the word 'synonymous' throughout. (125)

The upshot of Mates's argument is not—or not only—that multiply embedded contexts challenge substitutivity, but that even one level of belief-context sensitivity is sufficient to derail all possible standards for synonymous substitution. One does not have to resort to multiply embedded contexts to suggest that two putatively synonymous terms do not substitute freely. Single-level doubt is sufficient for that. (Fregeans, for instance, do not resort to multiple embeddings in order to demonstrate the failure of co-referring names to freely substitute in belief contexts.) Mates resorts to multiple embeddings because he is apparently unable to bring himself to personally doubt the substitutivity of synonymous or intensionally isomorphic pairs. But, Mates supposes, someone else could. So he formulates his challenge in terms of it being possible for at least someone, anyone, to doubt the substitution of synonymous words. But multiple embedding is not strictly necessary—simply feigning doubt or, like Sellars (1955), making up a name (i.e., "Jones") for someone who does doubt the substitution would have done just as well. Indeed, Putnam does not conclude that multiple embeddings present a special challenge to substitutivity. Rather Putnam (1954) reluctantly concludes: "If we take [Mates' argument] seriously, there is but one conclusion to which we can come: 'Greek' and 'Hellene' are not synonyms, and by the same argument, neither are any two different terms" (153, reprinted in Salmon & Soames 1988). Mates's argument indicates that single-level doubt is sufficient to undermine any possible account of synonymous substitutivity in the same way that Fregean arguments suggest that single-level doubt is sufficient to undermine co-naming substitutivity.

But Kripke's response to Mates's argument is simple: synonymous doubt is not a genuine possibility because synonymous doubt is merely interpreted as evidence of less

than full linguistic competence. When commenting on Mates, Kripke (1979) moves to short-circuit Mates's argument by not permitting synonymous doubt to take hold in the first place: "First," says Kripke (1979), "if someone were to assent to 'Doctors are happy', but refuses assent to 'Physicians are happy', *prima facie* disquotation does not apply to him since he is under a linguistic or conceptual confusion. . . . So there are as yet no grounds, merely because this has happened, to doubt that all who believe that doctors are happy believe that physicians are happy" (147). Doubt regarding the TP-substitution of synonymous predicates is not, at least by Kripke (1979), interpreted as evidence for the existence of hyper-intensional Fregean-like entities sufficient to semantically distinguish co-meaning synonyms. Nor do Church (1954), Feyerabend (1956), or Sellars (1955), for instance, endorse the proliferation of hyper-intensional entities (see Bealer 1993, for a brief discussion of this strategy). But nor are doubts regarding synonymous substitution interpreted as Quinean evidence that synonymy cannot, in principle, be distinguished from mere co-extensive predication. Instead, synonymy is held to be a linguistic requirement.

So while predicates generally apply contingently, synonymous predicates either both apply or both do not apply. There is, at least for Kripke, apparently no room for the "*a posteriori* discovery" of the truth that all doctors are physicians and all physicians doctors. Any "discovery" that synonyms are synonyms is interpreted as a belated linguistic epiphany—an indication that the terms must not have been applied correctly in the first place so that their "convergent application" could then later be discovered (see Feyerabend 1956). In short, synonymy is not subject to epistemic negotiation.

But if co-meaning predicates can be viewed as "directly," as it were, expressing the same property with epistemic factors dismissed as substitutionally irrelevant, structurally analogous considerations can be brought to bear upon the substitutional status of co-referring names. Co-naming doubt need not—anymore than synonymous doubt—be interpreted as Fregean evidence for sense-like entities. Instead it may be interpreted as evidence of impartial or incomplete linguistic competence. After all, even Fregeans agree that 'Tully' is Cicero's name just as much as 'Cicero' is. But rather than postulate intervening senses to explain the under-extension of 'Tully' (to restrictively refer only to Tully, and 'Cicero' only to Cicero), those who do not refer—or at a minimum, are unwilling to deferentially refer—to Cicero with both his names cannot be counted as unquestionably competent.

This is why I agree with Kripke (1979) when he rhetorically asks, "Has the parent mistaught the language?" if—aware of the identity of Hesperus and Phosphorus and, pointing to the morning star, calls it 'Hesperus'. Kripke suggests that the parent would not be misteaching the language, for he adds: "A parent who says 'Creatures with kidneys are called 'cordates', definitely has mistaught the language, even though the statement is extensionally correct" (146). But what about referring to Cicero with the name 'Tully'? Or referring to London with the name '*Londres*'? Would that be misteaching or misusing the terms? I do not think so. And that is because the full use—the deferential use—of 'Tully' includes Cicero in its denotation, not just Tully.

So while it is contingent that 'Cicero' and 'Tully' refer to anything at all, given that they do refer to the same (existent) thing, the Kripkean *a posteriori* discovery of the metaphysical truth that Cicero is necessarily Tully can be viewed—like Kripke wishes to

view synonyms—as a belated linguistic epiphany: the terms 'Cicero' and 'Tully' must have been incorrectly under-extended for their co-naming application to have ever diverged so that their convergence could then later be "discovered." Moreover, it is unclear whether Kripke's position on the linguistic necessity of synonymous substitution and the metaphysics of rigid designation are consistent with his Fregean-friendly position regarding the belief-context failure of co-naming substitution. (In "A Puzzle About Belief," Kripke (1979) reminds us that "*Naming and Necessity* never asserted a [co-naming] substitutivity principle for epistemic contexts" (139).) For unlike his treatment of co-referring names, Kripke does not, or does not uniformly, allow for the *a posteriori* discovery of a necessary truth relating two predicates to the same property. Kripke does not advertise 'being a doctor' and 'being a physician' as an *a posteriori* necessity relating two predicates to the same property. According to Kripke, substitutions between 'being a doctor' and 'being a physician' are a linguistic requirement of predicate synonymy. Perhaps 'being water' and 'being H_20' are predicates that express the same property, but unlike 'being a doctor' and 'being a physician' are not synonymous. Substitutions between 'being water' and 'being H_20' would not be a linguistic requirement. In this way, like 'being Cicero' and 'being Tully,' 'being water' and 'being H_20' would make for an *a posteriori* necessary truth. But isn't that what Mates would, or at least could, claim about 'doctor' and 'physician'?: *'D'* and *'D''* may very well be synonymous—*'D'* and *'D''* may express the same property—but that does not guarantee substitutivity. Kripke can maintain the non-synonymy of 'being water' and 'being H_20'—Kripke can make room for an *a posteriori* discovery of a necessary truth relating two predicates to

the same property—but only by reducing the force of his claim regarding the linguistic requirement of 'doctor'/'physician' co-meaning predication.

Perhaps the reason why 'being water' and 'being H_20' resist synonymous substitution is that 'water' and 'H_20' are more like 'Cicero' and 'Tully' than 'doctor' and 'physician.' As natural kind terms, 'H_20' and 'water' (like 'pain' and 'c-fibers' to use another Kripkean example) behave more like directly referring devices than predicates. But if this is indeed the case, if 'being water' and 'being H_20' are to be assimilated to 'being Cicero' and 'being Tully' to help account for the *a posteriori* discovery of their necessary identity, our original question regarding the substitutional status of co-referring names returns. It is not clear why the substitution of co-referring names should be held to an explicitly individualized epistemic standard while synonymous substitution is immune to such epistemic twist and turns. It is not clear, that is, why there should be this difference between synonymous and co-naming substitution independent of traditional Fregean intuitions regarding the substitution of co-referring names in belief contexts.

In this way, the linguistic requirement of co-naming substitution is the result of combining Kripke's anti-Fregean reasoning about names with his anti-skeptical reasoning about synonymous substitution. The result is that "psychologism" with respect to co-naming substitution is seen to be, in principle, no more satisfactory than "psychologism" is with respect to linguistic synonymy. Individualistic doubt is not a semantic trump card. This is why I share with McDowell (1977) "the suspicion that the ultimate source of the desire to see the differences in sense as underlying failures of substitution, and hence as capable of affording genuine explanations of them, is psychologism about sense, which Frege (officially) renounced" (157).

From a Fregean perspective, however, it would seem that the very possibility of "misapplying" one's own words by over- or under-extending them is not a coherent possibility (see Burge 1990, Kaplan 1989, Kimbrough 1998, Owens 1986, Santambrogio 2002). In fact, senses are precisely what license language users to under-extend 'Tully': 'Tully' need not be applied to Cicero as 'Tully' really only means what 'Tully' is *believed* to refer to—not what it might refer to by some other non-individualistic, non-epistemic standard. "[F]or how can it be meaningful," asks Higginbotham (1994) "to say that one 'partially grasps' one's *own* idiolect?" (528). As Kaplan (1989) explains,

> Perhaps [this] accounts for the feeling one has reading Russell on logically proper names, and even more so in reading Frege, that, like Humpty Dumpty, everyone runs their own language. When we speak, we *assign* meanings to our words; the words themselves do not have meanings. These assignments are, in theory, unconstrained (except by whatever limitations our epistemic situation places on what we can apprehend). In practice, it may be prudent to try to *coordinate* with the meanings others have assigned, but this is only a practical matter. (600)

But it is arguably words—words with pre-assigned content—which are the primary mechanism by which we uniformly refer to objects and properties independently of our own epistemic idiosyncrasies. "One need not fall in love to speak of love" says Kaplan (1989), "One need not have grieved to speak of grief. The poet who has never felt or observed love may yet speak of it if he has heard of it. The fact that the language to speak of it and to enable us to have heard of it exists may show that someone once felt love. But it need not be the poet. As with love, so with Samarkand (and red, and Aristotle)" (604).

Yet it should be kept in mind that only within the semantic frameworks provided by Putnam's "division of linguistic labor" and Burge's "anti-individualism" can the possibilities of incomplete or partial understanding of names and words generally be

more fully appreciated. Nevertheless, semantic norms are both liberating and constraining. What norms give with one hand they take with the other, for standards provide something to aim for just as much as they provide something for us to miss. We cannot fully enjoy the advantages of a division of linguistic labor without also being prepared to deferentially revise our linguistic usage and deferentially concede the synonymous substitutivity of co-meaning predicates or the strict substitutivity of co-referring names. Language—the communal language we share—does not offer any free rides. But while philosophers like Putnam and Burge have identified the personal benefits of a division of linguistic labor and communal linguistic standards, they have not addressed the personal costs of being deferential. In addition to allowing for the possibility of being convicted of lying (abusing words with the deliberate intent to deceive), deference is an explicit acknowledgement of partial or incomplete linguistic competence. But if language users are indeed typically deferential in this way, what we might attempt to express, and what might be inferred about our beliefs, through the use of words (e.g., "Tully is not Cicero," or "London is not *Londres*") we do not fully understand raises a number of issues which require careful dissection. Acknowledging incomplete linguistic understanding gives us the humble option of deferentially contradicting ourselves as opposed to the Humpty Dumpty-like option of defiantly pleading that "our own words" have been misunderstood. (See Burge 1978, 1979. The application of Burgean semantic deference to the proper or expert usage of names is taken up in chapter 3 and concluded in chapter 10.) In fact, claiming to genuinely doubt—no less deny—that Tully is Cicero (or all lawyers attorneys) is ultimately no more possible than genuinely doubting whether, for instance, TULLY is Tully.

But there is still yet a third problem awaiting an analysis of TP-substitutions premised upon an analogy with TK-substitutions. In virtue of not being the same word-type (nor any pre-theoretical commitment to "dot quote"-types), the substitution of co-referring names or synonyms is ungrounded unless interpretive considerations of semantic content are invoked. The reason why TP-substitutions involving co-referring names are unproblematic—at least in some contexts—is that co-referring names, as semantically interpreted, refer to one and the same object. And the reason why TP-substitutions involving synonymous terms are never unproblematic is that they too must be semantically interpreted, albeit as expressing the same property, in order to be equated. The substitution of co-referring names is trouble-free, when it is trouble-free, because the terms are co-referring. And the substitution of synonyms is trouble-free, were it to be trouble-free, because the terms co-express the same property. Unlike TK-substitutions among tokens of the same word-type, TP-substitutions among tokens of different word-types clearly require an interpretive appeal to semantic content—reference at a minimum for names, and properties for synonyms.

Both the TP-substitution of co-referring names and the TP-substitution of synonyms depend upon something like or at least related to semantic content. Phonological or orthographic form is wildly insufficient. Whether attempting to substitutionally equate co-referring names or synonyms, one must look beyond, or better, through (the word-tokens instantiating) these word-types in order to appreciate their equivalence. One reason, then, why synonyms and co-referring names attract the attention of philosophers is that there is typically nothing about the way a pair of co-referring names or a pair of synonyms look or sound like which gives any indication that

they are somehow related to each other. It is their semantic life, not their perceptual form, which relate 'Cicero' and 'Tully' (and 'attorney'/'lawyer').

Unlike TK-substitutions among tokens of the same word-type, substitutions among co-referring names and substitutions among synonyms are not orthographically or phonologically licensed. Neither synonyms nor co-referring names are the same word. Instead, their substitution must be grounded elsewhere. TP-substitutions are based upon semantic content (co-reference, at a minimum, for names and co-meaning for predicates), whereas TK-substituitions are based upon orthographic or phonological form.

This difference, then, between content-based TP-substitutions and orthographic/phonological-based TK-substitutions sets up an important disanalogy. On the one hand, TP-substitutions naturally have their basis, if they have any basis, in interpretive considerations of, or at least related to, semantic content, meaning or linguistic use. On the other hand, word-token TK-substitutions are licensed simply in virtue of being variants of one and the same orthographic/phonological word-type. Form is doing the work in TK-substitutions while something other than form underwrites synonymous and co-naming TP-substitutions. But this is a misleading disanalogy. TK-substitutions have much more in common with TP-substitutions then might be initially suspected.

The problem with setting up a distinction between content-sensitive TP-substitutions and form-sensitive TK-substitutions is that aligning them in this way suggests that only the former substitutions are based upon interpretive semantic considerations of co-naming or co-predicate content. But this is not how TK-substitutions work. The processes that give rise to basic TK-substitutions are themselves

dependent upon interpretive semantic considerations of co-naming and co-predicate content. The distinction between TP-substitutions which require an appeal to semantic content for their substitutivity and TK-substitutions which do not require any such appeal to semantic content breaks down.

This is because TK-substitutions are themselves also dependent upon semantic considerations. And this is because individual word-tokens are not, and cannot be, comprehensively classified into their language-specific word-type categories without relying on at least some interpretive semantic information. And this is because the phonemes word-types themselves are composed of cannot be individuated without some appeal to interpretive semantics. In terms of natural language phonology, basic phonological equivalence is "semantically conditioned" (Ullmann 1957): the classification of phonetically distinct word-tokens into phonemically equivalent word-types cannot be accounted for in terms which are exclusively non-semantic in nature because the classification of individual phonemes is itself semantically conditioned. In this way, both TP-substitutions and word-token TK-substitutions are semantically dependent. Semantic considerations typically viewed as vexing only philosophers of language are implicated even in the recognition of basic phonological equivalence. Like philosophers, phonologists also have to manage semantic issues when addressing issues of substitutional equivalence.

The observation that even TK-substitutions are semantically conditioned is noteworthy for two reasons: the first of interest mostly only for philosophers; the second of interest primarily for linguists. Consider that if basic phonemic categorization is dependent upon the semantic value of word-tokens, then not only is semantics implicated

in the language-specific phonological categorization of natural language (an interesting linguistic claim, see below), it would seem that it is word-tokens—not word-types—which have semantic properties (an interesting philosophical claim). If the semantic interpretation of various word-tokens is critical to the grouping of word-tokens into word-types, semantic content is being attributed to word-tokens. Indeed, attributing semantic content to word-tokens might be enough to suggest that the categorization of word-tokens into word-types cannot be based upon semantic information if only because it is abstract word-types—not word-tokens—that have semantic properties in the first place.

The short answer to this objection is that phones—ultimately unique space-time particulars—and the word-tokens they combine to create, and the sentence-tokens word-tokens themselves combine to create, exist. As linguists are fond of reminding us, no one has ever heard or pronounced a "phoneme." Nor has anyone ever pronounced a word-type or produced a sentence-type either. Abstract phonemes, the abstract word-types they serially combine to create, and the abstract sentence-types word-types serially combine to create, are all a reflection of language-specific, phonologically licensed substitutional practices among intersubstitutable word-token variants. They need not be attributed any independent existence apart from these phonological practices. Indeed, if there were no token-to-token variation among word-tokens of the same word-type, there would be little need to introduce anything like abstract phonemes or abstract word-types. The distinction between "token" and "type" itself would largely dissolve. Abstract phonemes, word- and sentence-types are the result of a language-specific classificatory system by which word-token particulars are categorized as the "same"—as phonologically intersubstitutable. And while these phonologically licensed substitutional practices have

a number of real empirical consequences, both developmentally and psycholinguistically (see chapter 8), it is the language-specific practice of treating phonetically distinct word-tokens as the "same"—not the fact that abstract linguistic types exist—that accounts for these linguistic effects.

Languages are spoken, written, signed, tapped, or felt, etc. with tokens, at least some of which are treated as phonologically or orthographically, etc. equivalent. In this way, semantic properties are properties of linguistic tokens: semantic content is a property of word-tokens and truth a property of sentence-tokens. This is not to suggest that one cannot attribute semantic properties to word- and sentence-types, or that it is incoherent to do so. Indeed, it is both natural and legitimate to define word-types as phonologically permissible strings of phonemes, and sentence-types as syntactically permissible strings of word-types—and to attribute semantic properties to them as we normally do—so long as one bears in mind that phonemes themselves are constructs representing equivalent classes of phonologically intersubstitutable phones.

In any event, the metaphysical status of abstract linguistic-types is a large topic, too large a topic—enough to sustain dissertation-length research unto itself (see, e.g., Hutton 1990). I do maintain that in the final accounting, the reality of language consists of tokens, and the abstract types into which they are grouped are in some sense less real. Having issued this promissory note, the question at hand concerns not the *ontology* of abstract linguistic types, but the *classificatory principle* of abstract types. And this is because language users routinely commit themselves to attributing the same semantic value to a wide variety of phonetically distinct but phonologically equivalent word-tokens. And this we do regardless of our philosophical proclivities, indeed regardless of

whether we are Quineans, Fregeans or innocent by-standers: basic phonological/orthographic competence demands as much. Rather than provide, no less prove, an ontological reduction of "type-talk" to "token-talk," I explain how types are individuated—how various tokens actually come to be categorized as the same abstract type. What is required is an accounting of how and why a variety of distinct word-tokens should come to be treated as phonologically intersubstitutable.

The long answer, then, to this objection requires an investigation into the basis, or classificatory standard, by which various word-tokens are grouped into word-type categories. Regardless of the philosophical status of abstract linguistic types, the challenge posed by the language-specific configuration of basic phonological word-types must be addressed.

Broadly speaking, explanations for the language-specific character of basic phonological categorization come in two varieties. One kind of explanation attempts to derive the language-specific character of phonemic types in terms which are exclusively non-semantic in nature. Information in the form of phonetic form, phonetic distribution, and sometimes even behavioral data are collected and analyzed so as to derive the phonemic structure of the language. The other type of explanation utilizes all this information, and more. For whereas the former purposely disavows any reliance on interpretive semantic information, the latter supplements the collection of non-semantic data with semantic content. The reason this is done is simple. The language-specific phonological categorization of phones into phonemes and hence phonetic word-tokens into phonemic word-types cannot be reconstructed merely on the basis of non-semantic information. Interpretive semantic information is also required. This claim is briefly

explained in the next chapter and explored at length in chapters 6 and 7. No amount of semantically blind listening or recording, no matter how phonetically detailed or technologically sophisticated, is sufficient to derive phonemic classification.

In this way, TK-substitutions and TP-substitutions have more in common with each other—at least with respect to their dependence upon semantic considerations—than might ordinarily be supposed. Both are semantically conditioned as even the basic phonological substitutions defining the language-specific phonological contours of natural language are not grounded in exclusively non-semantic considerations. This result, however, has philosophical implications. If semantics is theoretically necessary for linguists to account for basic phonological equivalence (chapters 6 and 7) and developmentally exploited by infants to acquire basic phonological competence (chapter 8), these theoretical and developmental results provide at least some initial motivation to re-approach traditional issues regarding the role the semantics of synonymy should or should not play, and the role the semantics of co-naming reference does or does not play, in grounding TP-substitutions for philosophers.

Such phonological considerations do not, however, prove that co-referring names are interchangeable without loss of truth value in belief contexts, or that meanings exist so that synonymous predicates may be said to be analytically equivalent. Instead, such considerations are negative in their impact. Pressing the semantic basis of phonology serves to highlight an unrecognized repercussion of traditional Fregean reasoning and Quinean eliminativism. On one hand, without considerations of synonymy and meaning equivalence, more than just analytic equivalence is lost. The basic phonological equivalence of various predicate-tokens is also threatened. On the other hand, if co-

naming reference is insufficient to guarantee substitutivity in belief contexts, it would seem that basic phonological substitutions might also be subject to Fregean-style substitutional doubt insofar as they are grounded in co-reference (rather than co-sense). In this way, the semantic basis of TP-substitutions—meaning for synonymous predicates and co-reference for names—cannot be evaluated in a philosophical vacuum. There are parallel issues in natural language phonology to consider. The semantic basis of natural language phonology constrains what can be consistently said about the substitution of synonymous predicates and the substitution of co-referring names.

Nor do these phonological considerations concerning the semantic basis of phonological equivalence entail that TP-substitutions and TK-substitutions are semantically dependent in exactly the same way. Matters are more complicated. Sameness and difference in word-token content may very well ultimately determine whether two individual phones do or do not constitute the same phoneme, and hence whether the word-tokens are or are not the same word-type (see chapter 2). Yet it is also clear that not all instances of phonetically distinct but semantically equivalent word-tokens are phonologically equivalent. Tokens of two synonymous predicates—though meaning equivalent—are not themselves the same word. Nor are tokens of two co-referring names—though referentially equivalent—the same word either.

So while it might be conceded that phonological word-type sameness or difference can ultimately be accounted for only with the help of word-token content, semantic content appears not to be a sufficient condition to guarantee phonological word-type equivalence. Phonemically distinct but co-referring names exist. And phonemically distinct but synonymous predicates also exist. Nor does phonemic equivalence always

appear to guarantee semantic equivalence. Semantically ambiguous homonyms also exist. These issues are discussed more fully in the next chapter, and solutions to these challenges are proposed.

The Philosophical Significance of Comparing Word-token Substitutions and Word-type Substitutions

Yet even assuming these difficulties can be resolved, the larger issue remains. How is the semantic basis of phonology supposed to interact with traditional philosophical concerns regarding the substitution of synonyms vis-à-vis Quinean meaning skepticism and the substitution of co-referring names in belief contexts vis-à-vis Fregean sense theory? A two-pronged connection is pursued, one associated with Quinean reasoning and the other associated with Fregean reasoning. Recall that Quine objects to analytic TP-substitutions involving synonyms. Quine objects to analyticity because it is dependent upon intuitions of synonymy. But Quine's objection to co-meaning substitutions is one of principle. Insofar as basic phonological TK-substitutions are also based upon considerations of predicate meaning, they too must be rejected. But whereas Quine welcomes the elimination of analytic equivalence, Quine does not wish to eliminate basic phonological equivalence.

In order to consistently reject analytic equivalence but retain basic phonological equivalence, Quine pursues a reductive account of phonological equivalence independent of interpretive meaning. He attempts to do this by grounding basic phonological equivalence in the non-semantic terms of acoustic, phonetic, or, as a last resort, behavioral data (see Quine 1953a, 1960, 1981, 1990). None of these attempts to "reduce the phoneme" prove successful, as the former analyses are well-known to be technically

insufficient and the latter is philosophically inadequate. But insofar as Quine is unwilling to allow meaning to play a role in grounding basic phonological equivalence, Quine is unable to provide an account of basic phonological equivalence acceptable in light of his own standards. Quine's position on phonology—at least with respect to the TK-substitution of predicate word-tokens—effectively replicates his position on analyticity: de-semanticize or eliminate.

In addition to co-meaning predicates, natural languages also have, or at least appear to have, co-referring names (and common nouns) which refer to the same object. Though Quine does not specifically consider a referential basis for some TK-substitutions, a referential basis for phonological typing does intersect with Fregean-like concerns. This is because Fregean intuitions regarding the substitution of co-referring names in belief contexts highlight the failure of reference to preserve truth. So while Quine objects to meaning, synonymy, and analyticity, and is suspicious of phonological equivalence insofar as it is dependent upon such notions, Frege observes that even co-naming reference is fundamentally insufficient to preserve truth in belief contexts.

But Fregean intuitions—like Quinean eliminativism—might not appear so definitive once issues of basic phonological substitutivity are considered. If co-naming reference is, in principle, incapable of preserving truth in belief contexts, and some phonological substitutions are themselves dependent upon referential considerations, this would seem to imply TK-substitutions also susceptible to Fregean doubt within belief contexts. So whereas Quine's meaning eliminativism threatens TK-substitutions based upon predicate meaning, Frege's observation regarding co-naming failures of substitutivity appear to threaten TK-substitutions based upon reference.

On the one hand, then, Quine rejects analytic-based TP-substitutions, but ends up being unable to accommodate phonologically based TK-substitutions insofar as they too are indebted to meaning. The Quinean dynamic between analytic equivalence and phonological equivalence is taken up in chapter 5. On the other hand, Frege observes that reference-based co-naming TP-substitutions are not immune from substitutional doubt. The structure of Frege's argument, however, suggests that referenced-based TK-substitutions might also be subject to doubt. Presumably Frege's argument—like Quine's—is one of principle. Perhaps co-naming TK-substitutions are not subject to doubt because they are based upon an equivalence of co-naming sense (rather than just mere co-naming reference, see chapter 2). But basing TK-substitutions upon an epistemic convergence of Fregean sense seems to demand a very high standard for phonological substitution. Phonological substitutions would, in principle, be no more likely nor interpersonally stable than co-sense TP-substitutions among co-referring names. Fregean senses may very well explain failures of co-naming TP-substitution, but they are not well-designed to explain why phonological TK-substitutions do *not* fail—independent of referential considerations. Instead, senses would explain why we should expect even TK-substitutions to fail. And this is because the semantics of Fregean sense is fundamentally divisive: co-referring word-tokens like 'TULLY' and 'Tully' would have to have the same sense (as well as referent) if they are to be phonologically intersubstitutable. Fregean senses do not encourage substitutivity; Fregean senses work to block substitutivity. The interaction between Fregean failures of co-naming TP-substitutivity and basic phonological TK-substitutivity is taken up in chapters 2, 3, and 4.

One way to defuse both these Quinean and Fregean complications is to ground basic phonological substitutions in something entirely independent of semantics—whether the semantics of synonymous predicate meaning or the semantics of co-naming reference. Phonology would be the result of strictly "bottom up" perceptual processing over word-tokens—independent of semantic content. In this way, Quine would be free to reject synonymy, and Frege free to postulate senses, without there being any phonological repercussions. Quine could eliminate synonymy without jeopardizing phonology and Frege could introduce senses without injecting senses into phonology. And to be sure, much of the structure of natural language phonology can be resolved independent of interpretive content. But not all phonological typing can be sorted without semantics. Chapter 2 explains how and why the phonemic classification of "free allophonic" variation demands the interpretive resources of semantic content.

This introductory chapter started with an attempt to address a number of problems confronting a comparison between word-type TP-substitutions and word-token TK-substitutions. The first concerned the inexistence of abstract linguistic types subsuming synonyms or co-referring names. This notational lacunae is eventually filled in chapter 9 with the help of Sellarsian "dot quotes." With the concretization of "dot quote"-types, it becomes no more possible to doubt the TP-substitutional equivalence of synonyms (and by extension, co-referring names, see note 2) than it is genuinely possible to doubt TK-substitutions among word-tokens of the same word-type. Synonyms and co-referring names become notationally incidental variants of the same abstract "dot quote"-type analogous to the way word-token variants are mere incidental variants of the same abstract word-type. Doubting co-naming and synonymous substitution is, as Ruth Barcan

Marcus might express it (see note 14), tantamount to doubting whether some object or property is identical to itself, applied, here, to linguistic types.

A second issue involved the related observation that while doubt regarding TK-substitutions is symptomatic of a linguistic deficiency, deviancy, or difference of some sort, TP-substitutional doubt has no such linguistic repercussions. Yet unlike Quine, Kripke maintains that synonymous TP-substitutions are a "linguistic requirement." Kripke (1979) distinguishes between co-meaning predication and co-extensional predication: synonymous substitutions cannot be doubted without jeopardizing one's linguistic competence. Yet is not obvious that co-naming doubt does not also have comparable linguistic consequences. Withholding the name 'Tully' from Cicero or using '*Londres*' to refer to only certain parts of London (the pretty parts) is not a robust indication of the full denotational use of these names. Our standards for the proper use of co-referring names are not intrinsically lower than our standards for the proper use of synonymous predicates.

A third issue concerned a distinction between the semantic basis of TP-substitutions in contrast to the non-semantic basis of TK-substitutions. The semantic basis of phonological TK-substitutions, however, is the heart of the dissertation. If it were the case that word-tokens were grouped into word-types strictly on the basis of their phonetic form and/or distribution, a philosophical comparison between TK-substitutions and TP-substitutions would be unmotivated. Unlike TP-substitutions, TK-substitutions would be reducible to the non-semantic facts of phonetic form and/or distribution. Appeals to abstract word-types would, in principle, be very different from appeals to "dot quote"-types, as the basis of these substitutional patterns themselves would be very

different. TK-substitutions would be based upon semantically uninterpreted data while TP-substitutions would be based upon semantically interpreted data.

But this is not the way TK-substitutions work. In particular, this is not the way language-specific phonemic categorization works. TK-substitutions are, in principle, no more grounded exclusively in uninterpreted perceptual form than TP-substitutions are. Neither TP-substitutions nor TK-substitutions are perceptually obvious or in some way perceptually inevitable. In particular, the substitution of "free allophonic" variants in natural language is not governed by perceptual similarity, nor even language-specific rules of "complementary distribution" (see chapter 2). Philosophically speaking, it is no more perceptually obvious why two allophonically distinct word-tokens should count as the same "abstract word-type" than it is perceptually obvious why co-referring names or synonyms would count as the same abstract "dot quote"-type. Instead, both kinds of equivalencies are semantically conditioned.

Introduction to the Philosophy of "Phonemic Reductionism"

The differences between philosophical TP-substitutions and phonological TK-substitutions, then, should not obscure the parallel issues they provoke. It is not, after all, the fact that two word-tokens either are or are not the same word-type for some language, and hence trivially equivalent—even within belief contexts—which is of philosophical or linguistic interest. Rather, it is the *standard* or *principle* by which various linguistic expressions are classified as either tokens of the same type or tokens of two different types which is of interest. There is, as it were, a "philosophy of phonology": what principles govern the basic phonological classification of natural language?

This is to step into a well-trafficked, though largely anachronistic, controversy. In fact, the basis of natural language phonological equivalence was long a topic of debate among linguists—and, with Quine, even between philosophers and linguists. In addition to being theoretically attractive for the philosophical reasons related to Frege and Quine outlined above, the relationship between semantics and phonology was an outstanding issue within linguistics proper. For while a diversity of psychophysical, perceptual, psychological, functional, and even behavioral accounts have historically competed for preeminence within linguistics (detailed in chapter 7), one theme remained dominant: whether interpretive semantic information is admissible, necessary and/or sufficient, or even relevant to the challenge of classifying token distinct expressions into phonologically equivalent categories.

In this way, Quine's philosophical interests in the basis of phonological equivalence can be placed in broader context for they are at least partially a product of his time. As will become clearer, Quine's commitment to providing a non-semantic reduction of basic phonological equivalence was by no unique. Various attempts to "reduce the phoneme" characterized much of early twentieth-century "neo-Bloomfieldian" phonological theory (though arguably little of their descriptive or field work, see Fought 1999a, 1999b). The claim that basic phonological categorization demands the interpretive resources of semantic content was hotly disputed throughout the structuralist period of linguistics both in the United States and abroad before the dawn of generative linguistics in the late 1950s (chapter 6). Indeed, one of the most interesting and controversial issues within twentieth-century linguistics, particularly as practiced within the structuralist framework of American descriptivist linguistics, was the attempt

to define phonological equivalence without the aid of any interpretive information. In fact, in addition to relying on the arguments provided by his contemporary "neo-Bloomfieldian" linguists, Quine would join the debate himself (chapter 7).

But nor were such asemantic demands peculiar to phonology. Within syntactic theory, for instance, reconstructions of natural language grammar independent of semantic content have also been viewed as methodologically preferable on account of their autonomy and perceived simplicity. From this perspective, grounding syntactic analyses in semantic considerations rather than just grammatical form and grammatical patterning was considered not only unnecessary in principle, but counter-productive in practice. Chomsky's early work in generative syntax (Chomsky 1957, 1965) is a clear example of this linguistic perspective. His invocation of 'colorless green ideas sleep furiously' was tailored to suggest the irrelevance of semantic considerations when considering issues of syntactic structure and syntactic wellformedness.[5]

So while the interface between semantics and syntactic theory represents one issue within linguistics (see chapter 9 of Bennett 1976 for a brief overview), an analogous controversy has historically also consumed phonology. This is because there has been a principled methodological interest in conducting linguistic analysis—whether grammatical, morphological, or phonological—without recourse to interpretive semantics. As Chomsky (1953) explains in the introduction to his first published paper:

> During the past several decades, linguists have developed and applied widely techniques which enable them, to a considerable extent, to determine and state the structure of natural languages without semantic reference. It is of interest to inquire seriously into [them] . . . and to examine the possibilities of applying [them], as has been occasionally suggested [a footnote favorably references the structural analysis of Zellig Harris and the work of Goodman and Quine on "constructional systems" and their "development of nominalistic syntax"] to a wide range of problems. (242)

In this way, systematic reconstructions of natural language phonology dependent upon semantic information were considered less desirable than analyses sensitive to just phonetic form and phonetic patterning (i.e., distribution). Indeed, the battle to define the relationship between semantics and linguistic structure was first fought on field of phonology (and morphology); the "autonomy of syntax" was more or less modeled on the semantic autonomy of phonology. (See Twaddell 1935 for an early version of this debate within phonology.) Yet by all accounts, the phonological battle was lost (Clark & Yallop 1995). Roy Harris's (1973) monograph and Ohlander's (1976) dissertation research are concerned with precisely this issue. And they, like Pike (1947b, 1952a), Ullmann (1957) and Fischer-Jorgensen (1952, 1956, 1958) among others—including even Bloomfield (1933) and a reluctant Chomsky (1955/1975) himself—conclude that semantics is ineliminable for phonological analysis.

Yet one should not lose sight of the positivist-like appeal of pursuing linguistic analyses free of semantic considerations. In fact, as Chomsky's (1953) allusion to Quine and Goodman suggests, Quine and at least a young Chomsky stood shoulder to shoulder on the merits of an asemantic linguistic methodology enforced, in particular, for phonology (see Heitner 2004). Challenging the semantic basis of phonological equivalence in favor of a non-semantic account based upon acoustic, phonetic, perceptual or, as a last resort, behavioral data, characterize both Quine's philosophical and at least Chomsky's early linguistic work. In one form or another, both Chomsky and Quine have advocated for a non-semantic reduction of basis phonological equivalence (see chapter 7). Indeed, for Quine, "saving" basic phonological equivalence from semantics was just as important as eliminating traditional meaning-based analytic equivalence. For Quine, it

was clearly not enough to wean philosophers from meaning-based analyticity. Linguists must also free themselves from their own meaning-based phonology.

Quine's motivation for rejecting meaning-based accounts of phonological equivalence is clear enough. If basic phonological equivalence were to require meaning equivalencies across word-tokens of the same predicate, then they too would be subject to the same sort of eliminative skepticism Quine directs at analyticity. The solution to this problem is to provide a non-meaning, reductive account of phonological equivalence. In a sense, Quine perceives how his attack on analyticity has phonological implications, and moves to ensure that phonology is not dependent upon meaning. But Fregeans are saddled with a similar dynamic, even if for different reasons.

Fregean accounts of the substitution of co-referring names in belief contexts implicitly assume that substitutions among word-tokens of the same word-type are not dependent upon considerations of co-reference, as only word-tokens of distinct word-types are held to provoke failures of substitutivity in belief contexts—not word-tokens of the same word-type. But there should be something philosophically principled which prevents Fregean substitutional skepticism from applying to TK-substitutions in addition to TP-substitutions. Short of relying on a divisive equivalence of co-sense (see chapter 2), the answer is that phonological substitutions are not dependent upon any interpretive considerations of semantic content in terms of reference *or* sense. Phonological TK-substitutions are based upon considerations of form.

There are convergent reasons, then, to either explicitly demand (as with Quine) or implicitly assume (as with Frege) an account of basic phonological TK-substitution independent of semantics. As for the role reference plays in accounting for basic

phonological substitutions, Fregean intuitions would have it that reference is either insufficient or unnecessary. As for the role meaning plays in establishing basic phonological equivalence, Quine would have it that philosophers do not want it and linguists do not need it.

But these claims are empirically misplaced in virtue of the cross-linguistic diversity of basic phonological classification, and developmentally dubious in virtue of the semantic demands the phenomenon free allophonic variation imposes upon the emergence of language-specific speech perception. Interpretive considerations of content are implicated even in the language-specific organization of natural language phonology (chapters 7 and 8). It is unsurprising then, as both Frege and Quine observe, that the behavior of co-referring names and synonyms—relations between phonologically distinct word-types—cannot be accounted for without appealing to something other than basic phonological form. Even the phonemic foundation of basic phonological word-type form is itself semantically conditioned.

Quine's rejection of the analytic substitution of synonyms is based upon a rejection of meaning. And Frege's invocation of sense is based upon the failure of reference to guarantee the substitution of co-referring names in belief contexts. But both Quine and Frege adhere to the common conviction that some substitutions—the "phono-logical"—are licensed strictly in virtue of non-semantic form. This is explicit in Quine's writings on the phoneme, but implicit in Fregean reasoning, or at least I shall argue. Given that Quineans and Fregeans generally agree upon so little, the Quinean contention and Fregean assumption that basic phonological equivalence is driven by form rather than content would appear to tap into a pervasive feature of analytic philosophy of language.

In doing so, it deserves a name: "phonemic reductionism."[6] Phonemic reductionism is the thesis that considerations of basic phonological equivalence are independent of semantic content.

"Phonemic reductionism," however, is false. Basic phonological equivalence cannot be asemantically reduced to perceptually accessible acoustic, phonetic, or even phonotactic distributional data. Indeed, the semantic basis of language-specific phonemic categorization is now universally acknowledged, with even some of its most ardent "neo-Bloomfieldian" opponents having long conceded that phonology is—as Leonard Bloomfield routinely reminded them—at least partially grounded by semantic considerations of interpretive content (chapters 6 and 7). According to Clark & Yallop (1995),

> It is generally agreed that the classic attempt to produce phonological descriptions that would make no reference to the meanings of words, let alone to native speakers' intuitions or insights is indeed inconsistent. Harris' "Methods in Linguistics" [sic] (1951) represents the claim that it is possible to discover phonemes purely by examining the distribution of phonetic segments. . . . But Harris' analysis in fact assumes the investigators ability to judge whether two utterances in a language are intended to be different words or whether they count as alternative ways of saying the same word. It can be argued that Harris' and others' efforts to define "objective" analytic procedures constantly presuppose access to native speakers intuitions into their own language. (83)

The philosophical repercussions—one Fregean and the other Quinean—the historical background, and some developmental implications of this failure to "reduce the phoneme" are developed in ten chapters. Chapter 2 introduces the linguistic terminology and phonological notation linguists use to describe the phonological organization of natural language, and explains why phonological equivalence is conditioned by semantic considerations. Though semantically conditioned, chapter 3 highlights how word-token "allophonic" substitutions are nevertheless immune to doubt and contrasts this

observation with a range of philosophical responses designed to address the perceived failure of co-referring names to freely substitute in belief contexts. Chapter 4 explains how the Fregean problem of co-naming substitution implicitly endorses the thesis of "phonemic reductionism," and why Quine would explicitly endorse it.

Chapter 5 traces the repercussions a semantically irreducible account of phonological equivalence has for Quine's attack on analytic equivalence. Chapter 6 details the positivist linguistic history behind various "neo-Bloomfieldian" attempts to reduce the phoneme. Chapter 7 takes a much closer look at these attempts to reduce the phoneme, guided in particular by Quine's own proposed reductions, including a behavioral reduction, similar to those favored by Zellig Harris (1951) and interestingly, even a young Noam Chomsky (1957).

Chapters 8 and 9 capitalize on the failures of phonemic reductionism by exploring a recent issue in developmental psycholinguistics, but also a traditional concern in the philosophy of language. Chapter 8 relates the failure of phonemic reductionism to developmental infant speech perception by detailing an experiment capable of confirming the hypothesis that infants actually lean on semantic information external to the speech stream during the process of phonological development. Chapter 9 relates Wilfrid Sellars' deployment of "star" and "dot" quotation to the standard "bracket" and "slash bar" notation (introduced in chapter 2) and used by linguists to distinguish "narrow" phonetic transcriptions of speech from "wide" phonemic transcriptions of speech. Sellars' use of "star" and "dot" quotation recapitulates the hierarchal relationship between phonetic brackets and phonemic slash bars at another level: phonemic slash bars represent categories of phonetically distinct but phonologically equivalent tokens; dot

quotes represent categories of phonemically distinct but, according to Sellars, "functionally" equivalent types. The structural parallels between phonemic typing and Sellarsian "dot quotation" are exploited to argue that not only should synonyms be regarded as variants of a single Sellarsian "dot quote"-type, but that co-referring names should also be viewed as "functional" variants of a single "dot quote"-type.

In chapter 10, the development of speech categories and the development of object categories are cyclically integrated: semantic information in the form of word meanings is used to help guide phonological classification, but linguistic information in the form word labels can also be used to refine object and property classification. This cyclical dynamic is then applied to Matesian problems of synonymous substitution and Fregean problems issuing from co-referring names.

In particular, this chapter defends the normative significance of asserting that two phonemically distinct but co-referring names are—regardless of what some may take themselves to believe—substitutionally equivalent. One's personal incredulity regarding the substitutivity of 'Cicero' and 'Tully' (like that of 'attorney' and 'lawyer') is no more relevant to their linguistic status as substitutionally equivalent than one's belief that Cicero is not the same man as Tully, or belief that bats are birds or dolphins fish, is relevant to the proper categorization of these objects. Psychologism with respect to the substitutional status of words (whether co-referring names or synonyms) in philosophy of language is no more *prima facie* acceptable than psychologism is with respect to the proper categorization of the non-linguistic world of objects. Pulling the work of Putnam, Burge, Kripke and some of Ruth Barcan Marcus together, the social pressures that encourage us to reconsider the meaning of our own words should also encourage us to

reconsider which words are substitutionally equivalent, even if, individualistically construed, we do not take ourselves to personally believe it. As a criteria of linguistic competence, language-specific phonological TK-substitutions are non-negotiable. As a criteria of linguistic competence, synonymous TP-substitutions are also non-negotiable. Armed with these two linguistic requirements, the substitutional status of co-referring names can be re-approached, as names are arguably also subject to normative constraints governing proper usage which we allow to over-ride whatever personal beliefs we may have thought we had about the substitutivity of co-referring names. Moreover, the semantic basis of phonology allows substitutions to be linguistically enforced even though they are dependent upon co-naming reference (vis-a-vis Frege) or co-meaning predication (vis-a-vis Quine).

In order to better appreciate the semantic dependence of basic phonological equivalence, it is helpful to introduce some of the basic terminology and notation used by linguists to explain how and why semantic content is implicated in the basic construction of language-specific phonological categories. With a better understanding of phonological substitutions, Fregean claims regarding the apparent substitutional failure of co-referring names in belief contexts and Quine's antipathy toward synonymous substitutions can be viewed from a different perspective. They can be considered from a "phono-logical" point of view.

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[1] In "Abstract Entities," Sellars (1967) explains that "Expressions in this rarified sense I have called—borrowing Peirce's term but putting it to a different, if related, use—linguistic types. Thus •red• is a type which is shared by the English word 'red,' the German word 'rot,' and the French word 'rouge'" (229). This atypical use "type" to subsume *phonemically* distinct word-types (e.g., 'rot', 'rouge', and 'red' or even 'father' and 'male sibling', see Sellars 1964a) indicates Sellars' desire to explicitly recognize an abstract hierarchy of linguistic types that transcends the phonemic level. For a justification of this use of the term "type" and his use of "token-class" Sellars (1950a) refers the reader to Sellars (1950b).

[2] Sellars does not apply his notation of dot quotation to co-referring names. Sellars uses dot quotation to reflect a "functional" equivalence only among synonyms and translations, not co-referring names. Presumably, co-referring names need not be "functionally" equivalent. Yet I do not see how this restriction of dot quotation to synonyms (and translations) is defensible. Insofar as two names are co-referential, then they do have the same "functional role"—even if this is not apparent from the behavior of some misinformed users. Just because there are some users who might not correctly apply 'Cicero' to everything they apply 'Tully' to does not imply that the words are not "functionally equivalent" anymore than someone who happens not use 'physician' and 'doctor' interchangeably refutes the claim that 'physician' and 'doctor' are functionally equivalent. Perhaps Sellars' use of "functional" is to be interpreted as strictly descriptive rather than prescriptive. But then it is no longer clear how Sellars can claim that 'father' and 'male parent' are "functionally" equivalent given that it is at least possible—according to Mates (1950)—that someone might not treat them as equivalent. Following Church (1954) and Kripke (1979) such synonymous doubt is dismissed as some sort of linguistic or semantic confusion or dealt with meta-linguistically. Why co-referring names cannot also be considered to be "functionally" equivalence despite co-referring doubt is taken up in chapters 9 and 10.

[3] "The older I get," says Fodor (1990) in the introduction to "Substitution Arguments and the Individuation of Beliefs," "the more I am inclined to think that there is nothing at all to meaning except denotation; for example, that there is nothing to the meaning of a name except its bearer and nothing to the meaning of a predicate except the property that it expresses it" (161) and proceeds to run down the usual reason:

> So tell me again: why does there have to be sense as well as denotation? What's wrong with the idea that denotation is all that there is to meaning? Here's what supposed to be wrong. The expressions 'Jocasta' and 'Oedipus' Mother' are co-referential *and must therefore be synonymous if denotation is all that there is to meaning.* But it's true that Oedipus believed that Jocasta was eligible and it's false that Oedipus believed that Oedipus' Mother' was eligible. So the expressions 'Jocasta' and 'Oedipus Mother' are not freely substitutable salva veritate. So they are not synonyms. So denotation can't be all there is to meaning. I'll call this kind of argument a 'substitution argument' (and I'll call the implied test for content identity the 'substitution test'). I think that substitution arguments are—and have been since Frege—a lot of what's behind the idea that there must be something more to meaning than denotation. But the older I get, the more I wonder whether substitution arguments are any damned good. I therefore propose to have a good look at substitution arguments. Starting now. On the face of it, substitution salva veritate in belief contexts doesn't look to be a test for identity of content. What it looks to be is a test for identity of belief state. . . . But it's one thing to admit that believing that J is eligible is a different state than believing that O's M to be eligible; it would seem to be quite another thing to admit that 'J' and 'O's M' are nonsynonymous. And it is, decisively, the latter conclusion that we need to be able to draw if we're to infer from the facts about Oedipus that there is more to meaning than denotation. (163)

[4] Like synonyms, 'cordate' and 'renate' are co-applying predicates in the sense that every cordate is a renate (and vice versa). However, these predicates are not co-applying in the sense that the property of having a heart is not the same property as having a kidney. In order to preserve a distinction, then, between synonymously co-applying predicates and contingently co-applying predicates, an intensional distinction between properties which are necessarily co-extensive and properties which are contingently co-extensive is required.

But this distinction between extension and intension has implications for characterizing the relationship between co-referring but sense distinct names. If predicates can be co-extensive but nevertheless still differ in meaning, why can't co-referring names be co-extensive but also differ in Fregean intension, that is sense? As Kripke has observed, this depends upon whether it is, in some way, coincidental that the object referred to by the name 'Tully' and the object referred to by the name 'Cicero' happen to be one and the same object. Kripke argues that this is not the case. With the apparatus of "rigid designation," Kripke (1980) argues that it is not contingent—given that 'Cicero' refers to Cicero and

'Tully' to Tully, and Cicero is Tully—that 'Cicero' and 'Tully' refer to the same thing. Unlike synonymous predicates and co-referring names, it is contingent that the terms 'renate' and 'cordate' are co-extensive. In this way, non-synonymous but co- extensive predicates like 'renate' and 'cordate' are analogous to co-referring definite descriptions. There is no "linguistic requirement" to equate 'renate' and 'cordate' anymore than there is a "linguistic requirement" to equate two co-referring definite descriptions. Linguistic competence does not require that Cicero be attributed the property of being a Roman orator, even if he is a Roman orator. Nor is there any "linguistic requirement" to apply definite descriptions, like the most famous Roman orator (see also note 15). There is no linguistic requirement to believe that Cicero is the most famous Roman orator, even if he is the most famous orator. There is, in general, no linguistic requirement to believe anything about Cicero, save perhaps for Kripke-like essential attributes, like being the genetic product of a particular act of conception.

[5] Note that while Chomsky's example indicates that syntactic wellformedness can be recognized independently of semantic intelligibility, this does not imply there is developmental independence—as opposed to the fully developed psycholinguistic processing independence—of syntactic categorization and semantic understanding. Nor, for that matter, does Carnap's (1937, 2) own prescient example of the pseudo-sentence 'Pirots karulize elatically' (indicative of the dual "autonomy" of both syntactic and phonological structure from semantics), succeed in establishing the developmental autonomy of either phonology or syntax from semantics.

[6] It is not altogether clear what the origin of this phrase is; it may be of my own making. Though American linguists were concerned with issues related to the basis of phonological categorization throughout the "neo-Bloomfieldian" structuralist period, neither the specific goal of providing a non-semantic definition of the phoneme or its failure, has been labeled as such. (See Koerner 1978, however, for a discussion of the motivations and personal subtleties behind the "neo-Bloomfieldian" movement most associated with such "reductionist" concerns.) Only two linguists, ironically both non-American, have devoted full-length, historically informed treatments of the relationship between phonology and semantics generally and discuss some of the issues related to "phonemic reductionism." The first is Roy Harris's (1973) *Synonymy and Linguistic Analysis* (the publication of his Ph.D. thesis at the University of London) where the issue is discussed "in the context of the debate between critics and defenders of what may conveniently be called 'semantically-based phonology'" defined as "the view that the basis for phonological classification of the sounds of a language is the extent to which meanings of words, phrases or sentences in the language remain invariant under sound substitution, phonologically distinctive and non-distinctive sounds being distinguished in terms of the possibilities of interchange without change of meaning" (21-22). Continuing, Harris explains that "It is characteristic of semantically-based phonology to define phonological units, such as the phoneme, in such a way as to involve an appeal to meanings, whereas it is characteristic of nonsemantically based phonology to deny that appeal to meanings enters into such definitions" (21-22).

The second is by a Swedish linguist, Solve Ohlander (1976), entitled *Phonology, Meaning, Morphology: On the role of semantic and morphological criteria in phonological analysis.* While Roy Harris addresses the relationship between phonology and semantics only in connection with a more general discussion of the role of synonymy within natural language, Ohlander's is a sustained exposition of the necessity of employing semantic criteria in the identification of basic phonological (and morphemic) categories. Also relevant is Paul Postal's (1968) monograph. While not defending or attacking the proposal that phonemic equivalence can be defined without the assistance of any semantics, Postal does offer a penetrating account of how a pre-occupation with this issue informed, or as he would have it, distorted "Bloomfieldian" linguistics.

Chapter 2
The Semantic Basis of Phonological Equivalence

Phonology is a semantically conditioned science.
S. Ullmann (1957, 32)

Natural language communication depends upon treating different linguistic specimen as the same. What linguists find so interesting and philosophers perhaps so intriguing is a fact of everyday life. So while it is clear that the typing of distinct speech-tokens into substitutionally equivalent word-type speech categories is a challenge routinely solved by infants and effortlessly implemented in everyday conversation, the basis of these substitutional habits is, or at least was, much more controversial. But as part of their efforts to explain what principles might govern this ubiquitous practice, linguists standardly recognize a distinction between linguistic tokens and linguistic types.

Phonetic Tokens and Phonemic Types

Linguistic types are said to be realized by linguistic tokens. In this way, any number of different linguistic tokens may instantiate or exemplify one and the same linguistic type. Linguists typically apply this token/type distinction to classifying what are known as phonetically distinct variants into phonologically or phonemically equivalent categories. Phonetic variation within one and the same phonological type is referred to as "allophonic" variation. All natural languages exhibit the phenomenon of allophonic variation, as each language has its own distinctive way of grouping phonetically distinct word-tokens into phonemically equivalent word-types. (Note for philosophers: This use of "token" to refer to allophonic variants—rather than the more restrictive philosophical use of "token" to refer to only spatially and temporally unique particulars—is standard

linguistic practice. So even though individual allophones are themselves categories of even more basic acoustic stimuli—ultimately unique "particulars"—the term "token" will be used to refer to phonetic categories. In this way, and following standard linguistic practice, phonological types are language-specific categories of phonetic tokens, even though phonetic tokens are, of course, themselves groupings of even more basic acoustic stimuli.[7])

In English, for instance, the way the sound 't' is articulated in the word 'top' is allophonically distinct from the way the same sound is produced in the word 'stop'. In (syllable) initial position, the phoneme 't' is always phonetically realized with an additional puff of air known as "aspiration." By contrast, in medial positions within a consonantal cluster like 'st', the phoneme 't' is always phonetically realized without aspiration. When the appearance of distinct allophones is governed by language-specific rules so that their distribution is *mutually exclusive* (wherever one variant can appear, the other cannot, and vice versa), the allophones are said to be in "complementary distribution." [8]

Sorting through patterns of complementary distribution is a powerful algorithm for resolving phones into language-specific phonemic categories for both linguists (Z. Harris 1954, Bloch 1948 and Bloch 1953) and infants alike (Peperkamp & Dupoux 2002), but it is also known to insufficient—even in theory (see Bloomfield 1933, Pike 1947b, Pike 1952a, and Pike 1958). In some positions—in final position as in the word 'mat' for instance—the phoneme 't' may be equally produced with or without aspiration with native speakers more or less randomly producing either phonetic variant. Indeed in English, this "free" allophonic aspirated/non-aspirated variation occurs for all voiceless

stop consonants, i.e., 'p,' 't,' and 'k,' in (syllable) final position. In these types of cases, the allophones are said to be in "free" as opposed to "complementary" distribution. So while allophony is jointly constrained by both language-general considerations of natural phonetic similarity (phones are not randomly equated) and language-specific rules of "complementary distribution" (distributional considerations play an important role), even the combination of these two sources of information is, in an interesting way, incomplete. The phonemic status of "free" variantion is underdetermined by even the joint application of language-general and language-specific phonetic data. Another source of information is necessary to account for the specific phonological contours of natural language. But this additional source of information is neither language-general nor even language-specific as it is external to the language itself considered as a pattern of phonetic data. Instead, this extra information concerns the semantic content the phonetic data is used to convey. It is the semantic interpretation of the phonetic data—what is semantically imposed upon the phonetic data—which effectively closes the gap between phonetics and phonology. (See below for how this actually works.)

So while invocations of abstract objects, including phonemic types, may raise the philosophical ire of some, there can be no doubt that various phones group into language-specific phonemic categories. But it is not necessary to attribute an existence to these types independent of the substitutional status of their token realizations. As suggested earlier in chapter 1, talk of linguistic types is short-hand codifying the observation that some tokens—ultimately unique particulars in space and time—are phonemically, and hence substitutionally equivalent with other tokens. The interesting philosophical issue concerns—and a central, albeit now defunct, debate in phonology concerned—not so

much the metaphysics of abstract linguistic types, but the *basis* by which phonetic tokens are categorized as phonemically equivalent: whether such linguistic categories can be constructed exclusively on the basis of perceptual information *intrinsic* to the language, or if semantic information *external* to the speech stream is also required.

Phonetic Brackets and Phonemic Slash Bars

Linguists also standardly deploy specialized notational devices by which to denote abstract phonemes on the one hand, and their token allophonic variants on the other. When referring to the singular sound that, for example, both aspirated 't' and non-aspirated 't' are token exemplars of, linguists surround the visual symbol representing this abstract t-sound with phonemic slash bars. In this way, the *phoneme* 't' is represented as /t/. By contrast, when referring to the various allophones a phoneme has, phonetic brackets are used. In this way, when the abstract phoneme /t/ is phonetically realized without aspiration, it is uniquely referred to as [t]. And when the abstract phoneme /t/ is phonetically realized with aspiration, it is uniquely referred to as [t^h]. (A superscript $^{'h'}$ phonetically denotes the presence of aspiration. This is, incidentally, also the full extent of phonetic symbolization used throughout the dissertation.)

Yet the phonological terminology and notation of phonetic brackets and phonemic slash bars linguist use to describe speech can be put to use to clarify an analogous distinction in the orthographic representation of the written word. By adopting the linguistic terminology for distinguishing between phonetic tokens and phonemic types, the visual word-tokens 'T' and '*t*', for instance, can be considered the orthographic analog to the natural language variation found at the phonetic level of speech. So just as

non-aspirated [t] and aspirated [t^h] are phonetic tokens of the phonemic type /t/, the inscriptional variation realized by italicization or capitalization can be notated by using brackets to individually denote italicized [*t*] and capitalized [T]. In this way, both [*t*] and [T] would instantiate the same orthographic type /t/. Neither token italicization nor token capitalization are linguistically contrastive in English orthography anymore than aspiration is linguistically contrastive in English phonology.

In fact, this move to co-opt the terminology and notation of linguists can be made complete by extending the linguistic notation of phonetic brackets and phonemic slash bars to the inscriptional variation found within a single orthographic word-type. Rather than adopt a new notation for dealing with the inscriptional token variation found in the written production of language, the notation of phonetic brackets and phonemic slash bars can be used to distinguish inscriptional word-tokens from orthographic word-types. Just as [mat^h] aspirated and its non-aspirated allophonic counterpart [mat], for example, are both phonemically identified as /mat/, [*Cicero*] italicized and [CICERO] capitalized would be "allographic" token variants of a single word-type, arbitrarily identified with slash bars as /Cicero/. (When identifying phonemic word-types, linguists generally choose the most frequent token variant to represent the standard or norm and surround it with phonemic slash bars. The choice of /Cicero/ to represent the word-type instantiated by both [Cicero] and [CICERO] is similarly decided, though in this allographic case, either word-token could be used to indicate the slash bar word-type. In addition, this practice of using brackets to identify linguistic tokens and slash bars to identify linguistic types will be adopted through out the rest of the dissertation: brackets refer to inscriptional/phonetic variants while slash bars refer to orthographic/phonemic types.

For this reason, the imprecise use of single quote marks will be discontinued until the reintroduction of the notation of brackets and slashes in connection with a more complete discussion of these notational devices, and in particular, their relationship to Wilfrid Sellars' parallel notation of "star" and "dot" quotation in chapter 9.)

Putting these notational devices to work, [*Cicero*] and [CICERO] would be "phonemically"—that is orthographically, and hence substutionally equivalent token exemplars of one and the same word-type /Cicero/. In this way, the notation of phonetic brackets and phonemic slash bars allows for the concise representation of the kinds of linguistic variation found in both the visual and oral production of language. Leaving such linguistic symbolization aside, token brackets and type slash bars serve to codify what native speakers already know: neither allophonic aspiration nor "allographic" italicization or capitalization affects the content of English sentences.

Free Allophonic Substitution

More specifically, word-token allophonic variation does not affect any change in semantic reference, semantic meaning, or truth. Whatever [mat^{h}] might refer to English, [mat] is co-referring. And whatever [mat^{h}] might mean in English, [mat] is co-meaning—even if both [mat^{h}] and [mat] ambiguously refer to or mean multiple things.[9] Similarly, whether one pronounces the sentence-type /the cat is on the mat/ in English with final stop consonant aspiration or without final stop consonant aspiration, either both are true or both are false. The substitution of allophonically equivalent word-tokens preserves sentential truth. Indeed, one may even indiscriminately *quote* someone as having said the sentence-type /the cat is on the mat/ in English by reproducing either free

allophonic word-token variant ([mat^{h}] or [mat]), as English phonology does not recognize any such distinction. (The standard for direct quotation does not normally require speech reports to be phonetically, no less imperceptibly, identical to the original, though some forms of quotation can be required to transmit with such artificial fidelity.)

Moreover, anyone who assents to the English sentence-type /the cat is on the mat/ tokened with final aspiration must—on pain of direct contradiction—also assent to /the cat is on the mat/ tokened without aspiration. They are, after all, the same (type of) sentence. This across-the-board truth-preserving phonological parity stands in sharp contrast to the phonemically distinct production, say, /the cat is on the map/ and its truth conditions. This is because the sounds [p] and [t] are phonemically distinctive in English. So whereas substitutions between [map] and [mat] in English will typically affect the truth of what was said—[map] and [mat] are said to represent a "minimal pair" (see note 8) in English—substitutions between [mat^{h}] and [mat] have no such semantic force. (Substitutions between phonemically distinct but synonymous word-types or co-referring names are discussed below.) In fact, all of this is just another way to make sense of the claim that word-types are phonologically permissible strings of phonemes and sentence-types are syntactically permissible strings of word-types: substitutions among phonemically distinct phones give rise to different word-types, and substitutions among phonemically distinct word-types give rise to different sentence-types. In this way, both sentence-types and word-types are premised upon a phonemic classification of phones.

Yet the only way to categorize "free" allophones—unlike allophones in "complementary distribution"—as phonemically equivalent in the first place is to rely on the common semantic interpretation imposed on phonetically distinct but free allophonic

word-tokens. Recall that some allophones alternate freely. One and the same word-type may be allophonically realized one way, or equally in some other way—just like [mat^{h}] and [mat] in the example above. But because allophones in free variation (unlike allophones in complementary distribution) can replace each other in exactly the same phonotactic position (e.g., both [t] and [t^{h}] can finish a word starting with [ma]), allophones in free variation end up mimicking the distributional structure of semantically contrastive phonemes—as if they were "minimal pairs." After all, though English phonology happens not to recognize aspiration as a phonemically distinctive feature capable of changing one word into another, other languages like Hindi and Thai do. (In Hindi, the word-token [p^{h}al] with aspiration refers to the edge of a knife, while without aspiration [pal] refers to taking care of something, see Akmajian, Demers and Harnish (1984). In Thai, [p^{h}aa] with aspiration refers to silk cloth and [paa] without aspiration refers to an aunt, see Aranoff & Rees-Miller (2001).) Yet seemingly trivial facts like this are not recoverable exclusively from an analysis of the intrinsic phonetic and/or distributional properties of the languages themselves. And this is precisely why an appeal to interpretive semantics is required.

Generally speaking (the phonetic details are provided in chapter 7), the problem with attempts to define phonological categories independent of semantics is that that free allophonic variation (e.g., [mat^{h}] and [mat]) cannot be distinguished from "minimal pair" phonemic alternation (e.g., [mat^{h}] and [map]). This is because the distribution of free allophones mimics the distributional pattern of semantically contrastive minimal pair phonemes. As Henry Gleason (1961) explained in a popular "Bloomfieldian" textbook of the time:

Free variation presents somewhat different problems. When two sounds are in free variation, they can obviously be found in similar environments—and, if the data is adequate, in identical environments. That is, they will occur in what might at first sight be taken for minimal pairs. These pairs are the same morpheme or sequence of morphemes occurring sometimes with one variant and at other times with another variant of the phoneme under consideration. They are not properly minimal pairs, because by definition minimal pairs must differ in both content and expression. To put it another way, given two utterances which are alike except in one feature, this may constitute evidence that the two sounds are phonemically distinct, or that they are variants of one phoneme. Which conclusion is to be drawn depends on whether or not the two utterances differ in content, and if they do, whether or not the difference of content is consistently correlated with the difference in expression. If there is no correlation of difference in content and expression, then we have free variation. (281)

This is why distinguishing free allophonic variation from minimal pair phonemic alternation requires an appeal to the common semantics of the allophonic pair, e.g., [mat] versus [mat^{h}] (where both refer to something you wipe your feet on) versus the distinct semantics of a real minimal pair, e.g., [mat] and [map] (where the former refers to something you wipe your feet on and the latter refers to some sort of pictorial representation). Similar considerations hold for free allophonic variation in the pronunciation of predicates, e.g., [hot^{h}] versus [hot]. It is the common property that both [hot^{h}] and [hot] express which distinguishes both of them from the semantically contrastive [hog].

Note that the fact free allophonic variation is itself typically subject to positional constraints, i.e., initial, medial, or final, does not, however, affect either the linguistic or philosophical significance of free variation vis-à-vis reductive attempts to define phonemic equivalence without invoking semantic considerations. Free allophonic variation—whether selectively free or "fully" free—undermines non-semantic accounts of phonological equivalence, for even two independent phones like [t] and [p] (which conspire to create minimal pairs like /mat/ and /map/), are not themselves fully free.

There are surprisingly complex positional constraints even on full-blown phonemes. For example, /tree/ is a permissible sequence in English but /nree/ is not (and /bank/ is but /batk/ is not), even though /t/ and /n/ are independent phonemes. Evidence, then, that even the distribution of free allophones is subject to incomplete or selective distribution does not succeed in distinguishing free variation from minimal pair alternation, as phonemes are also subject to incomplete and selective distribution. (See Bloch 1948 and Bloch 1953 for attempts to tame these sorts of complications.)

In any event, Pike (1947a) describes how some languages have allophones which—unlike [t^h] and [t] actually are subject to "fully" free allophonic variation. According to Pike, "free variation is of two general types: non-contrastive fluctuation of segments occurs in any and every environment in which either submember occurs ["fully free"]; or the variation is found only in certain restricted environments ["selectively free"]" (122), reporting that the allophonic variation of [n] and [ŋ] is fully free in a dialect of Kalaba, a West African language. (See also note 75.)

Homonyms and Synonyms

There are, however, two other and potentially much more damaging complications confronting the thesis that semantic content is integral to the individuation of phonemic types. If semantic content is a critical determinant for phonemic equivalence among free allophonic word-token variants, what about tokens of synonymous words and tokens of co-referring names? Even assuming that co-referring names have the same content and synonymous words express the same property, neither tokens of /Cicero/ and /Tully, nor tokens of /attorney/ and /lawyer/, are themselves phonemically equivalent. And what

about ambiguous homonyms? Homophones like /bat/ (the flying mammals) and /bat/ (the sports equipment)—though phonemically equivalent—are clearly not semantically equivalent.

The relation between semantics and phonology, then, appears to break down in both directions: word-type phonemic equivalence does not guarantee identity of semantic content and semantic content does not guarantee word-type phonemic equivalence. Semantics would appear to be neither sufficient (given synonyms and co-referring names), nor necessary (given homonyms) for phonological equivalence. Is this not a direct refutation—as Chomsky (1957) originally argued in *Syntactic Structures*—of the claim that semantics plays a critical role in determining whether two phonetically distinct word-tokens are phonemically equivalent? No. To see why, first consider homophones.

The fact that phonemic equivalence does not guarantee non-ambiguous semantic content does not contradict the claim that semantic content determines whether two phonetically distinct word-tokens are phonemically equivalent, as the observation that homophones exist is not even relevant to the claim that semantic considerations are required to resolve phonetically distinct free allophonic variants into language-specific phonemic categories. The fact that *one* phonemic word-type, say /bat/, may be semantically ambiguous so as to refer to both flying mammals and sports equipment is no threat to the claim that *two phonetically distinct word-tokens* are categorized as phonemically equivalent in virtue of their common semantics.

In fact, a thesis which asserts that two phonetically distinct word-tokens are phonemically equivalent on the basis of their common semantics is not committed in any way to the thesis that all phonemically equivalent word-types are semantically equivalent.

The direction of causality, as it were, is from semantics to phonemic equivalence, not from phonemic equivalence to semantics. Whether or not semantics is sufficient for establishing phonemic equivalence (and hence the problem of co-referring names and synonyms, see immediately below) is entirely independent of whether or not phonemic equivalence is sufficient for either co-reference or co-meaning. The contention that two word-tokens will be phonemically equivalent if they have the same semantics must address the problem of co-referring names and synonyms. But it does not have to deal with homophones, as the claim that two phonetically distinct word-tokens are phonemically equivalent if they have the same semantics does not exclude the possibility of there being phonemically equivalent but ambiguous homophones with distinct semantic content.

Given that homophonous words are not only phonemically equivalent, but also *phonetically equivalent* (see note 9), a claim concerning how two phonetically *distinct* word-tokens become phonemically equivalent does not entail any claims regarding how *one* phonemic word-type may be semantically interpreted, even if various distinct word-tokens cannot be categorized as phonemically equivalent in the first place unless semantic considerations are allowed to play the role they do in grouping distinct word-tokens into phonemically equivalent word-types. The claim, then, that the phonemic status of free allophones is determined by semantics does not entail the claim that phonemic equivalence determines semantic content. And whether the former claim is true or false is not going to be decided on the basis of the latter being true or, as is the case, false. Neither the claims of "phonemic reductionism" nor its denial—the claim that semantic information is required in order to categorize phones into phonemes, and hence

word-tokens into word-types—are affected by the existence of homophones. Just because phonology does not uniquely determine semantics, just because the words /bat/ and /bat/ are phonemically identical but semantically divergent, does not falsify a claim regarding how semantics may influence phonological categorization. But synonyms and co-referring names do present a problem.

Unlike homophony, the claim that two word-tokens are phonemically equivalent in virtue of semantic considerations does present an immediate problem. Synonyms express the same property, and co-referring names refer to the same object. But neither synonyms nor co-referring names are phonemically equivalent. Indeed there would seem to be the real danger of radically over-shooting phonemic equivalence: If tokens of synonymous predicates or tokens of co-referring names are classified as the phonemically equivalent merely on the basis of their common content, then tokens as phonemically disparate as [attorney]/[lawyer] (and [Cicero]/[Tully]) would count as tokens of the same word-type.

There are two responses to this "over-shooting" problem precipitated by co-meaning synonyms and co-referring names. The first is a technical phonological fix; the second an opportunity to express the philosophical conviction that, in an important way, synonyms and co-referring names really are just notational variants of each other. Not phonemically equivalent variants, but for all that, still strictly equivalent—even within belief contexts. The empirical observation that phonological equivalence is semantically conditioned provides the philosophical coverage by which to claim that synonyms and co-referring names are—notwithstanding their phonemic inequality—substitutionally

equivalent even within belief contexts, as if they were mere variants of one and the same abstract "dot quote" linguistic type.[10]

But this philosophical move presupposes that there is some way to get synonyms and co-referring names to be phonemically distinctive in the first place—that is word-type distinct—without giving up on the thesis that it is semantics which critically determines phonemic equivalence in the first place. And the linguistic answer to this phonological problem is nicely summed up by the phrase "Once a phoneme, always a phoneme" (Newmeyer 1980, 10).

Insofar as any phonetic difference is phonemically distinctive, at least once, any where in the language, the phonetic difference is uniformly elevated to the level of a phonemic distinction throughout the language. That is to say, insofar as a particular phonetic difference distinguishes at least one word-type from another word-type any where in the language, the phonetic difference is a phonemic distinction even if there exist cases (i.e., synonyms and co-referring names) where such phonetic differences do not precipitate any difference in semantic value. The reason why I am not (nor are infants) "phonological anarchists" (as Macnamara 1982, see chapter 8, puts it) for trying to phonemically equate, say [small] with [little] on the basis of their synonymy (or [Tully] with [Cicero] in virtue of their co-reference) is that there is no reason to phonologically equate them. For there need only be *one* minimal pair drawn from any where in the language distinguishing any of the sounds in a synonymous or co-naming pair (say the [t] in [attorney] and the [l] in [lawyer] as in [tip] and [lip]), to keep synonyms and co-referring names phonemically distinct despite their common semantics. And since there must always be at least one case where the relevant phonetic difference

does affect a semantic change (otherwise the phonetic difference would not be phonemic in the first place), synonyms and co-referring names do not undermine a semantically guided algorithm for establishing basic phonological equivalence—no less an account of basic phonological structure which exploits semantic considerations to keep independent phonemes apart in addition to keeping allophones together (see Roy Harris 1973 and Ohlander 1976). There are innumerable other opportunities for the phones in co-naming or synonymous pairs to prove themselves to be phonemic. (This is also why cases like the phonemic alternation between the long and short vowel pronunciations of /economic/ are not problematic for traditional phonemicists: the minimal pair semantic difference between /bed/ and /bead/ is sufficient to demonstrate that such alternations are phonemic even though in some cases this phonemic difference does not indicate any semantic difference.)

So while it is true that two word-tokens may be semantically equivalent—either in virtue meaning (e.g., tokens of /attorney/ and /lawyer/) or in virtue of reference (e.g., tokens of /Cicero/ and /Tully/)—without thereby instantiating the same phonological word-type, this does not falsify the claim that basic phonemic equivalence is guided by semantic considerations. And this is because the phonemic status of [attorney]/[lawyer] and [Cicero]/[Tully]—unlike [mat]/[mat^{h}]—*can* be decided merely on the basis of their phonetic form. Unlike [mat] and [mat^{h}], the phonemic status of tokens of /attorney/ and /lawyer/ can be determined merely on the basis of their phonetic differences, but that is because there are phonetic *differences*—not just a single phonetic difference. [attorney] and [lawyer] are not even close to being potential minimal pairs as they differ phonetically in more than one position. The reason why [attorney] and [lawyer] (and

[Cicero]/[Tully]) are not potential allophonic variants of each other is due to the fact that they are not potential "minimal pairs" either. But whereas the phonemic status of [attorney] and [lawyer] is phonetically over-determined (they would be phonemically distinct in any natural language), the phonemic status of [mat] and [mat^{h}] is phonetically under-determined.

The real test case, then, for the semantic basis of phonemic equivalence is to evaluate the status of name and predicate tokens which minimally differ only in one phonetic feature, say the names [Mat] and [Mat^{h}] or the predicates [hot] and [hot^{h}], by observing how these tokens are phonemically resolved when aspiration actually is used to semantically distinguish different objects and properties (as in Hindi) versus those linguistic environments where aspiration carries no such semantic force (as in English). When aspiration serves a semantic purpose, the tokens will be typed as phonemically distinct, as two different words. When aspiration serves no semantic purpose, the tokens will be typed as phonemically equivalent, as one and the same word. (See chapter 8 for an experimental paradigm.) But semantic considerations of this sort also allow us to distinguish between two ways of interpreting the semantic equivalence embedded within phonological equivalence. Does natural language phonology exploit interpretive equivalencies of semantic reference or does natural language phonology require, in addition to co-naming reference, equivalencies of Fregean co-sense?

Semantically Conditioned Phonology: Co-Naming Reference or Co-Naming Sense?

So while neither the existence of homonyms nor synonyms (or co-referring names) contradict the claim that semantic content is required in order to distinguish free

allophonic variation from minimal pair phonemic alternation, it is not clear whether this appeal to semantic content is referential or Fregean in character. After all, predicates are synonymous not because they refer to the same things, but because they refer to, or express the same property. And if the phonological individuation of predicates (e.g., /hot/) is dependent upon whether [hot] and [hot^{h}] express the same property, why shouldn't the phonological individuation of names and common nouns (e.g., /mat/) be dependent upon whether [mat] and [mat^{h}] co-express the same Fregean sense? Perhaps phonemic distinctions issue from perceived differences in Fregean sense rather than just differences in reference, while perceived equivalencies in sense, rather than reference, ground phonological equivalence. That is, why can't phonology track sameness and difference in sense as opposed to sameness and difference in reference?

To my knowledge, this is the first time a question like this has ever been asked. Such an issue never arose, to my knowledge, even during the heyday of phonemicist controversies surrounding the semantic basis of phonological categorization. The issue was one of pitting interpretive, semantic-based analyses against non-interpretive, non-semantic analyses, not reference-based against sense-based analyses. So while many linguists (not party to the excesses of "neo-Bloomfieldianism," see chapter 6) defined phonological equivalence in semantic terms, this semantic dependence was never explicated in a way that was sense-based. These linguists used the terms "semantics" and "meaning" to argue that phonemic equivalence could not simply be read from phonetic form, not that Fregean senses were needed. They only wished to stress that interpretive content—in addition to mere phonetic form—was required for phonemic classification. But they did not claim that meaning—in the sense of Fregean senses—was required.

When it was claimed that phonology is "semantically conditioned," they were claiming that perceptually accessible information in the form of psychoacoustic, phonetic, distributional, or, even behavioral data was insufficient to group word-tokens into phonemic word-types, not that Fregean senses capable of semantically distinguishing among co-referring names were necessary.

The appeal to semantics, then, was an appeal to semantics only in the sense that names refer to objects and predicates refer to, or express properties. What they meant by semantics—what they meant by "meaning"—was what philosophers call naïve content: properties for predicates and objects for names and common nouns. What exercised these linguists was the semantic equivalence among phonetically distinct tokens like [mat] and [mat^{h}] and the semantic difference (the "differential semantics" as it was called, see chapter 7) of phonetically distinct word-tokens like [mat] and [map]. The possibility that [mat] and [mat^{h}] might be referentially equivalent but sense distinct, or that [mat] and [mat^{h}] might somehow be both referentially and sense equivalent, did not engage them. Moreover, the arguments they constructed clearly indicate this. A typical example by Gleason (1961) was provided earlier. The allophonic variation cited by Bloomfield (1933) concerns the pronunciation of the word-type /man/ in contrast to /men/ (see note 30), and Hill (1967) cites the allophonic variation of the word-type /hat/ (see note 87).

In any event, there are both philosophical and empirical problems with this Fregean proposal. It's Achilles heel is that Fregean senses are fundamentally divisive. Fregean senses are very good for distinguishing semantic content, but they are not very good for doing the opposite. That is, Fregean senses serve to explain what co-referring

names need not have in common, but they do not readily explain what two co-referring names must have in common—independent of their common reference—to have the same "sense." But it is a specification of sense equivalence, not a mere differential in sense, which is required for phonological equivalence to be based upon considerations of co-sense as opposed to mere co-reference. In fact, if phonemic sameness and difference reflects only sameness and difference of sense rather than sameness and difference of reference, then phonological failures of TK-substitutivity are going to proliferate for the same reason that traditional failures of TP-substitutivity proliferate among co-referring names.

Recall that Fregean senses are designed to explain what even co-referring names do *not* have in common—why co-referring names do not substitute freely in epistemic contexts. Senses are semantically divisive. Applied to TK-substitutions, senses work to semantically distinguish phonologically equivalent word-tokens. But we want senses to enable phonological substitutions, not block phonological substitutions. For this reason, senses are ill-suited to explain what co-referring name-tokens must happen to have in common—independent of their common reference—in order to be phonologically equivalent. Fregean senses do not really help to grease phonological substitutions. Instead, Fregean senses would explain why even some phonologically equivalent word-tokens do *not* freely substitutable. But this is clearly not an explanation of phonological equivalence. It is closer to the denial of phonological equivalence. For while co-sense TK-substitutions might very well be typed as phonologically equivalent, co-referring but sense distinct word-tokens would be typed as phonologically distinct.

Worse, a sense-based explanation for basic phonological equivalence opens the door to a Mates-style counter-response whereby even a stipulated equivalence in "sense" is insufficient to guarantee phonological substitutions. Insofar as co-reference is insufficient to guarantee co-naming TP-substitutions as per Frege, and even synonymy is insufficient to guarantee synonymous TP-substitutions as per Mates, there exists the parallel threat that neither co-reference *nor* even Fregean co-sense would be sufficient to guarantee basic phonological equivalence. Following this Matesian line of reasoning, neither TP-substitutions nor TK-substitutions would survive substitutional doubt. With no possible standard for either TP- or TK-substitutions, the very possibility of phonological equivalence dissolves in the same way that Matesian arguments defeat any possible standard for TP-substitutions. "Phonological eliminativism" is the inevitable result of applying the structure of either Matesian or Fregean arguments to the basic phonological substitution of word-tokens, a result hinted at by Yagisawa (1984) and discussed more fully in chapter 4. Indeed, relying on the epistemic criteria of doubt to identify substitutional equivalence is an inevitably destructive force—an over-arching issue addressed more fully in chapter 10.

Senses may very well support some co-naming TK-substitutions (in the same way that /Cicero/ and /Tully/ might happen to express the same sense for some people), but senses will also inevitably block others—namely the merely reference-based ones. Unless, of course, sense and reference are tied together so that co-referring TK-substitutions are also co-sense TK-substitutions. But the whole idea of Fregean senses is to introduce a mechanism by which to *detach* sense from reference while still preserving a route back to reference. Senses are held to determine reference, but reference does

allow for the univocal recovery of meaning. There is a many to one relationship between sense and reference. Using senses, then, to individuate phonological types greatly increases the number of phonemic distinctions projected into the language.

Consider the following Fregean-like scenario. Imagine that two phonetically distinct word-tokens happen to be consistently associated with this object, but coordinated in such a way as to set up a relation between the tokening of one and the tokening of the other correlated with the different appearances of one and the same object. When entering the apartment, for example, a phonologically naïve person hears /Wipe your feet on the mat/ with aspiration and when entering school hears /Wipe your feet on the mat/ without final aspiration. This perceptive—arguably too perceptive—individual infers that the word-tokens [mat^{h}] and [mat] are *not* the same word at all: [mat^{h}] uniquely refers to apartment mats and [mat] uniquely refers to school mats. But even assuming there is no counter-evidence contra-indicating this conclusion, the reasoning itself is suspect.

The trouble with this sort of reasoning is that there is no end to hyper-individuating semantic content in this way. What if the mats are not located at different places, but happen to be different colors? The mats may even be located in the same place, with the same color, but differ slightly in shape. Even the same mat is viewed from one direction when leaving the house and another direction when entering the house. Different perspectives on one and the same object are a fact of life. It may even be a truism that the same object can never be re-perceived from exactly the same perspective. And while at least some of these perspectives will inevitably implicated in "fixing the reference" (Kripke 1980) of our terms (see also Marcus 1961), Fregean senses

are not going to provide a secure semantic basis for phonological substitutions anymore than Fregean senses secure a semantic basis for co-naming substitutions. Instead individuated content in this way provides the mechanism for blocking such substitutions.

Fortunately, different pronunciations of the same abstract word-type are not uniquely correlated with minimally distinct appearances of the same object. But even if they were, there are still two choices at least theoretically available. One the one hand, such cases may be interpreted as instances where two tokens instantiate two distinct word-types referring to (what is mistakenly taken to be) two distinct objects. On the other hand, such cases may be interpreted as instances where one and the same object is referred to by two allophonically related word-tokens of the same word-type. But Fregean arguments make it seem as if only the former option is available.

Fregean reasoning is premised upon the intuition that fully competent language users can be unaware (individualistically construed) that one object has been named twice. The ancients didn't realize (individualistically construed) that the names /Hesperus/ and /Phosphorus/ referred to one and the same planet. They did not realize that the name /Hesperus/ named Phosphorus (as well as Hesperus) in much the same way that, until recently, many of us did not realize, for example, that the term /mammal/ refers to both dolphins and bats (as well as cats and dogs). And the reason why Hesperus and Phosphorus were mistakenly considered to be different objects is that the way Hesperus appears in the sky is different from the way Phosphorus appears in the sky: one appears in the evening and the other in the morning. And the reason why dolphins were mistakenly not considered to be mammals is that they look and behave much more like fish than your typical land lumbering mammal. Similar considerations hold for

Superman/Clark Kent and Tully/Cicero. Different perspectives on one and the same object, and different appearances of one and the same object, occur all the time. But whether such a multitude of different perspectives—whether such "modes of presentation"—due to the rotation of the Earth, the superpower disguises of Clark Kent and all sorts of errors in re-identification should be built into the very semantics of our words is a question that I think at least phonology answers in the negative.

When confronted with two different word-tokens referring to what might appear to be two different things, one need not infer that the two tokens refer to two different things, and then introduce Fregean senses to explain this error in re-identification. One may equally move to infer that there may very well be just one (type of) word "directly" referring to one and the same multifaceted object. Rather than invoke senses to accommodate the above scenario, one may just as well move to allophonically ignore the phonetic difference between [mat^{h}] aspirated and [mat] non-aspirated and consider them to be token variants of one and the same type of word. Something like this arguably occurs in English (see chapter 8), and the failure of phonemic reductionism encourages opportunities for collapsing phonetic distinctions on the basis of non-Fregean semantic content.

As reviewed in chapter 8, language-specific phonological development consists in, among other things, the selective elevation of only some phonetic differences to the status of a phonemic distinction. The vast majority of phonemically viable phonetic distinctions available in natural language are not phonemic for any particular language. In this way, we—well, at any rate infants—are not so much challenged with the task of figuring out which phones are phonemically distinctive in their language, but are faced

with the reverse challenge of figuring out which phones are *not* phonemically distinctive. Our task is to reduce our accounting of the number of phonemic distinctions in our language. But senses are a mechanism for producing more phonemic distinctions than those allowed by reference alone. The semantic difference between [mat] and [map], for instance, is not merely a difference in sense. It is also a difference in reference. However, any semantic difference between between [mat^{h}] and [mat] can only be a difference in Fregean sense—at least with respect to English phonology. The claim, then, that a sense-based account of phonology can do everything a reference-based account of phonology can do is true. The problem is that sense-based accounts also do much more.

A sense-based account of phonological equivalence licenses many more possible phonemic distinctions in the language for the same reason that traditional Fregean sense-based accounts of the substitution of co-referring names license failures of co-naming TP-substitutions. In fact, what would merely be allophonic variation when considered from the perspective of reference, would be elevated to the status of a phonemic distinction insofar as each word-token variant could conceivably be assigned a distinct sense. And this is essentially due to the fact that a sense-based account of basic phonological equivalence individuates TK-substitutions in terms of doubt in the same way that Fregean reasoning individuates co-naming TP-substitutions in terms of doubt.

Interestingly, note that a similar but inverted dynamic obtains when predicate synonymy is rejected. For whereas Fregean senses are phonemically divisive, the Quinean dissolution of synonymy has the opposite effect of decreasing the number of phonemic distinctions. According to Quine, the synonymy of /attorney/ and /lawyer/ is not to be distinguished from the co-predication of /renate/ and /cordate/. Whatever the

predicate /attorney/ is true of, the predicate /lawyer/ is also true of. But all renates are also cordates, so everything which the predicate /renate/ is true of, the predicate /cordate/ is also true of. Without invoking the possibility that a creature with a heart could be a creature without a kidney, or an intensional distinction between what property a predicate expresses and what the predicate may happen to extensionally apply to, the distinction between synonymous co-predication and contingent co-predication dissolves. From Quine's perspective then, /renate/ and /cordate/ are just as "synonymous" as /attorney/ and /lawyer/ are since the extensional convergence of the former cannot be extensionally distinguished from the synonymy of the latter. The only kind of "synonymy" Quine is willing to allow for is co-extensive predication. And that is, of course, a rejection of synonymy. As far as Quine is concerned, there is nothing more to synonymy than mere co-extensive predication.

Applied to phonological categorization, Quine's reasoning encourages the phonologist to phonemically equate two phonetically distinct predicate-tokens—whether they express the same property or merely apply to the same objects. In this way, if say, [dap] and [daf] were—like tokens of /renate/ and /cordate/—predicate-tokens expressing different but co-extensive properties, [dap] and [daf] would be phonemically typed as the *same* word as the distinction between synonymous predication and co-extensive predication cannot be made on the basis of mere extensional application (nor on the basis of behavior conditioned only by "stimulus meaning," see chapter 7). On this analysis, tokens like [daf] and [dap] would *not* constitute a "minimal pair." Instead, [daf] and [dap] would be phonologically equivalent allophonic variants of each other. ([dap] and [daf] are chosen because in some languages, Hebrew, for instance [p] and [f] actually are

allophones of each other, as are [b] and [v].) Of course, phonemic categories are not drawn in isolation. There are innumerable other opportunities within a language like English for [b] and [p] to prove their phonemic distinctiveness. The extensional difference between [cap] and [cab] suffices. The point is that while Fregean senses philosophically license a sense-based *increase* in phonemic distinctions, Quinean meaning skepticism philosophically licenses an eliminativistic *decrease* in phonemically available distinctions.

The claim, then, that phonological typing is dependent upon semantic considerations is not undermined by ambiguous homonyms, co-meaning predicates or co-referring names. But while this semantic dependence respects the difference between synonymous predication and co-extensive predication, this semantic dependence is not dependent upon Fregean senses. Two word-tokens are categorized as the same phonemic word-type if and only if none of their internal phonetic differences occur so as to differentiate object reference or express different properties. And this is both a necessary and sufficient condition for phonemic word-types: two word-tokens are categorized as the same phonemic word-type if none of their internal phonetic differences ever serve to indicate a semantic difference in objects or properties, and no phonetic difference among word-tokens ever indicates a semantic difference in objects or properties if they are the same phonemic word-type.

In short, the phonological status of any phonetic difference—whether the phonetic difference is merely allophonic or phonemic—can be determined by a comparative search for semantically contrastive minimal pairs. When a semantically contrastive minimal pair, e.g., [mat] and [map], or [hot] and [hog], is found distinguishing [t] (and

[t^h]) from [p] or [g], the phones are phonemically distinctive and represented as /t/, /p/, and /g/. When no semantically contrastive minimal word pair is found distinguishing [t] from [t^h], [t] and [t^h] are not classified as phonemically distinctive but as mere allophonic variants of each other. Interpretive content—appeals to the objects named and the properties expressed—is an intuitive and efficient way to differentiate allophonic variation from phonemic contrast. This claim, however, is not very interesting and has never been disputed. It is the claim that this sort of semantic information is ultimately the only way to comprehensively sort phones into phonemes which ignites linguistic and philosophical discussion.

So while the basis of phonological equivalence was an issue for linguists to dispute (chapter 6), a debate for Quine to join (chapter 7), and a challenge infants must developmentally confront (chapter 8), everyday communication does not depend upon any such explicit knowledge. We just talk. This everyday communication does, however, require that we respect the substitution of free allophones. Problems with allophony do not inspire conversational confidence. Balking at the substitution of [mat] and [mat^h] in English is indicative of some sort of linguistic confusion or linguistic deviance. Doubting, for instance, the substitutional equivalence [mat] and [mat^h] is to forfeit one's basic English-speaking competence. Worse, actually denying the substitution of [mat] and [mat^h] amounts to nothing less than a direct contradiction in English.

Indeed, one of the most peculiar features of language-specific allophonic substitutions is that though they can sometimes be heard, they cannot genuinely be doubted without compromising one's linguistic competence. So while one may always

question word-token equivalencies in languages one does not speak fluently, one cannot question word-token equivalencies in languages one claims to already be competent in. Phonologists, for instance, routinely confront languages whose phonemic structure they are unfamiliar with. Phonologists, however, cannot question the phonemic equivalencies of their own native tongue without thereby undermining their own native language competence.

Yet this inability to genuinely doubt allophonic substitutions within a language one wishes to call one's own stands in sharp contrast to traditional Fregean reasoning regarding the substitution of co-referring names. Unlike doubt regarding the substitution of allophonic word-tokens, doubts regarding the substitution of co-referring names is not only permissible but philosophically revealing. In fact, the distinction between TP-substitutions and TK-substitutions appears to coincide with a distinction between which kinds of substitutions can be doubted with linguistic impunity and those which cannot be doubted with linguistic impunity. It is not obvious, however, why TP-substitutions and TK-substitutions should align, if they do align, themselves with respect to the possibility of doubt in this way. Perhaps only substitutions among distinct word-types can be doubted. And something like this is surely part of the answer. A more complete answer would also attempt to explain *why* only TP-substitutions, in contrast to TK-substitutions, can be doubted.

Challenged with such questions, we might eventually move to counter-intuitively conclude that TK-substituitions between word-tokens of the same word-type can, after all, be doubted. In fact, the injection of Fregean senses into natural language phonology tends to do just that. Or equally, we might move to counter-intuitively conclude that TP-

substitutions between co-referring names (and synonyms) cannot really be doubted. After all, only if TP-substitutions between co-referring names can be genuinely doubted (without contradiction or linguistic error) is there a reason to devise Fregean—or even anti-Fregean—solutions to account for these possibilities. But before the possibility of Fregean doubt can itself be doubted, a closer look at the belief-context status of phonological substitutions versus the belief-context status of TP-substitutions is required. In particular, a range of philosophical responses designed to either allow or disallow the possibility of co-naming Fregean doubt are reviewed. This review will in turn help reveal how the intuitive difference between the indubitability of phonologically based TK-substitutions and the dubitability of non-phonologically based TP-substitutions is itself premised upon considerations issuing from "phonemic reductionism."

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[7] Exactly how these more basic acoustic stimuli are grouped into phones is, strictly speaking, not the province of linguistics, nor even phonology, but the domain of articulatory and acoustic phonetics. In any event, no semantic information is required in order to categorize basic acoustic stimuli into (language-general) phonetic categories. Though individual phones are themselves categories of multi-dimensional, and often smoothly continuous psychoacoustic stimuli, they are "categorically perceived" (Repp 1984) as discontinuous phonetic categories, not only by human neonates (Jusczyk 1993), but apparently even by some non-primate mammals as well (Kuhl 1987). As such, there is no comparable problem of classifying acoustic variation at the level of (language-general) *phonetic* categorization. See Blumstein & Stevens (1981) and Stevens & Blumstein (1981) regarding how the language-universal pool of phonetic units selectively used by all languages can be more or less successfully described in either acoustic and/or articulatory terms.

[8] Phonetically distinct variants of the same phoneme are standardly referred to as "allophonic" variants, or "allophones" of the same phoneme. There is, however, an important distinction between allophones which are in "free variation" and allophones which are in "complementary distribution." Briefly, most allophonic variation is predictable on the basis of rule-governed (albeit language-specific) principles of mutually exclusive distribution which dictate which allophonic variant can appear under specific phonotactic conditions and which allophones are prohibited from appearing under these same phonotactic conditions. When allophonic variation is governed by phonotactic rules prohibiting allophones of the same phoneme to be used interchangeably, the allophones are said to be in "complementary distribution." "Free" allophones do not abide by the predictable rules of complementary distribution, however, because free allophones can replace each other freely in at least some phonotactic conditions. Semantic criteria are needed in order to distinguish between free allophonic variants and minimal pair words. In phonology, "minimal pairs" refer to words that differ by only one phoneme: [mat] and [man] are minimal pairs (and phonemically represented as /mat/ and /man/); [mat] and [mat^{h}] are free variants (and phonemically represented as /mat/).

[9] That is to say, even if a semantically ambiguous word-type is itself subject to free allophonic variation, as the word-type /bat/ is (it may be allophonically pronounced with or without final aspiration), this allophonic variation occurs regardless of semantic interpretation. Whether one is speaking about baseball bats or flying bats, the word-type /bat/ may be pronounced with or without aspiration. Homophones are defined as word-types with the same pronunciation, even if this same pronunciation allows for allophony, as the allophonic variation itself will be the same.

[10] Surely, as an exasperated MacDonald (1937) objected to a strikingly similar pattern of reasoning outlined by MacIver (1937a, 1937b, 1937c), "no ordinary person would think of saying that *quick* or *rapidly* are 'the same word'" (78). True enough. But by the same reasoning, no "ordinary person" would ever think of distinguishing among distinct allophonic variants either, pointing out that the [t^h] in /top/ is *not* the same as the [t] in /stop/. But if the /t/ in /top/ and the /t/ and /stop/ can, in fact, be different—notwithstanding ordinary English intuitions—then it is by no means clear that /quick/ and /rapidly/—notwithstanding ordinary English intuitions—could not also be the same. They will not, of course, be the same phonological word-type; let them be the same "dot quote"-type.

In any event, it is significant that Church (1954), relying on Langford's (1937) "translation test," makes precisely this same move, arguing that /fortnight/ and /fourteen days/ are the same, albeit with the convenience of a pre-established German (what Putnam 1954 calls a "neutral") translation. In this way, insofar as both /fortnight/ and /fourteen days/ receive one and the same German translation, Church (1954) argues for their strict synonymy (and the dispensability of one member of the pair) by pointing out that it is of no philosophical significance that some languages are internally redundant. It is just an accident of English that it is equipped with both /fortnight/ and /a period of fourteen days/. English suffers from what Church calls a "linguistic luxury." But German, for instance does not: both are uniformly translated as *'Zeitraum von vierzehn Tagen'*. And this is what "ordinary people" must do. In this way, doubt between /fortnight/ and /a period of fourteen days/ is simply not possible.

There is, in principle, no reason why this same pattern of reasoning cannot be applied—in addition to synonymous terms—to co-referring names. One need only imagine a language that has only *one* name for the planet Venus. In such cases, /Hesperus/ and /Phosphorus/ would have to be translated into the same word. In fact, Church (1989) returns to this issue, remarking "Languages are of course possible within which no two primitive constants denote the same thing. For given any primitive constant we may delete from the language all but one of the class of primitive constants that are concurrent with it" adding however, "Whether it is always effectively possible to cut down on the vocabulary of a given language so that no pair of concurrent but nonsynonymous primitive constants remains is an open question" (165).

FROM A PHONO-LOGICAL POINT OF VIEW

Chapter 3
Substitutivity within Belief Contexts

> Frege believed that any difference in the cognitive significance of two sentences must reflect an objective semantic difference. This view has in one form or another represented the received view in the philosophy of language since the appearance of 'On Sense and Reference'. But this, I want to argue, is a mistake—a mistake that has seriously retarded our understanding of the relation between though and language.
>
> W. Taschek (1987, 161)

Allophonic Substitutions within Belief Contexts

One of the most interesting features of language-specific word-token TK-substitutions is that they do not allow for doubt. In particular, assent, dissent, and epistemic contexts generally, are blind to allophonic variation. Consider again the English sentence-type /the cat is on the mat/ tokened with final aspiration and the same sentence-type tokened without final aspiration. One cannot agree to the former but disagree with the latter insofar as one wishes to still be speaking English. Or take the same sentence-type written in italics and the other written in all capitals. It is a contradiction in English to assent to one but not the other. A commitment to competently speaking, writing, and reading English requires that such token substitutions be treated univocally. Even transcriptions between the written and spoken word may be required. Literacy requires just such a competence.

Nor is it possible to report */S* believes the cat is on the mat/ (with final aspiration) but also report */S* does not believe the cat is on the mat/ (without final aspiration). Such allophonic equivocation again amounts to a contradiction. Language-specific allophonic variation seems to present cases where it really is not possible for doubt to occur. Reminiscent of Mates (1950) remarks, whoever believes the cat is on the mat, believes

that the cat is on the mat—regardless of how the words may be allophonically realized. And that's because such doubt doesn't make any sense, at least in English. English simply does not recognize such allophonic equivocation. (This point was inspired by Church (1954) who remarked, relying on the so-called Langford (1937) "translation test,"[11] that it is not possible for a German speaker to equivocate between saying or even believing an event lasts a fortnight and the same event lasts a period of fourteen days, as there is in German only one expression (*Zeitraum von vierzehn Tagen*) for both, see note 10.)

In fact, without resorting to the additional notational leverage afforded by phonetic brackets to isolate token distinct realizations of the same word, allophonic doubts cannot even be coherently formulated. (And to make a long dissertation story short, one cannot really formulate how distinct word-types like /Tully/ and /Cicero/ or /attorney/ and /lawyer/ can be the "same"—and hence substitutionally equivalent even in belief contexts—without in some way decreasing one's notational resources by relying on, for instance, the suppressive notation afforded by Sellars' "dot quotation." For while phonemic slash bars abstract away from phonetic differences, Sellarsian dot quotes abstract away from phonemic differences: phonemic "slash bar" notation secures the substitution of phonetically distinct word-tokens; "dot quote" notation secures the substitution of phonemically distinct word-types. Synonyms—and by extension, co-referring names (see note 2)—would themselves be distinct but "incidental" notational variants of one and the same abstract "dot quote" type. This notational symmetry is the topic of chapter 9.)

Similar, then, to the "allographic" variation between [Cicero] and [CICERO], allophonic variation appears to be a commonplace natural language example of semantically neutral truth-preserving substitutional equivalence. Some like it [hot] while others like it [hot^{h}].

Co-referring Names within Belief Contexts

Of course, it is not phonologists who typically speak in terms of "truth-preserving substitutional equivalence." It is philosophers who are charged with explaining what is going on when substitutions between co-referring names (or even synonyms) seemingly do not preserve truth by comparing someone's assent to /Cicero is speaking/ with their denial of /Tully is speaking/. So even though whenever Cicero is speaking, Tully is, of course, also speaking, it would seem that it might nevertheless be true to say of someone /*S* believes Cicero is speaking/ but false to claim that /*S* believes Tully is speaking/ (what Owens 1995 calls the "Fundamental Assumption" of Fregean theorizing). How can this be if in fact /Tully/ and /Cicero/ name the same person? This is Frege's puzzle.

It is important here to distinguish between two different claims. Nobody argues that /Cicero/ and /Tully/ cannot be freely substituted for each other simply because someone or another might mistakenly think they cannot, or should not, be substituted for each other. Mistaken beliefs and individual ignorance are supposedly not Frege's concern. Rather, it is the possibility of believing that Cicero is speaking while simultaneously not believing Tully is speaking. It is the difference between representing the Fregean argument for senses as simply premised upon an appeal to ignorance versus understanding the Fregean argument for senses as attempt to explain how it is even

possible for a seemingly rational agent to affirm that Cicero is speaking but deny Tully is speaking, given that /Tully/ is just another name for Cicero. Without this distinction, one might equally ask: how is it possible for Cicero to be speaking but for someone not to believe that Cicero is speaking? Obviously, this person is unaware—does not believe—Cicero is speaking. This person is apparently not, or not yet, epistemically positioned to appreciate Cicero's speaking. The problem arises when someone believes Cicero is speaking but does not also believe Tully is speaking. Obviously this person is unaware that /Tully/ and /Cicero/ name the same person. The question, however, is: how is it even possible not to be aware that /Tully/ and /Cicero/ name the same person, if indeed they are both names—just "Millian" name tags—for the same thing?

Fregean Senses, Propositional Guises and Hidden Indexicals

There are a variety a strategies to choose from in an effort to respond to these types of Fregean puzzles. Broadly speaking, however, this diversity can be usefully partitioned into four different techniques by which to attempt to reconcile a belief that Cicero is speaking with a belief that Tully is not speaking. One solution—the Fregean solution—postulates *senses* as an explanatory variable distinguishing the semantic content of co-referring names. The Fregean solution relies upon senses to distinguish the sense of /Cicero/ from the sense of /Tully/. In doing so, the intervention of distinct senses helps to explain an otherwise inexplicable failure of substitution and remove the threat of contradiction. Though the referent of /Tully/ and /Cicero/ are the same, the sense of /Tully/ and /Cicero/ are different. Assuming that belief clauses access, or express, the sense associated with a name rather than merely its referent, it is not, strictly speaking,

contradictory to believe that Cicero is speaking on the one hand, but not believe that Tully is speaking on the other. From this perspective, the claim that Cicero is really Tully can be just as informative as the claim that Cicero is really the emperor Nero, even though the former is true and the latter false. With respect to senses, /Cicero/ and /Tully are as sense distinct as /Cicero/ and /Nero/ are.

Another solution favored by direct reference theorists like Nathan Salmon and Scott Soames (see, for example, Salmon 1986, 1989 and Soames 1987) does not distinguish between the sense and referent of co-referring names. Instead of defusing a looming contradiction through the intervention of senses, their approach seeks to mitigate the inconsistency of believing Cicero is speaking in conjunction with not believing Tully is speaking through the intervention of propositional "modes of presentation" or propositional "ways of believing." Though the proposition expressed in both cases is exactly the same, one and the same proposition may be believed via distinct propositional "modes of presentation" or "ways of believing." These distinct "modes of presentation" or "ways of believing" help to explain how one and the same proposition can be believed in one way, but not be believed when presented in another way. Assuming that belief clauses access, or express, the "way of believing" associated with a proposition rather than merely its propositional content, it is not a contradiction—or at least not an "obvious" contradiction—to believe that Cicero is speaking but not believe that Tully is speaking. According to Salmon (1987),

> If $x \neq y$, then there is no such thing as the (possible) fact that $x = y$. The fact that $x = y$, if such a thing is a *fact*, is just the fact that $x = x$. These are the very same fact, described two different ways. Described as the fact that $x = x$, it is quite obvious that this fact obtains solely in virtue of logic and logic's applicability to x, and not in virtue of any further fact concerning the possible qualitative character or history of x. The same thing

is true of this fact (even if it is less obvious) when it is described as 'the fact that x = y', assuming there is such a (possible) fact. (518)

From this perspective, the proposition expressed by /Cicero is Cicero/ is exactly the same proposition as that expressed by /Tully is Cicero/, even though this proposition may bear very different epistemic, communicative, informational, contextual, conversational or pragmatic Gricean consequences in virtue of the same proposition being associated with different propositional vehicles. Rather than distinguishing /Cicero is Cicero/ and /Tully is Cicero/ in terms of distinct Fregean senses associated with /Cicero/ and /Tully/, /Cicero is Cicero/ and /Tully is Cicero/ are distinguished in terms of distinct propositional "ways of believing" associated with /Cicero is Cicero/ and /Tully is Cicero/.

I do not see much difference between these two views: both postulate intervening variables—either at the level of propositions or at the level of reference—in an effort to remove or at least mitigate the threat of inconsistency for those who might believe Cicero is Cicero but not believe Tully is Cicero. One relies on distinct senses associated with one and the same referent internal to the proposition, and the other relies on distinct propositional guises associated with one and the same proposition external to the proposition. The former introduces "modes of presentation" in the form of Fregean senses; the latter introduces "modes of presentation" in the form of propositional "ways of believing." In relation to propositional content, the Fregean solution amounts to an import model while the direct reference solution amounts to an export model. Both, however, invoke "modes of presentation" or "ways of believing." They only differ as to what this extra information is associated with—the referent of the name or the propositional content of sentence. In fact, "hidden indexical theories" associated with Schiffer (1992) and Crimmins & Perry (1989) form an intermediate position of sorts

between these two views. The epistemic, pragmatic or contextually sensitive information which would be associated with Salmon-esque propositional "ways of believing" external to the proposition is, like a Fregean account, incorporated into the very content of the proposition, but in such a way as not to be overtly realized in surface linguistic structure.[12]

So while both direct reference and hidden indexical theories reject Fregean senses as an account of the semantics of names, this anti-Fregean position is made more defensible by denying that propositional content is directly accessed, or uniquely expressed, by the various belief sentences used to convey such propositional content. (The suggestion that natural languages constructions involving belief clauses might not be "compositional" is symptomatic of this gap between propositional content and what belief clauses express.) According to theorists like Salmon and Schiffer, there is a one to many relationship between propositional content and any number of co-propositional but distinct ways of believing this propositional content But whether such ways of believing are external to the proposition or internal to the proposition (as with Frege), there is a one to many relationship between propositional content and belief content in the same way that there is a one to many relationship between reference and co-naming but distinct Fregean senses.

Though sympathetic to theories of direct reference, this dissertation is not an exercise in directly referential semantics. Rather, the present work seeks to undermine the original Fregean intuition that it is indeed possible to believe Cicero is not Tully without contradiction, or equally, without thereby compromising one's linguistic competence. Ruth Barcan Marcus, for instance, has adopted a minority position in a

number of publications (Marcus 1981, 1983, 1993), arguing that the belief Cicero is not Tully *is* a contradiction. She neither appeals to Fregean senses, nor propositional guises to preserve even the superficial consistency of such a belief. Instead, she moves to exclude the very possibility of believing Cicero is not Tully from the range of possible beliefs.

Marcus's Solution

Marcus does this by first considering someone who might claim to know that Cicero is not Tully. Given that Cicero is Tully, however, one may claim to know that Cicero is not Tully, but this claim would itself be false. Insofar as truth is a prerequisite for knowledge, and the proposition that Cicero is not Tully is not true, it is not possible to know that Cicero is not Tully. One could claim to know that Cicero is not Tully, but insofar as Cicero is Tully, this claim to "know" Cicero is not Tully would have to be withdrawn. But Marcus claims that just as truth is a precondition for knowledge, possibility is a precondition for belief. One cannot "know" a falsehood. But nor, argues Marcus, can one "believe" an impossibility. One can claim to believe that Cicero is not Tully, but this belief claim itself is mistaken. As Marcus reasons, if someone were to believe that Cicero were not Tully, then they would be believing that something were not itself. But, says Marcus, nobody can really believe that. According to Marcus (1993), "an agent can claim to have such beliefs but will be mistaken in so claiming" (252): "Just as the falsehood of '*P*' excludes knowing that *P*" says Marcus, "the necessary falsehood of '*P*' excludes believing that *P*, whatever the agent's knowledge claims or belief claims, respectively" (254). But this has struck many, to say the least, as counter-intuitive (see,

for instance, Salmon 1986b). How can agents be mistaken about which beliefs they can really call their own? How can agents (sincerely) say things they do not, no less could not, believe?

Clearly if Marcus's claim is to be made more palatable, some principle is needed by which to exclude those who take themselves to doubt whether Tully is Cicero (on the basis of their lack of awareness that /Tully/ and /Cicero/ are co-referring names), from being able to even get their "beliefs" off the ground in the first place. What is needed is some principle by which to exclude those who take themselves to doubt that Tully and Cicero are the same person from being able to even get their "beliefs" off the ground in the first place *merely on the basis that they are unaware that /Tully/ and /Cicero/ are co-referring names.* What is needed, in essence, is some principle, some gate-keeping mechanism, to prohibit access to our arena of disquotational discourse where we typically infer what someone believes by disquoting what was said. But this is not as difficult as it might seem. Those who do not—or at least are not deferentially prepared to—label Cicero with his other name /Tully/ cannot be unquestionably counted as fully competent with both his names. What we might attempt to express—and what might be inferred about our beliefs—through the use of words whose reference we do not fully appreciate, or are unwilling to deferentially accommodate, cannot simply be taken at "disquotational" face value.

In fact, such disquotational "no-fly zones" are by no means unusual. We do not disquote foreign speech. We do not unthinkingly disquote the misarticulated or misformulated speech of children. Even Burge (1978, 1979) acknowledges that there might be cases where disquotation is inappropriate, e.g., 'There is an orangutan in the

refrigerator'. Disquotation is a default, but no less defeasible, practice. Generally speaking, we do not automatically disquote speech when there is reason to suspect that the words are not being used to refer to what they properly refer to. Moreover, restrictions of this sort are not, as Burge has emphasized, imposed upon speakers, as if such semantic standards were somehow alien to them. Rather, such restrictions—and such corrections in linguistic usage—are typically expected and deferentially accepted.

As Burge has pointed out, it is we who typically re-interpret our own words—we unilaterally defer to the standards of our community—thereby relieving any pressure for our communicative partner to pick up the slack and do the re-interpretation for us. Children, for instance, do not make their elders continuously, no less permanently, re-interpret their idiosyncratic usage of a word. Instead children revise their usage in accordance with conventional standards. But whether we try to get others to speak like us or let others try to get us to speak like them, neither dynamic is suggestive of language users trying to linguistically go it alone.

This is why Burge rightly emphasizes that re-interpretation is not necessary. "Re-interpretation" has already occurred in virtue of deferential correction. We typically wish to deferentially express whatever it is that our words conventionally refer to—whatever that may turn out to be—and therefore do not need to be re-interpreted. And this is because "The willingness of the speaker to submit his statement to the arbitration of a dictionary indicates," says Burge (1978), "a commitment to having his words taken in their conventional sense, whatever that sense is" (130): "People are frequently held, and hold themselves to the standards of their community when misuse or misunderstanding are at issue. One should distinguish these cases, which seem to depend on a certain

responsibility to communal practice, from cases of automatic reinterpretation" (Burge 1979, 90). Despite mistakes in over-extending the application of, say, /arthritis/ to cover muscle ailments in addition to joint ailments, /arthritis/ deferentially refers only to ailments of the joints. It is not possible to have arthritis in the thigh. This is the reason why Burge urges us, correctly I think, not to re-interpret, or translate, the claim "There is arthritis in this thigh" as there is *tharthritis* in this thigh (as we would in some other community where /arthritis/ does refer to both joint and muscle aches). In this way, linguistic deference allows even the less than fully competent to be regarded as still within the relevant linguistic community rather than as partial to some other sociolinguistic allegiance. Everyone makes mistakes in lexical application, but linguistic deference—as opposed to linguistic defiance—allows partial or incomplete understanding to be resolved by correction.

What Burge neglects to point out, however, is that the deferential use of our words can unwittingly ensnare us in a contradiction of our own making, and with our own blessing. Consider that if the deferential use of /arthritis/ applies only to inflammations of the joints, then the deferential use of /arthritis/ by someone who fears that arthritis has spread to the thigh is the contradictory assertion that there is a pain in the thigh that can only occur in the joints. As Marcus might say, one can claim to believe that there is arthritis in the thigh, but the belief claim itself is mistaken. For if someone were to believe arthritis can occur in the thigh, then they would believe an ailment that can only occur in the joints is occurring in the thigh, and a pain in the thigh which cannot occur in the thigh is not possible. A pain in the thigh that cannot occur in the thigh is a contradiction, and nobody, Marcus would say, can believe that.

Yet analogous considerations hold for co-referring names. Just as /arthritis/ deferentially refers only to inflammations of the joints (despite its over-extended use to include non-joint ailments), /Tully/ deferentially refers to Cicero (and /Cicero/ to Tully)—despite their *under-extended* usage. /Tully/ refers to Cicero (and /Cicero/ to Tully) even if these terms happen to be restrictively under-extended. But neither the over- or under-extended use of /Tully/ or /Cicero/ is definitive of the deferential content of /Tully/ and /Cicero/ anymore than the over- or under-extended use /arthritis/, /gold/, /elm/, /beech/, /sofa/, and /contract/, etc., is definitive of their deferential content. From this perspective, names are no more immune to deferential correction than our words are generally. Doubting or denying, then, that Cicero is Tully is really no better or worse off than doubting or denying whether arthritis is a joint ailment.

On the one hand, then, if we are deferential, then we may be interpreted—because we wish to be interpreted—as intending to refer to what /Tully/ refers to, namely Cicero. As Burge observes, it is not as if those who confusedly say they have arthritis in their thigh, are using a word which just happens to be homophonously ambiguous with our word /arthritis/. Rather, they are *misusing* our common word /arthritis/ by *over-extending* it to cover both muscle and joint aches. But words can also be *under-extended* relative to their proper application just as much as words can be over-extended. /Tully/ and /Cicero/ are just such an example. The under-extended use of /Cicero/ to refer to Tully only under certain circumstances (and /Tully/ for the remaining circumstances) is just such a case. But here too, it is not as if those who might confusedly assent to the sentence /Cicero is not Tully/ are using words which just happen to be homophonously ambiguous with our words /Tully/ and /Cicero/. Rather, they are misusing their own words—our words—

/Tully/ and /Cicero/ by under-extending them in such a way so that /Cicero/ is not used to refer to Tully.

On the other hand, if we choose not to be deferential with the use of our terms, then we should not be interpreted—because we do not wish to be interpreted—as intending to refer to Cicero with the word /Tully/ for the same reason we might not wish to be interpreted as referring to Cicero with /Cicero/. In these cases, it is not that the speaker attempts to conform to some governing standard, fails and as a result, becomes entangled in some sort of contradictory linguistic confusion. Rather, the speaker has no intent (either for deliberately idiosyncratic or perhaps with alternative conventions in mind) to use the term to refer to what the term conventionally does refer to. When uttered by this person, /Tully/ no more refers to Cicero and /arthritis/ no more refers to arthritis than a Frenchman refers to a champ (i.e., a hero) with the word 'champ' (region, space). The non-deferential (or deferential by some other standard) use of these terms indicates that disquotation is inappropriate.

The upshot is that insofar as our words are used with the deferential intent of conforming to "proper" or "expert" usage, the possibility of incidentally contradicting ourselves arises in virtue of the possibility of lexical error (or what Burge calls "incomplete" or "partial" linguistic or semantic understanding). This result confirms Marcus's intuition that doubting whether Tully is Cicero is not a real possibility. Insofar as /Tully/ deferentially refers to Cicero, the belief that Tully is not Cicero is the belief that Cicero is not Cicero. And that is a contradiction. But insofar as our words are not used with the intent of conforming, the speaker is perhaps free of contradiction, but now ineligible for homophonous disquotation. So either "deferential disquotation" leads to a

potential contradiction (in virtue of less than full linguistic competence) or "defiant quotation" is irrelevant as there is little or no basis by which to infer speaker belief on the basis of speaker utterance in the first place.

Marcus's claim, then, that it is not really possible to believe that Cicero is not Tully can be substantiated. For when we try to believe that Cicero is not Tully—but wish to be deferentially interpreted as referring to what /Cicero/ and /Tully/ refer to rather than what we might happen to use /Cicero/ and /Tully/ to refer to—we would be believing that Cicero is not Cicero. The normative constraints governing Burgean "proper" and Putnamian "expert" usage indicate that we prefer to be wrong—even necessarily wrong—and accept linguistic correction, than plead we were somehow right all along because we had a different meaning in mind. Like claims of knowledge, claims of belief are also subject to retroactive correction. When we express a commitment to conforming to the full and proper usage of our words, we do this even if it requires us to view our past linguistic usage as either under-extended or over-extended relative to more proper usage (see, for instance, Putnam's 1975 discussion of Archimedes). These issues concerning the normativity of linguistic usage, and repercussions for substitutional equivalence, are picked up again in chapter 10.

Church's Solution

There is another technique, however, by which to undermine the Fregean intuition that it is indeed possible to believe Cicero is not Tully without contradiction, or equally, without undermining one's claim to be fully competent with the names. But rather than substantiate Marcus's reasoning by applying linguistic deference to co-referring names,

one may follow Church (1954) and Kripke (1979) and apply their anti-Mates reasoning to the substitution of co-referring names in belief contexts. Church's approach is essentially meta-linguistic in orientation, and it is this technique which is most relevant for a "phono-logically" inspired approach to traditional Fregean puzzles of substitutivity. So while we might agree with Marcus that those who deny or doubt whether Cicero is Tully cannot—on pain of trying to "believe the impossible"—be doubting whether Cicero and Tully are the same person, this leaves unanswered what such individuals actually are doubting. What is really being doubted, adopting the meta-linguistic strategy pursued by Church (1954) in his response to Mates (1950), is something "linguistic." Quoting from Church (1954):

> It must be understood that those who are supposed to have doubted that ['a fortnight lasts a period of fourteen days'] without doubting that ['a fortnight lasts a fortnight'] are supposed also to have had a sufficient knowledge of the English language so that the doubt was not, for example, a doubt about the meaning of the word 'fortnight' in English.
>
> Nevertheless, it is natural to suggest as a means of overcoming Mates's difficulty that it is after all not possible to doubt that ['a fortnight lasts a period of fourteen days'] without doubting that ['a fortnight lasts a fortnight']; and that the doubt which has been or may have been sometimes entertained by philosophers in considering the question of the criterion of identity of belief is not the doubt that ['a fortnight lasts a period of fourteen days'], but a doubt that does have reference to linguistic matters . . . the doubt whose existence or possibility Mates urges (as a difficulty in the analysis of belief statements) is about certain sentential matrices. (164)

The first thing to notice about Church's argument is that he relies on the *meaning* of the word /fortnight/ in English to counter Mates. The second thing to notice, however, is that this is not really relevant. The real force of Church's argument is not that Mates's argument some how trades on the difference between doubting the meaning of a word and doubting the referent of a name. Rather Church argues that Mates's argument is really about "sentential matrices," and presumably /Tully/ and /Cicero/ conspire to create different "sentential matrices" just as much as /fortnight/ and /a period of fourteen days/

do. Indeed, by replacing "meaning of the word /fortnight/" with "referent of the word /Cicero/," Church's argument provides a template by which to respond to Frege's original observations concerning the substitution of co-referring names. In fact, Mates's argument is really just a Fregean argument concerning the substitution of synonymous terms rather than the substitution of co-referring names, so it is unsurprising that Church's anti-Mates response could be re-worked into an anti-Fregean response. Replacing "sufficient knowledge of the English language so that the doubt was not, for example, a doubt about the *meaning* of the word 'fortnight' in English" with "sufficient knowledge of the English language so that the doubt was not, for example, a doubt about the *referent* of the word 'Cicero' in English" does not affect the structure of Church's argument, nor the conclusion which he draws, namely that such doubts do "have reference to linguistic matters": "The doubt whose existence or possibility [Frege] urges (as a difficulty in the analysis of belief statements) is about certain sentential matrices." Indeed, why Church does not deploy his anti-Mates strategy against traditional Fregean reasoning is an interesting question. It is an apparent inconsistency—"A Problem in the Frege-Church Theory of Sense and Denotation" according to Salmon (1993) and detailed at some length.[13] It is unclear, that is, whether Church—like Kripke (1979)—is consistent in rejecting Mates's reasoning so as to defend the strict substitutivity of synonymy in belief contexts on the one hand, only to endorse Fregean reasoning to defend the non-substitutivity of co-referring names in belief contexts on the other.

But what is distinctive about Church's "sentential matrix" approach is that, unlike Marcus, he does not directly appeal to the intuition that to believe a fortnight is not a period of fourteen days is to believe that something is not identical to itself. Rather,

Church goes meta-linguistic, arguing that those who doubt whether a fortnight is a period of fourteen days are actually expressing a doubt regarding linguistic matters: whether the *word* /fortnight/ is interchangeable with the *words* /a period of fourteen days/. (See chapter 4 below, where Peacocke 1975 and Kaplan 1989 are cited for providing more instances of this sort of reasoning.) But, counters Church, we are supposedly not concerned with matters linguistic, so Mates's argument misses its target. At best, Mates's argument indicates that it might be possible to doubt whether the *word* /fortnight/ is interchangeable with the *words* /a period of fourteen days/. But Church denies that even that is genuinely possible. For doubts which are clearly linguistic in nature inevitably raise the question whether the interchangeability of the word /fortnight/ and the words /a period of fourteen days/ can be doubted without thereby compromising one's command of the English language. And this, Church claims, is not possible: doubt about one's language transparently threatens one's claim to be linguistically competent. Specifically, Church suggests that those who doubt the substitution of /fortnight/ and /a period of fourteen days/ "are supposed to have had a sufficient knowledge of the English language so that the doubt was not, for example, a doubt about the meaning of the word 'fortnight' in English" (164). Summarizing some of his own reasoning on these matters, Pap (1955) concludes: "In other words, whether or not A knows that 'D_1' is synonymous with 'D_2' in L is irrelevant to the question whether 'A believes that D_1' entails, in L, 'A believes that D_2'; what is relevant to this question is only whether 'D_1' *is* synonymous with 'D_2' in L" (13).

As it stands, however, Church's response (and Pap's) is inadequate for it does not go far enough. Church's appeal to meaning (and Pap's to synonymy) only succeed in

begging the question against Mates. Recall that Mates (1950) designed his "Synonymy" argument to undermine any account of synonymous substitution. Mates's article raises an objection against any possible standard for substitutivity within belief contexts. Even when a pair of words is *stipulated* to be synonymous to the nth degree, Mates observes that it is always at least conceivable that someone might balk at the substitution. And this, according to Mates, proves that they are not synonymous. Church's counter-claim, then, that /fortnight/ and /a period of fourteen days/ do have the same meaning—are synonymous expressions—is clearly unresponsive to Mates. So is Church's move to uniformly translate both a /fortnight/ and /a period of fourteen days/ into a single German expression (see also Scheffler 1955, 44). For this move also depends upon meaning: the German expression */Zeitraum von vierzehn Tagen/* must having the same *meaning* as the two English expressions. (In any event, Mates could equally choose some other language where /fortnight/ and /a period of fourteen days/ are uniquely translated into a pair of words or expressions rather than a single expression, thereby circumventing Church's appeal to German. See Kripke 1979, 133-134 for related remarks on various "proofs" and "disproofs" of substitutivity.) Recall that Mates's argument was designed to demonstrate that no possible standard of substitutivity is immune to substitutional skepticism. So whether meaning does or does not guarantee substitutivity in belief contexts is not going to be decided by Church saying that meaning (synonymy, translation, etc.) does guarantee substitutivity in belief contexts and Mates saying that meaning (synonymy, translation, etc.) does not.

An adequate response to Mates's argument requires that a standard for substitution be invoked—a standard independent of, and somehow impervious to, doubt.

For unless a criterion for substitutivity independent of individualistic epistemic considerations is brought in, Mates's argument, like Frege's argument, will inevitably run its course. Indeed, the synonymy debate played out between Church and Mates recreates a co-naming debate between direct reference theorists like Marcus and Fregeans. But whether co-naming reference does or does not guarantee substitutivity in belief contexts is not going to be decided by a few saying that co-naming does and Fregeans saying that co-naming does not, any more than Church claiming, and Mates denying, that synonymy guarantees substitution. What is needed is some standard by which to enforce substitutions which even both parties to these kinds of debates can agree is doubt resistant. Mates (and Frege) suggest there is no such standard. Phonologically licensed substitutions are tendered in an effort to break this stalemate.

The way to response to Mates's challenge—and traditional Fregean puzzles generally (Kripke's Paderewski case demands separate treatment, see chapter 10)—is to emphasize that even if two terms are—like Putnam (1954)—conceded to be "different," such a concession does not, in itself, thereby guarantee that the two terms are not substitutionally equivalent. Well, not unless this claim is supported by the additional premise that all "differences" in linguistic "form" logically allow for failures of substitutivity. But that claim is false: allophonically related word-tokens differ in "form" but still freely substitute *salva veritate*. In fact, unless suitably qualified, Mates's pattern of reasoning, like Frege's, encourages a *reductio ad absurdum*, a reductio which helps to reveal how both Fregean and Matesian reasoning are implicitly premised upon the claim that sheer differences in linguistic form are sufficient to block both co-meaning or co-naming substitutions.

Here is how Yagisawa (1984) attempts to foist a *reductio* on Mates's argument, with the result that even minimally perceptible differences would precipitate failures of substitutivity. (This is not surprising as Mates used the minimally distinct /D/ and /D'/ as his example.) Consider /14/ and /fourteen/, suggests Yagisawa:

> Run the argument exactly as in the original argument . . . and you obtain the conclusion that *salva veritate* substitutivity in belief contexts of an English number name for the Arabic numeral for the same number fails. This conclusion is an *obvious* absurdity. (410)

What's most interesting from our "phono-logical" perspective is that Yagisawa even goes on to raise questions about the word-token variation which arises between capital and non-capital letters. But, suggests Yagisawa, such word-token inscriptional variation between capital and non-capital letters cannot be subject to doubt. Not unless we wish to deny that there are standards of orthographic/phonological equivalence which guarantee TK-substitutivity among distinct word-tokens.

But presumably neither Mates nor Frege were interested in pushing substitutional doubt so far as to question the substitutional status of allophonic or "allographic" word-token variation. Yet in order to draw the line at word-token variation, there must be some philosophically significant principle which serves to block doubt from disrupting word-token TK-substitutions. There must be something about the relationship among some distinct word-tokens (like [mat] and [mat^{h}]) which makes them the same word-type (unlike [Cicero] and [Tully]) so as to protect them from substitutional skepticism. Perhaps various word-tokens count as the same word-type in virtue of the way they look or sound; they instantiate the same word-type because they are somehow similar looking or sounding to each other. Or perhaps various word-tokens count as the same word-type not because they are—or not only because they are—as linguistic specimen perceived as

acoustically or visually similar, but because they either have the same meaning or the same reference. These are the two available choices in broad outline. But given the failure "phonemic reductionism," a Church-like content-based response cannot be so easily dismissed as question-begging in this word-token case. Nor can a direct reference inspired response be so easily dismissed as question-begging. Insofar as an argument can be made that basic phonological TK-substitutions among phonetically distinct word-tokens are at least partially based upon semantic considerations—meaning for predicate-tokens and reference for name-tokens—it is no longer question-begging to introduce such interpretive constraints when it comes to TP-substitutions, for now they have an independent justification. So while Church's appeal to synonymous meaning can be written off as question-begging against Mates, and direct reference appeals to naïve reference can be written off as question-begging against Frege, such appeals to synonymy and co-naming reference cannot be so easily brushed aside when it come to the semantic basis of natural language phonological equivalence. Without the semantic content of names and predicates, one does not even get token-to-token phonological TK-substitutions. It is unsurprising, then, that without synonymy and co-reference one would not get type-to-type synonymous or co-naming TP-substitutions either.

Church's response to Mates is good, but incomplete. It must be supplemented with the observation that even basic TK-substitutions are dependent upon semantics. Once phonological substitutions are themselves recognized to be indebted to semantic considerations, the distinctive philosophical motivation for seeking a basis for synonymous substitutions independent of meaning and co-naming substitutions independent of reference becomes less compelling.

The failure of "phonemic reductionism" suggests that if distinct word-tokens can be phonologically equivalent—and hence immune from substitutional skepticism—not despite, but ultimately in virtue of semantic considerations, then it is no longer obvious, philosophically speaking, why synonymous or co-naming substitutions should be held to a different standard, no less an explicitly individualistic epistemic standard. The important point that TP-substitutions and TK-substitutions are different—TP-substitutions involve tokens of distinct word-types and appear to be competently dubitable while TK-substitutions involve tokens of the same word-type and appear to be only incompetently dubitable—fails to explain *why* the possibility of genuine doubt should align with these two different kinds of substitutions in this way. Neither kind of substitution is perceptually obvious as both are indebted to interpretive semantic considerations. Nor does it seem that either kind of substitution can be doubted without contradiction, partial understanding, or linguistic deviance so great as to disable disquotation. Rather than the distinction between TK-substitutions and TP-substitutions marking a philosophically significant difference, we find that the distinction tends to mask a number of important symmetries. The next chapter sharpens the contours of this parity between word-token TK-substitutions and word-type TP-substitutions by analyzing how Fregean intuitions implicitly trade on "phonemic reductionism" and why Quine would explicitly commit himself to the thesis of "phonemic reductionism."

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[11] According, to Langford (1937), "There is . . . a simple test which helps us to determine whether a word is being used or talked about, namely, that of translation. A word that is being used is to be translated, while a word that is being talked about must not be (subject matter must remain unchanged under translation)" (53-54).

[12] Hidden indexical theories attribute the divergence in truth between '*S* believes Cicero is speaking' and '*S* believes Tully is speaking' to an implicit "mode of presentation" embedded in the proposition, but not linguistically overt (hence the "hidden" in hidden indexical). The nature of this covert mode of presentation is contextually determined on different occasions the belief is expressed (the "indexical" in

hidden indexical). So while the "mode of presentation" is part of the truth condition of the proposition, this mode of presentation will contextually vary from one circumstance to another. In this way, hidden indexical theories allow for the Gricean-like pragmatic features propositionally excluded by Salmon to be indexically incorporated into the proposition itself but without surface realization.

[13] Salmon (1993) detects an inconsistency in Church's reasoning about co-referring names, synonymys and his solution to the so-called "paradox of analysis." On the one hand, Salmon notes that Church does not invoke intensional entities when confronted with synonymous doubt. Instead, Church dismisses synonymous doubt as linguistically confused. On the other hand, Church does adopt a Fregean solution to the "paradox of analysis." But this is arguably inconsistent, observes Salmon (1993): "If Church were also to resolve the inconsistency by . . . . seeking an alternative solution to the Paradox of Analysis, his doing so would immediately raise the question of why he does not embrace an exactly parallel alternative solution to Frege's original puzzle about the informativeness of 'Hesperus and Phosphorus'—a solution that, by hypothesis, does not involve distinguishing co-denotational proper names as regards to semantic content. Adopting such an alternative solution would seriously threaten the Frege-Church theory. For once it is agreed that the apparent informativeness of an identity statement . . . is to be explained in some way other than by postulating a distinction between the senses of the singular terms involved, Church's and Frege's original argument for the theory of sense and denotation collapses" (163).

FROM A PHONO-LOGICAL POINT OF VIEW

Chapter 4
Phonemic Reductionism in Analytic Philosophy

> The second kind of argument is obtained . . . by assuming that 'D_1' and 'D_2' are the same sentence written in block letters and in script letters, respectively. The argument runs just as before. . . . And its conclusion is that typographical transparency—viz., *salva veritate* substitutivity of the same word written in different typographical styles—fails for belief contexts. This conclusion is an *obvious* absurdity.
>
> T. Yagisawa (1984, 411)

Initial reactions to Fregean arguments regarding the substitutional discrepancy between replacing /Cicero/ with the co-referring name /Tully/ versus replacing /Cicero/ with /Cicero/ in belief contexts, can be instructive as they are diagnostic of where one might sit in philosophical space, and the company to be found there. But in addition to being largely responsible for the present dissertation work, my reaction was atypical, though not entirely unique (see especially Sellars 1950b, 1964a, 1967 and MacIver 1937a, 1937b, 1937c, but also Meckler 1954, Rollins 1950 and Yagisawa 1984.) In many ways the present work finds its natural home within the "synonymity" discussions of the early 1950s sparked among Rudolph Carnap, Benson Mates, Alonzo Church, Hilary Putnam, and Wilfrid Sellars. For rather than only considering the advantages and disadvantages of various sense-like devices to explain the "cognitive dissonance" that accompanies co-naming (or even synonymous) substitutions in belief contexts, I was led to ask the following questions: What about the status of substitutionally switching the word-token [*Cicero*] for the word-token [Cicero] or [CICERO]? Are substitutions between italicized word-tokens and capitalized word-tokens "trivial" or "informative," and, if not, why aren't these kinds of substitutions (any longer?) informative?

After all, when considering the substitutivity of /Cicero/ and /Tully/, there are actually *two* questions which may be asked. But Frege only asks one. Frege asks how it is possible for substitutions between /Cicero/ and /Tully/ to be informative when substitutions between /Cicero/ and /Cicero/ are not. Yet one may just as well ask: how could substitutions between /Cicero/ and /Cicero/ be *uninformative* given that substitutions between /Cicero/ and /Tully/ are not? This is equally as puzzling. Perhaps switching tokens of /Cicero/ for tokens of /Cicero/ is uninformative because the two tokens are perceptually indiscriminable. (Kripke's Paderewski case of mistakenly interpreting one name as ambiguously denoting two different individuals seems designed for just this sort of response, see chapter 10). In any event, what about switching the perceptually discriminable tokens [Cicero] for [*Cicero*], or [Cicero] for [CICERO]? Is this substitution trivial or informative?

But these questions should not be dismissed as linguistically odd or philosophically idle. For in addition to the few listed above (e.g., Yagisawa), some prominent philosophers have turned their attention to the substitution of near minimally distinct linguistic expressions. Indeed it is Kripke (1979) who, in so many words, wonders whether tokens of /London/ and */Londres/* are freely substitutable in belief contexts, and if not, what this might mean for analytic philosophy of language. Nathan Salmon's concerns regarding /catsup/ and /ketchup/ and Scott Soames' regarding the phonologically very similar /Peking/ and /Bejing/ also come to mind here. In this way, traditional Fregean issues concerning substitutional equivalence have already mutated into questions concerning the substitutional status of minimally distinct linguistic

expressions. (The homophonous "ambiguity" of Kripke's Paderewski case is dealt with separately in chapter 10.)

And the pressing question this convergent interest in minimal-like substitutions raise is: Are there any limits to substitutional skepticism? If the substitution of tokens of /London/ for tokens of /*Londres*/ is a philosophically viable concern, what about [London] and [LONDON]? Is there no end to this sort of substitutional skepticism? And the answer is: Yes. There is a limit to substitutional skepticism—even in belief contexts—whether the items be as intuitively similar as [Cicero] and [*Cicero*], as intuitively distinct as [Cicero] and [Tully], or even as intuitively confusing as [London] and [*Londres*]. Indeed, it is not at all clear whether [London] and [*Londres*]—like [Peking] and [Bejing]—really should be considered two different word-types at all. (For what it is worth, Fiengo & May (1998) point out that "'London' and '*Londres*' are phonetic variants of the same expression" (401), without, however, drawing any conclusion about the viability of doubting their substitutivity.) But a full appreciation of the phonological nature of this limit—and in particular, the semantic basis of phonological substitutions among word-tokens of the same word-type—does not, in the end, permit substitutional skepticism to operate over co-referring names.

So while it may not be obvious what to say about [London] and [*Londres*], it is clear that intuitions governing the individuation of word-types is critically important. For it would seem only word-token variation of a certain critical mass—token variation which crosses a phonemic word-type boundary—is permitted to genuinely generate a Fregean-like puzzle. Presumably substitutional doubt involving [Cicero] and [CICERO]—as opposed to [Cicero] and [Tully]—is dismissible as philosophically

irrelevant. Such "doubt" is merely an indication of some sort of linguistic deficiency or deviancy philosophers need not directly concern themselves with. Such substitutions are not (or at least should not be) "informative" as each word-token instantiates or exemplifies one and the same English word-type. And substitutions among type equivalent word-tokens are, as it were, logically immune to substitutional skepticism. After all, the substitution of one token for another token of the same type is just to replace something by another version of itself. Since [*Cicero*] and [Cicero] are tokens of one and the same linguistic type—[*Cicero*] and [Cicero] are the same (type of) word—it makes no sense to doubt or question their substitutivity. They just are substitutionally equivalent, and doubts to the contrary—on pain of undermining the basic fabric of phonemic word-type categorization—must be dismissed as involving some sort of error or confusion. As the linguist Nigel Love (1990) observes, "Anyone who wishes to make the point that there is no such thing as a determinate unit of spoken English called *cat* runs up against the fact that in order to make it he seemingly has to invoke the unit whose existence he calls into question" (113).

Doubt whether [*Cicero*] and [CICERO] are substitutionally equivalent is philosophically uninteresting. And that's because [*Cicero*] and [CICERO] are word-token variants of the same word-type /Cicero/.[14] Indeed, it would seem that it is really not possible to doubt whether [*Cicero*] and [CICERO] are substitutionally equivalent—a point worth remembering as we are really interested in considering whether it is possible to doubt the substitution of /Tully/ and /Cicero/.

Frege's Implicit Phonemic Reductionism

But of course Frege was not, and philosophers of language generally are not, concerned with merely observing that tokens of /Tully/ and /Cicero/ seem not to be interchangeable within belief contexts (no less with the complementary observation that [*Cicero*] and [Cicero] appear to be freely interchangeable). Rather, Frege wishes to explain how tokens of co-referring names could provoke failures of substitution when placed in contexts involving the epistemic factors of belief and other propositional attitudes. And *senses* are his answer. Frege offers us a theory designed to explain why referentially equivalent names do not substitute freely within belief contexts. Words, and in particular proper names, are not "directly referring" devices. Instead, they are really disguised definite descriptions of some sort or another which succeed in picking out their referent in accordance with the descriptive content of their associated sense. For this reason, it is no mystery why two co-referring names might not substitute freely in belief contexts any more than two co-referring definite descriptions would fail to substitute in epistemic contexts. Definite descriptions are obviously referring devices that typically operate through the coordinated content of a multiple word description. Though contingently co-referential, definite descriptions are not meaning equivalent. (Nor are definite descriptions candidates for being the same "dot quote".[15])

By contrast, we have not been asking what seemingly prevents tokens of /Tully/ and /Cicero/ from freely substituting in belief contexts. Instead, we have been asking what actually underwrites the substitutional equivalence of token distinct variants like [Cicero] and [CICERO]. After all, Frege could have merely said that tokens of /Tully/ and /Cicero/ are not substitutionally equivalent simply because they exemplify two

different word-types, assert that no word-type distinct tokens (even if co-referential) are substitutionally equivalent, and leave it at that. (This is pretty much what Mates (1950) does with his */D/* and */D'/* example, though he adds that no word-type distinct tokens—even if synonymous—are substitutionally equivalent in belief contexts.)

But if it is the job of philosophers to explain why tokens of co-referring names (or even synonyms) do not substitute freely within belief contexts, it is the task of linguists, and at that, phonologists, to explain why some distinct word-tokens (e.g., [mat^{h}] and [mat]) are substitutionally truth-preserving in some languages—certainly within belief contexts, and arguably even within contexts of direct quotation. What is it, though, that actually determines whether two phonetically distinct word-tokens instantiate the same phonological word-type, and hence are protected against substitutional skepticism? According to Nigel Love,

> This idea [of invariance] lies at the heart of modern linguistic theory. Bloomfield, for instance, asserts it in the brusquely straightforward form of what he calls 'the fundamental assumption of linguistics': namely, that 'in certain communities (speech communities) some speech-utterances are alike as to form and meaning' (Bloomfield 1935: 144). For anyone who accepts this waiving of the theoretician's magic wand as providing a solution in principle to the problem of how to identify the recurrent 'sames' on which the kind of linguistic analysis Bloomfield envisages depends, the obvious next question is how it is to be done in practice. *Which* speech-utterances are alike as to form (and meaning)? (1998, 106-107)

On what basis, then, do phonetically distinct word-tokens group into phonologically equivalent word-types? This, like Love's questioning, requires an empirical investigation into the basis of natural language phonemic categorization. Yet regardless of the exact phonetic details, phonological substitutivity should not—at least from a Fregean perspective—be grounded in considerations of referential content, as this would tend to short-circuit an attempt to extract a Fregean difference in sense between

tokens of /Tully/ and /Cicero/. If phonological substitutions are at least partially premised upon an equivalence of reference, then it would seem that at least some phonological substitutions would also be subject to Fregean doubt. By contrast, if language-specific phonological substitutions are not susceptible to doubt, then it would seem that at least some substitutions are immune to substitutional doubt even if their equivalence is based upon referential (rather than sense) considerations.

That is to say, unless there is some principled reason to allow, on the one hand, epistemic contexts block the substitution of tokens /Tully/ and /Cicero/, but not allow, on the other, such epistemic factors to block the substitution of [CICERO] and [Cicero], Fregean reasoning threatens either to (i) overshoot its target and commit itself to injecting Fregean doubt into basic phonological substitutions, or (ii) Fregean reasoning undershoots its target and falls short of allowing epistemic considerations to block co-naming substitutions in the first place.

The first fork encourages what Yagisawa (1984) views as a *reductio* of Fregean-like reasoning, and what I call "phonological eliminativism." Fregean doubt would reach into phonology and provoke substitutional failures among token variants of the same word-type. This would allow [*S* believes TULLY is speaking] and [*S* believes Tully is speaking] to have distinct truth values (even though *S* is not guilty of a contradiction, nor subject to any linguistic or semantic confusion). Along with Yagisawa (1984), I view this as an unacceptable consequence. It allows substitutional doubt to cut "absurdly" fine, so fine it is tantamount to eliminating basic phonological equivalence.

By contrast, the latter fork preserves the integrity of phonological substitutions, but uses the parity of co-naming TK-substitutions and co-naming TP-substitutions to

immunize the substitution of co-referring names from doubt—just like phonological substitutions. Insofar as co-naming TK-substitutions are underwritten by referential considerations, an unqualified Fregean rejection of reference-based substitutions threatens the reference-based substitutions characteristic of free allophonic variation. So unless there is a principled philosophical reason to permit the typing of word-tokens into phonemic word-types on the basis of referential considerations, but by contrast, to prohibit the use of this sort of information when considering the substitutional status of phonemically distinct but co-naming word-types, TK-substitutions and TP-substitutions should be treated, philosophically speaking, in the same way.

Short of introducing Fregean senses at the level of word-tokens to ensure the phonological substitution of at least co-sense name-tokens (chapter 2), the way to avoid the force of this argument is to adopt the principled position of "phonemic reductionism." That way TP-substitutions and TK-substitutions would be kept distinct. So while co-sense might be necessary to guarantee the substitutional equivalence of co-referring names in belief contexts, senses would *not* be necessary to guarantee the phonological substitution of word-tokens simply because reference is not necessary either: phonological substitutions would be premised upon an equivalence of uninterpreted phonetic form, not interpretive content of any kind.

Though this pattern of reasoning is explicit in Quine (and the subject of chapter 5), no where does Frege address the basis of natural language phonological substitutions. Consider, however, the following passage. Recapitulating a *Begriffsschrift*-like account of co-naming substitutions, here is how one philosopher suggests how tautologically

trivial statements—unlike informative statements—can be identified in virtue of their perceptually inspected form rather than their semantically interpreted content:

> Frege correctly observes that there is a cognitive difference between 'a = a' and 'a = b'. He demonstrates that the properties of identity require a distinction of sense and reference to account for this difference. . . .
>
> [However,] [t]here is a most important difference between 'a = a' and 'a = b' which Frege discounts but which can, nonetheless, explain the difference in cognitive value between 'a = a' and 'a = b'. This is the difference between 'a' and 'b' *as objects* whether they function as signs or not, but particularly in the case that they do. This difference explains why 'a = b' can "express . . . proper knowledge" and 'a = a' is analytic.
>
> 'a = b' expresses proper knowledge because you can [properly?] use 'a' and 'b' without knowing that their referents are the same (i.e., identical; or, that a = b). That is, you must look at something besides 'a' and 'b' to know that a = b. *On the other hand you do not have to look at anything except 'a' and 'a' to know that a = a. 'a = a' is analytic precisely because examination of the signs as objects and not as signs reveal its truth.* (Wienpahl 1950, 487-488; emphasis added and 'properly?' inserted)

In the opening passages of "On Sense and Reference," Frege wishes to avoid his earlier *Begriffsschrift* conclusion that the difference between true statements of the 'a = a' form and 'a = b' form has anything directly to do with linguistic forms or "signs." It is instructive, however, to note that both the move to treat co-referring names as merely "sign distinct" and the move to treat co-referring names as sense distinct tends to assume that the signs themselves are individuated in non-semantic terms.

To see why, consider that Wienpahl, like Frege, does not invoke 'a' and 'b' as inscriptional tokens. 'a' and 'b' are clearly being mentioned as linguistic types, not tokens (though loose talk of signs as "physical objects" can obscure this). In fact, questions regarding the substitutivity of 'a' and 'b' (whether co-referring names or synonyms) must be construed as concerning the substitution of abstract types; otherwise the question degenerates into an issue of mere perceptual acuity. Directed at the level of

word-tokens, such substitutivity tests become a psycho-physiological measure of "just noticeable differences."

But if it is abstract types which are being compared, then the assertion that "you do not have to look at anything except 'a' and 'a' to know that a = a" amounts to either (i) the peculiar claim that, as two *abstract* symbol-types, /a/ and /a/ are somehow *perceptually* indistinguishable, or (ii) the false claim that (as two particular tokens instantiating the same type), [a] and [a] will be perceptually indistinguishable. The problem with the first interpretation is that it is not clear how types—abstract types—can be perceptually discriminated (though chapter 9 does touch on this issue). The problem with the second interpretation is that it is false. Two type-equivalent tokens need not be perceptually indistinguishable

Rather than force either of these interpretations, a charitable reading encourages an attempt to uncover and articulate the basic assumption guiding Wienpahl's discussion regarding the "examination of the signs as objects." It would seem that according to Wienpahl, various tokens of the symbol-type /a/ are recognized as exemplifying the *same* symbol-type in virtue of something intrinsic to the tokens themselves available through perceptual inspection. An examination of their perceptual form as linguistic specimen—as "objects" unto themselves—reveals their interchangeability. That is why equating [a] and [a] is uninformative and [a] and [a] freely substitute.

Yet given that it is our awareness of basic phonological equivalencies which govern the recognition of which tokens count as the same—which in turn decides which statements are trivially true tautologies and which are non-tautologically informative—it is by no means accidental that philosophers would inevitably describe the uninformative

truth of 'a = a' in terms of perceptual indistinguishability ("you do not have to look at anything except 'a' and 'a' to know that a = a" as with Wienpahl) or in terms of perceptual similarity (as with Quine, see below). If the basic phonemic categories of natural language phonology actually were determined exclusively by perceptual features, then this distinction would be both true and important. But this is not the way basic phonemic equivalence works.

Though with a different purpose in mind (and using synonyms rather than co-referring names), here is how another philosopher explains failures of substitutivity. Note how the author relies on a notion of "logical structure" built upon some notion of basic phonemic word-type equivalence:

> For even though [the sentences 'all lawyers are lawyers' and 'all lawyers are attorneys'] are themselves intensionally isomorphic, they also differ in their logical structure, for where [all lawyers are lawyers] contains *two occurrences of the same expression* (namely, 'lawyers'), [all lawyers are attorneys] contains *occurrences of different expressions* (namely, 'lawyers' and 'attorneys'). Thus ['all lawyers are lawyers'] has the logical structure exhibited by the following valid logical schema.
>
> *All Φ's are Φ's.* $[(\forall(\Phi x \rightarrow \Phi x)]$
>
> and is to be counted a logical truth, while ['all lawyers are attorneys'] has the logical structure exhibited by the non-valid logical schema,
>
> *All Φ's are Ψ's.* $[(\forall(\Phi x \rightarrow \Psi x)]$. (Taschek 1995, 74-75)

Similarly, Lambert (1956) puts it this way:

> For ['all lawyers are lawyers'] has the logical structure exhibited in the inscription "All F are F", while ['all lawyers are attorneys'] has the logical structure exhibited in the inscription "All F are G". In other words, ['all lawyers are lawyers'] and ['all lawyers are attorneys'] do not have the same logical structure . . . (71-72)

Both these passages are just long ways of saying that 'lawyers' and 'lawyers' are two token occurrences of the same type of word, while 'lawyers' and 'attorneys' are token occurrences of two different types of words. The distinction then, between the triviality

of 'a = a' and the informativeness of 'a = b' is clearly parasitic on some prior notion of basic phonological word-type equivalence. Trivial and informative substitutions are just the difference between TK-substitutions and TP-substitutions.

For that matter, whether one adopts or rejects Fregean senses, a basic distinction between phonemically equivalent TK-substitutions and phonemically distinct TP-substitutions is critically important. Like Wienpahl, Peacocke (1975) also explains how a *Begriffsschrift*-like technique can accommodate the "cognitive dissonance" between co-referring names:

> We have in this reply an instance of a general strategy that it is natural for the defender of the view that names are rigid designators to adopt; that is, of explaining any apparent difference in truth-conditions of surface structure sentences differing only in the occurrences of distinct proper names α and β, where α and β denote the same object, *by the differences between the expressions α and β themselves*. (128, italic added)

Even Kaplan (1989) suggests that it is the "syntactic" difference between 'Hesperus' and 'Phosphorus' that would explain away Fregean puzzles:

> Lately, I have been thinking that it may be a mistake to follow Frege in trying to account for differences in cognitive values in terms of *semantic* values. Can distinctions in cognitive value be made in terms of the message without taking account of the medium? Or does the medium play a central role? On my view, the message—the *content*—of a proper name is just the referent. But the *medium* is the name itself. There are *linguistic* differences between 'Hesperus' and 'Phosphorus' even if there are no semantic differences. Note also that the syntactic properties of 'Hesperus' and Phosphorus', for example their distinctiveness as *words*, are surer components of cognition than any purported semantic values, whether objectual or descriptional. (598-599)

And finally, in a more recent article, Napoli (1997) brings this line of reasoning to its anti-Fregean culmination:

> Frege had thought that to solve his puzzle, modes of presentation, i.e., senses, should be brought in. After him generations of philosophers have worked hard to make senses affable and effective. Senses have become all the more cumbersome and fat. To no appreciable effect. In the end the only successful move is to make the very expression itself part of the sense. So in the end all the belaboring with the attempt to encapsulate cognitive differential into semantic value has brought us near the simple truth. i.e., that

> the difference in cognitive value of co-referential expressions is accounted for by the differences of the expressions. Semantic values have indeed modes of presentations. Disappointingly the mode of presentation of a semantic value is no other than the expression of which it is the semantic value. (196)

So whether one favors or forsakes Fregean senses, it is "phono-logical" word-type sameness and phono-logical word-type difference that is fundamentally driving the discussion.

In the opening passages of "On Sense and Reference," Frege dismisses the claim that the distinction between 'a' and 'b' has anything directly to do with language. In particular Frege claims that the statements 'a = a' and 'a = b' are not statements about language. In fact, Frege recounts how in his earlier *Begriffsschrift* he did treat such statements as truths regarding the usage of signs, but eventually rejected such a maneuver because (as argued in "On Sense and Reference") the connection between names and objects is *arbitrary*: "You cannot forbid," says Frege in a somewhat Humpty Dumpty-like refrain, "the use of an arbitrarily produced process or object as a sign for something else. Hence, a sentence like 'a = b' would no longer refer to a matter of fact but rather to our manner of designation; no genuine knowledge would be expressed by it" (reprinted in Martinich 1990, 190)).

But it is not so clear that Frege can avoid this kill-joy conclusion anymore than the above comments suggest that direct reference minded theorists wish to avoid it. And this is because the very notions of "logical structure," "syntactic structure," "linguistic differences," and "the same expression" and "different expression" themselves presuppose some basis by which to recognize some linguistic differences as trivially or logically—that is "phono-logically"—insignificant and other linguistic differences as "phono-logically" significant. But how is this to be done? Language-specific

phonological equivalencies do not fall from the sky. But if basic phonological typing is itself dependent upon semantic considerations regarding which tokens refer to what, it is unpersuasive to claim that this sort of information is, in principle, insufficient to guarantee co-naming substitutions. Without "direct reference," failures of substitution in natural language phonology will occur in the same way that failures of substitutivity are held to occur between tokens of /Cicero/ and /Tully/ in belief contexts. In this way, whether identity statements between co-referring names are construed meta-linguistically so their informativeness is accounted for in terms of the designators themselves, or construed within a Fregean framework so their informativeness is explained through the operation of senses, the very conditions which allow for designators to be recognized as the "same designator" or "different designators" is itself conditioned by semantic considerations. This is what it means to say that phonology is "semantically conditioned."

Nevertheless, while the explanation for the informativeness of certain identity statements may just be the word-type difference in the expressions that occur in them, it does not follow that the statements themselves are about those expressions any more than Fregeans need conclude that identity statements are "really" about senses. Both the sentence /Cicero is speaking/ and the sentence /Tully is speaking/ are about someone speaking; the truth condition for both sentences is: Cicero is speaking. The question is not so much what identity statements involving co-referring names are about, but what accounts for their informative value. And whether distinct senses are invoked, or just the fact that phono-logically distinct word-types are involved, traditional Fregean senses are themselves individuated in terms of word-types. Senses are correlated only with distinct

word-types, not various word-tokens of the same word-type. Distinct Fregean senses are not attributed to token distinct exemplars of one and the same word-type (though Kripke's "Paderewski" case is seemingly designed to introduce precisely this complication, see chapter 10 below). As an account of informative identity statements, senses themselves are individuated in phonological terms, terms which are themselves conditioned not by sense-sensitive but reference-sensitive considerations.

The natural language distinction between trivial and informative identities is premised upon the phonological distinction between [a] and [b]. [a] and [b] are phonemically distinct symbol-types. But the typing of phonemes and hence words is itself a derivative product of semantic concerns. And if semantic reference is all that is needed for TK-substitutions, than semantic reference should be good enough for co-naming TP-substitutions. Unless of course, there is a principled philosophical reason to distinguish TK-substitutions from TP-substitutions.

Indeed, the way to block the assimilation of co-naming substitutions to the allophonic substitution of phonology (without risking "phonological eliminativism" via the epistemically divisive introduction of Fregean senses) is to maintain that there is an important difference between allophonic substitutions on the one hand, and co-naming (and synonymous) substitutions on the other. It must be, as Wienpahl (1950) suggests, that basic phonological equivalence can be read directly from *form* whereas co-naming (and synonymous) substitutions require more. In this way, the informative truth of 'a = b' aligns with the distinction between the semantically interpretive "you must look at something besides 'a' and 'b' to know that a = b" and the trivial truth of 'a = a' aligns

with the non-semantically perceptual "you do not have to look at anything except 'a' and 'a' to know that a = a."

Yet in order to press this distinction, the basis of "phono-logical" TK-substitutions must themselves be carefully considered. It would do little good to have phonological TK-substitutions underwritten by considerations of co-naming token reference if co-naming reference is, in principle, insufficient to guarantee substitutivity in belief contexts. But the failure of "phonemic reductionism" requires that semantic information in the form of reference is implicated in even the language-specific basis of natural language phonology. The most natural and intuitively appealing way, then, to prevent the application of Fregean reasoning to TK-substitutions is to assume the truth of "phonemic reductionism." Unlike traditional Fregean TP-substitutional doubt among word-tokens of two different types, TK-substitutional doubt among word-tokens of the same word-type is not possible simply because TK-substitutions are based upon, or better enforced upon us in virtue of, uninterpreted perceptual form.

So while Wienpahl's reliance on the verb "look" is suggestive of a non-interpretive, perceptual analysis of basic word-token equivalence, neither he nor Frege are properly considered "phonemic reductionists." Frege's original observation and Wienpahl's remarks above are merely consistent with "phonemic reductionism" and neither explicitly commit themselves to it. Instead, we are interested in how Fregeans can be Fregeans with respect to TP-substitutions without also being Fregeans with respect to TK-substitutions. A distinction between "phono-logical" TK-substitutions based upon uninterpreted phonetic form versus TP-substitutions based upon a semantic convergence

of sense would allow for a philosophically principled distinction to be drawn between TK-substitutions and TP-substitutions.

But while Frege and Wienpahl are arguably implicit "phonemic reductionists," Quine is explicit. For Quine, a non-semantic reduction of basic phonological equivalence is a specific objective. Whether typed by some measure of phonetic, perceptual, or perhaps behavioral similarity (see chapter 7 for details), basic phonological equivalence must not be grounded in anything semantic. In particular, if phonological equivalence is not to be subject to the same eliminative skepticism Quine subjects analytic equivalence to, meaning must not be implicated in the recognition of basic phonemic categorization. So while Fregean reasoning introduces a bias against co-naming TP-substitutions within belief contexts (thereby setting up complications for co-naming TK-substitutions in belief contexts), Quinean reasoning introduces a bias against synonymous TP-substitutions (thereby setting up complications for synonymous TK-substitutions)—unless, that is, "phonemic reductionism" is true. If true, the thesis of phonemic reductionism immediately defuses both these Fregean and Quinean phonological complications.

Quine's Explicit Phonemic Reductionism

Indeed, it is Quine (1953a, 1960, 1981, 1990) rather than those following in Frege's footsteps, who is most responsible for philosophically promoting the thesis of "phonemic reductionism." Quine does this for he perceives a meaning-based account of phonemic equivalence to be a threat to an overall philosophical agenda of a behaviorist philosophy purified of meaning. Nevertheless, Quine's reasoning is structurally identical to that of Frege, even if it is the skeptical elimination of meanings rather than the detection of

senses that motivates him. Consider that while Fregeans generate distinct senses to account for what *prevents* co-referring names from freely substituting, Quine moves to eliminate that which supposedly *permits* synonymous predicates (e.g., /attorney/ and /lawyer/) unlike co-extensive predicates (e.g., /renate/ and /cordate/) to substitute freely, namely their meaning.

Recall that in the opening pages of "Two Dogmas," Quine (1951) too invokes a distinction between 'a = a' versus 'a = b'. His examples are, of course, based on the strictly logical status of substitutions between /bachelor/ and /bachelor/ versus the analytic status of synonymous substitutions between /bachelor/ and /unmarried man/. But while traditional semanticists tend to use this contrast to argue for the existence of meaning equivalencies across phonologically distinct predicates, Quine—antecedently dissatisfied with the identity conditions for properties and meanings—uses it to argue for the inexistence of meaning-based analyticity. Both Fregean and Quinean arguments, however, trade on the paradigmatic distinction between 'a = a' truths on the one hand and 'a = b' truths, on the other. It is just that whereas Frege concentrates on co-referring names and moves to explain their substitutional failure in terms of sense, Quine concentrates on co-meaning predicates and moves to eliminate synonymy as a so-called explanation for their substitutional equivalence. In fact, Quine no more needs senses to keep co-referring names distinct, nor meanings to keep synonymous predicates together: Fregean puzzles are to be resolved not in favor of senses but at the expense of belief,[16] and intuitions of synonymy are at best holistically explained away rather than explained.

Nevertheless, it is clear that Quine does perceive a critical difference between tautological TK-substituitions involving word-tokens of the same word-type and TP-

substitutions involving tokens of distinct word-types. Insofar as a distinction between the strictly logical truths of the 'a = a' kind versus the analytic truths of the 'a = b' kind is drawn, Quine too must lean on some sort of basic token/type distinction to characterize the former in contrast to the latter. But the same question arises. What is it that licenses the substitution of one token of /bachelor/ for another token of /bachelor/? What is, in effect, the basis of "phono-logical" substitutions between [bachelor], [*bachelor*] and [BACHELOR]? Rather than asking Frege how name word-types are individuated, the question for Quine is: how are predicate word-types individuated? Are considerations of predicate-token meaning required to substantiate phonological substitutions among word-tokens of the same predicate word-type, or are non-semantic considerations sufficient?

In order for Quine's analytic eliminativism not to trigger his own version of "phonological eliminativism," Quine argues that the phonemic typing of phonetic tokens must be explained in terms of some sort of objective phonetic similarity based upon their token physicality. But while reductionist views like this once attracted a lot of linguistic attention, Quine's candid defense of "phonemic reductionism" as late as 1990 sets him apart. In fact even when faced with empirical pressures undermining phonemic reductionism, Quine pushed ahead with a behaviorist reduction of phonemic equivalence. But behaviorist accounts of phonemic equivalence are philosophically more misleading rather than phonologically revealing (chapter 7).

No doubt Frege and Quine make for strange philosophical bed fellows. But this is more a reflection of just how over-accommodatingly large the bed of "phonemic reductionism" is rather than an indication of substantive philosophical agreement. Both Fregean intuitions governing the substitutional status of co-referring names and Quinean

arguments governing the substitutional status of co-meaning predicates are premised upon drawing a distinction between those substitutions which are not "phono-logically" guaranteed—synonymous substitutions for Quine (and Mates, though for different reasons) and co-naming substitutions for Frege—and those which are phonologically guaranteed. Yet even phonological substitutions do not come for free. They too are semantically conditioned. Unlike Frege (who does not make any claims about phonology), Quine explicitly endorses "phonemic reductionism" in order for his "philosophy of phonology" to match his philosophy of language. The over-all success of Quine's meaning skepticism, then, is contingent upon successfully "reducing the phoneme," the topic of chapter 5.

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[14] One might even reason like Marcus. Marcus (1981) claims that it is not really possible to believe Cicero is not Tully—asserting that "If I had believed that Tully is not identical with Cicero, I would have been believing that something is not the same as itself and I surely did not believe that, a blatant impossibility, so I was mistaken in claiming to *have* [that] belief" (505). Those who doubt whether the *word* tokens [Cicero] and [CICERO] are substitutionally equivalent would be guilty of trying to believe that something was not the same as itself (i.e., /Cicero/), a blatant impossibility, so must be mistaken in claiming to have had that belief.

[15] "Dot quotation" is restricted to names and synonyms. A number of general considerations are relevant to this restriction (none of which are phonological in nature): (i) names, unlike descriptions, are typically monomorphemic, or at least not as polymorphemic, as descriptions deploy a concatenation of multiple words (with distinctive quantificational structure) to describe their referent rather than a single word to name it; (ii) though morphemic complexity is no doubt subject to all sorts of cross-linguistic diversity, the way definite descriptions behave in modal contexts arguably is not. The claim then, that /Cicero/ and /Tully/ are substitutionally equivalent (regardless of what any one in particular may happen to take themselves to believe), parallels the Kripkean claim that while Cicero is necessarily Tully (insofar as he exists), it is merely contingent, say, that Cicero/Tully happens to be the most famous Roman orator, and hence rightly subject to epistemic uncertainty. But even if modal intuitions are themselves subject to all sorts of cultural, linguistic, epistemic, and philosophical biases, presumably (iii) some basic distinction between words which refer (and even empty names that fail to refer) and phrases which describe is not to be rejected lightly. See also Marcus (1961).

In a footnote, Salmon (1987) also seeks to distinguish between names and definite descriptions when counter-intuitively proving that "the fact that x = y, if such a thing is indeed a fact, is just the fact that x = x" (518), referring the reader to Salmon (1986c) for further discussion: "The proofs apply no matter what values are assigned to these variables. The proofs can be extended unaltered to cases in which the variables are replaced with individual constants, indexicals, pronouns, or proper names (or any combination), but not to cases in which one (or both) of the variables is replaced with a definite description, because of a needed restriction on substitutivity . . . in such cases. For further relevant details see my [Salmon 1986c]" (518).

[16] Unlike Frege, Quine does not rely on senses to explain why co-referring names might not substitute freely. Instead, Quine points the finger at our "folk psychological" idioms of belief and the propositional attitudes generally for introducing systemic ambiguity into language. Quine argues that the propositional attitudes are, like modal and quotation contexts, defective for creating various forms of opacity and must be shunned for this reason.

FROM A PHONO-LOGICAL POINT OF VIEW

Chapter 5
Neutralizing Quinean Arguments Against Analyticity

Let's begin with the fact that even Quine has to admit that it is possible for two tokens of the same orthographic type to be synonymous, for that much is presupposed by his own account of logical truth.

P. Boghossian (1999, 343)

Logical, Analytic and Synthetic Truth

Ours is a philosophical environment deeply colored by Quine's arguments in "Two Dogmas." Yet it is important to recall that by way of introducing his critique of analyticity, Quine actually draws a three-way distinction among logical, analytic, and synthetic truths. Concerned as he is with the analytic/synthetic distinction, Quine (1951) briefly notes the logical status of tautological substitutions to contrast them with the semantic nature of analytic substitutions, and advances from there to pursue a sustained critique of the latter. Analytic truths are, in principle, no more immune to empirical revision than synthetic truths (and synthetic truths no less "analytically" invulnerable). According to Quine, there is no philosophically principled distinction to be had between semantically based analytic truth and empirically based synthetic truth. Analytic truths holistically bleed into empirical truths. From this perspective, "meaning" is just an elaborate network of firmly coagulated beliefs, deeper and more vivid in color, but different merely in degree rather than kind from all other beliefs. Quine does not, however, question a distinction between logically tautologous substitutions and analytic substitutions. For while undermining a traditional analytic/synthetic distinction, Quine helps himself to the presumption that substitutions merely between two word-tokens of the same type, say, [*bachelor*] and [BACHELOR]—in contrast to substitutions between

tokens from two different types, say, [bachelor] and [unmarried man]—are free of the kind of semantic considerations that trouble analytic substitutions.

But whereas Quine (1951) perceives a dogmatic distinction between the analytic and the synthetic, and proceeds to assimilate the former to the latter through the notion of epistemic centrality, there exists another though less visible dogma of empiricist extraction: a distinction between semantically independent tautological substitutions and analytic, that is semantically dependent substitutions. Central, however, to exposing the flawed nature of this distinction, and recovering from its philosophical liabilities, is a reassessment of basic natural language phonological equivalence, indeed, Quine's own account of basic phonological equivalence. Notwithstanding Quine's relatively recent and what appear to be his final remarks on the phoneme in an article entitled "The Phoneme's Long Shadow" (1990), still more light needs to be thrown on an issue basic to linguistics, but critical to the overall success of Quine's attack on analyticity. Propelled by the observation that even basic natural language allophonic equivalencies among phonetically distinct variants of the same word-type are, pace Quine, themselves dependent upon semantic considererations Quine takes to be illegitimate—any attack on analyticity on account of its semantic recalcitrance has a number of philosophical repercussions.[17]

Phono-Logical Truth: Tautological Substitutivity

On the one hand, and within analytic philosophy of language, questions of linguistic equivalence are traditionally addressed by considerations of meaning, synonymy, and analyticity and related considerations of intensional opacity. On the other hand, and within phonology, issues of linguistic equivalence revolve around the notion of the

phoneme: two word-tokens are phonemically equivalent—indeed the same (type of) word—if they are composed of the same phonemes. And distinct phones are themselves phonemically equivalent if their substitution (see the chapter 2 discussion of "once a phoneme always a phoneme") never has any semantic effect. Though phonologists are concerned with comparing phonetically distinct word-tokens and philosophers with comparing phonemically distinct word-types, their concerns, though different, run parallel to each other. In both cases, they are concerned with the conditions, if any, under which two distinct linguistic specimen are considered linguistically equivalent, and what these conditions might be—even though the kinds of equivalencies (TK-substitutions versus TP-substitutions) are different.

In natural language phonology, substitutions among different word-tokens are widespread and routinely occur among distinct allophonic productions of the same word-type. Within the philosophy of language, however, matters point in the opposite direction: Fregeans maintain that co-referring names (and perhaps even synonymous words) provoke failures of substitution (chapters 3 and 4), while Quineans views analytic substitutions with eliminative suspicion.

Given the prominence of Quinean arguments attacking analyticity, little time need be spent reviewing how a tight circle of analyticity, synonymy, and necessity makes it difficult to square the intuitive meaning equivalence of such linguistic pairs as /bachelor/ and /unmarried man/ or /attorney/ and /lawyer/. Suffice it to say that, unlike substitutions between, say, tokens of /attorney/ and /lawyer/ (substitutions between two different phonological types), strictly "phono-logical" substitutions between say, [*attorney*] and [ATTORNEY]—two tokens of the same type—are presumably not subject to Quine's

eliminative skepticism. Unlike basic "phono-logical truths" equating two distinct tokens of the same word-type, analytical truths equating synonyms can be converted into strictly logical truths only by appealing to the interpretive content the two synonymous tokens are assumed to have in common. As Quine (1960) explains in *Word & Object* (with respect to sentence synonymy),

> The general relation of intrasubjective sentence synonymy thus unsuccessfully sought is interdefinable with another elusive notion of intuitive philosophical semantics: that of an analytic sentence. . . . The interdefinitions run thus: sentences are synonymous if and only if their biconditional (formed by joining them with 'if and only if') is analytic, and a sentence is analytic if and only if synonymous with *self-conditionals* ('If *p* then *p*'). (65; italics added)

Rereading the opening pages of "Two Dogmas," it is clear that Quine considers analyticity to fall short of the more stringent standard of strictly logical or tautological—or as with the above passage from *Word & Object* "self-conditional" (e.g., $p \rightarrow p$)—substitutions among tokens of the same type. Simple inferences from [p] to [p] are not based upon semantic considerations of meaning. Rather inferences of this kind are premised merely upon basic phonological form. (Note that logicians typically use 'p' as a schematic letter-type, not letter-token. When logicians use schematic letters, it is an understood convention—often made explicit—that in a single argument context all instances of the same schematic letter must be replaced by instances of the same sentence. This convention of "instances of the same schematic letter"—whether explicit or implicit—guarantees that all substitution token-instances of the same schematic letter-type actually are drawn from the same schematic letter-type. These practices, however, clearly presuppose some basis by which to recognize various tokens as token instantiations of the relevant type.)

Unlike inferences informed by analytic considerations of meaning, then, self-conditional or tautological inferences (among tokens of the same type) are not indebted to meaning, but rely merely on considerations of uninterpreted linguistic form. If this were not the case, there would be no philosophically principled distinction distinguishing basic phono-logical substitutions from analytic substitutions. Indeed, if tokens of /attorney/ and /lawyer/ could be equated merely in terms of their perceptually accessible properties, say of phonetic quality or orthographic shape—rather than their semantically interpreted properties—the inter-related issues of analyticity and synonymy would presumably dissolve in the same way they apparently do with respect to the distinct but substitutionally equivalent tokens [*attorney*] and [ATTORNEY] (like allophonic speech variants generally), without any assistance from semantics. Unlike synonymous substitutions among phonemically distinct word-tokens, simple "phono-logical" substitutions among phonemically equivalent word-tokens are licensed merely in virtue of "form," not content.

Whether by some relatively simple measure of psychophysical similarity, or a more complex distributional comparison of phonetic features technically known as "complementary distribution" (Bloch 1953, Harris 1951; see Chapter 2 and note 8 for definition of "complementary distribution"), or even some sort of behavioral index (Quine 1987, 1990), any legitimate classification of linguistic items must, for Quine, be based upon some sort of "bottom-up," though undoubtedly liberal, measure of perceptual similarity. (Chapter 7 reviews these various proposals in detail.) After all, the force of Quine's argument against analytic equivalence, if indeed it is to be directed only at analyticity rather than also undermining natural language phonology, presupposes that,

unlike analytic equivalence, basic phonological equivalencies actually do yield to an explanation that is independent of semantic considerations. No doubt this is one reason why Quine's eliminative skepticism directed against appeals to meaning to prop up the so-called "analytic" equivalence of /attorney/ and /lawyer/ is matched by his reductive confidence that basic phonological equivalencies among various token pronunciations of one and the same word are not—could not be—beset by such semantic obscurities (Quine 1953a, 1981, 1990).

Apparently aware of the tactical importance of providing a non-semantic defintion of basic phonological equivalence to contrast with the semantically dependent definition of synonymy, it is perhaps unsurprising, then, that Quine would demand an account of basic phonological equivalence untainted by semantics. For that matter, both Quine and Nelson Goodman have argued for the non-semantic reduction of basic phonological or orthographic equivalencies (see chapter 7 for details). Notably Goodman's (1972) notion of visual "replication" and Quine's (1953a) adoption of psychophysical acoustics as the basis of phonemic categorization were intended to guarantee that the "bottom-up" perceptual cues of visual shape and acoustic sound are what ground basic natural language phonemic equivalencies—not meaning. Taking another example, Frank Ramsey (1950) writes that "tokens (like those of any sign) are grouped into types by physical similarity (and by conventions associating certain noises with certain shapes) just as words," adding "But a *proposition* is a type whose instances consist of all possible sign tokens which have in common, not a certain appearance, but a certain *sense*" (274; italics original). As for the role abstract propositions, meaning and semantics plays in

accounting for basic phonological equivalence, Quine would have it that philosophers do not want it and linguists do not need it.

But while the proposals suggested by Goodman (1972) and Quine (1953a) tend to reveal more of their commitment to individuating linguistic types independent of meaning rather than serious accounts of basic phonological or orthographic equivalence (and both acknowledge this[18]), this is not to suggest that no empirically credible accounts of basic phonological equivalence are available. Specifying precisely how two word-tokens may differ but still exemplify the same word-type is among the professional pursuits of phonologists. In fact, one of the most significant objectives of early twentieth-century phonology, particularly as practiced in the United States among a vibrant if ultimately overshadowed group of so-called "post-" or "neo-Bloomfieldian" distributional linguists (Hymes & Fought 1981, Koerner 1978), was to discover the underlying objective reality governing the intuitive distinction between significant and insignificant sound differences in natural language. Viewing themselves as the, though largely symbolic, heir to Leonard Bloomfield's linguistic leadership (Fought 1999a, 1999b), their positivistically oriented linguistics was premised upon the minimization (Harris 1951), if not elimination (Bloch 1953), of interpretive content in an effort to identify the objective basis of phonological equivalence among phonetically distinct tokens without the aid of any sort of semantic considerations. (This linguistic history is the topic of chapter 6.)

To their credit, if not success, such asemantic methodological restrictions are far from trivial. For not only do different languages differ as to how they cross-classify the same sounds (thereby spoiling any universal standard of phonological equivalence),

languages internally generate both systematically rule-governed and ungoverned variations in the pronunciation of its own sounds. More specifically, the phonologist is saddled with the task of explaining why native speakers reveal subtle patterns of phonetic variation whereby the "same" sound is sometimes produced one way and sometimes produced another way—a phenomenon technically known as "allophonic variation" or "sub-phonemic variability."

The *Reductio* Threat of "Phonological Eliminativism"

To be sure, none of the phonotactically conditioned variants in "complementary distribution" or phonotactically unconditioned variants in "free variation" are as substitutionally radical as those which typify analytic substitutions between synonyms or translations. In fact, for the native speakers who regularly produce and hear them, such allophonic discrepancies (like aspiration in English) typically border on the audibly imperceptible and articulatory unproducible (Strange 1995). But however seemingly negligible, neither is there a set of invariant acoustic, perceptual or even exhaustive distributional cues to signal phonemic equivalence among phonetically distinct tokens (see, e.g., Pike 1958, Twaddell 1935). "Phonemic reductionism"—in all its various forms—is false. The only feature which ultimately determines phonemic equivalence in the cases of naturally occurring free allophonic variation is the semantic equality/inequality of the tokens.

But this is really nothing new. As Bloch & Trager (1942) explain,

> [A] purely phonetic description makes it impossible to distinguish the really significant features of the vocabulary and the grammar from the accidental and personal features which inevitably form part of every utterance; as a scientific procedure it is about as

fruitful as it would be for a biologists to assign two cats to different species because one had more hairs on its tail than the other. (39)

But given the failure to substantiate strict phonemic reductionism, the fact that two identifiably distinct phonetic forms are considered to be "phonologically equivalent" raises at least as many issues as does the philosopher's quest to explain—or explain away—intuitions of "analytic equivalence" between forms as diverse as /bachelor/ and /unmarried man/. Yet unlike linguists who have conceded the irreducible role interpretive semantics plays in the basic phonological typing of phonetic tokens, Quine has always remained committed to phonemic reductionism.

Indeed, picking up where some prominent "neo-Bloomfieldian" linguists left off, Quine endorses a behavioral test in an effort to avoid tainting basic phonemic equivalence with semantics. But raw behavior stripped of interpretive significance is no more a key to unlocking the phonemic structure of a language than raw speech stripped of its interpretive significance is. In fact, Quine's attempts, like those tendered by Zellig Harris and a young Noam Chomsky in the form of their behavioral "pair test," are false, trivial or circular—discussion of which is postponed until chapter 7.

In any event, the short-circuited future of "phonemic reductionism" is not nearly as interesting as its philosophical repercussions vis-à-vis Quine's influential critique of analyticity. For once the tempting but false premise of phonemic reductionism distinguishing basic "phono-logical" equivalence from analytical equivalence is uncovered, the Quinean case against analyticity—for being dependent upon meaning—looks different. Analyticity cannot be painlessly faulted for being meaning dependent because our "phono-logical" substitutions—at least those premised upon word-tokens instantiations of the same predicate—are also dependent upon considerations of meaning.

Even the basic "phono-logical" truths of language governing allophonic variation fall short of Quine's standards insofar as TK-substitutions involving predicates are to be individuated in terms of properties rather than mere co-extensionality. So while all of [cordate], [renate], and [RENATE] are tokens of co-extensive predicates, only [renate] and [RENATE] are word-token variants of the same word-type. An inability to distinguish between contingent co-extensive predication and synonymous co-meaning predication dissolves the distinction between word-tokens of the same predicate word-type and word-tokens of co-extensive predicate word-types.

As discussed in Chapter 2 in connection with a Fregean appeal to word-token senses to license basic phonological equivalence among tokens of co-referring names, Quine could dismiss word-token predicate meaning in favor of mere word-token predicate extension. (In fact, Quine's behavioral analysis of phonemic contrast based upon "stimulus meaning" has this effect, see chapter 7). But basing phonological equivalence upon considerations which do not distinguish between tokens which express the same property and tokens which apply to the same objects collapses the phonemic distinction between tokens of co-extensive predicates. Phonology, like analyticity, would be blind to synonymy: phonetically distinct tokens of co-extensive predicates which are typed as "minimal pairs" on the basis of a difference in meaning would be typed as mere allophonic variants in virtue of their extensional equivalence. So whereas Fregean senses inspired by the need to explain substitution failure *increase* the number of phonemically available distinctions, Quine's rejection of predicate meaning decreases the number of phonemically available distinctions. The Fregean introduction of senses asks us not to phonologically equate tokens like [mat] and [mat^{h}] so long as their co-reference can be

doubted. By contrast, the Quinean rejection of meaning asks us to phonologically equate tokens like [dap] and [daf] (the "minimal pair" analogue of [renate] and [cordate]) so long as their predicate extension is the same.

If the problem with analytic equivalence is that it requires a distinction between contingently co-extensive predicates and synonymously co-property predicates, then it would seem that the individuation of basic phonological equivalence incurs the same burden. This is because phonology itself distinguishes between predicate-tokens which are merely co-extensive and predicate-tokens which are phonologically equivalent. Without meaning, both analyticty and phonology are undermined. After all, as Boghossian (1999) notes "Let's begin with the fact that even Quine has to admit that it is possible for two tokens of the same orthographic type to be synonymous, for that much is presupposed by his own account of logical truth"[19] (343).

There is, then, philosophical parity between analytically equating tokens of /attorney/ and /lawyer/ and phonologically equating free allophonic word-token variants of the same predicate, at least when it comes to the issue of predicate meaning. Tokens of /attorney/ and /lawyer/ do not obviously look or sound anything alike, but surprisingly, neither do free allophonic variants. Free allophonic variants do not "naturally" sound alike—just ask someone unfamiliar with the basic allophonic rules governing English phonology. In languages like Hindi and Thai, for instance, alternation between aspiration and non-aspirated voiceless stop consonants is phonemically contrastive, not allophonic. There is, in this way, no reason to expect the analytic equivalence of tokens of /attorney/ and /lawyer/ to be any more accessible without meaning than the phonological equivalence between word-tokens of the same predicate. Both the phonological

equivalence of free allophonic word-token variants of the same predicate and the analytic equivalence of /attorney/ and /lawyer/ are equivalencies conditioned by meaning independent of phonetic considerations intrinsic to the language and that of mere predicate co-extensionality.

This basic symmetry between phonological and analytic equivalence, however, has been overlooked because Quine's attack on analyticity highlights the semantics of synonymy without taking into account the semantics of phonology. Further obscuring the connection between analyticity and phonology is a widespread assumption among philosophers that phonology is just about the sounds of words of no philosophical relevance. But it is Quine's own writings on the phoneme intended to free phonology from semantics which reveal that at least Quine was aware of the tactical importance of providing a non-semantic account of basic phonological equivalence. Without it, Quine's argument against analyticity is an argument against phonology. Phonology—the basis of our everyday intuitions governing simple tautological truth—is semantically conditioned. The information which determines which linguistic forms are tautologically equivalent is itself at least partially based upon interpretive semantics. For this reason, a linguistically more accurate, a developmentally more realistic, and a philosophically more consistent approach to synonymy must expose this widespread dualism between form-based "phono-logical" substitutions and meaning-based analytic substitutions—implicit in Quine's attack on analyticity—as yet another dogma-like presumption within empiricist philosophy of language. Linguists have, in their own way, sought to recover from a semantically irreducible phoneme. [20] But it is philosophers—ironically largely responsible for the "neo-Bloomfieldian" pre-occupation (see Koerner 1978, Bloomfield

1944) with "reducing the phoneme" in the first place—who have yet to come to grips with some of its philosophical implications. A closer look at the positivist historiography of "neo-Bloomfieldian" linguistics, the topic of the next chapter, helps to clarify the relationship between Quine's philosophy of language and their style of linguistic philosophy.

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[17] Given the unresolved debate between Quine and Strawson, it is important to distinguish Strawson's argument from the present argument, though both are critical of Quine for relying on an artificial notion of logical truth independent of any semantic issues. In response to Quine's attack on analyticity, Strawson (1957) argues that Quine's own account of logical truth is parasitic on the very notions Quine seeks to dismiss. Strawson does this by pointing out that logical substitutions require one to rule out homonyms and indexical expressions which vary in reference depending upon their semantic interpretation: only substitutions that keep the same reference are logically valid, using the example 'If he is sick, then he is sick' to stress that the first occurrence of 'sick' can be interpreted as mentally sick while the latter can be interpreted as physically sick, conspiring to make a potentially false conditional. How are such basic logical inferences to be protected from such ambiguity? "The apparently unavoidable answer to these questions" says Strawson, "is that typographical identity of sentences or recurrences of sentence-letters (as Quine calls them) represent or symbolize identity of *statements* or *propositions* or whatever we call those things which are true or false. (The terminology of 'sentence-letters' is objectionable in so far as it obscures this important fact.) If this apparently unavoidable answer is right then we have no adequate characterization of logical truth until we have an adequate criterion of identity of statements or propositions. . . . We may, that is to say, characterize the difficulties as follows. Some arise from the fact that typographical identity of referring expressions does not guarantee identity of reference; others arise from the fact that typographical identity of predicate expressions does not guarantee identity of sense" (19-20). Though Quine (1960) reports that this criticism is "an interesting one, which I cannot claim to have answered anywhere" (65; footnote 3), at no point does Strawson attempt to undermine Quine's critique of analyticity by questioning the nature of "typographical" identity itself. So while Quine (1960) expresses reservations about "embrac[ing] the analyticity of the truths of logic as an antecedently intelligible doctrine" (65) (and refers the reader to his article "Truth by convention"), he does this on the basis of a *holistic* rejection of a sharp demarcation between truths of logic and truths of fact. Both Quine, and it would seem Strawson too, however, take it for granted that at least non-homophonous "typographical identity" is an unproblematic notion which does not require the introduction of any semantic concerns in order to be defined.

[18] See chapter 7. Both Goodman and Quine freely share their own doubts regarding "similarity" and the viability of defining basic phonological equivalence in non-semantic terms. Goodman (1972) concedes this as his second of "Seven Strictures on Similarity." As for Quine, after outlining a psychophysical account of phonemic equivalence, Quine (1980) notes that his attempt at phonemic reductionism is severely inadequate. This does not, however, stop him from then re-asserting his philosophical conviction that the phoneme can, or rather must, be defined independently of semantic considerations.

[19] This quote from Boghossian (1999) continues with: "As we saw in the passage I quoted above, Quine describes a truth of logic as: 'a statement which is true and which remains true under all reinterpretations of its components other than the logical particles.' Clearly, the idea isn't that such a statement will remain true no matter how the non-logical particles are substituted, but rather that it will remain true provided that the non-logical particles are substituted in a uniform way, with multiple

occurrences of the same word receiving the same substitution in every case. But what should we count as the same here? . . . So even Quine has to admit—what in any event seems independently compelling—that two tokens of the same type can express the same meaning" (343-344).

[20] Not so much, however, by accepting a semantically defined phoneme, but more by re-orienting linguistics and moving on to different concerns. Indeed, the Chomskyan framework of generative phonology neither provides, nor aspires to provide, any account of basic phonological classification (semantic-based or otherwise). So while fundamentally rejecting the "phoneme" as an independent unit of linguistic analysis and representation, Chomsky is content to treat phonological equivalence, and its opposite phonological contrast, as "primitive" notions (Chomsky 1964, 83). See also Postal (1968) who also wishes to treat phonemic categorization as primitive rather than attempt to explain the basis of these language-specific categories.

Chapter 6
Linguistically Applied Logical Positivism: "Neo-Bloomfieldianism" and the Philosophical Foundations of American Distributionalism

> In our wording, we shall avoid all semantic and psychological criteria. The implication is, of course, that such criteria play no part, or at least need not play one, in the theoretical foundations of phonemics. . . . The basic assumptions that underlie phonemics, we believe, can be stated without any mention of mind or meaning.
>
> B. Bloch (1948, 5)

Distinctive among the objectives of a dominant, if ultimately superceded, movement in pre-Chomskyan American structural linguistics was the desire to identify the underlying reality governing the intuitive distinction between linguistically significant and insignificant sound differences within a language. Dissatisfied with the linguistic methodology of various contemporaneous circles of linguistic thought, but confident that Leonard Bloomfield's linguistic ideals were the solution, a group of American linguists began to chart an agenda of linguistic analysis which was, both knowingly and unknowingly, indebted to the philosophical principles of Logical Positivism. Commonly identified as a potent if not entirely homogeneous force within American structuralist and descriptivist linguistics, the term most often associated with this program is "neo-" or "post-Bloomfieldian" distributionalism, though this somewhat self-appropriated eponymous label tends to obscure rather than reveal some of the Leonard Bloomfield in "Bloomfieldianism" (see especially Crawford 1985, Fought 1999a, Fought 1999b, Fowler 1965, Fries 1961, Hall 1987a, Hall 1987b, Murray 1993, Teeter 1970[21]).

Viewing themselves as the heir to Bloomfield's linguistic leadership and moreover, as the first generation of linguists positioned to fully implement some of his more postivistic aspirations, they sought to bring the study of language completely into

the scientific fold of objective observation and quantitative analysis. Among their most notable goals was a definition of phonemic equivalence untainted by semantic considerations (though see Koerner 1978; Hall 1987a, 1987b; and Hockett 1968 for qualifications regarding "extreme" and more "moderate" "Bloomfieldians" on this issue[22]). Yet the lighting rod conductivity of this position has tended to obscure the basic motivation, and in particular, the classically empiricist rationale and justification for "phonemic reductionism." Reducing the phoneme may have characterized an extreme, and in any event unsuccessful movement in linguistics, but such was part of a larger linguistic ambition of—or as some of its soon-to-follow detractors would have it, myopic obsession with (see Chomsky 1964, Lees 1957, Postal 1968) [23] —observational foundationalism and terminological reductionism; a scientific vision that might be usefully summed up as linguistically applied logical positivism. For better or worse, "neo-Bloomfieldian" linguistics answered the question of what linguistics would look like if the philosophical force of logical positivism were brought to bear on the science of linguistics.

Though in the end, these linguists helped to prove themselves wrong—basic token-to-type phonemic classification depends (just as Bloomfield had always maintained[24])—upon an ineliminable residue of semantic knowledge, they did not just haphazardly make up their asemantic linguistic convictions. Indeed, a theoretical commitment to "phonemic reductionism" tended to over-ride its intermittently self-recognized empirical shortcomings. In this way, at least some of "neo-Bloomfieldian" linguistics was an instance, as some participants confessed at the time (Bloch 1948, Bloch & Trager 1942, Hockett 1949), where theory really was more important than

practice.[25] So even though, as Kenneth Pike (1947b) complained, "[t]here must be something wrong with . . . phonemic theory if workers agree on the practical value of a procedure (and of evidence) in the field which they then rule out in theoretical discussion and in presentation" (159), such discrepancies between theory and practice are ideal for exploring the philosophical basis and aspirations of "neo-Bloomfieldian" linguistics. A commitment to reducing the phoneme was not a simple empirical oversight. Instead, a distinctive philosophy of phonology issued from the combined goals of a certain kind of methodological consistency and theoretical clarity.

Guided in large part by the philosophical rhetoric if not actual descriptive work of Bloomfield, neo-Bloomfieldian linguists tended to proudly, almost defiantly, approach the study of language with a concentrated form of anti-metaphysical, anti-subjectivist and anti-mentalistic rigor. As even Bloomfield (1939) himself explained in his linguistic contribution to the Positivists' focal publication, the *International Encyclopedia of Unified Science* (edited by Otto Neurath and Rudolf Carnap), the methodological commitments he outlined there were not—and were never considered to be—substantive empirical discoveries, but rather necessary theoretical prerequisites in order for scientific progress in linguistics to actually occur. Bloomfield and his followers "hitched their wagon" (Anderson 1985) as it were, to the philosophy of Logical Positivism, confidently propelled by a scientific positivism then sweeping through Anglo-American philosophical circles. Yet even then, not everyone was so sanguine.

While some contemporary linguists in the 1940s and 1950s merely noted that "A general characteristic of the methodology of descriptive linguistics, as practiced by American linguists today, is the effort to analyze linguistic structure without reference to

meaning" (Carroll 1953, 31), other linguists were not so disinterested in their remarks. As the British linguist John Firth observed, "Certain leading linguists especially in America find it possible to exclude the study of what they call meaning from scientific linguistics, but only by deliberately excluding anything, in the nature of mind, thought, idea, concept. 'Mentalism' is taboo" (1951, 82). Similarly, Lounsbury (1962) observed, "An avoidance (one could even say abhorrence) of meaning as a criterion in linguistic analysis" was a "distinguishing feature" of American linguistics in the 1940s and 1950s (281; reprinted in Fought 1999a, 146). No doubt, as one linguist complained, "The minimizing of meaning as a factor in linguistic description was at first a healthy reaction against the misuse of meaning in establishing linguistic categories" but it had since "become almost a fetish with some linguists" (Haugen 1951; as reprinted in Joos 1957, 362). (See Hymes & Fought 1981, however, for a critical reappraisal regarding the extent of this "rejection" of meaning among American linguists.)

But "far from being disturbed by these criticisms," Stark (1972) reminds us that these "American linguists seem to feel that it is precisely this neglect of meaning that has made their methods uniquely scientific and rigorous" (146). And it is for this reason that neo- or post-Bloomfieldian distributionalism should not be viewed just as a behavioristically oriented linguistics. Rather, the fountainhead of "Bloomfieldianism" runs deeper. If behaviorism can be viewed as the psychological partner to Logical Positivism (Smith 1986), neo-Bloomfieldian distributionalism can be thought of as the progeny of a marriage between Logical Positivism and linguistics. In short, neo-Bloomfieldian distributional linguistics was more the convergent result of applying classically empiricist epistemological principles of knowledge reconstruction—given

renewed and sharpened expression through Logical Positivism—directly to the incipient American institutionalization of the modern scientific study of language.

The Autonomy of Linguistics

So while the distributionalist prohibition against the use of meaning to identify basic phonological equivalence was neither philosophically or methodologically arbitrary nor, as we shall also see, linguistically trivial, it was also partially fueled merely by a desire to establish an autonomous science of synchronic linguistics. Some one hundred years ago, as the primacy of the comparative method of the nineteenth century gave way to the synchronic structuralism of the twentieth century, linguistics—as a discipline onto itself—self-consciously sought to articulate its boundaries and methodology. In this way, the new science of linguistics was to be distinguished from the historically related disciplines of philology, psychology, anthropology and philosophy, as well as the on-going concerns of historical and comparative linguistics (see Bugarski 1999, Joseph 2002, Lane 1945). Languages, it was argued, could be accurately described independently of both their own history, as well as the cultural, social and individual peculiarities of their users.[26] And over the course of several decades, Leonard Bloomfield was the American epicenter of these efforts. As early as 1925 Bloomfield helped to create the Linguistic Society of America, wrote its very first flag-ship article entitled "Why a Linguistic Society?" and served as its president in 1935. Bloomfield's inaugural article for the journal *Language* was a veritable declaration of independence from "the existing societies, Philological, Oriental, Modern Language, Anthropological, Psychological, and what not" given that most of them, according to Bloomfield, were not

even aware "that there is a science of language" (1925, 1). As Randy Harris (1993) succinctly put it, "the modifiers on either side of *Society* say it all" (22): specifically linguistic and separated from Europe. And if distinguishing the future of a modern, scientific linguistics from the past of European linguistics was an objective, then avoiding semantics was one strategy that could hardly fail. As Newmeyer (1986c) explains:

> To the Europeans, understanding the role of language in conveying meaning was paramount; consequently they devoted considerable attention to the semantic function of the units they arrived at in their structural analysis. Their preoccupation with meaning meant that they were constantly abutting on fields like philosophy, psychology, and criticism, which also studied meaning. (6)

In this way, an avoidance of semantics not only divided American structuralists from their European counterparts, it simultaneously contributed to the institutionalization of linguistics as an autonomous discipline.

Even Bloomfield's early "neo-grammarian" commitment to the autonomous regularity of sound change is indicative of a desire to ensure that linguistic phenomenon are explained in linguistic rather than psychological terms. According to some, Bloomfield's insistence that word meanings and psychological considerations should not be invoked to account for diachronic phonetic change provided a kind of methodological template by which to study the synchronic structure of language generally (see Fries 1961).[27] Revising his earlier stance on the relationship between psychology and linguistics, Bloomfield ultimately came to believe that if anything, it was psychology and the social sciences generally that were dependent upon linguistic theory rather than the other way around (see Bloomfield 1936).

However, it was this yearning to validate linguistics as an autonomous scientific enterprise that led Bloomfield and many of his interpreters to almost inevitably draw

inspiration from the prevailing scientism of the day in their attempts to formulate a scientific basis for the study of language. As one historian of linguistics explained,

> Each period in our intellectual history somehow defines itself, at least implicitly, in terms of a particular metaphor. The metaphor associated with the historical-comparative period was, of course, the Darwinian notion of the "family tree" and of genetic relations, a useful metaphor which persists to this day. . . . The structuralists metaphor seems to have been that a language is mindless, independent of its speakers and their minds. (Prideaux, 68, 1999)[28]

In particular, the neo-Bloomfieldian approach reveals a strong, if somewhat naïve, affinity towards the positivist ideal of describing all natural phenomenon, be they physical, biological or social, from a purely scientific perspective that admitted of no mysticism, animism or mentalism. On the occasion of the twenty-first anniversary of the Linguistic Society of America, Bloomfield (1946) would proudly write:

> Only those who before the year 1925, worked in almost complete isolation can appreciate the change that came about with the existence of the linguistics Society. The improvement in the professional happiness of linguistic workers is only the subjective aspect of an improvement in the continuity and social cohesion of our work. Linguistics has come more to resemble, in its social complexion, the type of the better established branches of science—say physics, chemistry, and biology. (2)

For Bloomfield, language was nothing more than—but nothing extraordinarily less for that reason—the physical signaling mechanism by which individuals efficiently manage their collective behavior. As Bloomfield once put it: "The language which is studied by linguists is the noise you make with your face" (quoted in Ohlander 1976, 190). As such, linguistics was entirely within the realm of the objectively definable with considerations of meaning strictly beyond the reach of linguistics. At most, the obvious fact that language does, of course, convey meaning—that morphemes and word forms are correlated with semantic content—was relegated to the status of a primitive axiomatic postulate: "In every speech-community some utterances are alike in form and meaning"

(Bloomfield 1933, 78), providing, in fact, all the leverage Bloomfield needed in order to establish distinctive phonemes via a simple comparison of contrastive semantics.[29] According to Pike (1989), a typically reserved Bloomfield once stood up upon hearing that phonemic analyses should be done without any reference to meaning or grammar and said, "That is like going into battle with one hand tied behind your back" (218).

So while later linguists like Bernard Bloch, George Trager, and Zellig Harris would attempt to eliminate even this weak concession to semantics in their efforts to combat semantically based phonological analyses,[30] they viewed their goals as an extension of Bloomfield's linguistic principles. The neo-Bloomfieldian dismissal of semantics is perhaps best viewed as a symptom of an ambitious attempt to describe and rationally reconstruct language on the strict basis of quantifiable observables coupled with a hierarchy of explicit definitions rather than as a substantive target unto itself. And it was this foundationalist project of terminological reductionism which is most clearly of empiricist extraction and philosophically of one mind with Logical Positivism. If meaning had to be sacrificed, even if only temporarily, for the epistemological goals of providing an empiricist reconstruction of language, such merely served to confirm a related but distinct behavioristic suspicion of semantics.

The Taxonomic Reconstruction of Language

In this way, the basic rationale of neo-Bloomfieldian linguistics was to provide a non-circular foundation for the identification, classification and combination of observable linguistic entities which conspicuously respected the various taxonomic levels of linguistic structure (Fudge 1995). In this regard, distributionalism was unique, though it

did have significant structuralist precursors in Saussure, for instance.[31] Nevertheless, contemporary European linguists showed comparatively little interest in rigorously "reverse engineering" language with the primary intent of isolating its elementary "building blocks" (Stark 1972) only to put them back together again.[32] Yet this type of painstaking reconstruction is one of the most distinctive calling cards of a classically empiricist philosophy. One need only to think of the British empiricist philosophies of Locke, Hume or Berkeley and the "reconstruction" of objects, events and all of everyday knowledge as an accumulation of basic percepts and their combination—with Rudolph Carnap's *Aufbau* representing a modern culmination of these epistemological goals.[33] By carefully first recording the distribution of phones and phonemes, and then morphemes and words, the structure of natural language could be observationally rebuilt without relying on any data or information "higher up" the structural stream than that which is concretely observable—the raw "sense data" of acoustic speech impressions immediately present to the human ear.

Starting with the perceptually given data of continuous speech, it was the phonologist's job to identify the basic sound pattern of the language, pass this phonological data onto the morphologists, who would then pass it on to the grammarian. Individual sounds were to be objectively grouped into phonemes, phonemes into morphemes, morphemes into words, words into phrases and finally phrases into complete sentences. As Stark (1972) explains,

> [A] typical Bloomfieldian description (1) establishes the phonemes, and then (2) states the occurrences of phonemes within morphemes, (3) of morphemes within words, and (4) of words within constructions and finally, (5) of constructions within sentences. . . . The principle vertical relationship was 'made up of' or 'composed of' so that a sentence was composed of constructions, constructions were made up of words, words were made up of morphemes, and morphemes were composed of phonemes. . . . The units of these

> levels fitted together to give an extremely homogeneous if somewhat monolithic representation that consisted of just two basic parts: (1) and inventory of units and (2) their tactic patterns or arrangements. (131)

Such is, no doubt, the "taxonomic" impulse behind the taxonomic epithet-cum-epitaph affixed by Chomsky.[34]

The integrity of this feed-forward procedure, however, critically requires that such relatively concrete linguistic items as word, sentence and phrase, no less abstract grammatical categories such as noun and verb, if they were to yield to observational reduction, or at least operational definition, had to be definable in terms already defined in more basic terms. As Bloomfield (1936) himself stressed, for example, "The term *noun* . . . must then be defined, for English grammar, and the term *word* for language in general, as technical terms of linguistics; this definition, moreover, must be made in terms of the postulates, undefined basic terms, and earlier definitions of linguistics—not by definitions of meaning and not in metaphysical terms" (90).[35]

In the hands of neo-Bloomfieldian linguists, if such useful linguistic terms as "phoneme," "morpheme," and "word" were to earn their place in a science of language, they each had to be strictly recoverable from an analysis of the speech stream. But this hierarchy of definitions had to be preserved at each level (i.e., phonological, morphological, and syntactic) of linguistic analysis. In particular, it was held that the basic sound structure of a language—its phonology—could not be described by appealing to such structural units as morphemes or words (as these units were themselves defined in phonological terms), if the terms of phonology were to avoid circularity. Given that phonology was taken to be the most basic level of linguistic structure, it was clear that the sound system of a language would have to be reconstructed entirely from scratch. As

Zellig Harris reiterated as late as 1988, "The one type of element that is precisely established is the set of phonemes, the characteristic sounds of a language; indeed the discovery of phonemes is the beginning of a precise science of language" (2). But the primacy of phonology was not, at least by those committed to the program, viewed as problematic. After all, it is fortuitously the sounds of a language which can, it was held, be fully and accurately appreciated without any knowledge of the language, nor the role these sounds might happen to play in creating words, sentences and eventually meaning. All you have to do is open your ears, listen and record.

In this way, the suppression of semantics in distributionalist methodology reflected an empiricist commitment to systematically building up a complete description of language from the data considered to be the most concrete and perceptually accessible—individual sound elements. Everything else was "theory," guilty as it were, until proven terminologically innocent. And while some considered meaning to be the final stage of such linguistic reconstructions[36] (with Trager 1951 dismissively once remarking, "It will be time enough to worry about meaning when we have worked out all our morphemic (morphological and syntactic) analyses" (127)), others considered meaning to be outside the purview of linguistics (Hockett 1948 considered semantics to be part of some "bio-social psychology"[37] better handled by philosophers, logicians, sociologists, or psychologists, etc, see Lyons 1968). But only at the expense of undermining the foundational objectivity of phonology, and by implication the entirety of a scientific linguistics, could meaning be leveraged to identify basic phonemic equivalence among phonetically distinct phones. Given that various phonemic arrangements are what eventually give rise to words, and words to sentences, phonemes

will be used to define these latter structures. Just as obviously, word meaning could not be relied on to define phonemic units on pain of circularity.

Kenneth Pike, while no fan of this type of structural reductionism (in fact Pike was perhaps the most rebellious among these American descriptivists), did recognize its appeal. In an article appropriately entitled the "Interpenetration of Phonology, Morphology, and Syntax," Pike (1958) writes:

> The thrust of a such a view is toward separation of levels . . . The data from compartments must be rigidly separated, in theory, or the mixing of levels will—it is maintained—result in confusion, nonscientific results, and archaic procedures. Specifically, data from a higher level must not be used in treating a lower one, since the scientific progression is from lower to higher, the higher builds on the lower, and to use the higher in the analysis of the lower will lead, it is felt, to circularity of a vicious type. That is, grammatical considerations must not be used in phonemic analysis, phonemic analysis is ideally accomplished without reference to grammar or meaning, and morpheme identification likewise must ideally be accomplished, and the morphemic distributional structure described, without reference to meaning. . . . A quasi-mathematical statement of complex distributions of a minimum number of small units is the criterion of correctness and elegance in the system. Intuitive judgments and concepts are to replaced by formulas for distributional relations.

Continuing Pike notes,

> The key to its [distributional] strength lies, perhaps, in its simplicity plus the hope it holds out at the same time of being able to treat the most complicated problems with a mathematical rigor which would bring linguistics in line with the great progress achieved in the past century in the disciplines where mathematical and quantitative objectivity have been achieved. It leads to a philosophical elegance which often receives mystical—and sometimes even passionate—support. (364-365)

In contrast, then, to what soon became known among some as the "Pikean heresy" (Fudge 1995)—that is, letting information cyclically flow from one level to another—Charles Hockett's (1942) concluding remarks in "A System of Descriptive Phonology" clearly reveal a commitment to an epistemological hierarchy of levels and the interrelated themes of a positivist science of linguistics:

This completes the presentation of the principles of phonology. . . . The branch of grammar which deals with the phonemic shape of morpheme, words, and constructions, without regard to their meaning, is *morphophonemics*. . . . There is no circularity; no grammatical fact of any kind is used in making phonological analysis. . . . The writer has attempted to satisfy six requirements which seem essential for a correct system, but which no other system that he knows of completely fulfills. (1) Range and criteria must be accurately and unambiguously defined. (2) There must be no mentalism. (3) The terminology must involve no logical contradictions; terms defined as variables, class names, and quality names must be consistently used in those values. (4) No material should be excluded which might prove to be of grammatical importance, and none should be included which cannot be of grammatical importance. (5) There must be no circularity; phonological analysis is assumed for grammatical analysis, and so must not assume any part of the latter. The line of demarcation between the two must be kept sharp. (6) The way should be left open for the introduction of any criteria whatsoever on the grammatical level, barring mentalism. (112).

Providing a different perspective—and again indicative of a widespread concern against "mixing levels"—Bar-Hillel once argued that this fear of circular interdependence could be at least partly assuaged if meaning were to be first purged from syntax, in the way Chomsky would soon formulate his syntactically autonomous transformational grammar.[38] Bar-Hillel (1954) reasoned that if the autonomy of syntax was assured, then linguists would be that much less concerned about meaning bleeding in through any phonological-syntactical interface:

There has been recently a lively discussion of the degree of interdependence of the various structural 'layers.' It seems to me that part of the purists' insistence on a sharp demarcation between phonology and grammar is based on the assumption that a treatment of the syntactical-transformational aspects of language is in constant danger of succumbing to an infestation by meaning, an evil from which those aspects of language that can be shown to be independent of syntax can be saved. Indeed, so long as syntax, as traditionally handled, was a *meaning syntax*, it was methodologically worth while to adopt the procedure of Trager and Smith in *An Outline of English Structure* (1951), which, based upon purely distributional analysis, rigidly discriminates between ascending levels of complexity of organization. If only one could establish phonemics without recourse to grammar, the danger of letting semantic considerations creep in would be considerably reduced if not completely eliminated. I believe that some of the attractiveness of this attitude is reduced by recognizing that a description of the transformational aspects of syntax can be just as free of meaning as other parts of a linguistic treatment. (234)

And though not as generous as Pike, this is how Katz (1964)—in a generative shot across the Bloomfieldian bow entitled "Mentalism in Linguistics"—described the goals of "taxonomic linguistics":

> Utterances are stretches of physical sound. Since the primary data for a taxonomic linguistic investigation is a set of utterances elicited from informants or obtained from texts, the linguist begins with observable physical events, sounds or inscriptions. At the first stage of classification—the cataloguing of phonemes on the basis of these stretches of sound or some grouping of them—the linguist erects classes of significant sounds. At the next stage he forms classes of sequences of phonemes, thus producing a catalog of the morphemes of the language. Finally he classifies sequences of morphemes as sentential constituents of various types. Even if at some point the linguist should also consider an aspect of the speaker himself (such as his intuitive judgements about well-formedness) or an aspect of the speaker's environment (such as what he is referring to), such consideration is restricted to just those aspects that are capable of being observed by anyone who cares to carry out the same investigation. Therefore, on the taxonomic conception of linguistics, there is nowhere from the beginning to the end of a linguistic investigation, any appeal to mental capacities or mental processes. Alternatively, the taxonomic conception is a very narrow form of reductionism, which holds that every linguistic construction, at any level, reduces ultimately, by purely classificational procedures, to physical segments of utterances. (124)

More succinctly, Zellig Harris, the most sophisticated of neo-Bloomfieldian distributional linguists along with Bernard Bloch, explained his linguistic goals as such: "Here we will discuss how each language can be described in terms of a distributional structure, i.e., in terms of the occurrence of parts (ultimately sounds) relative to other parts, and how this description is complete without intrusion of other features such as history or meaning" (1954, 26). Equally representative is Charles Fries' (1957) affirmation that "One of the basic assumptions of our approach here to the grammatical analysis of sentences is that all the structural signals in English are strictly formal matters that can be described in physical terms of form, correlations of these forms, and arrangements of order" (58).

According to Householder (1971),

> the chief domain of scientification [sic] for linguists of the thirties and forties . . . was in procedures for discovering the structure (phonological and morphological, at first; later

syntactic) of new languages. By scientification here I mean reduction to rules and principles such that (after due perfection) any two properly trained linguistics independently approaching a hitherto undescribed language, if they spent sufficient time in applying them to the data of the language, would come up with the same description, in the same form, order and notation. (194)

And finally, though by no means an impartial by-stander to these issues, Chomsky (1955/1975) recapitulates the difference between "empiricist" and "rationalist" approaches to the study of language:

Traditional empiricism may be understood as an elementary 'data-processing approach.' The mind contains a system of properties that provide an initial analysis of the data of sense. Systems of knowledge and belief are developed by the procedures of generalization, analogy, induction, association, and habit formation that are developed in the several varieties of empiricist psychology and philosophy. It is, I believe, appropriate to regard the procedural approaches of structural linguistics as an unusually refined, detailed, and sophisticated development of a theory of this general character. Rationalist approaches, in contrast. . . (13).

Designing and implementing an algorithmic "discovery procedure" based upon the formal hierarchal structure of language was, in this way, a direct application of these basic principles valued by like-minded positivists. As Frederick Newmeyer once remarked, "many linguistics felt that the procedures had been so well worked out that computers could take over the drudgery of linguistic analysis. All one would have to do (in principle) would be to punch the data into the computer and out would come the grammar!" (1986b, 307).

Guarding Against Linguistic Biases

In addition, however, to satisfying the constraints imposed by an independent and terminological precise science of linguistics (as well as giving expression to the "autonomy" of both of phonology and syntax from semantics, as would be later

emphasized by Chomsky (1953, 1957, 1955/1975)), this proscription against "mixing levels" was also recognized as an effective prophylactic against the intrusion of wholly unscientific notions. Terminologically grounding a science of language was one objective, insuring that the familiar categories of the linguist's native language did not unintentionally—and worse, pre-conceived philosophically-motivated principles of "universal thought" did not intentionally—distort linguistic analysis, was another.

Reiterating the corrupting influence of traditional Indo-European concepts and grammars, Bloomfield (1939) recalls in his contribution to the Positivists' *International Encyclopedia of Unified Science* mentioned above, how the therapeutic effect of Native American languages instilled a sense of objectivity all too often taken for granted, but frequently absent even among linguists:

> The study of languages of diverse structure broke down parochial misconceptions according to which special features of Indo-European grammar were universal in human speech; it destroyed the pseudo-philosophical dogmas which were built on these misconceptions. The Greek and medieval notions of linguistics, which still hold sway, it would seem ineradicably, in our schools, have thus been long out of date. (220)

Mindful that a science of language had scarcely begun and questions of language when asked were, by default, often answered by "a kind of philosophical reasoning, which operates with terms such as 'subject,' 'object,' 'predicate' and so on," in the opening pages of *Language*, Bloomfield warns against this "common sense way of dealing with linguistic matters. Like much else that masquerades as common sense, it is in fact highly sophisticated, and derives, at no great distance, from the speculations of ancient and medieval philosophers" (1933, 3).

In order to regain a sense of linguistic innocence, and guarantee the scientific objectivity of their investigation, linguists had to approach each language *de novo*. Much

like Quine's own vision of "radical translation" but concretely acted upon, the neo-Bloomfieldian field linguist was charged with the task of deciphering the structure of a language without initially relying on anything other than that which was immediately audible, nor ultimately relying on anything which could not be reduced to such. Consistent with empiricist principles, any observer, with access to the same corpus of linguistic data and the same hierarchal algorithm of reconstruction, would come to the same, or structurally equivalent, conclusion. In this way, linguistic analysis was protected against terminological circularity, as well as the intrusion of subjective intuitions, linguistic hunches and scholarly caprice. "[I]t was largely through the observation of American Indian tongues" Bloomfield (1945) reported, "that linguistic students learned to describe the structure of languages in realistic and intelligible terms—and not in the traditional pseudo-philosophical jargon of our school grammars" (630).

According to Sampson (1980), this was in fact a real issue: "[D]uring the early part of [Franz] Boas's career, more orthodox linguistic scholars sometimes flatly refused to believe the results he was publishing" (59) on native American Indian languages. "In fact, what previous scholars had identified as the vague and variable sounds—in contrast to the definite sound system and patterning of European languages—was in actuality sensitivity to phonemic contrasts that were only allophonic in the target language" (60). Indeed, rather than model their grammars of English on the template provided by traditional Indo-European languages, it's almost as if these descriptivists approached English as if it were an exotic native American language.

In some ways, however, these self-imposed methodological restrictions were also a practical necessity (though see Hymes & Fought 1981 for a dissenting critique

regarding the anthropological and field work origins of "Bloomfieldianism"). Following in the footsteps of such prominent late nineteenth-century and early twentieth-century American anthropological linguists as Franz Boaz and Edward Sapir, American linguists, including Bloomfield, were committed to accurately documenting a number of rapidly vanishing Native American languages. As Bloomfield complained, some of the "American Indian Languages . . . are disappearing forever, more rapidly than they can be recorded" (quoted from Murray 1993, 278). Given the rate of extinction, field workers could hardly take the time to actually learn the language before beginning descriptive analysis lest the language were to disappear before getting started. In any event, as one linguist pointed out, "When the linguist himself does not speak the language under analysis he cannot analyze his own intuitions about it, and he can only rely on properties of the acoustic signal and study their patterns" (Bierwisch 1971, 39).

Here is how Trager & Smith (1951), in their influential *Outline of English Structure*, explained the situation:

> In the present state of morphemic analysis it is often convenient to use the meanings of utterance fractions as a general guide and short-cut to the identification of morphemes. This is especially so in the case of languages that are more or less well known to the analyst, as has been true for most morphemic work done up to now. When we are confronted, however, with a language that we know little about in terms of the relation of the linguistic behavior of the speakers to the rest of their cultural behavior, it becomes clear that meaning can be of little help as a guide. The theoretical basis then becomes evident: it consists of the recognition of the recurrences and distributions of similar patterns and sequences. The analyst must therefore constantly keep in mind this theoretical basis, and be aware that his hunches about what goes with what are really short-cut conclusions about distributional facts. (54)

By the same token, the linguist Einar Haugen attributed the tendency of some American linguists to avoid or exclude semantics as a result of successful code breaking efforts during the Second World War, where, in fact, the only information initially

available to the cryptologist was exclusively distributional (Diderichsen 1958, 213)—the sort of early computational linguistics Joseph (2002) calls "algebraic" structuralism. More generally, "As long as the investigator," noted Roman Jakobson, "knows no *signatum* of a given language, and has access to nothing but *signans* he willy nilly has to strain his detective capacities and obtain any possible information about the structure of this language from the external evidences. The present state of Etruscology is a good example of such a technique. . . . It is difficult, however" continued Jakobson "to simulate the ignorance of a familiar code: smuggled meanings distort one's allegedly cryptanalytic approach" (1961, 249).

Henry Gleason offers a slightly different explanation for the distinctly American linguistic aversion to meaning—the problem of translation. While translation difficulties encountered between so-called standard European languages and Native American languages led some to question the universality of "conceptual frameworks," culminating in the conceptual relativity of the Sapir-Whorf hypothesis,[39] according to Gleason (1965), problems with translation also encouraged a systemic mistrust of meaning as a tool of linguistic investigation:

> Working as they did with languages using very different conceptual categories than does English, American linguists became acutely conscious of the great difficulty of translation. Using informants whose knowledge of English was sometimes limited, they found that their recorded meanings for utterances were often inadequate and sometimes downright misleading. The traditional oriented grammars commonly parroted the classical, grammatical definitions—usually meaning-based—and then tried to identify the same familiar parts of speech in the Indian language on the basis of these definitions. All these experiences conspired to create a suspicion of meaning. The movement of thought among American anthropological linguists was toward a total rejection of meaning as a basis for analysis. Some of these leaders in this development found it necessary to apologize for the use of meaning as a "convenient shortcut," while others struggled valiantly to eliminate it from their procedures . . . In due course, another characteristic American tenet was established: meaning is not relevant for linguistic analysis. . . [though

careful to add in a clarifying footnote that] This has been a fruitful source of misunderstanding. The objection is to basing analysis on meaning, not, for most linguists, to the study of meaning. (44)[40]

Anti-mentalism

In addition to empiricist reasoning—and undoubtedly the implicit assumption that real-life child language learners[41] initially only have access to low-level distributional information (see chapter 8)—neo-Bloomfieldian linguists also held a specific animus against "mentalism" generally.[42] Though ostensibly disavowing any dependence on "any one psychological doctrine" so as to insure "the findings of the linguist" should not be "distorted by any prepossessions about psychology," Bloomfield and the neo-Bloomfieldians he inspired were by no means apologetic about their anti-mentalistic convictions. Here is how W. Freeman Twaddell (1935)—perhaps best known for comparing "physical," "mentalistic," and "fictitious" accounts of the phoneme—defended a positivist ideology as the only acceptable scientific methodology in linguistics:

> The principles presented in this section are limited strictly to applications of scientific methodology, and accordingly do not involve a discussion with those whom behaviorism is unacceptable as a psychological assumption or (irrelevantly) as a 'philosophy.' Whatever our attitude toward mind, spirit, soul, etc. as realities, we must agree that the scientist proceeds as though there were no such things, as though all his information were acquired through processes of his physiological nervous system. In so far as he occupies himself with psychical, non-material forces, the scientist is not a scientist. The scientific method is quite simply the convention that mind does not exist: science adopts the nominalistic attitude toward the problem of the universals, in matters of procedure. (1935, 57)

Likewise in a letter to Edward Sapir (in response to a potential appointment to the Victoria Memorial Museum in Ottawa, Ontario) Bloomfield himself explained:

> As a behaviorist, I don't put as big a gap as you probably do, between non-human and human phenomenon. The break in the natural order which is viewed as the plus of a mental factor, choice, etc., is for me simply the plus of socialized behavior—in principle

and chiefly—language. The enormous difference between non-human and human events is for me simply this, that man has, beside the "direct" reactions given to the higher mammals, another set, the language-reactions, which (1) give very specific stimuli to other members of the social group, which thereby becomes, to this extent, a new and more complex type of organism, in which the interchange of stimulus and reaction is more detailed than between individual animals, and (2) give man the ability to stimulate and react as a result of law of consequences. All this I have from [Albert Paul[43]] Weiss, our psychologist. The first appeal this made to me was due to the fact that in our actual work we do exclude "mental" factors, i.e, sound-change won't stop for desires or fears. (Some of the Germans, I see from the Streitberg volume, are going in for the theory that "useless" sound-groups are "more easily lost"—which seems to me to be pure bunk, resting upon vague ideas of what "uselessness" and "meaning" etc. are. . .) (quoted in Hockett 1987, 53).

Similarly, Bernard Bloch (1943)—responding to a colleague who skeptically challenged the recent ascendance of the "mechanistic" school over the "mentalistic" (and citing Sapir as a notable counter-example to this trend)[44]—sharply retorted:

It is to be noted, however, and remembered by those who regard the undisputed greatness of Edward Sapir as an argument in favor of 'mentalism', that although Sapir's terminology and his interpretation of linguistic patterns emphasized the psychological correlates of the facts of speech, his METHOD—his manner of observing, recording, and classifying those facts—was always rigidly 'mechanistic'. What 'mechanists' usually criticize in the work of 'mentalists' who lack Sapir's profound knowledge of scientific procedure is the circularity of their argument: the explanation of a linguistic fact by an assumed psychological process for which the only evidence is the fact to be explained. If 'mechanism' is inadequate because of its insistence on objectivity and its occasionally somewhat nervous avoidance of common terms with undesirable psychological connotations, surely 'mentalism,' with its failure to separate inference from observation and its tendency to substitute imagination for analysis, is less adequate still to the task of teaching us how language works. The 'mechanists' have been blamed for being more interested in the facts of speech than in the possible conclusions to be drawn from them. May not the 'mentalist' be blamed for the converse prepossession? (199)

In defense of his own acceptance of the label "non-mentalist," Bloomfield (1944), with tongue in cheek, would point out: ". . . I use terms like *mechanist* or *non-mentalist*: in a community where nearly every-one believed that the moon is made of green cheese, students who constructed nautical almanacs without reference to cheese, would have to be designated by some special term, such as *non-cheesists*" (49).[45] According to Bar-

Hillel (1954), Bloomfield "deplored the mentalistic mud into which the study of meanings had fallen, and tried to reconstruct [the field of linguistics] on a purely formal-structural basis" (234-235). But Bloomfield was not alone in so colorfully criticizing his mentalistic opponents. Esper (1968), in study detailing the origin of Bloomfield's psychology of language,[46] shares the anonymous quote: "Once we start admitting the semantic element into the study of forms, we find the laboratory door torn from its hinges, [and] life, like a gang of louts, rampaging in" (5).

With positivist poise second only to the Logical Empiricists themselves, Bloomfield (1936) was fond of emphasizing that "Linguistics as actually practiced employs only such terms as translatable into the language of physical and biological science . . ." noting that linguistics differs in this respect "from nearly all other discussion of human affairs." With this in mind, Bloomfield would confidently predict

> Within the next generation mankind will learn that only such terms are usable in any science. The terminology in which at present we try to speak of human affairs—the terminology of 'consciousness,' 'mind,' 'perception,' 'ideas' and so on—in sum the terminology of mentalism and animism—will be discarded, much as we have discarded Ptolemaic astronomy, and will be replaced in minor part by physiological terms and in major part by terms of linguistics. (1936, 19)

It was, according to Bloomfield (1930), precisely the scientific study of language which would be the leading edge in the general advancement of the sciences, as a scientific linguistics would uniformly de-mythologize such notions as "want," "choose," "will," "wish" desire" or "conscious striving" for the benefit of all the sciences:

> It is certain that the actions of man involve some factor which is not present in the actions of plants and animals, just as these involve a factor which is not present in the actions of inanimate matter. It used to be thought that in plants and animals there was a 'vital principle' which is wanting in lifeless things. That was animism; today we know that the peculiar factor in living organisms is a highly specialized unstable chemical combination, the protoplasm. Let me now state my belief that the peculiar factor in man which forbids our explaining his actions upon ordinary plane of biology, is a highly specialized and

unstable biological complex, and that this factor is none other than language . . . (quoted in Fries 1961, 223)

In this article entitled "Linguistics as a Science," Bloomfield (1930) expressed his hopes for linguistics to aid the progress of scientific development:

> In speaking of linguistics as a science, I shall not deal with technical matters, but, presuming your patience, I shall speculate upon the service which the study of language may render to the sciences that deal with man, to humanistic studies, and to our view of the world. . . . I believe that in the near future—in the next few generations, let us say—linguistics will be one of the main sectors of scientific advance, and that in this sector science will then win through to the understanding and control of human conduct. . . . I believe that the study of language will be the ground where science gains its first foothold in the understanding and control of human affairs. (149-152)

And it is for all these reasons that Bloch (1949) would extol Bloomfield's commitment to scientific clarity as a cure for specious metaphysical philosophy and linguistic frivolity in his obituary for Bloomfield:

> There can be no doubt that Bloomfield's greatest contribution to the study of language was to make a science of it. Others before him had worked scientifically in linguistics; but no one had so uncompromisingly rejected all pre-scientific methods, or had been so consistently careful, in writing about language, to use terms that would imply no tacit reliance on factors beyond the range of observation. To some readers, unaware of the danger that lies in the common-sense view of the world, Bloomfield's avoidance of everyday expressions may have sounded like pedantry, his rigorous definitions like jargon. But to the majority of linguists, the simple clarity of Bloomfield's diction first revealed in full the possibilities of a scientific discourse about language. . . . In his long campaign to make a science of linguistics, the chief enemy that Bloomfield met was that habit of thought which is called mentalism: the habit of appealing to mind and will as ready-made explanations of all possible problems. Most men regard this habit as obvious common sense; but in Bloomfield's view, as in that of other scientists, it is mere superstition, unfruitful at best and deadly when carried over into scientific research. In the opposite approach—known as positivism, determinism, or mechanism—Bloomfield saw the main hope of the world; for he was convinced that only the knowledge gained by a strictly objective study of human behavior, including language, would one day make it possible for men to live at peace with each other. (92-93)

Yet it was only towards the end of his life that Bloomfield's words would find their largest and most sympathetic philosophical audience. In a 1939 monograph commissioned by the editors of the *International Encyclopedia of Unified Science* entitled "Linguistic Aspects of Science," the convergent positivist themes of behaviorism, mechanism, operationalism and physicalism are each given their due praise:

> It is the belief of the present writer that the scientific description of the universe, whatever this description may be worth, requires none of the mentalistic terms, because the gaps which these terms are intended to bridge exist only so long as language is left out of account. If language is taken into account, then we can distinguish science from other phases of human activity by agreeing that science shall deal only with events that are specifiable in their time and place to any and all observers (strict *behaviorism*) or only with events that are placed in co-ordinates of time and space (*mechanism*), or that science shall employ only such initial statements and predication as lead to definite handling operations (*operationalism*), or only terms as are derivable by rigid definition from a set of everyday terms concerning physical happenings (*physicalism*). These several formulations, independently reached by different scientists, all lead to the same delimitation, and this delimitation does not restrict the subject matter of science but rather characterizes its method. It is clear even now, with science still in a very elementary stage, that, under the method characterized, science can account in its own way for human behavior—provided, always, that language be considered as a factor and not replaced by the extra-scientific terms of mentalism. (230)

Nevertheless, Bloomfield was humble enough to recognize that "A prophecy of this sort, no matter how deep the conviction from which it springs, is so pitifully subject to individual prejudice and errors, that even more than most statements it needs to be confirmed or refuted," no doubt precisely why Bloomfield was happy to remind us that "Within the last years a group of philosophers and logicians, known as the Vienna Circle, has arrived at the same conclusion concerning language. Subjecting various branches of science to logical scrutiny, Rudolph Carnap and Otto Neurath have found that all scientifically meaningful statements are translatable into physical terms—that is, into statements about movements which can be observed and described in co-ordinates of space and time" (1936, 20-23).

Though at the time some scholars expressed some criticism, both the obvious benefits of increased analytic clarity and a widespread climate of positivist science combined to mute much dissent (either linguistic, as with Kenneth Pike, or philosophical as with Leo Spitzer[47]). Indeed reservations often took the form of back-handed compliments rather that direct criticism: "While appreciating the importance of careful analysis of speech forms," Fitch (1940) would pause to point out that "one is inclined to feel that Bloomfield, like Carnap and Morris, has not yet faced the most difficult problems." "In any case," continued Fitch, "the work of the logical empiricists is important not so much for their negative criticisms, which are likely merely to reflect misunderstandings on the part of the critic himself, as for their positive and fruitful scientific insights and theories" (680). Similarly, after having noted some caveats and concerns, Collinson (1948) sums up his more positive reflections: "I should like to express my conviction that the opponents of mentalism in linguistics have swept like a cleansing wind through the dead leaves cumbering many a description or analysis of language. It is good that they should push to the utmost limit their efforts to make linguistics an autonomous, self-sufficient and scientific discipline and to go on improving to greater precision the neat and compact techniques they have already evolved" (310). George Miller—one of the founding fathers of cognitive science— would remark that "one can only admire those hardy linguists who would solve all their problems without recourse to meaning, but to an outsider who regards meaning as the major distinction between linguistics and cryptanalysis, such efforts seem imprudent and unnecessary" (quoted in Ohlander 1976, 191).

Linguistically Applied Logical Positivism

A convergence, then, of theoretical, methodological, practical and ultimately deep philosophical convictions, led to the distinctively neo-Bloomfieldian disclaimers regarding meaning and its relationship to phonological and linguistic structure generally. "Bloomfield was not merely passively influenced by logical positivism," says Sampson (1980), "but after a flirtation in his twenties with very different views became an active proponent of positivist ideas as they applied to the study of human language" (63). Given their self-imposed hierarchal structuralism and aversion to semantics, it would be a foundationalist failure if the most basic task of the linguist—classifying phonetically distinct segments into phonemically equivalent groups—demanded not only an objective distributional analysis of those phonetic elements, but also required a semantic interpretation of how those sounds, in contrastive combination, actually did or did not differ in meaning. After all, the essence of distributionalism, and descriptivism generally, was description, not interpretation, with Martin Joos (1957) once scolding a European colleague for "offering too much of a phonological EXPLANATION where a sober TAXONYMY would serve as well" reminding us that "Children want explanations, and there is a child in each of us; descriptivism makes a virtue of not pampering that child" (96; emphasis original).[48]

Two sounds may very well be intuitively recognized as phonemically distinctive or phonemically equivalent because substitution of one for the other does or does not effect a change in meaning, but such is merely a principle of, as it were, our "folk phonology." But as Bloomfield once warned: "Accept everything a native speaker says in his language and nothing he says about it" (quoted in Sampson 1980, 63). The fact

that two phones are semantically contrastive—in "commutation" with each other in the terminology of the Copenhagen Linguistic Circle—is not really an explanation of phonemic contrast, but instead only a restatement of precisely what demands an explanation. It is not as if two sounds are phonemically equivalent simply because they are incapable of supporting a semantic contrast. Rather, two sounds are unable to signify a semantic contrast because they are phonemically equivalent in virtue of some non-semantic regularity, either some psychophysical acoustic property or perhaps their complementary phonetic distribution. It is, or so it was argued, precisely these phonetic and/or distributional features which determine whether two phonetic tokens are, in fact, of the same linguistic type (so as to have the same meaning, save for homophonous ambiguity). As such, two minimally distinct tokens have the same meaning because they are of the same type; they do not instantiate the same type because they have the same meaning. According to Bloch, Smith, and Trager, complementary allophonic distribution (and some measure of phonetic similarity) was the real key to unlocking phonemic equivalence.

Even Pike, while recognizing the inevitable shortcomings of this sort of distributional analysis, was careful to point out the strengths of strictly distributional derivations of phonemic structure. In a conference session devoted to discussing Diderichsen's (1958) "The importance of distribution versus other criteria in linguistic analysis" contribution to the Eighth International Congress of Linguistics (characterized by a number of European scholars criticizing the asemantic procedures of Harris and Bloch), Pike (1958) pauses to reaffirm their distinctive contributions to linguistics:

> It is important, however, for European readers to understand the very exciting results which would be certain to appear if the formal approach by Harris, Bloch, and others,

could be made to work. Many of us find this outlook very exciting, and of great value as far as it has already gone. Some of us, however, have found it necessary to state that due to limitations in the structure of language we think certain of these goals cannot be completely attained. Those of us who have sensed such ultimate limitations nevertheless feel that the contribution of this approach has been, and will continue to be for some time, very great and cannot be dismissed lightly. Theirs is an attempt to reduce language to a formal analysis of great simplicity, elegance and mathematical rigor, and they have come astonishingly close to succeeding. This drive and this outlook is basic to the American work referred to, but has not been reflected by the writers of these reports. (204)

So while the phenomenon of free allophonic variation (whereby a pair of phones mimics the distributional structure of two contrasting "minimal pair" phonemes but is nevertheless considered by native speakers to be mere allophonic variation) undermined these distributional proposals, there are no doubts regarding the intent. "Complementary distribution" represents the most accurate and sophisticated attempt to "reduce" the phoneme and its failure the culmination of years of phonological research and, in a sense, an entire linguistic era.[49]

Summarizing Chapter 1 of his linguistics dissertation, Ohlander (1976) emphasizes:

Indeed it has been something of an antimentalist dream to devise an analytic system where, as it were, form is the input and function is the output, i.e., a system where formal (including distributional) facts alone will automatically lead to the relevant linguistic facts, "all semantic and psychological criteria" being avoided (Bloch 1948: 169). Twaddell expresses his belief in the 'reality' of this antimentalistic dream in the following general terms: 'Any correlation of phenomena which can be established on the basis of mental entities or events can also . . . be established on the basis of the phenomena themselves.' It has been one of the purposes of this introductory chapter to stress the untenability of such views. Both formal and functional, i.e., psychological," criteria are required for determining linguistic relevance. (20)

The phoneme—if not in classical "taxonomic" terms at least in terms of generative "distinctive features" (Chomsky & Halle 1968)—has outlived attempts to reductively define it away. In fact, as Morris Halle (1964) once pointed out, despite all the problems

encountered with trying to square phonemic equivalence among phonetically distinct realizations, such difficulties did not result "in the wholesale abandonment of the phoneme." On the contrary says Halle, "Only a few easily frightened souls have been ready to do without the phoneme," (325) and I might add, none without the notion of "contrast" or at least contrastive distinctive features. To suggest, as Chomsky and Postal do in the context of generative phonology, that "'contrast' must be taken as a primitive notion" (Chomsky 1964, 83) or that "the notion may be considered primitive" (Postal 1968, 7) is not to provide a non-semantic account of basic phonemic categorization, but to consider such issues uninteresting or orthogonal to certain other concerns.[50] "Phonology is," as Ullmann (1957) simply says, "a semantically conditioned science" (32) and this is true regardless of whether one wishes to avoid semantics for neo-Bloomfieldian reasons, or merely to generatively distance oneself from the "taxonomic" concerns of pre-generative phonology.

Yet independent of the failure to actually deliver on the promise of phonemic reductionism—and many self-acknowledged "short cuts of practical convenience" to the contrary[51]—neo-Bloomfieldian distributionalism, much like other empiricist attempts to reduce our everyday knowledge to combinatory percepts, deserves respect if only for its philosophical aspirations and self-imposed methodological standards. As Postal (1968) remarks with unusual generosity,

> It was, incidentally, not totally implausible for Bloomfield to maintain a radical empiricist philosophy of science during the 1930's, since this was widely accepted at the time by many among the best of philosophers. What proved unfortunate for the linguists that followed was a failure to keep up with work in the philosophy of science, work which led to the abandonment of this radical empiricist position when it was found incompatible with the nature of actual scientific theories. (295)

Expressing a form of cross-party-line respect, even Katz (1996) remarked, "Bloomfield's attempt to construct such a methodology was a novel development in the history of nominalistic thought. Never before had the problem of the abstractness of language been put in a fully general form and never before had an attempt to solve it brought to bear the full resources of the science of language" (276).[52] Nevertheless, without a theoretical framework to recognize, no less exploit the meta-linguistic intuitions of native speakers—whether phonological, morphological or grammatical—distributionalism was eventually seen to have painted itself into an empirically attractive but increasingly cramped linguistic corner. Only a different approach could, as it were, save linguistics from its own positivist ambitions.

In fact, soon the leading figures in generative linguistics would emphasize that the goals of linguistic theory is not to "reduce" our natural language intuitions, but to use them as *evidence* in the construction of a theory intended to explain the principles which govern their overall linguistic operation. According to Lees (1957), there is nothing intrinsically illegitimate with linguists consulting their own "*Sprachgefűhl*." Indeed, "it is precisely this *Sprachgefűhl*," says Lees, "this intuitive notion about linguistic structure, which, together with the sentences of a language, forms the empirical basis of grammatical analysis; and it is precisely the purpose of linguistic science to render explicit and rigorous whatever is vague about these intuitive feelings" (399). Lees' appeal to *Sprachgefűhl* is, in this way, similar to Hockett's (1955) much quoted "empathy" requirement:

> We know of no set of procedures by which a Martian, or a machine, could analyze a phonological system—an entity, that is, to which even the basic biological and cultural common denominator of humaneness would be alien and would require specification. The only procedures which can be described are rules for a human investigator, and

depend essentially on his ability to empathize. To refuse to do this, to insist on avoiding it as much as possible in the name of a spurious 'objectivity'—as for example, Franz Boas did—is to place oneself in much the same position as that of a bacteriologist who would refuse to stain his slides. . . . Any newcomer to the community, say a newborn child, in due time learns the language of the community, including its phonologic system: learns, that is, to identify and differentiate acoustic stimuli in much the same way as do those already in the community. . . . All that we can ask of an investigator is that he do as good and precise a job along this line as a child does—but that he keep a record as he does it, so that we will have the materials to which to apply the other aspect of analysis, collocation. . . (1955, 147)

Bringing us full circle, even Chomsky (1955/1975) would eventually concede that the so-called behavioral "pair-test" (see chapter 7 for details) is parasitic on the semantic knowledge of the native informant, contradicting or at least retracting his claim in *Syntactic Structures* that "there is no warrant for interpreting the responses studied in the pair test as semantic in any way" (1957, 99).[53] According to Chomsky (1955/1975),

in a certain sense the ultimate criterion remains the speaker's intuition about linguistic form, since only this can tell us which behavioral tests are to the point. We might hope that some more general account of the whole process of linguistic communication than we possess now may permit us to reconstruct the criteria of adequacy for linguistic theory in more convincing and acceptable terms. But for the present, it seems that we must rely, at least to some extent, on the speaker's intuitive conception of linguistic form. (102)

As Hutton (1990) describes it,

The history of linguistics in the 20th century is conventionally divided into two distinct phases. The first, the so-called 'structuralist' period, involved a corpus-based study of linguistic systems. The linguist would collect a corpus of data 'in the field' and then attempt to analyze it by inductively dividing it into units of analysis on the levels of phonology, morphology, and sentence grammar. The second approach to the data was that developed by transformational linguists. Informants would be asked to reject or accept sample sentences put them. These judgments were held to provide direct access to the linguistic competence of the speaker. This approach was compared favorably by transformational grammarians with that of 'structural' linguistics, who, it was argued, had no criteria for distinguishing between those elements of their data that reflected linguistic competence, and those that arose from a variety of extra-linguistic factors such as lack of concentration, tiredness, distractedness, etc. (138).

So while ironically no more genuinely satisfied with semantic-based phonology than the Bloomfieldian "taxonomic" linguists he criticized,[54] Chomsky's generative

approach did provide a new (or according to Chomsky, an old but positivistically dismissed) palette by which to paint language; a technique as colorfully rationalist in its philosophical methodology as distributionalism was monotonously empiricist. Then again, it would not be Chomsky, nor really any linguists for that matter, who would continue to associate themselves with the positivism—and in particular the "phonemic reductionism"—of neo-Bloomfieldian linguistics long after it had fallen out of favor in linguistics. For inasmuch as it was an empiricist philosophy which provided the positivist blueprint for neo-Bloomfieldian linguistics, it is unsurprising that an empiricist philosopher would continue to pursue Bloomfieldian-like linguistics long after Bloomfield's death in 1949. One need only look as far as W. V. Quine. Indeed, Quine's own contributions over the course of four decades to the philosophy of phonology represent another chapter in the history of "phonemic reductionism," presented here as chapter 7.

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[21] Generally, these authors seek to correct, deflect or deflate various misconceptions about Bloomfield revolving around three inter-related issues: (i) the extent and significance of Bloomfield's behaviorist rejection of meaning; (ii) an apparent theory versus practice inconsistency regarding, on the one hand, Bloomfield's claim that linguistic analysis must start with *form* rather than meaning, but on the other, his claim that the very basis of phonological form could not be identified without appealing to sameness and differences of meaning; and finally (iii) some of the specific academic and personal relationships among Bloomfield and his immediate neo-Bloomfieldian followers. See Hymes & Fought (1981), Fought (1999b) but also Koerner (2002) for discussion of the Leonard Bloomfield in "Bloomfieldianism."

For a taste of the give-and-take within Bloomfieldian scholarship, this is how Langendoen (1979) faults Uhlenbeck (1979): "I do not believe that Bloomfield's adoption of the radical behaviorism of Weiss seriously affected the development of American linguistics. In fact, I do not find that radical behaviorism affected even Bloomfield's linguistic work (what he said in his polemical articles and reviews is another matter)" (157). Though writing from the "anti-Bloomfieldian" position of generative phonology, Postal (1968) observed that while "Bloomfield gave an analysis which involved recognition of the existing nontransitivity of contrast, he maintained a theory which denied this. This is only one of many different examples of several distinct types which could be brought forward to show the superiority of Bloomfield's linguistic practice over his theorizing. In later developments in linguistics which are often referred to as 'Bloomfieldian,' linguists consistently ignored his practice and based themselves on his theorizing, leading to the inadequate taxonomic linguistics of the 1940's and 1950's, whose phonological aspect we have been criticizing throughout the present study" (223-224).

[22] Following Fries (1954, 1961), Hall (1987a, 1987b) and Koerner (1978, 2002) identify the "extreme" neo-Bloomfieldians as Bernard Bloch, Henry Lee Smith, George Trager and Zellig Harris and some of the more "moderate" Bloomfieldians as Eugene Nida, Henry Gleason, Archibald Hill and Robert Hall. Charles Hockett (and Charles Fries for that matter) are more difficult to place. A certain ambivalence seems to characterize their own thought.

[23] Though Chomsky (1964) would represent generative linguistics as a fundamental break from earlier "taxonomic" distributional and descriptivist linguistics, Chomsky's interest in the "autonomy" of phonology and syntax from semantics suggests certain "Bloomfieldian" continuities. See especially Chomsky (1955, 1955/1975, and 1957). In *Syntactic Structures* Chomsky (1957) argues how a reliance on semantics has stunted the growth of both phonological and syntactic theory. But Chomsky can be somewhat of an occupational hazard for historians of linguistics. In the process of charting the development of Chomsky's thought over a period of three decades (in an extensively annotated article provocatively entitled "The Anti-Mentalistic Skeleton's in Chomsky's Closet") one researcher details passages from Chomsky's early career which "Chomsky would probably now like to forget" (Steinberg 1999, 269). See also Joseph (2002) and the references therein for more details about Chomsky's "quirkiness as a historian" (165). In fact, in response to a criticism of the methodology of Bloomfieldian linguistics, Chomsky (1955/1975) actually came to Bloomfield's defense, calling on W. V. Quine of all people for additional support. While conceding that

> It is true that many philosophers have given up, as a general requirement for significance, the kind of reductionism that our restatement of Bloomfield's program has as its goal, and have held that dispositional terms and other theoretical constructs must be introduced into scientific theory even if not amenable to the kind of analysis once sought. However, I do not believe that this is relevant to Bloomfield's antimentalism, or to the approach to linguistic theory that we have outlined. The fact that a certain general criterion of significance has been abandoned does not mean that the bars are down and that "ideas" and "meanings" become proper terms for linguistics. If this rejection of an old criterion is not followed by construction of a new one, then it simply has no bearing on the selection of the legitimate terms for a scientific theory. If it is followed by a new analysis of 'significance,' then if this is at all adequate, it seems to me that it will rule out mentalism for what were essentially Bloomfield's reasons, i.e., its obscurity and inherent untestability. Thus Quine rejects reductionism, suggests an alternative, and rejects mentalism. (85-86)

These comments tend to sit uncomfortably with Chomsky's polemic attacks on Bloomfield's "taxonomic linguistics." In *Current Issues in Linguistic Theory* Chomsky observes how Bloomfield's anti-mentalism "fitted well with the operationalism, verficationism and behaviorism that formed a dominant intellectual mood in the early 1930's" (1964, 70).

[24] Well not exactly always. In his legendary article "On Defining the Phoneme," Twaddell (1935) quotes Bloomfield as having optimistically once claimed that "The physical (acoustic) definition of each phoneme of any given dialect can be expected to come from the laboratory within the next decades" (63). Of course in *Language* Bloomfield (1933) repeatedly repudiates any such physical reduction of the phoneme: "Only by finding out which utterances are alike in meaning, and which ones are different, can the observer learn to recognize the phonemic distinctions" (93). As Bloomfield (1933) emphasizes, "For this reason even a perfected knowledge of acoustics will not, by itself, give us the phonemic structure of a language. We shall always have to know which of the gross acoustic features are, by virtue of meanings, 'the same,' and which are different for the speakers" (128).

[25] Illustrative of this tension between theory and practice, Fudge (1995) notes that even Bloch had difficulties implementing his own (Bloch's) principles of phonemic analysis: "In the final section of the 1950 ["Studies in colloquial Japanese: Phonemics"] paper, however, Bloch in effect has to admit that his earlier transcription [which violated some of these hierarchal principles] was superior for many practical purposes. He allows that, once a strict phonemic transcription has been used to study the behavior of morphemes, new information discovered on this basis may be used to modify the transcription. Such a modified transcription, Bloch states, is usually far better adapted to the study of the morphology and syntax of the language. . . . Significantly, although these advantages were clearly apparent, they were not felt by

post-Bloomfieldians to carry sufficient weight to justify abandonment of the strict phonemic transcription. This illustrates the basic importance which they attached to the two theoretical principles [no "mentalism" and no "mixing of levels"] with which this article began" (309). Similarly, Carroll (1953) complains that "For all the emphasis that some linguists have put, as a matter of theory, upon the analysis of linguistic structure apart from meanings, these same linguists have put at least an equal emphasis on getting meanings and behavioral and cultural correlates, when studying a language in the field" (32).

[26]According to Bugarski (1999):

> Throughout its history, modern linguistics has been crucially marked by an effort to lay the conceptual and institutional foundations for a science of language which would no longer be subordinated to philology, philosophy, sociology, or some other larger discipline, but rather a self-contained and legitimate member of the circle of modern sciences. And the way to achieve this goal was sought in an attempt to define language as a distinct system, irreducible to subsets of its manifestations which could be individually and adequately covered by the other sciences just referred to. In other words, as opposed to disciplines whose areas of interests include some aspect or another of language as well, and which therefore tend to see in language a means of gaining knowledge of extralinguistic phenomena of some kind, linguistics came to perceive in it a coherent whole and a worthy object of concentrated scholarly study. Such a conception presupposed the existence of a linguistic system which could in principle be described independently of the history, culture, social context, or individual peculiarities of its users, and in that sense it may properly be identified as the postulate of the autonomy of language and linguistics. (29)

[27] Reviewing Bloomfield's attitude toward linguistics, Anderson (1985) concluded "The central factor underlying both his maintenance of neogrammarian assumptions about change and the shift in his point of view on psychology was undoubtedly Bloomfield's passion, a product of his times, for scientific explanation based solely on propositions relating observable factors and influences by principles of logic and mathematics alone" (50). Turning the table around, Postal (1968) makes very similar observations. According to Postal, neogrammarian linguistics is, with the benefit of generative hindsight, really just a diachronic version of neo-Bloomfieldian taxonomics, and must be rejected for the same reasons: ". . . the truth value of a classical view of the nature of sound change which, while predating the development of autonomous phonemics, is closely linked to it theoretically, factually, and historically. It is concluded that this widely accepted Neogrammarian position is false and that the theory of sound change must be based on a more flexible and abstract conception of phonology within the framework of systematic phonemics. More generally, it is concluded that sound change, like other aspects of language and language change, is an essentially *mentalistic* phenomenon and that a major aspect of the error of autonomous phonemics and its historical corollaries is the futile attempt to maintain an antimentalistic methodology" (xiv-xv, see also 24, 231-233, 240). See also the papers in Hoeningswald (1979), especially Jakobson (1979), Langendoen (1979), Malkiel (1979), and Uhlenbeck (1979) for the European background to American structuralism and also the ensuing exchange among Langendoen and Uhlenbeck for further subtleties on Bloomfield.

[28] The related "organismic" idiom was also common. Franz Bopp, a prominent nineteenth-century German comparativist, wrote "Languages must be regarded as organic bodies, formed in accordance with definite laws; bearing within themselves an internal principle of life, they develop and they gradually die out, after, no longer comprehending themselves, they discard, mutilate or misuse . . . components or forms which were originally significant but which have gradually become relatively superficial appendages" (quoted in Newmeyer 1986c, 23).

[29] The full quote from Bloomfield (1933) reads:

> Evidently the working of language is due to a resemblance between successive utterances. Utterances which in ordinary life we describe as consisting of 'the same' speech-forms—say, successive utterances of the sentence *I'm hungry*—evidently contain some constant features of sound-wave, common to all utterances of this 'same' speech-form. Only on this assumption can we account for our ordinary use of language. The phonetician however, cannot make sure of these constant features, as long as he ignores the meaning of what is said. Suppose, for instance, that

> the phonetician had records of an utterance which we could identify as representing the syllable *man*, spoken on two different pitch-schemes. If the language of these utterances were English, we should say that both contained the same speech-form, namely the word *man*, but if the language were Chinese, the two records might represent two different speech-forms, since in Chinese differences of pitch-scheme are connected with different meanings: the word *man* with a rising pitch, for instance means 'deceive,' and the word *man* with a falling pitch means 'slow'. As long as we pay no attention to meanings, we cannot decide whether two uttered forms are 'the same' or 'different'. The phonetician cannot tell us which features are significant for communication and which features are immaterial. A feature which is significant in some languages or dialects, may be indifferent in others. . . . To recognize the distinctive features of a language, we must leave the ground of pure phonetics and act as though science had progressed far enough to identify all the situations and responses that make up the meaning of speech-forms. In the case of our own language, we trust to our everyday knowledge to tell us whether speech-forms are 'the same' or different.' Thus, we find that the word *man* spoken on various pitch-schemes is in English 'the same word,' with one and the same meaning, but that *man* and *men* (or *pan* and *pen*) are 'different words,' with different meanings. In the case of a strange language we have to learn such things by trial and error, or to obtain the meanings from someone that knows the language. The study of significant speech-sounds is phonology or practical phonetics. Phonology involves the consideration of meanings. The meanings of speech-forms could be scientifically defined only if all branches of science, including, especially psychology and physiology were close to perfection. Until that time, phonology and, with it, all the semantic phase of language study, rests upon an assumption, the fundamental assumption of linguistics: we must assume that in every speech-community some utterances are alike in form and meaning. (76-78)

Bloomfield (1943) would reiterate his commitment to contrastive meaning as a principle of phonemic classification: "In language, forms cannot be separated from their meanings. It would be uninteresting and perhaps not very profitable to study the mere sound of a language without any consideration of meaning. . . . It is only the differences of meaning which decide that most of the inevitable variations of sound are irrelevant and only certain ones play a role in communication. In short, the significant features of sounds (the phonemes) of a language are, of course, those which involve a difference of meaning" (reprinted in Hockett 1970, 400-405).

[30] See Bloch (1948, 1953), Trager & Smith (1951) and Harris (1951, 1954).

[31] In the posthumously published *Cours de Linguistique Générale*, Saussure (1959) writes: "It would be interesting from a practical point of view to begin with units, to determine what they are and to account for their identity by classifying them. . . . Next we would have to classify the subunits, then the larger units, etc. By determining in this way the elements that it manipulates, synchronic linguistics would completely fulfill its task, for it would relate all synchronic phenomena to their fundamental principle" (111). But Saussure, like Bloomfield, agreed that "To divide the chain [of sounds], we must call on meanings. . . . analysis is impossible if only the phonic side of linguistic phenomenon is considered" (1959, 103).

[32] See E. M. Uhlenbeck (1950) who writes: "[I]n determining what are phonemes in a language we do not accept the point of view that one should draw a distinct dividing-line between the grammatical and the phonological analysis. Some American linguists favor this view. . . . Thus—purely by means of criteria of a phonal nature—[t]he[y] arrive at the phoneme, which therefore for [them] and the other American linguists can be nothing but relevant features of sound, whereas according to our definition phonemes are relevant moments of the form of the word and the morpheme. This aversion from the use of grammatical distinctions in phonological analysis has been—and in our opinion rightly—criticized by Pike" (254-255).

[33] According to Carnap (1928), "One object (or type of object) is called epistemologically prior with respect to another if the second is known by means of the first object, and therefore, knowing the first object is a precondition to the knowing of the second object" (74). In this way, Carnap conceived of his *Aufbau* system as a "rational reconstruction of the formation of reality, a formation which in the actual process of cognition is made intuitively" (139).

[34] Other terms associated with the distributional program are "item and arrangement" or "segmentation and classification." Postal (1968) prefers the label "autonomous phonology" or "autonomous phonemics" so as to contrast it with the "systematic phonology" of generative linguistics.

[35] But as Robins (1988) reasons the threat of terminological circularity and the inclusion of unacceptable mentalistic assumptions are distinct, as the latter does not "mean that one must follow the strict separation of levels or the biunique phonemic requirement, dear to the heart of some 'Bloomfieldians' but never a part of Bloomfield's own practice. But it does require that terms like word, morpheme, noun, verb, etc. taken as basic descriptive elements, should be defined in linguistic terms, not assumed as primitives, available and understood, on the evidence of dictionaries, spaces in alphabetic printing, existing grammars, and the like" (246).

[36] Hill (1958): "Linguists assume that the description and analysis of language must begin with description of the sounds and their patterning and that description of meaning must be put off until the first task is done. Such an attitude is often misunderstood to be a denial of meaning, but this is not true. The linguist's desire to put off analysis of meaning is no more than an application of the principle of working from the more knowable to the less knowable . . ." (409-410). Similarly—and indicative of his "moderate" Bloomfieldian status—Hall (1964) explains: "[L]inguistics deals with the simplest subject matter of all the social sciences, specifically because it eliminates from its initial consideration (though not from its ultimate concern!) the factor of meaning, which is the principal concern of the social sciences in general. Meaning then returns at a later stage in linguistic analysis, after the functional units and their groupings have been determined" (419).

Not all linguists, particularly those of European background and training, supported such methodological postponements. R. Jakobson would complain that "One of the most symptomatic features of this conference [an American conference of anthropologists and linguists held in the early 1950s and published as a supplement to the *International Journal of American Linguistics* 19, 11-21] was that we lengthily and passionately discussed the question of meaning. . . . Thus, meaning remains a No Man's Land. This game of give-away must end. For years and decades we fought for the annexation of speech-sounds to linguistics, and thereby established phonemics. Now we face a second front—the task of incorporating linguistic meaning into the science of language" (quoted in Palmer 1968, 50).

[37] As has often been remarked in the secondary literature, not one of Hockett's sixty-four chapters in *A Course in Modern Linguistics* (1958) was devoted to semantics. Rather, only grammatical, phonological and morphophonemic structure was considered central to language precisely "because they have nothing directly to do with the non-speech world in which speaking takes place" (137).

[38] As Chomsky (1955/1975) reminds us, "The question of 'independence of syntax' arose in a specific context of discussion and had to do with an important technical question: can such notions as 'phonemic distinctiveness,' 'grammaticalness,' 'syntactic category,' 'word,' and other notions of grammatical theory, be defined in a general and systematic way in terms of a basis of primitive notions including 'synonymy' and 'significance' (meaningfulness), or other notions of semantic theory? It was argued that addition of these notions to the primitive basis for the definition of the linguistic notions in question solves none of the problems that arise when the problem of defining these notions is seriously faced" (19).

[39] In this way, the American anthropological linguistic tradition of Sapir and Whorf, with their enthusiastic respect for the phonological, grammatical and semantic exotica of "non-SAE" (Standard Average European) languages was conducive to Quine's observations concerning the relativity of one's ontological commitments: "[A]s Cassier and Whorf have stressed, there is in principle no separating language from the rest of the world, at least as conceived by the speaker. Basic differences in language are bound up, as likely as not, with differences in the way in which the speakers articulate the world itself into things and properties, time and space, elements, forces, spirits, and so on" (Quine 1980, 61).

[40] Linguists associated with the neo-Bloomfieldian program would long suffer the charge of ignoring meaning *toute courte*. Hockett (1950), responding to such charges, was careful to emphasize the methodological, rather than substantive, prohibition on meaning: "No one has ever demonstrated that

semantic criteria are valueless. We have demonstrated that *certain things* of value can be determined with either no resort to meaning, or with small resort of a specific and well-defined kind. It is important to see just how far we can go without resorting to further semantic criteria. To say this is far different from labeling semantic considerations 'unscientific,' 'mentalistic,' 'mystical' or unbehavioristic,' none of which they need be" (55). Similarly, Fries (1952) would point out that "This challenge of the conventional use of meaning as the basic tool of analysis must not lead to the conclusion that I have ignored meaning as such, nor that I deny that the chief business of language is to communicate meanings of various kinds, and that the linguistic student must constantly deal with meanings" (8). And "This challenging of certain uses of meaning . . does not constitute a repudiation of all meaning in analysis. Meaning of some kind and of some degree always and inevitably constitutes part of the framework in which we operate (Fries 1954, 60-61).

Bloomfield's own reaction, however, is perhaps the best known; certainly the most heart felt. Writing to Kenneth Pike in a personal letter dated January 29, 1945, Bloomfield would complain:

> It has become painfully common to say that I or rather, a whole group of language students of whom I am one, pay no attention to meaning or neglect it, or even undertake to study language without meaning, simply as meaningless sound. It seems to be harmful and unjust that you should introduce remarks on the subject of meaning, even in a letter, by such a statement about me. . . . It is not just a personal affair that is involved in the statements to which I have referred, but something which, if allowed to develop, will injure the progress of our science by setting up a fictitious contrast between students who consider meaning and students who neglect or ignore it. The latter class, so far as I know, does not exist" (quoted in Fries 1961, 215 and Pike 1989, 220).

Chomsky (1955/1975) too would also have to fight against similar misunderstandings, for similar reasons:

> I will merely emphasize again that the 'legitimacy of semantics' (whatever that might mean) has never, to my knowledge, been challenged, nor has there ever been any question of the importance of incorporating a study of reference, meaning, language use, and related topics within a full theory of language that will deal, in particular, with the highly significant relations between formal structure and semantic interpretation. The appeal to meaning must be clearly distinguished from the study of meaning. The latter enterprise is unquestionably central to the general theory of language, and a major goal of the SS-LSLT [Syntactic Structures-Logical Structure of Linguistic Theory] approach is to advance it by showing how a sufficiently rich theory of linguistic form can provide structural descriptions that provide the basis for the fruitful investigation of semantic questions. But the appeal to meaning within the theory of linguistic form has all too often served simply as a way of side-stepping serious inquiry. (21-22)

[41] In rare cases, this learning-theoretic rationale was made explicit whereby distributionalist procedures were viewed as modeling what the child language learner must actually go through. Hockett (1948) writes: "The analytical process thus parallels what goes on in the nervous system of a language-learner. . . . The essential difference between the process in the child and the procedure of the linguist is this: the linguist has to make his analysis overtly, in a communicable form, in the shape of a set of statements which can be understood by any properly trained person, who in turn can predict utterances not yet observed with the same degree of accuracy as can the original analyst. The child's 'analysis' consists, on the other hand, of a mass of varying synaptic potentials in his central nervous system. The child comes to behave the language the linguists must come to state" (279-280).

[42] Here I seem to disagree with Bar-Hillel (1954) who inverts these priorities: "The tendency exhibited by many contemporary structural linguists to set up sharp demarcation lines between the various linguistic subfields is presumed to be based, first, on the attempt to keep linguists as far as possible independent of concepts open to the intrusion of meaning and, secondly, on a fear of circularity in the definition of basic terms" (237).

[43] See Esper (1968) and Beyli (1967) for an account of Weiss's influence on Bloomfield. Bloomfield's (1926) article "A Set of Postulates for the Science of Language" was modeled directly upon Weiss's "One Set of Postulates for a Behaviorist Psychology" (1925) and Bloomfield encouraged Weiss to contribute an article entitled "Linguistics and Psychology" to the inaugural issue of *Language*.

[44] Swadesh (1948) too would question the perceived hegemony of the "mechanists": "It would be interesting to have some concrete measure of the extent to which mechanical materialism pervades American linguistics today. I believe the mechanists tend to be credited with being much more numerous and representative than they really are" (259).

[45] On a more personal note, Hall (1987a) reports that Bloomfield's aversion to mentalism ran so deep that he resisted seeking psychotherapeutic help for his wife's depression because "He was too much a 'mechanist' to allow himself to place any trust in treatment of 'mental' phenomenon. (In fact, he had a strong block against the words mind or mental, which he refused to interpret in any way except that of reference to some kind of non-physical, unobservable, and hence irrelevant factors in human life.)" (65).

[46] While Bloomfield traced this view of language to the behaviorist psychology of Albert Weiss (see Bloomfield's 1931 obituary of Weiss reprinted in Hockett 1970, 159-161), Fries (1961) also sees many behaviorist features in Bloomfield prior to his encounter with Weiss. Esper (1968) and Schlauch (1946) locate Bloomfield's conversion to behaviorism within the wider context of both European and American psychology. Mediating somewhat between these two views—and consistent with the positivism being attributed to him in this chapter of the present dissertation—are comments by Hymes & Fought (1981). They argue that what "Bloomfield got from Weiss [was] not linguistics, but a conception of science" (105). (See also Ohlander 1976, 25.) Bloomfield's views regarding the science and philosophy of linguistics were picked up and further amplified by Charles Hockett, most notably in an article entitled "Biophysics, Linguistics and the Unity of Science" (1948). Hockett (1999) also discusses Bloomfield's behaviorism.

[47] As is perhaps characteristic of positivist movements generally, criticism is often viewed as reactionary at best and as unintelligible at worst. According to Gray (1980), "The Americans held themselves to be scientific and therefore impartial, so that they did not feel obliged to take too seriously the arguments of the few outsiders who seriously disagreed with them. They knew what science is: it is what they thought and did. This attitude, if it did not always curtail debate, kept it one-sided. The Americans evidently disdained argument as unseemly and unnecessary in science" (7). Twaddell (1935) provides a telling example of this:

> The principles presented in this section ["The phoneme as a mental or psychological reality"] are limited strictly to applications of scientific methodology, and accordingly do not involve discussion with those to whom behaviorism is unacceptable as a psychological or biological assumption or (irrelevantly) as a 'philosophy'. Whatever our attitude towards mind, spirit, soul, etc., as realities, we must agree that the scientist proceeds as though there were no such things, as though all his information were acquired through processes of his physiological nervous system. In so far as he occupies himself with psychical, non-material forces, the scientist is not a scientist. The scientific method is simply the convention that mind does not exist; science adopts the nominalistic attitude toward the problem of universals, in matters of procedure. (56, as reprinted in Joos 1957)

And, later, here is how Hall (1946) would express his own exasperation:

> The present-day intellectual atmosphere in Europe is influenced by an essentially reactionary hostility to objective science, and by a return to doctrines of 'spiritual activity,' 'creativity of the human soul,' and socially based value-judgments which European scholarship has inherited from the aristocratic, theological background of medieval and Renaissance intellectualism. This reactionary attitude is present in the theorizing of many modern European students of language, who sacrifice positive analysis of concrete data to discussion of purely imaginary, non-demonstrable fictions like 'thought' and 'spirit' as supposedly reflected in language. In American work on language, the burning question at present is whether this same anti-scientific attitude is to be allowed to block the further development of linguistics and its contribution to our understanding of human affairs, especially in our teaching. (33-34)

The dogmatic tenor of the well-known exchanges between Leo Spitzer and Bloomfield (and Hall too—Spitzer once accused Hall of setting up an "academic FBI" to patrol against any dissent) is unsurprising in this regard. After reviewing the charges of "medieval speculation" leveled against him, Spitzer responds by pointing out that Bloomfield "does not seem to realize that such a [positivist] statement itself constitutes a 'philosophy' (albeit a very shallow decoction of eighteen-century theories of the *philosophes*) and that a

linguistic treatise based on this antiphilosophic philosophy is necessarily bound to be a mentalistic philosophy (of the Leonard Bloomfield brand of mentalism, of course). I surmise that the antimentalists can only afford to call themselves 'unbiased by philosophy' because they suppose that what they ignore (or are ignorant of) can be eliminated at will and does not exist . . ." (quoted in Bloomfield 1944, 54). See Spitzer (1944, 1946) and Gray (1980) for more details concerning these acrimonious debates.

[48] The full passage reads: "Trubetzkoy phonology tried to explain everything from articulatory acoustics and a minimum set of phonological laws taken as essentially valid for all languages alike, flatly contradicting the American (Boas) tradition that languages could differ from each other without limit and in unpredictable ways, and offering too much of a phonological EXPLANATION where a sober TAXONYMY would serve as well. Children want explanations, and there is a child in each of us; descriptivism makes a virtue of not pampering that child" (Joos 1957, 96; emphasis original).

[49] Looking back at all the attempts to provide a non-semantic reduction of the phoneme, one linguist wistfully observed that Bernard Bloch's (1953) "Contrast" article "probably represents the apogee of American descriptivist phonemic theory and practice. It was the last such major paper to appear in *Language,* and in the years immediately following the problems and methods with which it concerned itself—as well as the full-blown 'phonemic paper' itself as a genre for linguistic publication—would begin to hold less and less interest for many linguists" (Miller 1969, xxv). Similarly Hill (1967) remarks:

> Bernard Bloch's postulates (1948) occupy a curious place in linguistic discussion. Since the whole concept of a phonological base for grammatical recognition and interpretation is now under vigorous [generative] attack, one might expect that the article would be forgotten or dismissed as of only antiquarian interest. Instead, the usual position seems to be that it was a work so painstakingly explicit that it revealed all the weaknesses of the neo-Bloomfieldian position. It is certainly true that the work was explicit, and that by being so it raised important issues, but it seems to me that to regard it as no more than a brave but inevitable failure is a curiously roundabout way of bringing it into line with currently fashionable thinking. (203)

[50] The Chomskyan framework of generative phonology neither provides, nor aspires to provide, an account of either basic phonemic equivalence or basic phonemic contrast (semantic or otherwise). Neither Chomsky nor Postal claim that generative phonology provides a way of either defining phonemic equivalence or phonemic contrast. So while generative phonology may be considered to have definitively undermined the basis for postulating an independent phonemic level of linguistic analysis/representation, at no point does generative phonology do away (or aspire do to away) with the notion of phonemic "contrast," nor offer an alternative non-semantic explanation for, or account of, the basis of such contrast. Postal's (1968) monograph is very clear on this point (see, xiii, 5, 7-15, 20-22, 27-31). Postal argues that he and the phonemicists differ on the claim whether or not generative phonology can *represent* the difference between phonemic contrast and free variation repetition—without the postulation of phonemes. Postal argues that not only can generative phonology represent the difference between contrast and free variation; it can do so better than taxonomic phonemics. Postal criticizes the phonemicists for falsely believing that the phoneme was required in order to *represent* the difference between contrast and free variation, not because they believed (as does Postal) that contrast and free variation are fundamental to phonology. As Postal summarizes in his conclusion, "Autonomous phonemics and systematic phonemics are not completely opposed, but they differ in the overwhelming body of assertions they make. Their agreements are limited to little more than *the recognition that there is a notion of free variation/contrast,* that some phonetic features are predictable, and that there is some kind of representation of sentences between that of morphemes and phonetics" (310; emphasis added). There is, then, nothing wrong with the notion of contrast (indeed, it is an essential "primitive" according to generative phonology); it is the phoneme which is theoretically problematic and descriptively expendable without any loss (in fact a gain, according to Postal) in linguistic accuracy. It is the *phoneme* which is superfluous; not the notion of contrast embedded in it. This is why Postal argues that "The argument [for the necessity of representing contrast], does not in other words, show the need for the former [the phoneme]" (5) and "We see then that the claim that a grammar *without* autonomous phonological representation cannot correctly describe the facts of repetition is completely groundless" (15; emphasis original).

And this is why, when Postal correctly maintains that generative grammar can, at a minimum, equally "describe" ("represent," 12; "characterize," 15; "predict," 31) contrast and repetition as autonomous phonemics, he is not crediting generative phonology with any explanatory force regarding the basis of this distinction. Indeed, says Postal, "the exact basis or nature of this [contrastive] relation is and has been quite controversial. There is, for example, a large literature dealing with the question of whether this basis is semantic, a conclusion which many linguists have seen as self-evident. We need not be concerned within such questions here, and for present purposes the notion may be considered primitive. All linguists are agreed that there is such a notion, and working on any particular language there is, in practice, almost complete agreement on the assignment of utterance tokens to contrasting or noncontrasting [repetition] sets" (7). What Postal ignores is that—unlike generative phonology—at least "semantic-based" phonology provided some explanation of the difference between free variation and phonemic contrast, even if unpalatable to some.

[51] See Bloch (1948, 5), Harris (1951, 186 and 365), Hockett,(1968, 24) and Trager & Smith (1951, 54).

[52] See Postal (1968) where "In view of the generally negative position taken in this work on autonomous phonemics, it is fair to indicate what I take to be its real contributions" (311-312) including descriptive analyses of vast number of languages, development and use of complementary allophonic distribution, and generally increased formal clarity to linguistics.

[53] Even Postal would criticize Chomsky for thinking that such operational tests deliver what they advertise. According to Postal (1968), "Chomsky was concerned with suggesting an operational approach to defining linguistic features and excluding nonlinguistic audible features such as rate of speed, absolute loudness, etc. I see no reason today to believe any such operational test exists. Certainly none has ever been proposed. . . ." (227).

[54] While summarizing the general thrust of "Bloomfieldian" linguistics, Koerner (2002) points out Chomsky's continuity:

> In general their approach was inductive, starting with the smallest, empirically identifiable units, trying to build from the ground up, as it were, aiming at the highest level, i.e., that of syntax. While the post-Bloomfieldians could pretend, on the level of phonology, that they could do without reference to semantic content by simply asking (themselves or an informant) whether a phonic structure was the 'same' or 'different', the trouble with meaning began to surface on the level of morphology, not to mention syntax (which few of them were concerned with at the time). As we have seen, in his early work Noam Chomsky, in particular his *Syntactic Structures* of 1957, continued to operate within the (post-) Bloomfieldian tradition, especially that line which held that semantic considerations weren't really needed when analyzing language in a 'rigorous' manner. (105)

Similarly Joseph et al. (2001) observe:

> Perhaps the most significant continuity between Harris's *Methods in Structural Linguistics* (1951) and Chomsky's *Syntactic Structures* (1957) is the exclusion of meaning. Zellig Harris's whole enterprise, in fact, is to show how phonological and grammatical units of language can be identified without reference to the meanings of the utterances in which they may be held to occur. In Chomsky's early work, the setting aside of meaning arose from an argument that what makes a particular segment of words in a language grammatical has nothing to do with meaningfulness or significance. *Color green ideas sleep furiously* and *furiously sleep ideas green colorless* are both nonsensical, but the former is grammatical and the later is not. Such examples suggest to Chomsky (1957: 15) that 'any search for semantically based definition of 'grammaticalness' will be futile'. (125)

Chapter 7
Quine and the Phoneme:
The Search for a Non-Semantic Definition

> There are abundant reasons to suspect that neither this oversimplified account nor anything remotely resembling it can possibly provide an adequate definition of the phoneme; and phonologists have not neglected to adduce such reasons. As a means of isolating other points of comparison between grammar and lexicography, however, let us make the unrealistic assumption that our grammarian has some such non-semantical definition of the phoneme. . . . The basic point of view is that the class *K* is objectively determinate before the grammatical research is begun.
>
> W. V. Quine (1953a, 53)

Among the giants of analytic philosophers of language, Quine stands alone in attempting to directly relate his distinctive views on language to a specific issue central to phonology. Though largely assumed among philosophers of language to be philosophically inconsequential, and in any event linguistically peripheral, discussions of the phoneme have played an unusually prominent role within Quine's philosophy. This is, in itself, notable. Though perpetually engaged with semantic issues and (via questions of logical analysis) also with syntax, philosophers almost universally tend to ignore the third pillar of this standard triumvirate of linguistic structure. Though a few linguists have tried to explain this lapse, and even express some of their concerns,[55] phonology remains essentially neglected among philosophers of language.[56] This is how the British linguist Christopher Hutton expresses his frustration:

> There has been no focused debate within 20th century linguistics and philosophy of language as to the nature, applicability and validity of the terms type and token . . . [and] once attention is directed to the topic and precise definitions are sought, it is realized that it is by no means simple to give substance to the distinction. What is striking is the isolation of these discussions from mainstream philosophical considerations both of the nature of language and the nature of categorization. The type-token relation is granted a sacrosanct status that reflects a fundamental division between language and non-language. . . . Even those philosophers who are interpreted as questioning the notions of meaning that underlie this division have not yet perceived the theoretical problems

involved in setting up classes of similar forms, relying on the solidity of the orthographic word. (1990, 60)

But unlike his philosophical colleagues, and to his credit, Quine has repeatedly gone out of his way to ask—and answer—questions usually only addressed by linguists, and among them, only phonologists: How should the term "phoneme" be defined within linguistics? What is the basis of our native natural language phonemic intuitions governing the relationship between different token allophonic pronunciations of the same type of word? Quine's responses, though variable in detail, do have one thing in common: they are all uniformly committed to an account of phonological equivalence untainted by semantics. And while Quine should be admired for attempting to bring the flora of phonological theory in line with his preferred philosophical landscape devoid of meaning, what started out as a small but resistant phonological weed has proven strong enough, when appropriately watered, to overshadow the very basis of Quine's seminal attack on analyticity (chapter 5). Given that basic phonological equivalence is not strictly asemantic in nature, but recoverable only with the help of interpretive semantic considerations, the observation that analytic equivalence is not definable without meaning comes as no surprise.

The history of Quine's career-long relationship to the phoneme is clear enough. As early as 1951 and as late as 1990, the pursuit of "phonemic reductionisms"—that is, the project of providing a definition of phonemic equivalence in strictly non-semantic terms—has consistently attracted Quine's attention. There are several convergent reasons for this persistent interest in the phoneme. One is Quine's philosophy of language. Empiricist in orientation and behaviorist in implementation, Quine's philosophy of language came of age during the pre-Chomskyan period of American

descriptivism where both the style and substance of Quine's no-nonsense physicalism matched the prevailing scientific outlook among contemporary "neo-Bloomfieldian" linguists. Accessing the positivist descriptivism of American distributional linguistics characteristic of the 1940s and 1950s required little effort and undoubtedly confirmed Quine's own suspicions of what linguistics was and, more importantly, should be about. In a sense, Quine's linguistics was their "philosophy of language" just as much as their "neo-Bloomfieldian" philosophy of language was Quine's linguistics.

Then there was also, in addition to this convergent linguistically applied positivism, the phoneme. The then-current debates raging around a proper definition of phonemic equivalence—and in particular provocative talk of an "irreducibly semantic" component to phonemic analysis—represented a ripe target for Quine's asemantic principles. Encouraged by both the theoretical aspirations and positivist rhetoric of several prominent "neo-Bloomfieldian" linguists, Quine found an issue that was eminently hospitable to his brand of philosophical linguistics. Indeed, few issues within American linguistics were so divisive. With even Leonard Bloomfield, the guiding inspiration for a generation of "neo-Bloomfieldian" linguists, having openly conceded—as one of the "fundamental assumptions of linguistics" (1933, 78)—that semantics was indispensable as an entry into phonemic analysis, the basic character of the phoneme was hotly disputed. And it was precisely Bloomfield's claim that "It is important to remember that practical phonetics and phonology presuppose a knowledge of meanings: without this knowledge we could not ascertain the phonemic features" (1933, 137) which led some "extreme" neo-Bloomfieldians to actually reprimand Bloomfield himself.

Rather than specifying an objective procedure for defining phonological sameness and difference, Hockett (1949) would complain that Bloomfield relied on "our everyday knowledge to tell us whether speech-forms are the 'same' or 'different' [Bloomfield, 1933, 77]" (reprinted in Makkai 1972, 206).[57] Linguistic theories indebted to such "folk phonological," as it were, procedures based upon semantics were dismissed as insufficiently objective to properly ground phonology or a true science of language generally. For this reason, successfully reducing the phoneme, for both the good of linguistics and philosophy, would represent a significant advancement toward a more thoroughly scientific approach to language. This is why in a review of Harris's *Methods in Structural Linguistics* (1951), Hockett would proudly (though incorrectly, see below) report that, with Harris's "pair-test" method, "Bloomfield's other fundamental element—meaning—is entirely eliminated" (reproduced in Hymes and Fought 1981, 163-164). Given that the basic building blocks of language were assumed to be phonemes[58]—and languages were to be non-circularly described without presupposing any of the information (no less semantic information) which the linguist was charged with actually reconstructing (or as with Quine eliminating)—there could be no better, indeed no other, place to start empirically studying language than at the very bottom.

Broadly consistent, then, with both classically empiricist approaches to knowledge construction as well as a positivist aversion to meaning, it is unsurprising that Quine would join the debate on phonemic reductionism. The debate consumed much of linguistics proper, but occasionally also attracted philosophers—Hilary Putnam (1961) for example (see below)—in addition to Quine. As Chomsky (1957) himself argued, the claim that phonology was somehow dependent upon semantic considerations would

entail "that in order to construct a phonemic system it is necessary to know which utterances are different in meaning. To know difference in meaning is to know synonymy, and this is the central term of the theory of meaning. If accepted, then, this claim is an open admission that linguistic analysis must be based on precisely the most dubious part of semantic theory" (95). Sounding more like Quine than some linguistic-cum-philosophical conventional wisdom can comfortably accommodate, "the central objection to meaning as a criterion of analysis" says Chomsky (1955), "has always been the obscurity of semantic notions" (141-142).

More serious than just upsetting a positivist methodology then central to neo-Bloomfieldian linguistics, an irreducibly semantic phoneme would, from Quine's perspective, generate a dynamic entirely internal to his own philosophy (chapter 5): If non-semantic reduction is the criterion distinguishing the Quinean permissible from the Quinean intolerable, then, depending upon the success of phonemic reductionism, not only must we learn to live with analytic eliminativism, we must also make peace with a new form of Quinean skepticism—phonological eliminativism.

Phonemic Reductionism

Historically, competing accounts of the phoneme have ranged from the explicitly semantic (Bloomfield 1933)—as well as implicitly semantic, i.e., "functional" (Vachek 1966), "theoretical" (Twaddell 1935), or psychological (Sapir 1933)[59]—to various "physicalistic" definitions (Jones 1967).[60] Among the latter are physicalist theories committed to either (i) a direct *psychophysical* reduction of the phoneme to a specific invariant acoustic property (see, for example, an early Bloomfield (1926) where the

phoneme is described as "recurrent acoustic fraction of sound-waves"[61]); (ii) a *perceptual* reduction to a "family" of similar sounds organized around a central prototype (Jones 1957)[62]; (iii) a *distributional* reduction underwritten by the structuralist principle of "complementary allophonic distribution" (Bernard Bloch 1948, 1951, 1953); and finally (iv) a *behaviorist* reduction based upon the observed behavior of a native informant (Chomsky 1957, Harris 1951, Quine 1990). Since Quine is specifically interested only in theories which are non-semantic in nature, and at that, discusses only a version of perceptual prototype theory (1953a, 1960) only to reject it in favor of a behavioral reduction (1981, 1990), we shall concentrate only on accounts (ii) and (iv). Nevertheless, by way of orientation, it is perhaps useful to provide a summary—dated, but appropriately contemporaneous with Quine's early interest in the phoneme—of some of the techniques used to arrive at a non-semantic definition of phonemic equivalence.

Writing in 1950, J. Lotz provides the following review of the most significant of these non-semantic proposals:

> There have been attempts to try to define the units of speech without recourse to semiotic notions:
>
> 1. Some investigators assume that the phonemes will appear as clustering in an n-dimensional system. It seems, however, that it will never be possible to distinguish between allophones, phonemes, or classes of phonemes sharing one sound feature, by means of measurement alone. There is one concrete case discussed by E. Zwirner; he analyzed the German vowels, which—as is known—have two distinct degrees of length. The unclassified data did not show two maxima; but, when the classes were distinguished according to linguistic [semantic] criteria, the distribution of the two classes followed the Gauss curve.
>
> 2. B. Bloch thought that the use of distributional criteria would yield phonemes, since in most languages the environments of the phonemes are not always identical; in English, e.g., we have seal/zeal, but only zebra. It could be assumed that if the difference between two sounds is phonemic, only one of them could occur in a given environment. This theory, however, would not be valid in a language in which the phonemic net is completely utilized. Nor could one expect that utterances would re-occur in all variations before other similar problematic cases turn up. Bloch himself lapses tacitly into semiotic considerations.

3. Some scientists have attempted to use child language and code breaking as arguments in favor of a non-semiotic approach to the problem of sound. This, however, cannot be accepted, since in both cases fully developed semiotic systems are given in advance. Thus it seems that, although the sound wave contains all the information about the phoneme, this information cannot be extracted by analysis of the sound wave alone, because the phoneme implies the use of a descriptive function, the elective predicate of which is semiotic. (1950, 714)

Phonemic Reductionism I: Prototypical Norms of Similarity

Such are the short-comings of phonemic reductionism. But evidently not content to leave the project of phonemic reductionism to phonologists themselves, Quine has expressly argued against a semantic-based definition of the phoneme. Sitting right alongside "Two Dogmas" (as Chapter Three of *From a Logical Point of View*), Quine takes up the phoneme in "The Problem of Meaning in Linguistics." In this article (apparently Quine's first published remarks on the nature of phonemic categorization), Quine emphasizes the importance of an observation-based, empirical account of basic phonological equivalence. For Quine, the very legitimacy of linguistic analysis demands an account of basic phonological equivalence free of semantic considerations. And the solution is to be found in the "natural clustering" of sounds into phonemes.

Invoking the same "radical translation" strategy later deployed in connection with the "indeterminacy of translation" and the "inscrutability of reference," Quine (1953a) assumes the linguistic burden of a grammarian "at work on a hitherto unstudied language, and that his own contact with the language has been limited to his field work" (49). As the linguist John Fought (1999a) explains with reference to the objectivist methodology of descriptivist linguistics, "We are not just admonished to avoid preconceptions here; we are also shown what it is like to work without them" (5). For Quine, as for his "neo-Bloomfieldian" counterparts, the ideal of approaching language from the "outside" was

supposed to be more than just an important methodological canon, but ideally, an empirical challenge to be implemented in practice.

With the task of empirically reconstructing a natural language thus defined, Quine proceeds to delineate the prerequisite steps for the linguist to arrive at a methodologically sound description of languages generally. According to Quine, the linguist's first challenge is "to discover the bounds of the class *K* of significant sequences of the language" (1953a, 49). But the grammarian is also charged with isolating which "parts of [these] significant sequences count as significant, down to the smallest adopted unit of analysis; so such units, whatever they are, are the shortest members of *K*" (49).

With this objective in mind, Quine notes that even this first task is much easier said then done. In addition to specifying the "length dimension" of individual words, there is, Quine realizes, also the dimension of what he calls "thickness" to consider:

> For given two utterances of equal and arbitrary length and fairly similar acoustical make-up, we must know whether to count them as occurrences of two slightly different members of *K* or as two slightly different occurrences of one and the same member of *K*. The question of thickness is the question what acoustical differences to count as relevant and what ones to count merely as inconsequential idiosyncrasies of voice and accent.
>
> The question of thickness is settled by cataloguing the phonemes—the single sounds, distinguished as coarsely as possible for purposes of the language. Two subtly differing sounds count as the same phoneme unless it is possible, by putting one for the other in some utterance, to change the meaning of the utterance. [Here a footnote references Bloch and Trager (1942) and Bloomfield (1933).] Now the notion of phoneme, thus formulated, depends obviously and notoriously on the notion of sameness of meaning, or synonymy. Our grammarian, if he is to remain pure grammarian and eschew lexicography, must carry out his program of delimiting *K* without the help of a notion of phoneme so defined.

Recapitulating the positivist methodology of American descriptivism—and in particular the prohibition against "mixing levels" (Hockett 1942) basic to neo-Bloomfieldianism and central to Zellig Harris's sophisticated structural linguistics—Quine is unhappy with

this meaning-based approach to defining the phoneme. Yet "[t]here seems indeed," continues Quine, "at first glance, to be an easy way out" for the linguist:

> [H]e can simply enumerate the phonemes needed for the particular language at hand, and dispense with the general notion of phoneme defined in terms of synonymy. This expedient would be quite admissible as a mere technical aid to solving the grammarian's problem of specifying the membership of *K*, if the problem of specifying the membership of *K* could itself be posed without prior appeal to the general notion of phoneme. But the fact is otherwise. The class *K* which it is the grammarian's empirical business to describe is a class of sequences of phonemes, and each phoneme is a class of events. (It will be convenient to swallow this much Platonism for present purposes, though some logical maneuvers might serve to reduce it.) The grammarian's problem is in part objectively set for him thus: every speech event which he encounters in his field work counts as a sample of a member of *K*. But the delimiting of the several members of *K*, that is, the grouping of mutually resembling acoustical histories into bundles of proper thickness to qualify as linguistic forms, needs also to have some objective significance if the task of the field grammarian is to be made sense of as an empirical and objective task at all. . . . Thereupon the grammarian's business, with respect to a language *L*, can be stated as the business of finding what sequences of phonemes of *L* are significant for *L*. Statement of the grammarian's purpose thus depends not only on 'significant,' as we had been prepared to expect, but also on 'phoneme'.
>
> But we might still seek to free grammar of dependence on the notion of synonymy, by somehow freeing the notion of the phoneme itself from such dependence. It has been conjectured, for example, by Bühler,[63] that this might in principle be accomplished. Let the continuum of sounds be arranged in acoustical or physiological order in one or more dimensions, say two, and plotted against frequency of occurrence, so that we come out with a three-dimensional relief map in which altitude represents frequency of occurrence. Then it is suggested that the major humps correspond to the phonemes. There are abundant reasons to suspect that neither this oversimplified account nor anything remotely resembling it can possibly provide an adequate definition of the phoneme; and phonologists have not neglected to adduce such reasons. As a means of isolating other points of comparison between grammar and lexicography, however, let us make the unrealistic assumption that our grammarian has some such non-semantical definition of the phoneme. Then his remaining task is to devise a recursive description of a class *K* of forms which will comprise all and only those sequences of phonemes which are in fact significant.
>
> The basic point of view is that the class *K* is objectively determinate before the grammatical research is begun. . . .

And so immediately after outlining the next level of an "orthodox" (referencing Bernard Block and Bloomfield again) recursive specification of *K* (what linguists refer to as the morphophonemic level of analysis), Quine pauses to reaffirm his commitment to these

self-imposed non-semantic credentials: "There is no denying that the grammarian's reproduction of *K*, as I have schematized it, is purely formal, that is free of semantics" (1953a, 53). Having clearly explained the importance of identifying a non-semantic basis by which to individuate the "thickness" of individual sound segments, yet cognizant of the many difficulties of doing so, Quine awkwardly concludes that his "schematized" methodology adheres to the demands of an objectively determinate empiricist analysis unfettered by semantic considerations.

There are two features of Quine's discussion of "phonemic reductionism" to specifically note. First, Quine does not contend that he has avoiding meaning by adopting an extensional test for phonemic equivalence. Quine does not respond to meaning-based accounts of phonological equivalence by providing a reference-based account. Quine does not argue that phonological equivalence can be defined in terms of reference as opposed to meaning. Instead, Quine argues that *uninterpreted phonetic form* is sufficient: no interpretive appeal to the semantics of meaning *or* the semantics of reference required. This is consistent with the historical neo-Bloomfieldian background leading up to Quine's discussion. Linguists committed to a non-semantic reduction of phonemic categorization did not distinguish between extensional reference and intensional meaning. Meaning for them was the reference of common nouns and the properties expressed by predicates.

The second feature to note about Quine's discussion is a certain ambivalence between the philosophical importance of a semantic-free phoneme on the one hand and the empirical short-comings of phonemic reductionism on the other. But Quine is not alone in this respect. An all too familiar theme within neo-Bloomfieldian linguistics

generally was the tension, periodically recognized by the participants themselves, between the idealized objective of defending a non-semantic-based analysis of phonological structure versus the actual necessity of relying on semantics to identify phonemic equivalence. This ambivalence explicitly re-appears in Hockett[64] and Bloch[65] (and arguably implicitly throughout the writings of Bloomfield), with the renegade Kenneth Pike (1947b, 1952a, 1958)[66] performing the thankless job (along with some other, though mostly European, linguists, see Diderichsen (1958), Fischer-Jorgensen (1958), and Uhlenbeck (1950)) of repeatedly pointing this out.

In any event, Quine (1960) returns to the issue of non-semantic phonemic reductionism in *Word & Object*. Having apparently resolved to his satisfaction his earlier skepticism regarding phonemic "thickness," Quine again invokes the statistical "humps" of prototype clustering previously dismissed as unrealistic:

> Linguists handle the phonetic norms with the help of their concept of phoneme. The phonemes of a language are to speech in that language what letters are to writing. . . . The phonemes of a language can be viewed as short segments of the norms of utterances in that language. Linguists choose them short enough so that they can keep their number down and still represent every longer norm as a string of them. Talking of phonemes enables linguists to abstract from all phonetic minutiae not germane to the grammar and lexicography of the language; for each phoneme is just the norm, as against the innumerable more or less passable deviations from that norm.

Indeed seemingly aware of the then-current linguistic debate raging between "hocus pocus" linguists favoring an instrumentalist view of phonological theory and partisans of a "God's truth" view of phonological objectivity,[67] Quine continues:

> The law of phonetic norms gives substance to the phonematic approach, assuring us that any utterance has as its norm the sequence of those phonemes approximated in the utterance. But note that that law affords no basis for any particular snipping of phonemes to length. Whether to treat 'cheer' as segmented merely into two syllables 'chee' and 'er,' or into a consonant 'ch,' a vowel 'ee,' and a vowel 'er' [sic], or into a consonant 't,' a consonant 'sh,' a vowel 'ee,' a glide 'y,' and a vowel 'er,' is indifferent to our law of phonetic norms and to the child's learning of language as well. The language has its

utterances and its norms, and then the linguist imposes a technical segmentation upon the norms to implement his business of specifying the lot. (89)

Here, Quine again appears confident that some asemantic measure of phonetic similarity accounts for phonemic classification. But it is Quine's philosophy—not the linguistic data—which encourages Quine to be so sanguine in the face of his own doubts and linguistic counter-evidence. But even Quine is not philosophically alone in this regard. Quine's nominalist colleague Nelson Goodman would also appeal to perceptual similarities in his own attempt to account for word-type equivalencies among distinct word-tokens without appeal to abstract linguistic types. As Goodman (1972) complains:

> In platonistic terms, the distinction between the general pattern or *type* of a word or sentence and its particular instances or *tokens* was drawn many years ago by Pierce. Too often, however, those who have noticed the distinction have looked upon it as a matter of isolated interest and assumed that thereafter one need be concerned only with the types. . . . Indeed, it is the types that we can do without. Actual discourse, after all, is made up of tokens that differ from and resemble each other in various important ways. Some are "now"s and others "very"s just as some articles of furniture are desks and others chairs; but the application of a common predicate to several tokens—or to several articles of furniture—does not imply that there is a universal designated by that predicate. . . . Although each utterance and inscription is a separate word (or statement or letter, etc.), the difference between two words often has no practical importance. For most purposes, we need not distinguish among several "Pisa"s, all of which name the same thing, even though they differ widely in size, shape, color, sound, place, date, etc. It is true that a given 'Pisa' may be more like a given 'Paris' than like some other 'Pisa,' just as a given mushroom may look more like a given toadstool than like some other mushrooms; but in both cases we must discern just that overt difference that is correlated with a difference in appropriate use. . . . In the case of "Pisa"s and "Paris"s, and in many other cases, some certain difference of shape or sound-pattern is the clue to a difference in what the words name. (207-209)

Like Quine, Goodman is interested in providing a perceptually-driven account of word-type equivalence. The problem is that Goodman does not actually specify how "significant" word-token differences are to be distinguished from the "insignificant" word-token differences—nor what "difference in appropriate use" is supposed to amount to if not semantic content. Primarily concerned with repudiating abstract types (linguistic

or otherwise), Goodman is content to rely on an intuitive appreciation of linguistic form to avoid rather the resolve the problem of natural language word-type equivalence. But it is no more sufficient to invoke a "certain difference of shape or sound-pattern" than to say, along with Linsky (1950), that "For convenience the reader may think of printed expressions as sign-events and, for the relation of similarity, have in mind some close approximation to geometrical congruence" (232). Perceptual similarity is too weak to ground basic word-type equivalencies.

Nor is Goodman's reliance on "replication" sufficient. According to Goodman (1972), various tokens are catalogued as the same "type" when they are "replications" of each other. But Goodman provides no principle by which to constrain "replication." And this is why what Goodman famously says about "similarity" is equally applicable to his own notion of replication. Though "ever ready to solve philosophical problems and overcome obstacles," replication, like similarity, is in fact "a pretender, an impostor, a quack . . . professing powers it does not possess" (1970, 437). Appeals to "replication" do not solve the problem of linguistic equivalence; they only displace it in favor of questions concerning the basis of "replication." After all, Goodman introduced the term "replication" as a convenient way of "speak[ing] about words (or letters or statements, etc.) that are catalogued under a single label" (1972, 209) without appealing to abstract linguistic types. But this is obviously not an explanation for why word-tokens would be classified as "replicants."

Here is how one contemporary commentator critically appraised Goodman's suggestion regarding "replication", in this case, orthographic replication:

> If it is true that all tokens which are not "replicas" of each other, must differ in meaning, how much alike are 'dog,' 'DOG,' '*dog*' and 'dOg' in meaning? And do any of these

compare more favourably than the rest with (say) 'canine'? In fact, can we even say that 'dog' and 'dog' are mutual "replicas," since we know that they do, after all, differ in microstructure?. . . The only way in which this situation can be remedied is to say that various occurrences *like* 'dog'—*i.e.* "replicas" of it—are instances of the "same word." . . . But such a device presupposes some suitable analysis of 'like' or 'replica'. . . . Is a script-version of 'dog' "more like" the printed word than is the hand-sign for a dog? Is the German adjective 'gut' a "replica" of the English noun 'gut' [and hence of the same meaning] or not? Perhaps we should want to answer this last question in the negative—but this could be only because we feel that these two usages exhibit different meanings—yet, as we can see, this would be a circular procedure for analyzing the difference in meaning between 'gut' (German) and 'gut' (English). In short, the implications of the Goodman thesis are hopelessly complicated and confusing. . . (Meckler 1954, 74)

Similar considerations prompt Rollins (1950) to complain that Goodman's account is unworkable for, "unless still further qualifications be added, it would make 'TRIANGLE' and 'triangle' differ in meaning because 'TRIANGLE-description' and 'triangle-description' have different extensions; and likewise would make 'toffee' and 'toffy' differ in meaning because the extension for the 'the expression 'toffee' in Webster's' is not the same as that for 'the expression 'toffy' in Webster's'" (43-44).

In any event, in "Seven Strictures on Similarity," Goodman (1970) himself concedes (as his "Second Stricture"):

Similarity does not pick out inscriptions that are 'tokens of a common type' or replicas of each other. Only our addiction to similarity deludes us into accepting similarity as the basis for grouping inscriptions into several letters, words and so forth. The idea that inscriptions of the same letter are more alike than inscriptions of different letters evaporates in the glare of such counterexamples as those in Figure 1.

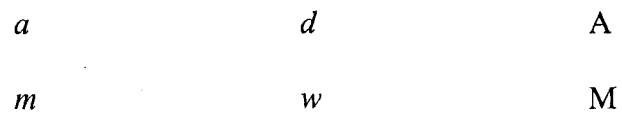

FIGURE 1

One might argue that what counts is not degree of similarity but rather similarity in a certain respect. In what respect, then, must inscriptions be alike to be replicas of one another?. . . We have terrible trouble trying to say how two inscriptions must be alike to be replicas of one another—how an inscription must resemble other inscriptions of the letter *a* to be itself an *a*. I suspect that the best we can do is to say that all inscriptions

that are a's must be alike in being a's. That has the solid ring of assured truth, but is hardly electrifying. Moreover, notice that to say that all a's are alike in being a's amounts to simply to saying that all a's are a's. The words "alike in being" add nothing; similarity becomes entirely superfluous. (438-439)[68]

As MacIver (1937a) argues in an extended exchange in the pages of *Analysis* (continued in MacIver 1937b, 1937c),

The real question at issue in these discussions is: what is meant by saying that two tokens are "of the same type"—or, in the paradoxical language of everyday life, by the saying that "two words are the same word," that "two sounds are the same sound," that "two letters are the same letter," and so forth? Though Mr. Hardie professes to reject the [implicit abstract ontology] of the type/token distinction altogether, in fact he is only offering his own answer to this question—which is: that all that is meant is that the tokens are *similar.* But this will not do. (60)[69]

Even Bertrand Russell (1961) would contribute to these discussions of linguistic similarity:

Similarity is neither necessary nor sufficient to make a shape a member of the class which is the word 'cat'. The word may be written in capitals or in small letters, legibly or illegibly, in black on a white ground or in white on a blackboard. If I write the word 'catastrophe,' the first three letters do not constitute an instance of the word 'cat'. The most necessary thing in an instance of the word is *intention.* If a piece of marble happened to have a vein making the shape 'cat' we should not think this an instance of the word. . . . It thus appears that we cannot define the word 'word' without a) a logical theory of class, and b) a psychological understanding of intention. These are difficult matters. . . (140)

Yet despite such philosophical hand-wringing, it is important to appreciate why philosophers like Quine and Goodman would be so attracted by an account of token-to-token linguistic equivalence which, in virtue of being based upon some objective measure of similarity, would obviate the need to semantically interpret the tokens in order to "see" their equivalence. Though similarity, as Goodman would have it, "tends under analysis either to vanish entirely or to require for its explanation just what it purports to explain" (1970, 446), there is no denying its philosophical appeal. So while Goodman's animadversions against similarity are quite general, and do not especially target

perceptual, no less phonological, similarity, it is clear that by grounding word-token classification in perceptual similarity, the problem of typing linguistic tokens would resolve itself through "bottom up" processing, independent of "top down" semantic interpretation.

Whether by some measure of perceptual similarity or a more complex distributional comparison of phonetic features technically known as "complementary distribution," any scientifically acceptable typing of linguistic tokens must, for Quine, be based on some objective, though undoubtedly liberal, measure of similarity, not interpretive semantics. But this is not the way natural language phonology works. The extraction of perceptually definitive cues of phonemic classification is not a scientifically challenging exercise but is, as Quine (1953a) correctly conceded, an unrealistic enterprise. As Lyons (1977) reminds us matter-of-factly,

> Ever since Bloomfield (1926) explicitly formulated the principle (as one of his postulates for linguistics) that every utterance is wholly made up of forms (as every form is wholly made up of phonemes), it has usually been taken for granted by linguists that the question of type-token identity for any given language is decidable. Great difficulty has been experienced, however, in giving empirical content to the linguists' theoretical commitment to Bloomfield's postulate. (17)

And as Roy Harris (1980) once pointed, such "great difficulty frequently accompanies attempting the impossible" (135). This is because—at least since the advent of the sound spectrograph and acoustic speech synthesizer—it has been more or less obvious to speech perception theorists that no range of perceptual parameters constitute phonemic (and hence word) identity (Kuhl 1980, Strange 1995).

The problem with both invariant acoustic theories and those predicated upon a range of family resemblances is that phonemic "families" are not acoustically or even phonetically well-formed "bell-shaped" curves centered around a phonemic prototype.

Instead of a single "hump," phonemic categories actually cluster into multiply distinct allophonic categories. As Bloch (1948) explains: "Some members of the same phoneme are phonetically different. If this were not true, the term phoneme would mean merely a class of phonetically same segments or spans. It is of course a commonplace that phonemic analysis very often groups together in the same phoneme sounds that are objectively different" (38). In terms of Quine's three-dimensional relief map, what we find are several frequency spikes, all of which (according to native intuitions) correspond to a single, albeit allophonically disjointed phoneme. Phonemic equivalence, explained Fries, "must not be taken as the engineer's 'norms' with margins of tolerance—statistical norms clustering around averages. . . . They are 'sames' as the various types of 'strike' in baseball are functionally the same. . ." (reproduced in Twaddell 1952, 609).

There is, of course, no way for Quine to be fully aware of these phonological complications. (One is, however, reminded of Twaddell's remark: "Any non-linguist who is tempted to seriously discuss or investigate linguistic phenomena has two choices; resist the temptation, or read [the relevant] writings. If he does neither, he becomes an object of merriment for the judicious, and is very likely to waste a lot of his time and other people's money" (1952, 608).) As Lyons (1977) points out:

> At first sight the type-token distinction may appear to be quite trivial if not pointless. After all, we do not usually fall victim to type-token ambiguities in everyday life. We know which sense of 'word' is intended when we are told telegrams cost so much a dozen words (or whatever it might be). Consider, however, such questions as the following. Does a capital letter instantiate the same type as the corresponding lower-case letter? Does a word printed in italics instantiate the same type as a word printed in Roman? Is a word handwritten by X ever the same as a word handwritten by Y? The answer to these questions does not depend upon some notion of absolute identity. The relationship of instantiation involves the recognition of identity relative to some purpose or function. What kind of identity is involved may be clear enough in most practical situations. But it is important to realize that it cannot be specified in terms of a certain degree of physical or perceptual similarity. Indeed, it is doubtful whether any sensible

measure of physical or perceptual similarity can be determined that is totally independent of functional considerations in questions of pattern recognition the kind that are at issue here. . . . The identification of spoken forms as tokens of the same type is far more difficult. . . (15-16)

Phonemic Reductionism II: Behavioral Assent

So while linguists soon turned their attention away from perceptual similarity to considerations which also exploited rule-governed "complementary distribution" in an attempt to non-semantically resolve phones into phonemic categories (e.g., Bloch 1948), Quine abandoned an analysis of the speech stream all together. In "Use and its Place in Meaning" (Chapter Five of *Theories and Things*), Quine (1981) returns to the problem of phonemic classification, but pursues phonemic reductionism through the strategy of behaviorism. Here, the critical notions are not that of physical similarity, statistical norms or prototypes, but simply that of behavioral equivalence.

In this 1981 collection of essays, Quine begins his criticism of semantic accounts of phonemic equivalence on the grounds that they are circular. In connection with explaining his use of the expression "expression," Quine informs us that

An expression, for me, is a string of phonemes—or, if we prefer to think in terms of writing, a string of letters and spaces. . . . The point is that the notion of an expression must not be allowed to presuppose the notion of meaning. One may suspect, however, that my identifying expressions with strings of phonemes is then self-defeating, because the notion of a phoneme is itself commonly so defined as to presuppose the notion of sameness and difference of meaning. Two acoustically distinguishable sounds are counted as occurrences of the same phoneme if substitution of one sound for the other leaves meanings unaffected. Now this appeal to meaning is happily not needed. We can simply say that two sounds count as occurrences of the same phoneme if the substitution has no effect on a speaker's readiness to assent to any string of sounds.

Expressions, then, are strings of phonemes in this innocent sense, and it is expressions that are to have meanings. . . . (44-5)

This passage is significant, for in addition to abandoning the speech stream in search of phonemic equivalence, Quine embraces a behavioral analysis similar to that

adopted by some prominent linguists themselves, most notably Zellig Harris (1951), Chomsky (1957) and Charles Hockett (1948). Though referencing neither Chomsky nor Harris, this type of behavioral reduction became the favored route among even so-called distributional linguists, as it had become increasingly obvious that phonemic categories were not recoverable simply on the basis of any information intrinsic to the speech stream, whether that be acoustic, perceptual, or even the type of distributional cues available through an analysis of complementary distribution. Behavioral reduction became the fall back position. Rather than relying on an analysis of the language itself, one only had to observe the behavioral reaction of a cooperative native speaker subjected to the manipulation of certain sounds. If an informant indicated that the substitution of one sound for another was "significant" in the sense that it would change an affirmative response into a negative response or was consistently detectable (these two criteria are different), the difference was deemed phonemic. And these procedures were held to be objectively determinate because—just like listening, recording, and analyzing the primary linguistic data of speech itself—the informant's behavior was merely observed, not interpreted.

Similar in this respect to Quine's remarks in "Use and its Place in Meaning" is his entry for "phonemes" in *Quiddities*. Building on the rhetoric and bottom-up constructivist imagery of a classically empiricist approach to language Quine (1987) writes:

> Two distinguishable sounds belong to the same phoneme, for a given language, if switching them does not change the meaning of any expression in that language: such is the ordinary uncritical definition of the phoneme. But meaning is a frail reed; surely the phonemes, the very building blocks of the language, are firmer than that. They are indeed, despite occasional misgivings to the point. There is an easy behavioral criterion of sameness of phoneme that presupposes no general notion of sameness of meaning.

Two sounds belong to the same phoneme if substitution of one for the other does not affect a speaker's disposition to assent to any sentence. (150-151)

And finally, after reminding us that the phoneme is "here to stay" (a somewhat anachronistic remark in a post-Chomskyan world of generative phonology), Quine reiterates this behavioral reduction as late as 1990. Reflecting upon "The Phoneme's Long Shadow"—the title of a short article included in a cultural anthropology volume entitled *Emics and Etics: The Insider/Outsider Debate*—Quine affirms:

[For] besides providing the very backbone of phonetic [sic] theory, the phoneme idea is gratifying in its clarity. This is not evident at first, for the idea is usually defined with the help of the nebulous idea of meaning. Sounds belong to the same phoneme, for a language, if they can be interchanged without changing any meaning. However, there is happily a direct criterion in *assent*. Switching allophones in a sentence must not alter an informant's disposition to assent to it. (164)

Of course, more technical formulations of this behavioral reduction were proposed by linguists themselves. Among the most notable attempts to behaviorally "operationalize" the phoneme were those detailed by Zellig Harris (1951) and Chomsky (1957). For both teacher and former student, the critical criterion was that of "repetition": two distinct sounds were operationally defined as phonemically equivalent if, when substituted, a native informant would consider them to be "equivalent repetitions" of each other. Linguistic specimen not considered "repetitions" were catalogued as phonemically distinct.

Here is how Chomsky (1957)—taking his cue from Harris (1951)—explains the advantages of this behavioral reduction:

Fortunately we do not have to pursue any such far-fetched and elaborate [semantic] program in order to determine phonemic distinctness. In practice, every linguist uses much more simple and straightforward non-semantic devices. Suppose that a linguist is interested in determining whether or not "metal" and "medal" are phonemically distinct in some dialect of English. He will not investigate the meanings of these words, since this information is clearly irrelevant to his purpose. He knows that the meanings are

different (or he is simply not concerned with the question) and he is interested in determining whether or not the words are phonemically distinct. A careful field worker would probably use the pair test, either with two informants or with an informant and a tape recorder. For example, he might make a random sequence of copies of the utterance tokens that interest him, and then determine whether or not the speaker can consistently identify them. If there is consistent identification, the linguist may apply an even stricter test, asking the speaker to repeat each word several times, and running the pair test over again on the repetitions. If consistent distinguishability is maintained under repetition, he will say that the words "metal" and "medal" are phonemically distinct. The pair test with its variants and elaboration provides us with a clear operational criterion for phonemic distinctiveness in completely non-semantic terms. (96-97)

And again in *The Logical Structure of Linguistic Theory*, Chomsky (1955/1975) reiterates his preference for the putatively non-semantic pair-test:

The paired-utterance test, discussed by Harris, is a good first approximation to an operational test for phonemic distinctness of utterance tokens, which might be refined in a number of ways. In practice, some variant of this test must be used when a nontrivial problem of determining contrast arises, and it does appear to capture presystematic intuitions fairly well. As noted earlier, it was widely assumed in the early 1950s that grammar depends upon meaning in terms of "synonymy" ("difference of meaning" or "differential meaning" was the term often preferred). As far as I could make out, there was not then (nor is there now) any coherent formulation of this proposal, or of other proposals that the concepts of syntax and phonology must (or even can) be defined in terms of semantic primitives. (34-35)

Similarly, Morris Halle (1954)—co-author with Chomsky of *The Sound Pattern of English* (1968), a seminal work in generative phonology, claims that

It has been shown by Harris [1951] that this decision [of determining whether two utterances are the same or different] can be established without recourse to "meaning" or "difference of meaning." Two utterances as spoken by the same informant are recorded on tape and a test tape is prepared by re-recording the original two utterances in a random order . . . The two original utterances are played to an audience of native speakers who are instructed to make a check on their ballots (or signify in some other manner) whenever they hear the first utterance during the playing of the test tape. If the utterances are "different," i.e., phonemically distinct, the audience will obtain an almost perfect score; if the utterances are homophonous the score will be in the vicinity of 50 percent. (200)

In an effort to avoid implicating semantics in an account of basic phonemic equivalence, linguists should rely solely on the behavior of their native informants. Rather than

observing the language itself, Quine would have it that "two sounds belong to the same phoneme if substitution of one for the other does not affect a speaker's disposition to assent to any sentence" (1987, 150) and leave it at that. On the other hand, the "pair-test" proposals of Harris and Chomsky rely on the native's discriminative ability or inability to discern various stimuli. And while Quine's proposal is only superficially devoid of meaning (see below), the "pair-test" is so devoid of meaning it is liable to give false results.

Consider first the paired discrimination test. There is the dual danger of under- as well as over-discrimination. Surely the standards of phonemic categorization are not to be "blindly" determined by just anyone, say, the hard of hearing. But worse, neither should phonemic categorization be determined by the hyper-sensitive either. It cannot really be the case that insofar as two linguistic stimuli can be consistently re-identified as "different" that they are, ipso facto, phonemically distinct. That assumes distinct allophones of the same phoneme cannot be consistently discriminated, but as Bloch & Trager (1942) point out, this is simply not true—all the less true for people like Chomsky, Harris and Halle. For while most anyone can detect the mispronounced speech of a foreigner speaking English with an allophonic accent, phonologists (or more specifically phoneticians) are precisely those specialists who, by profession, are trained to attend to those slight phonetic differences that they themselves acknowledge to be sub-phonemic. One had better not ask them if allophonically alternating "repetitions" of one and the same word can be consistently discriminated.[70] And it is clearly begging the question, as Chomsky (1957, 95) himself notes, to restrict phonemic distinctiveness to those changes in pronunciation which would change "one word into another word," as

this is precisely what is at issue.[71] Phonetic differences, in particular allophonic discrepancies—whether detectable or not—are not coincident with phonemic distinctions. For even if a phonetic difference is detectable, it need not change assent to dissent. The question arises, then, *why* don't allophonic differences "make a difference"?

The behavioral pair-tests proposed by Zellig Harris (1951) and favored by a young Chomsky (1957) clearly presuppose that at least the native informant is accessing semantic information, if the test is not to degenerate into a test of auditory acuity. In fact, Harris (1951, 20) himself explicitly acknowledged, and Chomsky (1955/1975, 102) would eventually concede, that much. So despite some misleading commentary by Hockett and Lees,[72] it is Harris (1951) who tells us in the very first footnote to the chapter entitled "Phonemic Distinctions" exactly why meaning must inevitably be invoked—even when performing a paired repetition test:

> Objection might be raised here to the effect that meaning considerations too, are involved in the determinations of elements, since, for example, when sounds (or sound features) x and y occur in identical environments they are assigned to different phonemes if the complexes containing them constitute different morphemes (e.g., (l) and (r) in the environment /-ayf/: *life*, *rife*). However, this differentiation of *life* and *rife* on the basis of meaning is only the linguist's and the layman's shortcut to a distributional differentiation. *In principle, meaning need be invoked only to the extent of determining what is repetition.* If we know that life and rife are not entirely repetitions of each other, we will then discover that they differ in distribution (and hence in 'meaning'). (7; italics added)

And later, while discussing the paired repetition test, Harris (1951) observes that:

> In many cases this involves asking an informant to "say it again" or "what?" or asking another informant who is present "Would you say that?" In some cultures and in some social situations there may be difficulties in obtaining repetitions. . . . When what we obtain is not an admitted repetition (and, sometimes even when it is), we have to judge whether utterance *B* is indeed a repetition of utterance *A*, by considering the situation, meaning and sounds. . . . This is equivalent to Bloomfield's 'fundamental assumption of linguistics: we must assume that in every speech-community some utterances are alike in form and meaning' (*Language* 78). (29)

In fact, in a section of the 1958 Internal Congress of Linguistics entitled "Native Reaction as a Criterion in Linguistic Analysis" (see Hoijer 1958), one participant, noting the irony of relying on native informants, remarked:

> Insofar as our principal problem is concerned, it would appear that the Bloomfieldians [identified as Bloch, Trager, Smith and Hockett] are least concerned with native reaction as a criterion in linguistic analysis. They emphasize the observable sound features and their distribution in speech, and attempt to disregard semantic substance entirely, or to reduce consideration of it to a minimum, on the ground that semantic considerations do not lend themselves to objective scientific treatment. . . . As a matter of textual exegesis and historical accuracy, the claim that Harris's pair test is conducted without appealing to meaning is incorrect, particularly when Harris (1951) clearly states, "In principle, meaning need be involved only to the extent of determining what is repetition" [7] . . . It is empirically discoverable that in all languages which have been described we can find some part of one utterance which will be similar to a part of some other utterance" [20].

Continuing, Hoijer emphasizes:

> 'Similar' here means not physically identical but substitutable without obtaining a change in response from the native speakers who hear the utterance before and after the substitution: e.g., the last part of 'He's in' is substitutable for the last part of 'That's my pin'. In accepting this criterion of hearer's response, we approach the reliance on 'meaning' usually required by linguists. Something of this order seems inescapable, at least in the present stage of linguistics: in addition to the data concerning sounds we require data about the hearer's response. . . . According to Chomsky and Halle, Harris's test is specifically designed to *determine*, rather than *assume*, whether two utterances are repetitions. But this is incorrect. (573-91)

In fact it was none other than Bloch & Trager (1942)—two, along with Z. Harris, of the most committed phonologists dedicated to a non-semantic reduction of the phoneme—who had already deflated such optimism:

> A phonetically trained foreign student of English, recording the utterances of an informant, is likely to observe many differences of which the average speaker of English is unaware. He will record an aspirated [p'] in *pin* and *appear*, an unaspirated [p^h] in *spin* and *upper*, and an unreleased [$p^|$] in *napkin* and (occasionally) in *up*. He will note that the vowel of *bid* is longer than the vowel of *bit*, and that a similar difference obtains between the vowels of *bet, bat, but* and those of *bed, bad, bud*. He will distinguish the prevelar [ĝ] of *geese* and *give* from the mediovelar [g] of *goose* and *gone*; the weakly palatalized or neutral [l] of *let* from the velarized [l_u] of *tell*; the short [n] in *hence* from the longer [n'] in *hens*. And if his ear is sharp enough (that is, if he has been well enough trained), he may record half a dozen different vowel shades in successive utterances of

dog. In the early stages of his work, of course, he will not know that these utterances all contain one and the same word: for all he can tell, [dog], [dɷg] and [dɔg] may be as different from each other as they are from [dAg]. This uncertainty cannot be resolved simply by asking the informant. If the latter is sophisticated enough to understand such finespun questions, he is probably literate in his native language and hence likely to be misled by the way in which words are written, by the tradition of the schools, and by other equally fallible guides; and if he is unspoiled by education, the chances are that questions about the identity of words will only baffle him. (38)

Observing that an uncritical reliance on "repetition" is to take advantage of, but not explicitly acknowledge, (the native's own) semantic knowledge, Hockett (1949) would also criticize Harris's notion of repetition:

Harris only apparently carries us closer when he prescribes that we look for *repetitions*; that is we hear someone say something, and perhaps record it, and then ask him to say the same thing again, or even try to say it back to him, thus, if we are lucky, producing a repetition ourselves. . . In a way, we are really quite uninterested in the individual utterance-event with its date and place of occurrence; and yet that is all we can ever observe. Harris's advice leaves the fundamental problem unsolved, for how can we be sure that the second [utterance] event is actually a repetition of the first? (206, as reprinted in Makkai 1972).

In fact, even Chomsky would, as noted earlier in chapter 6, reluctantly concede that

in a certain sense the ultimate criterion remains the speaker's intuition about linguistic form, since only this can tell us which behavioral tests are to the point. We might hope that some more general account of the whole process of linguistic communication than we possess now may permit us to reconstruct the criteria of adequacy for linguistic theory in more convincing and acceptable terms. But for the present, it seems that we must rely, at least to some extent, on the speaker's intuitive conception of linguistic form. (1955/1975, 102)

For that matter, even Paul Postal would criticize Chomsky for thinking that such operational tests deliver what they advertise. Quoting Postal (1968), "Chomsky was concerned with suggesting an operational approach to defining linguistic features and excluding nonlinguistic audible features such as rate of speed, absolute loudness, etc. I see no reason today to believe any such operational test exists. Certainly none has ever been proposed. . . ." (227). As Teeter (1964) described it, "the post-Bloomfieldian

fallacy is in these terms simply a methodological requirement that any evidence which would suggest the postulation of mind is to be excluded. This would really leave us only with physical records of speech, though fortunately linguists have never taken the assumption literally enough to exclude native speaker's judgments of sameness or difference among utterances" (202).[73]

But why is this? Why are native informants so important? Exactly what is it that the native has that an impartial fieldworker does not? After all, if the phonemic structure of a language really can be objectively determined strictly on the basis of overt behavior—rather than what the sounds of the language actually mean as semantically understood by the native—then any linguistic observer should be capable of perceiving this structure just as well. To repeat: What, exactly, does the linguist need informants for in the first place? The Quinean field linguist should just observe the behavior of the native informants. If meaning is not relevant, the linguist really need not bother informants with even questions of Quinean assent and dissent. Just observe them in their natural activities. If it is behavior which is critical, then there is nothing the native has which cannot be behavioristically observed by an attentive fieldworker. Given that fieldworkers can (with the aid of external equipment) overcome any of their native language perceptual biases, the only reason to rely on the isolated pair-test behavior of a native would be to surreptitiously access the very semantic information native speakers, unlike nonnative speakers, possess.

Behavioral reductions like Quine's involving comparisons of assent and dissent, then, do not solve the problem of providing a non-semantic definition of phonemic equivalence; they merely surrender to it. Basing phonemic equivalence on the behavior

of any number of native informants is not a solution because such a move is just a diversionary tactic, successful only in postponing what will eventually amount to an obvious concession. It is important to remember that when confronting an alien language, it is supposedly the job of the uninformed linguist who knows nothing about the language to determine if two sounds are or are not phonemically equivalent. Relying on a native informant for this information is not to answer the question, but merely to have someone else carry out the "dirty work" (as Ohlander 1976, 81 put it) of semantically answering it for you. So while judging phonemic equivalence on the basis of isolated pair-test behavior may very well be, as Quine (1990) would have it, "methodologically harmless," it is also trivial, as the question of phonemic equivalence has not been answered but only pushed one step backwards. On what basis do the informants make their own "behavioral" distinctions of assent and dissent?

Assuming such questions can be made intelligible to the average informant, the answer will inevitably be based upon whether the substitution affects a change in meaning or not. As Lounsbury (1956) argued:

> If a linguist who knows no English records from my lips the word *cat* first with a final aspirated stop and later with a final preglottalized unreleased stop, the phonetic data will not tell him whether these forms contrast or not. It is only when he asks me, his informant, whether the meaning of the first form is different from that of the second, and I say that it is not, that he will be able to proceed with his phonemic analysis. (190)

But perhaps this overtly semantic diagnosis of phonemic intuitions is to, in effect, beg the question against Quine. Perhaps our native informants come to phonologically categorize their own language merely on the basis of observing the overt behavior of others in their language community. So while informants may have no explanation for their linguistic intuitions regarding what counts and what does not count as a "repetition"

other than semantic contrast, this is arguably irrelevant. What really matters is the *origin* of these intuitions, not their introspective status. In any event, is it not always possible for non-native linguists to "go native" and figure out the phonemic structure of a language on their own by immersing themselves in the behavior of a community of native language users. After all, we were all originally non-native in our own language. In this sense, the answer is of course, yes: going native is always an option. Note, however, that this procedure, if true to the principles of a strictly non-semantic behavioral analysis, would not be anything like the typical language learning experience, for it would be prohibited from leveraging any of the interpretive semantic significance of any one's behavior (verbal or otherwise). Rather, the only tool that the behaviorist, qua behaviorist, has is behavior.

So instead of acquiring phonemic categorization strictly on the basis of perceptual cues derived from the speech stream itself (accessible, as it were, exclusively through a radio or loud speaker, see chapter 8 for a brief discussion of this), this behaviorist proposal is equivalent to merely watching the behavior of language users without, however, attributing any interpretive value to their behavior, specifically their speech behavior. While such an analysis is surrealistically analogous to watching live drama without attributing any semantic content to the actors' speech behavior—and on this strictly behaviorist basis, trying to figure out what sounds count as the same and which count as different—Quine might contend that this "meaning-blind" analysis might, in principle, succeed in cracking the phonemic code.

But this will not work. For now the question has become: what distinguishes one overt behavior from another? A classificatory standard distinguishing "assent" and

"dissent" is required just as much as a classificatory standard distinguishing the phonemic and the non-phonemic. In order to be free from meaning, the behavioral cue distinguishing simple Quinean dissent from assent must be transparently accessible, or at least reducible in principle to some sort of a purely behavioral description—perhaps in terms of physiological movement. But dissent and assent, as relevant here, are fundamentally voluntary, intentional, acts subject to culturally-specific norms of expression. For this reason, judging which behaviors count as the "same" or "different" is subject to the same kind of initial uncertainty as which *sounds* count as the same or different. The question of "phonemic equivalence" has merely been transformed into a question of what is to count as "behaviorally equivalent," but this is not progress.

So despite the clarity of their methodological intent, the behavioral tests of Zellig Harris, Chomsky and Quine do not avoid interpretive considerations of meaning. It is instructive to note, however, how this debate was received outside the linguistic community—how the aims of "phonemic reductionism" were viewed by non-phonologists. In connection with a discussion of Chomsky's new transformational grammar among Quine, Chomsky, Goodman, Roman Jakobson, and Max Black in a *Symposia on The Structure of Language and its Mathematical Aspects* held in New York City (April 1960), Putnam (1961) addresses the interface between semantics and grammar, including the rules of phonology:

> An issue that we can hardly bypass, if only because it has generated so much controversy, is the issue between those who assert and those who deny that grammar can be done 'independently of meaning'. Among American linguists, Zellig Harris was the first I know of to emphasize this claim. (33)

Putnam continues by asking, "Can one discover the phonemes (morphemes, form classes, etc.) of a language without learning the language (learning the meaning of any form,

learning that any two forms are synonymous?" and answers that, in principle, it merely "depends on how good a detective you are" (33):

> Thus, some linguists apparently maintain (at least when they are arguing this question) that one cannot discover the phonemes of a language without learning the meanings of the forms in that language (or at least learning that certain pairs are pairs of non-synonymous expressions). I am sure that these very same linguists, however, would not be surprised (provided the statement were not concerned with this "controversy") to hear that some linguist had inferred the phonemes of a language X from the way the X-speaker spoke English (*a fortiori*, without learning X). . . . I am not saying that any of these procedures is practicable today or will ever be practicable, but only that no issue of principle is involved. Every linguist believes that phonemics have an "obligatory" character . . . But then how on earth could it be impossible in principle to discover the phonemes of an alien language except in one way? . . . Viewed in this light, Harris's methods do not appear so surprising. Harris discovers the phonemes of a language in roughly the following way: the linguists recites a sequence of expressions, e.g., 'cat, cat, cad, cab, cab, cad' and the informant describes what he heard. If the informant says: 'you said A twice and then B and then C twice and then B' . . . then one would conclude (tentatively, of course!) that b, c, t were phones of different phonemes in the alien language. On the other hand. . . (34).

But here I think Putnam is making the same mistakes. First, phonemics does not have an exclusively "obligatory" character. The "free" in free allophonic variation allows for enough phonetic noise to disable a phonemic algorithm predicated merely upon the obligatory patterning of complementary distribution. Second, the question is not whether native informants are capable of sharing their linguistic knowledge of which sounds are phonemically equivalent in their language. As native speakers they are implicitly aware of this, and typically can be encouraged to meta-linguistically divulge such knowledge to a linguist. The question is: on what basis are the natives themselves aware of their own phonemic intuitions. Where did they come from? Given that the phonemic structure of a language cannot be resolved simply by analyzing the phonetic structure nor phonetic distribution of the speech stream itself, it would seem that interpretive information is also required for the natives to have acquired the phonemic knowledge of their own language.

Moreover, consider how Putnam (1961) expresses some related reservations concerning a behavioral reduction of phonemic equivalence:

> We might say that two sequences of phones in a natural language are structurally identical if a speaker of the language counts them as the same expression and otherwise [as] structurally non-identical. Of course, this raises a number of problems: It is not crystal clear what it means to say that a speaker of a natural language counts two phones sequences as the same expression, apart from contexts in which we have available a bilingual informant who is willing to make explicit metalinguistics statements, at least of a very simple kind ("You said the same word twice"), and a linguist who is willing to rely on such explicit metalinguistic statements. If we ask for characterization of what it means to say that an informant counts two phone sequences as the same, where the informant is a speaker of only one language x, and the characterization is to be wholly behavioral, and not to refer to dispositions to make certain kinds of explicit metalinguistic statements about x, then we shall probably not be able at present to say very much. Moreover, even if we succeed in thinking of a number of things that we might call "symptoms" of the disposition to count two phone sequences as the same expression, it would still be a mistake to think that one could arrive at an explicit definition of this disposition in terms of such symptoms. I will not go into this last point any further, since to do so would take us far afield into familiar controversy pro and con operationalism. Instead I will just remark that here we are reminded that linguistics is after all a social science and that its fundamental concepts have the same kind of dispositional and human character as do the fundamental concepts of any other social science. (36-37)

In short, behaviorism is not really a solution to our problem. Instead, behaviorism is more a restatement of the problem, for there is no avoiding the meaningful in "meaningful difference," whether this be a phonemically meaningful difference or behaviorally meaningful difference.[74] As one critic put it, "Meaning, as at least one linguist has expressed it, has become a 'dirty word'; but if the name tends to be avoided, there is no doubt that every linguist employs the concept, though some would be unwilling to admit to such improper thoughts" (Allen 1957, 22). So while superficially devoid of semantic considerations, the effort to semantically sanitize phonemic equivalence by probing the auditory discrimination of native informants or their behavioral reaction to certain sound substitutions is either irrelevant or itself semantically conditioned.

Phonemic Reductionism III: Stimulus Meaning?

Given that behaviorism is more a tactical avoidance of, rather than an alternative to, a semantic basis for phonemic classification, is perhaps unsurprising that meaning is, in one way or another, introduced at some point back into the analysis. But Quine's philosophy of language is equipped with notions which are not to be found within the distributionalism of Harris and Bloch, nor even the positivist-minded descriptivism of neo-Bloomfieldianism generally.

Among Quine's distinctive contributions to philosophy of language is his notion of *stimulus meaning*. Though Quine does not present "stimulus meaning" as a mechanism by which to defuse the semantic basis of phonemic classification, it is worth exploring how Quine could have used stimulus meaning to counter a semantic definition of the phoneme. But before exploring how stimulus meaning could be used to define the phoneme in non-semantic terms, it is important to review what our problem is, and what counts as a solution. Mindful that behavioral pair-tests were misrepresented as a non-semantic solution to phonemic classification, it is important not to lose sight of what is demanded of a non-semantic reduction of phonemic equivalence.

One of the central concerns within pre-Chomskyan structural linguistics was whether the distinction between phonemically relevant and phonemically irrelevant phonetic distinctions could be "objectively" defined without the introduction of any non-linguistic factors extraneous to the linguistic signal itself. This was a contentious issue, separating not only American descriptivist linguistics from contemporaneous European centers of linguistic thought (e.g., the Copenhagen school of "glossematics" propounded

by L. Hjelmslev and the "functional phonology" associated with the Prague Circle of linguists), but also divisive within American structuralism. The use of meaning in linguistic analyses tended to divide "moderate" from "extreme" descriptivists, with Pike and like-minded European linguists emphasizing the phonological futility of asemantic analyses and the methodological absurdity of prohibitions against the cyclical "mixing" of various structural levels of linguistic analysis generally. As one European linguist—Roy Harris's dissertation advisor (see note 6)—wryly commented: "Phonemics however means something rather different according to the school in question. The Prague school does not pretend to dispense with semantics and the Yale school [the reference is to Bernard Bloch] does no more than pretend" (Bazell 1952, 276).

For a variety of reasons, the most important being the phenomenon of "free allophonic variation," attempts to define phonemic equivalence without appeal to semantic information failed. As the Danish linguist Eli Fischer-Jorgensen (1956) explains:

> Reference to meaning is necessary to distinguish contrast from free variation. . . . Now Bloch has of course never claimed that [his method, even if sound] would be a useful method, only that it would be theoretically possible. Perhaps it could be maintained that it is possible to define the difference between contrast and free variation on the basis of distribution alone (and in his last paper Bloch [1953] does not seem to maintain more than this). It cannot, however, be maintained that it is possible to analyze a concrete language by methods based on these definitions. The possibility of avoiding any reference to content in the definitions [of phonology] is of course of great theoretical interest. It does not imply, however, that this reference is simply a practical short-cut, for the difference between contrast and free variation can also, and in simpler way, be defined by reference to content, that is, by means of a relation which is fundamental to language as a system of communication, and the commutation test, derived from this definition, can be, and indeed is, used in practice in all types of structural linguistics. (585-587, as reprinted in Makkai 1972)[75]

Similarly, Charles Fries writing in 1961 reminds us that "A number of American linguists, however, who have been considerably influenced by Bloomfield, have tried to

go beyond him in the exclusion of meaning—at least they have proposed, as a theoretical possibility, the total exclusion of the use of meaning in analysis. No examples of descriptive analyses accomplished on this basis have appeared" (216).

These failures in turn led a number of later linguists, including Chomsky (1957), to defend—as a fall back position—a behavioral analysis of phonemic equivalence. So while no longer based strictly on the perceptual or even phonetic/distributional structure of the language, it still, at least superficially, adhered to the basic methodological principle of identifying phonemic equivalence without the introduction of any interpretive information extrinsic to either the language or the behavior of the language users themselves. If careful analysis of the linguistic signal could not expose phonemic structure, then at least it would be revealed on the behavioral sleeves of a cooperative informant. In either case, or so it was argued, no interpretive value was attributed to the linguistic signal or linguistic behavior. But while acoustic, perceptual, and distributional theories represented exciting research programs potent with significant empirical results, the latter offers no more than behaviorist circumlocution.

In any event, as Ohlander (1976) keenly argues, if phonetic structure or behavior alone were sufficient to determine phonemic categorization, this would be quite a remarkable empirical result. After all, in order to be credible, an independent non-semantic analysis of phonemic categorization must, at the very least, empirically allow for the possibility that such an analysis would *not* coincide with an analysis of phonemic categorization driven by semantic criteria. In fact, such a non-semantic analysis sets up a potential conflict between two competing analyses, with the non-semantic analysis presumably trumping the semantic analysis. "However," Ohlander points out, "such a

result has not generally been envisioned by advocates of non-semantic phonemics. Instead, another result has by and large been taken for granted, namely that the units established by non-semantic criteria would in fact have the capacity to differentiate meanings. We would thus have a situation where certain units, arrived at without reference to meaning, are found to have, as an interesting side-effect, a meaningful function" (34).[76]

So either (i) it would remain a lucky accident that non-semantic criteria would perfectly match semantic criteria, or (ii) one might conclude that semantic criteria are in fact the basis of phonemic categorization. "How else," Ohlander (1976) rhetorically asks, "could such an extraordinary coincidence be explained" (81) other than by concluding that semantic information influences phonemic differentiation?[77] Summing up his argument, Ohlander is at pains to stress that so "long as proponents of a purportedly non-semantic approach to the question of phonological contrast, based upon native-speaker responses, do not state what considerations other than ultimately semantic actually sway native informants in their decision as to sameness or difference between forms, the burden of proof is wholly theirs" (1976, 81).[78]

The first thing, then, to notice about Quine's stimulus meaning is that it is, of course, a type of meaning. "Stimulus meaning" is a particularly rarefied type of stimulus-response associative meaning, but—and just like naïve reference to objects and properties—is information external to the language only contingently associated with it. Recall that Quine defines "stimulus meaning" as the class of all stimulations that prompt assent to a linguistic probe. ('Rabbit' and 'gavagai' are thus stimulus synonymous because the conditions—"the ocular irradiations"—which give rise to an assent to

"rabbit" are the same as those which prompt assent to "gavagai.") But given that stimulus meanings are, according to Quine, the value of words, it is clear that it is not anything intrinsic to the speech stream which governs phonemic equivalence. Rather, it is the external, non-linguistic similarity of ocular irradiation that would guide phonological equivalence. This is, in itself, a semantic concession. It is not however, a semantic concession sufficient to distinguish phonological equivalence based upon co-extensive predication versus phonological equivalence based upon predicate synonymy. And this is because if mere stimulus meaning is the criterion for phonemic equivalence, there is no difference between word-tokens which are word-type equivalent because they express the same property or word-type equivalent because they apply to the same objects. After all, the stimulus meaning of [renate] is the same stimulus meaning as that of [cordate] for the same reason that the stimulus meaning of [renate] is the same stimulus meaning as that of [RENATE]. In this way, stimulus meaning does not distinguish between tokens of two different but co-extensive predicates and tokens drawn from one and the same predicate. All of [cordate], [renate] and [RENATE] are stimulus equivalent, but only [renate] and [RENATE] are tokens of the same word-type. Of course, [cordate] and [renate] are phonetically too far apart in phonetic space to be typed as phonologically equivalent in any natural language. But if stimulus meaning is the principle governing phonological equivalence, then—all other things phonetically equal (see chapter 2)—two minimally distinct predicate-tokens (e.g., [dap] and [daf]) which would be typed as phonemically distinct "minimal pairs" in virtue of their distinct predicate meaning would be typed as phonemically equivalent in virtue of their convergent predicate application. As should be expected, Quinean restrictions on

synonymy collapse the distinction between co-extensive predication and synonymous predication at both the level of word-tokens and word-types. Insofar as [cordate] and [renate], like [renate] and [RENATE], are stimulus meaning equivalent, all of them would count as the same phonemic type. So much the worse for both synonymy and phonology, Quine might say: like traditional synonymy, phonology liberally accommodates distinctions which are better buried than nurtured. There is then, not only something deeply wrong with synonymy, there is something deeply wrong with phonology too. Quinean meaning eliminativism requires not only a rejection of analytic equivalence, consistent Quinean eliminativism requires a reformation of basic phonological equivalence.

The second thing to note is that some Bloomfieldians, indeed, Bloomfield himself (having already conceded the semantic nature of phonemic categorization) entertained the idea of explaining semantics, and hence the phoneme, in terms of stimulus-response pairs. But Bloomfield rejected this strategy as unworkable. According to Bloomfield, the "meaning" of a linguistic form is "the situation in which the speaker utters it and the response it calls forth in the hearer" (1935, 139). But whereas Bloomfield concluded that stimulus-response meaning requires exhaustive knowledge of what people would say in a particular situation, and thus useless as a methodological tool,[79] others simply argued that that this type of extrinsic semantic information, regardless of any difficulty in obtaining it, was neither necessary nor legitimate.

In fact, when confronted with the concession by some of the more "moderate" Bloomfieldians that "only what has been called 'differential meaning' is relevant" (Hill

1958, 409) for phonemic analysis, this suggestion was rejected as a violation against the exclusion of meaning. As the "moderate" Fries (1954) explains,

> We must approach every linguistic analysis with a large body of 'meaning' in hand. The question is not, then, whether we can dispense with all meaning in linguistic analysis but rather, more specifically, whether we can proceed with a valid and useful analysis without some knowledge or some control . . . of the meanings of the language forms which we are analyzing. . . [I]t is necessary to control in some way enough of the lexical meaning to determine whether forms showing certain differences . . . are, for the particular language, 'same' or different.' (62-68)

But for the "extremists," neither the type of meaning, nor the extent of meaning was at issue. At issue was whether any interpretive leverage extrinsic to the speech signal was required. As such, even the minimal pair "differential meaning" distinguishing, e.g., [mat] from [map] (in contrast to [mat] and [mat^{h}]) was considered to be unacceptable to those committed to the goal of phonemic reductionism.[80]

Here are a few passages indicating a "moderate" acceptance of differential meaning. According to Smith (1956),

> Differential meaning is simply a statement from a person who has been brought up according to a particular culture to the effect that one cultural event or component of a cultural system is the same as or different from another event. If you ask a fellow American, for example, whether *pin* is the same as *bin* you are likely to get one of two responses: "Oh, no, *pin* is something you use to stick things together, and *bin* is a place where you store coal or grain"; or, "No one begins with a *p* and the other begins with a *b*." The first answer is phrased in terms of referential meaning and the second in terms of differential meaning. *Pin* and *bin* are not the same word, first because they contain different "isolates" of the sound structure of English—they begin with different initial sounds. Secondly, all who know English and participate in Western culture also know that this basic difference then can be tied directly to different items in the material systems of our culture—words mean different things, refer to different objects in our cultural milieu.
>
> Now it is the linguist's job to analyze and describe the contrasting components of the structure of language on ascending levels of complexity. . . . The way the sounds group into forms and words and the way these in turn pattern into constructions and sentences must be ascertained and stated through the use of differential meaning only; for if referential meaning is resorted to, we immediately open the door to all the other systems of the culture and can never state the structure of the system we are endeavoring to describe and analyze in its own terms. (12-13)

Here is how Lounsbury (1963) somewhat defensively (note the scare quotes) explains his reliance on semantics:

The method of phonemics is not one of psychological experimentation; rather it involves the search for circumstantial evidence as to the distinctiveness or nondistinctiveness of phonetic features in naturally occurring speech data. The only "psychological test" which the linguist performs is to ascertain whether two phonetically different forms are the same or different as linguistic stimuli to natives. The specific purpose of the same-or-different test is to determine whether a particular feature of phonetic difference is the result of free variation or of contrast. (23 as reproduced in Postal 1968)

And here is how Charles Hockett—"seeking to allay," according to Lees (1957, 398) "feelings of guilt at the use of apparently semantic criteria when testing for distinctness of units . . . emphasized a strict separation of 'meaning' from 'differential meaning'"—explains the difference between them. The passage from Hockett is juxtaposed with Bernard Bloch's (1948) dismissive appraisal:

Hockett:
It has been granted—nay, proclaimed—that we cannot decide which utterance-events are "the same" for purposes of further analysis without using meaning (as defined). This use of meaning is (1) differential, rather than specific, in the sense that we use it to tell us whether two utterance-events had more or less similar meanings or not, but are not then concerned (at this stage) with just what those meanings were; and (2) applied to *whole utterance-events*, not to parts of utterances. Having once concluded which utterance-events should be classed as "the same" utterances, and having thus assembled a corpus of utterances (not utterance-events) for further analysis, that further analysis can proceed without any further reference to meaning. Our avowed use of meaning here, therefore, does not constitute a concession to Pike's desire to use meaning for the comparison of recurrent parts of utterances. . . . It is important to recognize that the criterion of biosocial function used in grammar is not all the same as the criterion of biosocial equivalence used in phonology. The latter involves only the observation of whether two situations are the same in general outline, whereas the former involves extensive analysis of situations. (1942, 112)[81]

Bloch:
Since our approach differs in some respects from Bloomfield's—chiefly in that Bloomfield invokes meaning as a fundamental criterion and arrives at his definition of the phoneme without stating in detail the intermediate assumptions that lead to it—we shall begin at the very beginning, instead of attaching our postulates to his . . . Bloomfield's [1926] phrasing does not tell us in what respects successive utterances are

alike; his discussion states that the likeness lies partly in 'vocal features' (auditory fractions), partly in 'stimulus-reaction features' (meaning), but gives no clue to its essential character . . . (6-7)[82]

Yet despite a desire to distance himself from a reliance on any semantic assumptions concerning the individuation of phonemes, Bloch (1948) continues a page later with "While we grant that this sameness is no doubt ultimately a matter of biosocial equivalence [a term he apparently picked up from Hockett 1942], we prefer to account for it here simply in terms of recurrent auditory fractions that are (by assumption) the same" (8).

In any event, Bloch's non-semantic aspirations remained undiminished. In a much quoted footnote to this same 1948 article Bloch declares:

Theoretically it would be possible to arrive at the phonemic system of a dialect entirely on the basis of phonetics and distribution, without any appeal to meaning—provided that in the utterances of the dialect not all the possible combinations of phonemes actually occurred. Given a sufficient sample of the dialect—say twenty or thirty hours of connected speech by an informant, recorded either on a high-fidelity machine or in a minutely accurate phonetic transcription—a linguist could probably work out the phonemic system without knowing what any part of the sample meant, or even whether any two parts meant the same thing or different things. (But he would need some kind of guarantee that every part of the sample meant something.) (5)

No discussion, however, of Bloch's preeminent role in attempting to exclude all semantics from phonology would be complete without quoting from the opening lines of his 1953 "Contrast" article—the culmination both of years of personal phonological research and, in a sense, of an entire linguistic era. In this paper, notable for both its brevity and obscure terminological innovations, Bloch triumphantly declares "Contrast between sounds can be defined, I think, on the basis of distribution alone, without the customary appeal to meaning" (1953, 59; reprinted in Makkai 1972, 224).

Though the phenomenon of free allophonic variation proved technically insurmountable for even Bloch's distributional proposals,[83] the existence of these non-semantic attempts to define the phoneme clearly demonstrates there have always been numerous ways to introduce meaning—either overtly (as with Bloomfield) or covertly (as with various "functional" accounts, see Ohlander 1976, 16-20 for criticism)—into considerations of phonological equivalence. But the critical distinction was whether any interpretive semantics was to be relied upon, not what type of meaning, nor how much meaning, was to be included. As Hockett (1970) seemingly never tired of reminding his colleagues, "The signals can be analyzed but not the things signaled about. *This reinforces the principle that linguistic study must always start from the phonetic form and not from meaning*" (162).

There are, then, two different issues stimulus meaning raises. On the one hand, stimulus meaning involves just as much meaning as even the minimal "differential meaning" rejected by extreme neo-Bloomfieldians. Various stimulus meanings differ in their stimulus-response conditions from other stimulus meanings, and these differentials in stimulus meaning would underwrite phonemic distinctions between phonetic differences that make a stimulus difference and those which do not make a stimulus difference. Grounding phonemic categorization on differences of stimulus meaning is still to ground phonemic categorization on differences other than the non-interpretive facts of phonetics and phonetic distribution. On the other hand, unlike traditional appeal to meanings, stimulus meanings are blind to a distinction between co-extensive predication and co-meaning predication. Applied to the phonemic classification, tokens of co-extensive predicates would be phonologically equated as if they were tokens of one

and the same predicate. Stimulus meanings do not discriminate between tokens expressing the same property from tokens truly applying to the same objects.

Taking phonology seriously means taking the individuation of semantic content seriously. Both Frege and Quine, however, challenge the individuation of semantic content, each in their own way. Frege challenges a naïve individuation of the content of names sensitive merely to considerations of "direct" reference. Quine challenges a naïve individuation of the content of predicates overly sensitive to considerations of "synonymous" meaning. These challenges, however, have phonological implications. If co-naming reference is insufficient to guarantee substitutivity within belief contexts, then co-naming reference is presumably insufficient to guarantee phonological substitutions conditioned by mere reference. If synonymous predication is not to be distinguished from co-extensive predication, then tokens of two different but co-extensive predicates are not to be phonologically distinguished from tokens of one and the same predicate. But natural language phonological substitutions are immune to doubt while natural language phonology does recognize a phonemic difference between tokens of /renate/ and tokens of /cordate/. However, the thesis of "phonemic reductionism," if true, would solve both of these problems with one claim: the typing of word-tokens into word types is not dependent upon any semantic considerations of either naming reference or predicate meaning.

But phonemic reductionism is false, and a Fregean invocation of word-token senses is phonologically divisive while a Quinean rejection of word-token meaning is too phonologically coarse. But more than just linguists and philosophers must confront the challenge of natural language phonological categorization. Infants too must address

problems related to free allophonic variation. Consider that if phonologists realize that language-specific phonological categories cannot be resolved without relying upon semantic information, then those challenged with actually acquiring these phonemic categories are presumably no better off. But this would make the acquisition of basic phonological competence, as a matter of developmental prerequisites, dependent upon a semantic appreciation of the language, or at least parts of the language.

Yet given the rapid pace of phonological development—and in particular the onset of language-specific speech perception within the first year of life—developmental psycholinguists would be in the awkward position of granting infants a precocious, albeit limited, appreciation of the semantic value of word-tokens before they could grasp which phonetically distinct word-tokens count as phonologically equivalent. This dynamic, however, suggests that it might be possible to experimentally catch, as it were, infants in the act of opportunistically leveraging their incipient semantic knowledge to track the particular phonemic contours of their target language.

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[55] In an extended footnote to the "The Ontology of Phonology," co-authored with Morris Halle, Sylvain Bromberger (1992) suggests several instructive diagnoses for this near universal neglect:

> Though many philosophers of language have views on empirical linguistics, few, if any, have given serious attention to phonology. Recent anthologies and books in the philosophy of language either don't mention phonology at all, or at best perfunctorily restate crude and outdated notions on the subject. This is somewhat surprising since the facts that phonology studies are critical to the individuation of expressions and to their character as objects of speech perception. But for these facts, there would be no syntax or semantics of natural languages beside sign languages (also neglected by philosophers), and philosophers deliberating about such languages would have to be silent. There are a number of explanations for this neglect. To begin with, recent philosophers of language generally belong to an intellectual tradition that admits no essential difference between natural languages and some of their contrived extensions. This was pointed out a long time ago by Strawson (1950), though he had other shortcomings in mind. Philosophical discussions thus generally abstract not only from differences between English, German, Japanese, and other natural languages, but also from differences between these real languages and notational systems used in mathematics, logic, physics, chemistry, biology, linguistics, etc. Such notational systems do have a syntax (albeit usually one that has very little in common with the syntax of natural languages), a semantics, and a pragmatics, but happen to have no phonology. . . . So it is not surprising that though Frege and his successors include signs in their Sign/Sense/Nominatum triad, they have nothing of interest to say about signs as things uttered and heard. . . . Furthermore, philosophers generally seem to believe that there can't be anything of philosophic interest about phonology.

> This attitude flows naturally from the previous one. The ideographs used by scientists are adopted through explicit published conventions that determine everything true of them qua signs. Since they are also semantically word-like, their sub-segments have no autonomous status and raise no philosophic problems. How could there be anything of philosophical interest about the shape of simple numerals, or about the horizontal segment in an inverted A in a quantifier, or in the vertical line of the *F* for Force? It is easy to pass from this outlook to the view that there can't be anything philosophically challenging about the spelling of words, and thence to the view that there can't be anything philosophically challenging about their pronunciation. And isn't phonology "just" about pronunciation? Whatever the explanation for philosophers' neglect of phonology, we think that it has a cost. To begin with, we think that it is a mistake to lump all lexical systems together as forming some kind of natural kind family. It blurs too many differences, and attention to phonology can highlight important ones. Furthermore, we think that no theory about the relation between natural language signs and their referent (or their meaning) can be trustworthy that nonchalantly takes signs for granted. Finally we think that an adequate understanding of the ontology of language—of the objects whose existence constitutes the reality of language—must include an adequate conception of the objects investigated by phonology. More specifically, an adequate conception of language must check our tendency, when we reflect about language, to slip thoughtlessly between talk about individual utterances and talk about types, as if such slips were always innocuous and easily fixed ways of pedantry. Spoken tokens are transitory events that occur in time and space, that can be perceived, that are shaped by their speaker's occurent intentions, and that are subject to norms fixed by their speaker's mental make-up. Types—if there are types—are abstract entities, neither in time nor space, devoid of causal histories or casual consequences, hence beyond perception. Types—if there are types—outdistance tokens. Tokens and types—if there are types—are thus utterly different. Conflating them is bound to lead to confusions and incoherence. But giving each its due, and understanding their connection, won't be possible unless we see how the type-token distinction fares in phonology, a topic which we discuss in what follows. (209-211)

[56] One notable example within the philosophical literature (discussed in chapter 9) is an extended spate of articles among MacDonald and MacIver in the 1935-1936 issues of *Analysis* (volumes 3 and 4) initially provoked by an article entitled "What is Logical Syntax?" (Bronstein 1935) exploring Carnap's claim (Carnap 1935, 59) that the "formal" mode of speech concerns linguistic expressions "without any reference to sense or meaning." Hutton's discussion of this *Analysis* debate is found in his *Abstraction and Instance: The Type-Token Relation in Linguistic Theory* (1990).

[57] This is how Hockett (1968) explained the situation: "We post-Bloomfieldian descriptivists of the 1940's were dissatisfied with several aspects of Bloomfield's portrayal of language. We believed that our own views were derived at bottom from his, that we were clarifying confusing details in his view by applying procedures and ways of thinking that he had taught us. But our views actually differed from his in two important respects":

> In the first place, as we have just seen, Bloomfield had repeatedly insisted that the discussion of meaning is beset with difficulties; from this he inferred, not that scientific linguistics is impossible, but merely that our characterization of a language should always start from form rather than meaning. The approach via meaning held too much danger of introducing irrelevant philosophical apriorisms, or of imposing on one language semantic categories actually relevant only for some other. During the 1940's some of us suspected that it might be possible to determine the forms of a language, and all the patterns by which they combine into larger forms, without any reference to meaning at all. Some decided that this was not only possible, but indeed, the only rigorous procedure, even if occasional resort to meaning might be a useful practical shortcut. A few even went so far as to interpret Bloomfield's recommendations as implying that any resort to meaning was 'mentalistic' and thus 'taboo'—something that he never said or implied, and a procedure that he never followed in his own work and that he would have rejected as impossible. (24-25)

[58] Unlike their structuralists predecessors—and exploiting the work of the Russian linguist Roman Jakobson—Chomsky and Halle (1968) would argue that the "phoneme" is generatively superfluous, a misleading artifact of "taxonomic" linguists obscuring a more basic derivation founded upon distinctive features. Nevertheless, generative phonology does not—any more than the distributional phonology it superceded—circumvent the need for semantics in the identification of basic phonemic contrast, for the same complaint leveled against the distributionalists by Diderichsen (1958) applies equally to generative phonologists: what does "distinctiveness" mean if not distinctiveness as to meaning? To suggest, like Postal (1968) and Chomsky (1964) that "'contrast' must be taken as a primitive notion" (83) (see note 50) is to effectively concede its semantic nature, not to avoid it, given that a semantic-based proposal explains the ostensively primitive difference between free variation and phonemic contrast.

[59] Consistent identification among various linguists from different eras and schools is complicated by a number of factors, including terminological obsolesce, ambiguity and unclarity, as well as personal evolution, with Bloomfield having expressed support for all of these views at one point or another. Symptomatic of these difficulties is Edward Sapir's "psychological" definition of the phoneme which sounds eminently functional: "To say that a given phoneme is not sufficiently defined in articulatory or acoustic terms but needs to be fitted into the total system of sound relations peculiar to the language is, at bottom, no more mysterious than to say that a club is not defined for us when it is said to be made of wood and to have such and such a shape and such and such dimensions. We must understand why a roughly similar object, not so different to the eye, is no club at all, and why a third object, of very different colour and much larger and heavier than the first, is for all that, very much of a club" (Sapir 1933; reprinted in Katz 1985, 65-66).

[60] According to Fudge (1970),

> The extreme form of the 'physical' view (as propounded in 1950 by Daniel Jones, 1881-1967) excludes all reference to non-phonetic criteria in the grouping of sounds into phonemes. . . . Furthermore, the fact that members of different phonemes are capable of differentiating meanings (English [p] and [v] as in pan *v.* van), whereas members of the same phoneme (English [k] and [q]) are not, is said to be a *corollary* of the definition of the phoneme, and not it *basis* (Jones, 1950: 13-15). (80)

[61] As described by Twaddell (1935) in his "On Defining the Phoneme" article, the "acoustic" position maintains that the phoneme can be defined as a "constant, characteristic acoustic fraction which is present in the sound-wave at each occurrence of that phoneme" (62). Vachek (1935), a proponent of a "functionalist" view, describes attempts to provide an acoustic-based account of phonemic categories in this way:

> Some of the linguists have tried to discover [the principle of phonemic classification] in the subordinate units themselves, i.e., by examining speech sounds as to their physiological and acoustic qualities. Such is the procedure adopted by Professor Leonard Bloomfield. The American scholar tries to answer the question why certain groups of sounds are treated as belonging to separate phonemes. The answer he arrives at is that all sounds within each group exhibit a common vocal (i.e., phonic) feature; this feature only is characteristic of, and relevant in, all the sounds of the group. It acts as a signal upon the hearer whenever he meets any of the sounds of the group in an acoustic context. In short, this very feature itself is the phoneme. (245)

It is the search, as Morris Halle (1954) remarked, for "a sort of common denominator" (336) or in the words of Kenneth Pike (1952b) for "physical absolutes" (618).

However, with the advancement of more sophisticated speech transduction techniques and recording equipment it became clear that rather than revealing a single acoustic signature for each phoneme in all its token instantiations, more detailed analyses simply revealed more and more variation. As Bloch (1948) stated in his introduction to his influential "A Set of Postulates for Phonemic Analysis," "Bloomfield's view of the phoneme as a feature present in the sounds or sound-waves has been shown to be untenable, and is no longer widely accepted among descriptive linguists" (4).

Similarly, Fischer-Jorgensen (1958), noted that "The new techniques cannot be expected to help us find the functional units of a language. Some of the new machines are known as 'phoneme detectors'. This sounds very promising, but the name is misleading. They are not detectors of phonemes, but of

sounds. They can be constructed to react to sound differences which by previous analysis have been found to be distinctive in a given language, but they cannot detect phonemes which have not been detected beforehand. The new techniques can, however, help us in giving a better acoustic description of the linguistic units" (479).

[62] According to Jones (1957), "viewed from the 'physical' angle a phoneme is a family of uttered sounds (segmental elements of speech) in a particular language which count for practical purposes as if they were one and the same" (191). Similarly, Jones (1964) described the phoneme as "a family of sounds consisting of an important sound of the language (generally the most frequently used member of that family) together with other related sounds which 'take its place' in particular sound-sequences or under particular conditions of length or stress or intonation" (49). Summarizing this "family resemblance" approach, Twaddell (1935) writes, "A phoneme may be defined as a family of sounds in a given language, consisting of an important sound of the language together with other related sounds, which take its place in particular sound-sequences" (64).

[63] Karl Bühler (1879-1963) was a prominent Viennese psychologist centered at the Psychological Institute of Vienna University, famous for its studies on child development. Bühler's research included contributions to developmental psychology, human perception and cognition, and laboratory studies on language comprehension, as well as theoretical discussions regarding the nature of expressive behavior and the Gestalt principle. Bühler emigrated to the United States in 1938, but it is through his association with the Linguistic Circle of Prague and, in particular, with Roman Jakobson that his linguistic work became widely known (see Asher 1994). Bühler's main linguistic monograph *Sprachtheorie: Die Darstellungsfunktion der Sprache*, first published in 1934, was translated into English by D. F. Goodwin and published in 1990 by John Benjamins Publishing Company under the title *Theory of Language: The Representational Function of Language.*

[64] As Hockett (1968) puts it, "During the 1940's some suspected that it might be possible to determine the forms of a language, and all the patterns by which they combine into larger forms, without any reference to meaning at all. Some decided that this was not only possible, but indeed the only rigorous procedure, even if occasional resort to meaning might be a useful practical shortcut" (24; reprinted in Fought 1999a, 151).

[65] Here is Bloch's (1948) linguist mea culpa:

> [I]n our wording we shall avoid all semantic and psychological criteria. The implication is, of course, that such criteria play no part, or at least need not play one, in the theoretical foundation of phonemics. But we do not imply—in view of what has just been said—that appeals to meaning, or even (in desperate cases) to the informants own judgment of like and unlike sounds, are therefore necessarily ruled out as practical devices in the actual work of making phonemic analysis. The basic assumptions that underlie phonemics, we believe, can be stated without any mention of mind and meaning; but meaning, at least, is so obviously useful a shortcut in the investigation of phonemic structure—one might say, so inescapable—that any linguist who refused to employ it would be very largely wasting his time. (5)

This would not, however, stop Bloch from "wasting" his own time as he would redouble his non-semantic efforts in the 1953 "Contrast" paper discussed below.

[66] In reaction to these discrepancies between theory and practice Pike (1952a) would complain, "Yet I suggest that such a procedure is not only 'almost inescapable' on a practical plane, but is probably so on a theoretical plan as well—with the investigator's knowledge of some semantic differential between some utterances as a prerequisite for certainty in phonemic analysis" (216). Continuing, Pike protests:

> They make 'statements' that place morphemes (and/or phonemes by extension) and meaning on different levels of theory, insisting that they are theoretically separate, and then to proceed to ignore the difficulty by using 'shortcuts' which allegedly do not destroy the theory of the essentiality of separate analytical levels but which in practice are their actual method of operation without such separation. I fail to find any place where they attempt to teach us *how* to go the long way around, eliminating their own short cut. I am troubled by this use of 'statements' where I

> need demonstration or evidence to appreciate and to utilize their material. In my experience problems assumed or defined out of existence have a troublesome habit of reappearing. Until a theory is translatable into *workable procedure*, i.e., until it is testable by experience in the fact of a *hard core of reality in communication*, I do not assume it to be proved. (218, emphasis original)

[67] Here is how, in his review of Harris's *Methods of Structural Linguistics*, Householder (1952) framed the debate: "The theory of the 'God's truth' linguistics . . . is that language *has* a structure, and the job of the linguist is (a) to find out what that structure is, and (b) to describe it as clearly, economically, and elegantly as he can, without at any point obscuring the God's truth structure of the language. The hocus-pocus linguist believes . . . that a language (better, a corpus, since we describe only the corpus we know) is a mass of incoherent, formless data, and the job of the linguist is to somehow arrange and organize this mass, imposing on it some sort of structure (which must, not of course, be in any striking or obvious conflict with anything in the data)" (260). Anderson (1985) provides a description of the nature of this debate within American structural linguistics. As Anderson recalls it, claims of the tenor: "I don't care if it *is* the 'right' answer; how do you justify having found it?" (284) sharply divided linguists as they challenged the linguist to consider linguistic systemization as either an instrumental calculus (a "hocus pocus invention") or as a unique discovery ("God's truth").

[68] Interestingly, Goodman goes on to state (in his "Third Stricture") that "Similarity does not provide the grounds for accounting two occurrences performances of the same work, or *repetitions of the same behavior* or experiment" (439, emphasis added), an observation relevant to Quine's attempt to provide a strictly behavioral reduction of phonemic equivalence.

[69] MacIver (1937a) continues:

> In the first place, similarity is a matter of degree, and tokens which are "of the same type" are certainly not always *exactly* similar—as any knows who has had to correct examination-papers and has compared the first token of the type *the* which occurs in script, written at leisure, with the last token of the same type, in the same script, written in the hurried scrawl at the end. This would be no objection if tokens of the same type were always *more* similar to one another than to tokens of other types, for then we could say that tokens are "of the same type" if they had *more than a certain degree* of similarity. But in fact a token may be *less* similar to one that is "of the same type" than to one that is "of a different type" . . . (60)

MacIver then correctly goes on to point out the language-specific cross-classification of "formal types," i.e., phonological categories, by comparing various dialects of English within Britain: "The norm and the permitted limits of divergence both depend on the particular language in question, so that tokens which are of the same formal type in one language may be of different formal types in another: this is recognized in what philologists know as 'the theory of the phoneme'"(61).

[70] Here is how Roy Harris (1973) attacks Chomsky's position:

> It is far from clear why anyone should regard the 'pair test' in the form described by Chomsky as a test of phonemic distinctness, when what it is is manifestly something quite different, namely a test of auditory distinguishability of utterances. Such a test would, of course, become a test of phonemic distinctness if we had some guarantee that the utterance-tokens in question were not distinguishable by any other than phonemically relevant features. But any such proviso must of necessity involve the phonologist in circularity. Nor can this circularity be broken by the proposal to define the phoneme simply by reference to the 'pair test': e.g., by saying 'Let us mean by *phonemically distinct* nothing more than *distinct as measured by the pair test.*' For the effect of such a proposal would be simply to equate 'phonological unit' with 'auditorily distinguishable unit,' and for purposes of phonological analysis this equation is clearly intolerable. (35)

Similarly, Hill (1958) conceded that

> Until a few years ago, it was an assumption almost universal among linguists that a speaker, even without special training, would infallibly and automatically hear the contrasts in his own speech, and that the only things he would not hear would be sounds which were not contrastive. Consequently, it would at that time have been necessary to say that any speaker who had trouble

> in hearing four grades of stress would be one who had only three contrasts. We have taken the position that there are speakers who have four contrasts but who still have difficulty in hearing all the distinctions they make. Such difficulties occur not only in the system of stress, but with other sounds as well" (17-18).

But not only may speakers apparently fail to discriminate phonemic contrasts, speakers can discriminate non-phonemic (allophonic) differences. As Strang (1958) complains, "we have heard too much of the ordinary man's phonetic obtuseness, of the limitations of his phonetic discrimination to phonematic distinctions" (reproduced in Hutton 1990, 50).

[71] While debating Lounsbury (in *Syntactic Structures*) Chomsky (1957) points out that,

> We can make Lounsbury's position more acceptable by replacing the question 'do they have the same meaning?' with 'are they the same word?' This avoids the pitfalls of the essentially irrelevant semantic question, but it is hardly acceptable in this form, since it amounts to asking the informant to do the linguist's work; it replaces an operational test of behavior (such as the pair test) by an informant's judgment about his behavior. The operational tests for linguistic notions may require the informant to respond, but not to express his opinion about his behavior, his judgment about synonymy, about phonemic distinctiveness, etc. The informant's opinions may be based upon all sorts of irrelevant factors. This is an important distinction that must be carefully observed if the operational basis for grammar is not to be trivialized. (97)

What, then, is the basis for phonological classification? "Consistent identification," says Chomsky: "If consistent distinguishability is maintained under repetition, [the field worker] will say that the words . . . are phonemically distinct" (99). For the reasons discussed above, this test is liable to degenerate into a test of mere auditory acuity.

[72] In a review of Harris (1951), Hockett falsely reports that, in addition to telling us how to determine sameness or difference of sound (which Harris does indeed do), "Bloomfield's other fundamental element—meaning—is entirely eliminated." This claim by Hockett is reproduced in Hymes and Fought (1981, 163-164) who correctly emphasize that Harris's pair test was merely an operationalization of Bloomfield's fundamental meaning assumption, not a replacement. Ohlander (1976, 81) makes this same point. Perhaps the most egregious example of misrepresenting Harris's pair-test as a non-semantic procedure is Lees (1957) who actually "corrects" another linguist for "construing] Harris's references to the use of meaning criteria in that work as the major cause of his failure to provide a discovery procedure (though Harris's methods in fact make no use of semantic criteria)" (Lees 1957, 381). Pace Lees, Halle (1954, 200) correctly emphasizes that Harris's method is *not* exclusively non-semantic: "It is to be noted here that in Harris's presentation recourse is had to meaning in the instructions given the original speaker, where the two utterances are identified 'by translation or otherwise.'"

The twists and turns of all this get very tangled; I encourage the reader to have copies of the relevant texts at hand before trying to read any further. Part of the problem with these entangled reports is, I suspect, that Harris himself is not clear about his exclusion of meaning. This leads Lees to confusingly claim that "Harris avoids this confusion in phonemic analysis by advocating the use of pair-tests (*Methods* 31-2)." Lees claims that Harris does this by distinguishing "between informant response and semantic criteria in the phrase 'meaning-like distinction between utterances which are not repetitions of each other' (7 fn. 4)" (Lees 1957, 369). It is not clear how the invocation of "meaning-like distinction between utterances which are not repetitions of each other" avoids meaning. Is it the distinction between "meaning" and "meaning-like" which, according to Lees, frees Harris from semantics? But not only is the pair-test based upon the semantic intuitions of the informant, and not only does Harris, as matter of methodology, explicitly recognize this, the references provided by Lees direct the reader to precisely those pages where Harris (1951) actually concedes the semantic-basis of the pair-test, including the passages from pages 7 and 29 quoted in the text above. Lees (1957) also goes onto to misleadingly assert "That the informant response test is not a semantic criterion has been clearly demonstrated by Chomsky [1957] especially for the case of phonemic analysis" (396) and falsely reports that Halle (1954, 200) and Hockett (1955, 144-145) also avoid meaning in their account of phonemic contrast.

[73] Here is how Bloomfield (1933) expresses a similar complaint:

> Practical phoneticians sometimes acquire great virtuosity in discriminating and reproducing all manner of strange sounds. In this, to be sure, there lies some danger for linguistics work. Having learned to discriminate many kinds of sounds, the phonetician may turn to some language, new or familiar, and insist on recording all the distinctions he has learned to discriminate, even when in this language they are not distinctive and no bearing whatever. . . . The chief objection to this procedure is its inconsistency. The phonetician's equipment is personal and accidental; he hears those acoustic features which are discriminated in the languages he has observed. Even his most "exact" record is bound to ignore innumerable non-distinctive features of sound; the ones that appear in it are selected by accidental and personal factors. There is no objection to a linguist's describing all the acoustic features that he can hear, provided he does not confuse these with phonemic features. He should remember that his hearing of non-distinctive features depends upon the accident of his personal equipment, and that his most elaborate account cannot remotely approach the value of a mechanical record. (84-85)

[74] After summarizing the technical faults of purely distributional accounts of phonemic equivalence, Diderichsen (1958) comes to a similar conclusion regarding an ineliminable residue of meaning:

> The most fatal feature, however, is the desperate attempt at working out methods of linguistic analysis totally avoiding any reference to meaning or signification. Indeed it is frankly admitted that methods based on this principle are highly cumbersome in practical work. But it is urged that they are more objective and open to check than methods based upon commutation viz. distinctiveness. I hope that the report has made it evident that this principle is due to the adoption of Daniel Jones's anti-phonemic [i.e., "physicalistic"] definition of the phoneme in the phonemic theory of Sapir [sic] and Bloomfield. . . and that the methods, instead of being objective, are fundamentally inadequate, neglecting the constitutive character of language elements, *viz.* their distinctiveness. For what does distinctiveness mean if not 'distinctiveness as to meaning'? (211)

[75] While discussing "free variation," Fudge (1970) points out, using Turkish as an example, that "it is not easy to see how [e] and [æ] in Turkish could be assigned to the same phoneme on any other grounds than that substitution of one for the other does not give rise to different words and different meaning (for instance, the apparently contrasting [bana gel] and [bana gæl] both mean 'come here'; in fact any cases in which type 1 variation [unconditioned free variation] is sufficiently wide [as to cross a potential phonemic boundary] will cause trouble unless the differentiating function of phonemes is taken as a basis and not a corollary of the definition" (80).

[76] Zellig Harris (1951) concedes this much too when he stresses "The use of instruments which permit exact measurement, as the ear does not, may enable us to employ tests of measured similarities (of sound waves or body motions) instead. But only those tests will be linguistically relevant which will accord with (even if they are not based on) the speakers' actions. This ultimate correlation is the only one which has so far been found to yield a simple language structure" (31).

[77] I take this argument, implicitly assumed but rarely articulated among advocates of "semantic-based phonology," to fill a lacuna pointed out by Chomsky (1957, 100), Lees (1957, 399) and Postal (1968, xi). Here is Postal's version of it: "Bloch tried to show that the notion of contrast was itself definable in terms of phonetic properties and their distributions. It is generally agreed that this attempt was a failure. . . . But the usual inference, namely, that therefore contrast must be defined in terms of semantic properties is a *non sequitur*" (1968, xi).

[78] In probably the most remarkable passage penned on these issues, Lees (1957) actually bites on this bait: Lees embraces the counter-intuitive consequences of taking merely "consistent identification" as the basis of phonemic structure, arguing that if an individual can consistently identify allophonic pairs as distinct, then they are in fact phonemic: "[F]or if an informant does in fact distinguish ["light"] [l] and ["dark'] [ɫ] consistently in all examples tested, then the distinction is not a case of free variation at all, but one of synonymy . . ." (397).

Nevertheless, Lees (1957) does concede that "If, then, because of the [semantic-based] arguments offered above, semantic criteria, such as 'difference in meaning', have been withdrawn as useful evidence

for grammatical structure, there would seem to be nothing in the linguistic data to indicate to the analyst which utterances or utterance fractions must be compared and contrasted to test which tentative structural units" (399). Undaunted, Lees—shifting the burden of proof like Postal (1968) and echoing Chomsky (1957)—goes on the offensive, continuing with "First, this is not a valid argument in favor of semantic criteria anymore than it is in favor of any other possible criteria, say chemical, political, or theological. Second, as we see from the practice of linguists, though unfortunately not from their own descriptions of linguistic methodology, the criterion actually used in all crucial cases is either the informant's response in carefully designed pair-tests or other elicitation techniques, so constructed as to be completely indifferent to meanings, or else" and this is the real kicker, "the linguist's own *Sprachgefühl* is called upon to provide the correct analysis. . ." (399).

But whether it is the linguist's 'Sprachgefühl' (glossed by Lees as an "intuitive notion about linguistic structure") or the informant's, meaning is implicated in the analysis. Quoting again from Lees, "it is precisely this Sprachgefühl, this intuitive notion about linguistic structure, which, together with the sentences of a language, forms the empirical basis of grammatical analysis; and it is precisely the purpose of linguistic science to render explicit and rigorous whatever is vague about these intuitive feelings. This is apparently what Hockett means in his discussion of 'empathy' in phonemic analysis, *Manual* 146-7" (399). Whose side is Lees on?

[79] Bloomfield (1933) writes:

> [O]ur knowledge of the world of which we live is so imperfect that we can rarely make accurate statements about the meaning of a speech-form. The situations (A) which lead to an utterance, and the hearer's response (C), include many things that have not been mastered by science. Even if we knew much more than we do about the external world, we should still have to reckon with the dispositions of the speaker and the hearer. We cannot foretell whether, in a given situation, a person will speak, or if so, what words he will use, and we cannot foretell how he will respond to a given speech. (74-75)

And latter in *Language*, "In order to give a scientifically accurate definition of meaning for every form of a language, we should have to have a scientifically accurate knowledge of everything in the speaker's world. The actual extent of human knowledge is very small, compared to this. . . . The statement of meanings is therefore the weak point in language-study, and will remain so until human knowledge advances very far beyond its present state" (139-140). It is for this reason Bloomfield (1943) writes, "It follows from this that in all study of language we must start from forms and not from meanings" (133).

> But clearly this "must" is the must of a should rather than the must of practical necessity given that, according to Bloomfield himself, phonemic forms can not be individuated until (some) meanings have been. What follows is that, however unpalatable, contrastive semantics is indispensable for establishing phonemes and that the phonologist must *start* with (at least some differential) meanings in order to recognize (at least some) basic phonological forms. As Love (unpublished manuscript) recently writes, "Meanings are so problematic, in Bloomfield's terms, that there is little or nothing to say about them. But if the identification of forms depends on meaning, that ought to be no less problematic. [Yet] on the contrary, in *Language* and elsewhere Bloomfield has a great deal to say about forms. . . ." (17).

[80] In following passage, Hall (1987b) explains:

> From Bloomfield's justified insistence on formal, rather than semantic features as the *starting point* for linguistic analysis, this group (especially Harris) set up as a theoretical aim the description of linguistic structure exclusively in terms of distribution. They did not deny the existence of meaning, nor its relevance to the use of language. They did envisage linguistic structure, however, as pure patterning, and existing wholly independently of meaning. It is (or should be) widely recognized that this aim was only theoretical. In practice, whether they admitted it or not, even these extremists made use of semantic criteria wherever necessary. But many post-1945 European critics did not realize the purely theoretical nature of these considerations, nor did they know the background of confusedly semantic analysis against which Harris's and the others' were reacting. As a result, these critics treated Harris's and the others' notions as if they were (a) observed in practice, and (b) characteristic of all American linguistics of the 1940's and 1950's. (156)

The problem with this account of things resides in its inconsistency, with Hall (1964) himself—immediately after stressing that "considerations of form must take precedence over considerations of meaning"—explains that when doing phonology, "we have to make use of meaning for the differentiation of functional units" (32-33).

[81] With the same intent, Fries (1954) writes: "Sometimes it is insisted that we use 'differential' meaning, not 'referential' meaning. Perhaps this statement means that the linguistic analyst seeks basically to establish the fact whether two instances differ in meaning content or not. He does not need to know what that content is or in what ways the two may differ. If they differ in meaning he assumes that there must be some difference in formal features, and sets out to find, prove, and describe that difference" (68). Even Harris, when explaining his so-called "behavioral" pair-test method, favorably distinguishes between meaning and "differential meaning" (further evidence that Harris recognized the semantic nature of the pair test): "To see how the description of language structure is achieved, we note first how the elements can be established. First it is possible to determine the phonemic distinctions in a language by a behaviorist test that does not involve the specific meaning of words or the investigator's judgment of phonetic similarity" (1988, 4).

[82] Trager (1959) would also pile on Hockett for similar reasons: "A morphemics based on meaning cannot be successful in analyzing language. It is not meaning but distribution and arrangement of recurrent partials that is the basis of morphemes, as well as for any other level of grammatical analysis" (79). According to Koerner (2002), such critiques were leveled directly against Bloomfield too: "Many years earlier, but quite in line with Trager's position on 'meaning', the sociologist Paul Hanly Furfey (1896-1992) of the Catholic University in Washington, D.C., published an article in the journal edited by Trager entitled 'The Semantic and Grammatical Principles in Linguistic Analysis', in which Bloomfield is taken to task for not being rigorous enough in his elimination of semantic considerations in distributional analysis" (83).

[83] This is not to deny the significance of distributional analyses as an important mechanism for identifying phonemic equivalence but to emphasize that they are incomplete. See, for instance, Smalley (1955) "A problem in phoneme identification without differential meaning."

FROM A PHONO-LOGICAL POINT OF VIEW

Chapter 8
Revisiting Roger Brown's "Original Word Game": An Experimental Approach to the Semantic Basis of Language-Specific Speech Perception

The child passes through words to the phonemic perception of speech. N. Kh. Shvachkin

Developmental Speech Perception

Intrigued by the robust pace of language acquisition, linguists have proposed a variety of cognitive pathways which support, and explain the experimental details of children's linguistic progress. In particular, speech theorists have documented neonates' precocious language-like speech discrimination (Eimas 1975, Kuhl 1987, Werker 1994), while developmental psycholinguists have detailed the assumptions or biases guiding preschoolers' verbal-conceptual maturation (Gelman & Wellman 1991, Keil 1991, Markman 1989). Though developmental constraints are often portrayed as innate, linguistic accelerants need not be specified in the genome. Rather, they may be part of what Roger Brown (1958) dubbed the "Original Word Game." As current experimental research demonstrates, external information provided in the form of word labels can facilitate children's superordinate and atypical object categorization (Gelman & Coley 1990, Waxman & Markow 1995). By contrast, however, the inverse hypothesis that the external linguistic labeling of objects or properties can developmentally support the language-specific "phonological reorganization" (Best 1994, Jusczyk 1993, Werker & Tees 1992) of infant speech categories has yet to be theoretically isolated and experimentally explored.

Broadly speaking, contemporary investigations into developmental speech perception represent an attempt to synthesize two strains of empirical research that lead in opposite directions. On the one hand, a variety of now-standard experiments in infant speech perception indicate that neonates are innately prepared to discriminate among most, if not all, phonetic speech contrasts potentially employed by any natural language (Best et al. 1988, Jusczyk 1985, Jusczyk 1997, Kuhl 1987, Kuhl & Meltzoff 1997, Trehub 1976, Werker et al. 1981, Werker & Tees 1984; for reviews see Strange 1995 and Jenkins & Yeni-Komshian 1995). As the late Peter Jusczyk (1985) explained, "By and large, these studies indicate that infants are capable of discriminating virtually every type of phonetic contrast that they have been tested on," suggesting that infants are "universal phoneticists" biologically endowed to perceive all possible phonetic contrasts (205).[84] In particular, there is no evidence that certain populations are naturally predisposed to discern some phonetic contrasts better than others.

On the other hand, these experimental results with infants do not predict adults' mature speech perception. So while language-specific phonological biases are not genetically based, they are clearly observable in adult populations. Unlike neonates, adults exhibit language-specific biases against discriminating between phones which do not appear in their native language (Pisoni et al. 1982, Trehub 1976, Trehub 1976, Werker 1989, Werker et al. 1981, Werker & Logan 1988, Werker & Tees 1984), and even more importantly, against those that *do* appear in their target language but happen to be sub-phonemic allophones (Werker 1995a, 1995b). In light of this, researchers have turned their attention to documenting when this transition from language-general speech perception to language-specific speech perception occurs. They soon discovered, at least to their initial

surprise, that within their very first year infants undergo a process of phonological specialization characterized, at least in part, by a language-specific *inability* to discriminate among various non-phonemic contrasts (Jusczyk 1994, Werker & Tees 1992). As Eimas (1985) concludes,

> it would seem that the infant comes into the world biologically endowed with the ability to distinguish and categorize virtually all of the information that is relevant to the phonetic categorization of natural languages. Experience with the parental language serves to maintain and perhaps enhance those categorizations for which there is an early correspondence. On the other hand, experience will eliminate a category or alter the boundary location between categories in situations where the correspondence is quite disparate. (171)

Data, however, outpaces theory in this line of developmental research. For while it is obvious that this transition is related to the particular linguistic environment of the infant, it remains obscure whether mere acoustic exposure to the target language is sufficient, or perhaps interpretive experience with the language is also necessary, to explain the dynamic of this selective perceptual desensitivity.

"Linguistic experience": Acoustic or interpretive?

To be sure, one reason why these two linguistic variables (i.e., acoustic experience and interpretive experience) are so easily confounded is the obvious point that 9- to 11-month old infants' acoustic and incipient interpretive experiences are quite naturally entangled. Infants typically do not hear human speech in deliberately contrived interpretive vacuums, and it would seem that no (ethical) experiment which attempted to control for this variable by exposing infants only to phonologically correct—but perpetually meaningless—speech can be conducted.[85] Moreover, if the development of language-specific speech perception is indeed at least partially underwritten by interpretive information, this argument incurs the

empirical burden that approximately 9- to 11-month old infants are not only capable of organizing their environment into objects, events, and properties, but are also capable of associating particular speech tokens with these non-linguistic categories. Nevertheless, experimental data marshaled from pre-linguistic infant categorization (Eimas 1994, Mandler 1992, Younger 1993), combined with studies investigating infant "cross-modal" speech-object association (Baldwin & Markman 1989, Mandel et al. 1995, Waxman & Markow 1995)—not to mention early infant comprehension studies (Bates et al. 1994, Benedict 1979, Harris et al. 1995, Huttenlocher 1974)—suggest that such speech-referent associations might very well be in place. Be that as it may, the causal relationship between this associative ability and the development of language-specific speech perception has yet to be theoretically established.

Consider, however, how the interpretive skills of early speech-object associations could potentially suppress discriminability of certain phonetic contrasts, resulting in language-specific speech perception: Insofar as

1) infants associate some semantic value with some speech utterances (the premise of early speech/referent association); and

2) at least some of these utterances are subject to sub-phonemic free allophonic variation (the premise of free allophonic variation); then

3) the fact that two phonetically distinct speech tokens are consistently associated with the same object, event, or property might "semantically" motivate the phonologically naive infant to collapse those phonetic differences that, as it were, simply do not make a difference (the hypothesis of semantic-based phonological development); and

4) begin to perceive them in accordance with the target phonology (the prediction of suppressed speech discrimination).

No doubt this is why Walley (1993) notes that the language-specific changes in infants' speech perception documented by Werker and colleagues occur "perilously close to the nine-month point often cited as the age at which infants first begin to comprehend words" (173).

"Free" Allophonic Variation

In addition, however, to capitalizing on the suggestive but largely circumstantial evidence provided by empirical studies of early infant speech-referent association, the logical dynamic sub-phonemic allophonic variation imposes on phonological development must also be considered when evaluating the proposal that language-specific speech perception might be at least partially driven by interpretive semantic cues. In particular, accounts of phonological development which implicitly assume that language-specific speech perception can emerge in an interpretive vacuum are arguably incomplete on purely methodological grounds. To see why, consider the fact two languages may draw upon the same phonetic resources but nevertheless instantiate different phonological systems whereby the same pair of phones is phonemically contrastive in one language but phonemically irrelevant in the other. Moreover, and as one might expect, these language-specific phonological differences are correlated with psycholinguistic processing differences in the ability of adults to discriminate among phonetically distinct but allophonically related phones in their language (Pegg & Werker 1997).

The issue, then, reduces to whether, say, an English-learning infant could, at least in principle, eventually come to correctly categorize English speech tokens merely by listening to extended bouts of English heard exclusively over, say, the radio. Yet because this

auditory stimulus will include speech tokens which are phonetically distinct, but (at least in English) phonemically equivalent (e.g., in utterance-final positions /t/ may be aspirated or non-aspirated), there is no acoustic reason for the infant to treat them as mere allophonic variants, or to eventually desensitize to the phonetic contrast. After all, aspiration is phonemically distinctive in some languages just as much as voicing—the difference between [cap] and [cab]—is phonemically distinctive in English. This is why two word tokens, though differing merely in aspiration, count as distinct words in languages like Hindi and Thai, but as mere allophonic variants in English.

Cross-linguistic phonological variation like this is a challenge for infants on route to acquiring the language-specific contours of their language. Aspiration is a phonemically distinctive feature in Hindi, but merely allophonic in English. But insofar as both aspirated word-tokens and non-aspirated word-tokens appear in both languages, both Hindi-learning infants and the English-learning infants are exposed to both aspirated and non-aspirated word-tokens. And the question is: how are infants to figure out whether aspiration is phonemically distinctive in their (Hindi) target language or mere allophonic variation in their (English) target language? How are infants supposed to figure out what they are trying to learn? Clearly, one way is to appreciate the semantic difference between two Hindi words that differ only in aspiration versus the semantic equivalence of two English words that also differ only in aspiration. In this way, the infant would be positioned to infer the semantic, and hence, phonological relevance or irrelevance of this phonetic distinction. Insofar as aspiration appears—ignoring rule governed patterns of complementary distribution for the moment (see below)—phonetically more or less equally in the speech stream of both English and Hindi, no amount of mere auditory exposure is sufficient to

induce the phonological reorganization of the phonetic contrast as both contrasts appear in both languages. The only distinguishing feature, at least with respect to "free" allophonic variation, which serves to distinguish the presence of aspiration is that aspiration is semantically significant in one language but semantically neutral in the other.

While some researchers have skeptically speculated about the role interpretive cues might play in inspiring language-specific speech perception in infancy (Jusczyk 1985 Kuhl et al. 1992, Macnamara 1982, Vihman 1996, Werker & Lalonde 1988, Werker & Tees 1984), only MacKain (MacKain 1982, MacKain & Stern 1985) has been particularly forceful in highlighting the role semantic content, in contrast to mere acoustic exposure, could play in phonological development:

> [E]xperience listening to the speech of a particular linguistic community will not induce discrimination of a language-specific contrast, shift a boundary to correspond to that of the language user, or attenuate discrimination of a distinction that may be discriminable by virtue of its psychoacoustic characteristic until infants are aware that certain sounds contrast to convey differences in meaning (MacKain & Stern 1985, 31).

To be sure, MacKain's case is overstated. To the extent that some phonetic distinctions do not appear in the infant's target language simply because not every phone appears in every language, the infant may develop language-specific patterns of speech perception on the basis of what amounts to the phonological equivalent of negative evidence. Moreover, insofar as some allophonic variation is predictable on the basis of language-specific patterns governing the phonetic realization of the same phoneme in different contexts (see Jusczyk 1993, Jusczyk & Aslin 1995, and especially Peperkamp & Dupoux 2002), then the phonology of the language can, at least in part, be inferred by more or less passively tabulating these phonotactic regularities. As such, the answer to MacKain's (1988) question: "Can exposure to the speech of their native language influence infant's

perception of phonetic contrasts prior to the onset of symbolic functioning?" (55) is clearly: Yes. On the other hand, as MacKain (1982) emphasizes and Jusczyk (1993) seems to implicitly recognize, the presence of "free" allophonic variation presents a formidable challenge to the establishment of language-specific speech categories simply because free variation undermines the "well-behaved" phonotactic regularities of complementary distribution available to the infant in the speech stream. In order to resolve free allophonic variants, semantic information is required.

Ohlander (1976), for instance, is conscientiously explicit in detailing the developmental relationship between semantics and phonology:

> In this way the phonological stage presupposes the semantic stage. Exclusion of semantic criteria in phonological analysis would deprive the linguist, as well as the child learning his native language, of the only means available to him for introducing some kind of organizing principle into an otherwise more or less chaotic multitude of phonetic data. The function of semantic criteria in phonology is thus, so to speak, to provide the incentive for the linguist and the language-learning child to look for and try to establish systematic phonological relations between phonetically different forms having the same meaning. In other words, considerations of meaning constitute an indispensable part of the linguist's and the child's discovery procedure as regards phonological structure. The basic heuristic principle involved in the child's language acquisition strategy can be seen as resting on a kind of tentative biuniqueness assumption concerning form-meaning relations. . . namely that semantic identity is likely to be matched by phonological identity. This, it seems, is what prompts the child to look for, discover, and internalize the systematic phonological rules of his native language, and thus to learn the differences between accidental and systematic neutralization of semantic contrast, i.e., between synonymy and allomorphy. (79-80) [86]

It is for these reasons, then, that the phonological structure of language cannot, and moreover need not, be induced merely through asemantic acoustical exposure. But nor can we, for obvious reasons, attempt to empirically confirm this reasoning by wholly depriving infants of the opportunity to leverage meaning with phonological consequences. Instead, we must concentrate on whether interpretive-based semantic information can be implicated in the *acceleration* of 9- to 11-month old infants'

perceptual desensitivities to phonetically distinct speech tokens. There are two ways to experimentally explore the relationship between semantics and phonological development. One way would be to deprive infants of potentially useful semantic information and document their phonological development as it occurs in a semantic vacuum. Another way is to provide some infants with extra semantic information relative to other infants and compare their phonological development. So rather than underrating the role perceptually accessible phonological regularities play in supporting phonological development, we should look for evidence that, in addition to information intrinsic to the speech pattern of their target language, infants may also capitalize on extrinsic semantic information concerning what the speech patterns are actually about.

Revisiting the "Original Word Game"

With this in mind, it should be clearer how to experimentally substantiate the role nascent semantic categories might play in underwriting the development of language-specific (allophonic) speech perception. In fact, it is merely a version of Brown's "Original Word Game." Using the "Shvachkin-Garnica" word-object training procedure originally devised by Shvachkin (1973) and Garnica (1973) for testing phonemic speech *discrimination* in preschoolers—recently adapted by Werker & Pegg (1992) for infants—but inverting it to test for phonemic speech *equivalencies*, the hypothesis that infants use non-linguistic categories to help convert universal speech perception into language-specific speech perception can be empirically tested. Simply by associating both tokens of a phonetically contrastive minimal "word" pair with either the same referent (to encourage suppressed discrimination), or uniquely associating each phonetically contrastive speech token with two

different referents (to maintain discrimination), infant phonological development should be susceptible to experimental manipulation.[87] Interestingly, Kuhl & Meltzoff (1997) report that even a mere 15 minutes (5 minute intervals over three days) of laboratory exposure can be effective in influencing infant responses to various phonetic stimuli.

In short, once the trajectory of infant speech perception is recognized to consist in language-specific desensitivities, the basic experimental tasks designed to explore this phenomena can be revised to reflect this change in theoretical orientation. This is why Werker's (Werker & Pegg 1992, Stager & Werker 1997) hesitancy to grant 10- to 12-month infants a language-specific phonological system because they do not interpretively respond to words they do understand differently from even phonemically contrastive nonsense words they do not understand (e.g., /stop/ and /stok)/) is noteworthy but misplaced. Pace Werker, it is not the case that "the strongest evidence for the functional (phonological) use of perceptual categories would be provided if it could be shown that infants differentiate real words on the basis of minimal pair contrasts" (Werker & Pegg 1992, 300). Instead, the relevant litmus test for early "functional" (i.e., phonemic) language-specific speech perception is for the infant be able to apply their evolving perceptual sensitivities to the task of NOT distinguishing between speech tokens on the basis of semantic allophonic equivalencies.

Testing infants for phonemic equivalencies is more diagnostic of phonological structure than testing for phonemic distinctions because, as reviewed above, infants are essentially born with the ability to discriminate most if not all phonetic contrasts—whether they happen to be phonemic in their target language or not. What is symptomatic of a semantically-informed language-specific phonological system in late infancy, then, is not

the interpretive ability to discriminate between, say [mat] and [man], but the interpretive ability *not* to discriminate between [mat^{h}] aspirated and [mat] non-aspirated.[88] Rather than indirectly correlating a constellation of cognitive skills (e.g., object categorization, goal-oriented "means ends" thinking, arbitrary association) with language-specific speech perception (Lalonde & Werker 1995), experiments should be designed to directly examine the influence of linguistically labeled categories on the development of speech perception. As Walley (1993) reminds us,

> In light of the rather slim three-month difference between current estimates for the onset of language-specific speech perception and for word comprehension, it would seem essential in future research to assess directly those core cognitive/semantic abilities that infants are claimed to lack, in addition to demonstrating that they possess a certain perceptual ability at a particular age. That is, it will be important to directly link or unlink, as the case may be, performance across different linguistic subdomains within subjects. This approach would yield the strongest and most convincing data for evaluating different theoretical accounts of phonetic development (174).

The design of the experiment proposed here, then, consists in three parts: an initial speech discrimination task, a phonemic training procedure, and a follow-up speech discrimination task. Infants are first tested on their ability to discriminate a particular phonetic contrast. Infants with comparable discrimination skills are grouped together so as to then be exposed to two different phonemic training procedures. In the phonemic training procedures, infants are trained to associate a phonetically distinct speech pair with either two different referents (the control subjects), or trained to redundantly associate both members of exactly the same phonetically distinct speech pair with one and the same referent (the experimental subjects). In this training phase, both groups of infants are exposed to exactly the same speech pairs for exactly the same time, and in exactly the same sequence; only the semantic props are manipulated. In the experimental condition, one and the same referent is repeatedly presented to the infant regardless of which member of the speech pair the infant

is exposed to. This redundant pairing is designed to encourage suppressed discriminability of the phonetic distinction. In the control condition, subjects are presented with the same two speech tokens, but the speech tokens are uniquely presented in conjunction with two different and dissimilar referents in alternation: one referent is consistently associated with one member of the speech pair while a different referent is consistently associated with the other member of the speech pair. This control group phonemic training is intended to encourage the subjects to preserve the phonetic discriminability of the speech pair. So while both groups are exposed to the same speech stimuli, the subjects in the control condition are trained to uniquely associate two referents with what are intended to be interpreted as two distinct word labels associated with two distinct referents, while the subjects in the experimental condition are trained to redundantly associate one and the same referent with what are intended to be interpreted as two allophonic realizations of the same word-label.

Because this is an acceleration experiment, however, the infant subjects may be drawn from either different linguistic populations or the same linguistic population. In a cross-linguistic study, the control condition (where the subjects are trained on associating the linguistic stimuli with two referents) may be satisfied by Hindi-learning infants trained on a phonetic distinction which is phonemic in their language, while the experimental condition (where the subjects are trained on associating the linguistic stimuli with one referent) can be satisfied by English-learning infants trained on the same phonetic distinction which is allophonic in their language. Alternatively, one could conduct the experiment within a single language and consider the non-experimental infant population of the same language to represent the "control group" of infants. The effectiveness of an experimental phonemic training procedure in inducing suppressed discrimination of an

allophonic contrast can be determined simply by comparing the speech discrimination skills of infants before and after their phonemic training sessions.

The post-training speech discrimination phase of the experiment is straightforward. Simply by comparing the speech perception of the control and experimental subjects on a standard speech discrimination task, the effect of semantics on speech categorization can be directly examined. If, after training, statistically significant differences in discrimination are observed when the subjects are tested on novel speech pairs utilizing the very same phonetic contrast used in the training procedure, then it would seem the variable of non-linguistic object categorization can influence speech perception—just as Roger Brown presciently suggested some thirty years ago.

After all, the "original word game" experiments Brown (1958) conducted in the 1950s demonstrated not only the effects of phonemic speech perception on object categorization, but also the effects of object categorization on speech perception. With respect to the former dynamic, Brown experimentally engineered a situation whereby his native Navaho subjects perceived a total of four phonemically distinct word-types whereas the English speakers perceived only two phonemically distinct word-types. But Brown observed how this difference in language-specific speech perception immediately influenced the object categorization behavior of the (adult) subjects. Upon hearing what they perceived to be four distinct words, the Nahavo subjects categorized four somewhat similar objects into four categories. Upon hearing the same four speech tokens, however, the English subjects categorized the same four objects into only *two* categories. The English subjects perceived only two phonemically distinct words, each allophonically pronounced twice. While successfully demonstrating, as he put it, "speech categories operating as a guide to

referent categories," Brown succeeding, albeit crudely by today's standards, in implicitly demonstrating the more general effect of linguistic labeling on object categorization that has since become a major focal point of current developmental psycholinguistic research (Davidson & Gelman 1990, Waxman & Markow 1995).

But by also running the experiment essentially in reverse, Brown discovered that non-linguistic referent categories could serve as a guide to the categorization of the experimenter's *speech*. By deliberately biasing the initial categorization of the objects available to subjects, Brown found English subjects could be encouraged to parse the speech tokens according to non-native contrasts. As Brown (1958) concluded, "This result demonstrates a facet of the Word Game that we have not yet discussed. It is evidently possible for nonlinguistic reality to serve as a guide to the categorization of speech. The isomorphic relationship can be useful in either direction. An inescapable visual difference leads us to look for a speech difference" (216). Of course, the counterpart of this last claim—namely that "inescapable" *similarities* among objects may lead us, and in particular 9- to 11-month old infants, to "look for" speech *equivalencies* among phonetically distinct phones—is the thesis explored in this chapter. And though collapsing phonetic distinctions merely on the basis of interpretive identity is obviously fallible—infants would be, as Macnamara (1982) points out, "phonological anarchists" if they attempted to phonemically equate [small] and [little] merely because they mean very nearly the same thing—this source information could serve as a valuable developmental mechanism supporting the appearance of language-specific "perceptual accents" in late infancy and deserves experimental scrutiny.

So while ignoring the distinction between allophonically related speech-tokens on the basis of semantic content may be a developmental pathway for infants, ignoring the distinction between phonemically distinct word-tokens like [small] and [little] on the basis of semantic content is not. But equating phonetically distinct word-tokens on the basis of semantic content does, however, set a philosophical precedent. Given that some phonetically distinct word-tokens are categorized as substitutionally equivalent on the basis of their common semantic content (rather than just their form), categorizing phonemically distinct word-types as substitutionally equivalent on the basis of their semantic content (rather than form) may be phonologically anarchic, but this does not speak to the philosophical issue of doing so. No doubts infants would be phonologically disorientated if they were to equate synonyms (and co-referring names, assuming they too have the same semantic content). But the issue is not whether synonyms and co-referring names are phonologically equivalent. Synonyms and co-referring names are not phonologically equivalent.

The issue is whether in virtue of the semantic basis of basic phonological equivalence, the philosophical move to substitutionally equate synonyms and co-referring names *salva veritate* on the basis of semantic content now has a rationale independent of traditional arguments pro and con synonymy and the substitution of names in belief contexts. The semantic basis of phonology provides an independent route by which to re-approach issues related to synonymous and co-naming substitutions. Phonology is one example of content-driven substitutional equivalence. Synonymy is another, and co-naming arguably another. But unlike phonemically equivalent word-tokens, phonemically distinct word-tokens do not instantiate the same abstract word-type. Instead, they may be said to

instantiate the same "dot quote"-type. The next chapter details how Sellarsian "dot quotation"—like phonemic slash bar notation—can be deployed so as to give concrete notational expression to even "trans-phonemic" equivalencies.

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[84] One wonders how this could not be the case: How could infants be expected to "learn" speech discriminations they are not even capable of psychoacoustically detecting? This suggests that the notion that infants developmentally "tune" *into* the phonemic distinctions of their native language is somewhat misguided (see Werker 1989 for a discussion). Instead, phonological development principally consists in selectively "tuning" *out* many phonetic distinctions which happen not to be phonologically relevant. This is why a "pruning" rather than "planting" metaphor better describes phonological development: language-users specialize in only those phonetic distinctions that are phonemic in their native language, coming to perceptually ignore those that are not. Analogously, infants appear to be "universal babblists," initially capable producing the sounds characteristic of any human language, again regardless of linguistic environment, but by the second half of the first year their babbling patterns selectively approach the sound system of the target language (Werker 1989). And this too seems to be truistic. How could an infant be expected to learn to produce sounds which it was physiologically incapable of articulating in the first place?

[85] In an article in Slobin (1971) exploring the new science of developmental psycholinguistics, Susan Ervin-Tripp, reports one misguided self-instructional Russian language-learning course which actually had this dynamic as a built in feature: "The most dramatic case of prolonged absence of meaning during language learning was brought about in a self-instructional Russian program written under the supervision of Rand Morton. Intensive training occurred for 17 weeks, with over a month with no meanings, but drills in writing, discrimination, and imitating syllables and phrases in Russian. Late in the program, meaning clues were added to the workbook incidentally. At the end of the program, taken by students from the Defense Language Institute, Western Branch, the students scored below chance on the Army language achievement test for Russian. They entered the usual intensive program of the DLI for acquisition of Russian along with other students with no experience at all in Russian. The [meaning-deprived] trained students remained worse throughout the program and never were able to learn as well as the neophytes." Ervin-Tripp reports that one researcher familiar with the program concluded that "the students either learned that Russian is meaningless, or they [had] generated meanings which interfered with the conventional ones" (195).

[86] Ohlander (1976) continues, quoting from other linguists on the importance of "situational semantics":

> In the speaker's linguistic universe form and function are not to be found in separate compartments, completely sealed off from each other. Above all, the acquisition of language does not take place in some kind of all-linguistic, formal vacuum having no connection with the outside world. On the contrary, and as pointed out by Halliday (1970: 322), "What the child learns he learns in situations of use, and the structures he builds up reflect the functions which he himself internalizes." Thus the child discovers and internalizes language structure on the basis of its communicative functions, as manifested in concrete situations: the acquisition of language implies the continual establishment of relations between linguistic forms and communicative functions, i.e., meanings in a broad sense. In other words, semantic considerations must be assigned an all-important role in the speaker's discovery procedure. As noted by Schlesinger (1971: 86), "already the fact that semantic cues are supplied to the child in abundance is sufficient to make it implausible that they should learn language without using these cues." Therefore a linguistic model that purports to be exclusively based on the formal aspects of linguistic signs, including their distribution, can hardly be considered realistic from the point of view of the speaker. (135-136)

See also Gleason (1961, 260) where situational semantics is viewed as an "incentive" for children to sharpen their phonemic discrimination as well as preventing allophonic over-discrimination.

[87] In fact, Hill (1967) incidentally describes a field work procedure substantively identical to this. In order to identify phonemic structure, explains Hill,

> The jury may be made up of nearly mythical 'native expert speakers,' or it may be made up of linguists. For the [pair] test to prove linguistic difference, the jury must be able to identify which sentence is which, *without reference to context* [italics added]. . . . Suppose, however, that the situation is different. A learned Choctaw (to use one of Bloch's favorite fictions) has heard the word 'hat' pronounced sometimes with release of the /t/, sometimes without. He makes guesses that there is a difference of identity between hat_1, which is released and is made of straw, and hat_2, which is not released and is made of felt. He asks the native speaker to use the sentence 'Here's your hat', meaning a straw hat, and 'Here's your hat' meaning a felt hat, and asks the native jury to tell which is which. Note that he does not merely pronounce the sentence with released /t/ and unreleased /t/, asking the jury whether the two utterances sound different. If that had been the question, he might well have gotten merely identification of the difference between release and its absence. In the method I am describing, the tester is asking 'are these two utterances instances of the same [type of] sentence, or of different [types of] sentences?' It is true, of course, that a single pair test, like that of *hat* just given, does not tell our Choctaw linguist that there is not some other pair, like a pot_1 and a pot_2, where the presence or absence of the release is a difference marker, but at least he knows that there is no difference between a supposed hat_1 and hat_2. (204)

This is a great passage. But why does Hill use two different hats, one made of felt and one made of straw? This needlessly complicates the acquisition process, for there is no *a priori* reason why hat_1 and hat_2 should be identified as phonemically equivalent, even under the conditions Hill describes. The problem is that there is no reason, at least in principle, to assume that hat_1 and hat_2 are not in fact *distinct* labels for *different objects*, namely a hat made out of *felt* and one made out of *straw*. After all, there are different words for drinking receptacles made out of glass ('glasses') and those made out of paper or plastic ('cups'). This is why it is preferable to use exactly the same object in an experimental effort to induce a phonemic collapse between phonetically distinct word-tokens.

[88] Part of the confusion here stems from the awkward evidence indicating that even older preschool children do not always respect the difference between full-blown phonemic contrasts, like /b/ and /d/. This is not fully understood yet; it may be an attentional phenomenon. However, if infant phonological growth consists in decreased sensitivity to many initially accessible phonetic contrasts because they are perceived to be referentially inconsequential, then young children's tendency to conflate phonemically contrastive minimal word pairs might very well be the result of what amounts to functional *over*-assimilation. After all, for subjects whose lexicon is sparsely populated, and hence generally consists of very few word pairs of minimal phonemic contrast, lexical comprehension can—with relative impunity—be accomplished merely by recognizing the "holistic" or "skeletal" acoustic shape of words (Ferguson 1986, Jusczyk 1992) rather than incorporating (what seem to them to be) irrelevant phonological detail. Given that a number of theorists argue that it is an *expanding* vocabulary that propels attention to phonemic segmentation (e.g., Walley 1993b) and both Jusczyk's WRAPSA model and Werker's Functional Reorganization Hypothesis specifically predict that speech perception is *functionally* warped due to semantic pressures, the efficacy of semantic-based training sessions in inducing language-specific phonemic speech perception represents an important test of the proposal that semantic information can support phonological development.

Chapter 9
Brackets and Slashes; Stars and Dots: Understanding the Notation of Linguistic Types

> Logicians have used the word 'type' to refer to linguistic expressions as *abstracta* in contrast to individual linguistic occurrences. They have failed to see, however, that the linguistic entities which they call "types," and which they contrasted with linguistic events or tokens, are, in a genuine sense, embodiments of still more 'abstract' linguistic entities which therefore more properly deserved the term. Thus, logicians speak of a certain class of visual patterns as the type word and members of this class are said to be tokens of this word type. Yet a word, it is clear, is not a mere class of patterns as such. It is surely a class of patterns as performing a specific linguistic function.
>
> Wilfrid Sellars (1950c, 519)

The Status of Linguistic Types

The principle by which linguistic expressions are categorized as the "same" has long been a source of controversy among, and sometimes even between, linguists and philosophers. This problem has a phonological aspect concerning the classification of word-tokens into word-types as well as a philosophical aspect regarding the substitutional status of phonemically distinct but synonymous expressions and co-referring names. This chapter pulls these two issues together by adopting, and then extending, the notational innovation of Wilfrid Sellars' dot quotation. For while a diversity of phonological theories have each attempted to account for the substitutional dynamic governing allophones, on the one hand, and a multitude of philosophically theories have attempted to account for the behavior of co-referring names and synonymous words on the other, one problem affects every position: how to actually refer to an abstract linguistic-like entity—whether this be a linguistic abstraction subsuming phonetically distinct word-tokens as incidental variants of each other or a linguistic abstraction of one sort of another which subsumes synonyms or co-referring names as incidental variants of

each other.

As discussed in chapter 2, phonologists consider various allophonic word-tokens to instantiate or exemplify the same word-type. In fact, independent of debates concerning the basis on which word-tokens are classified as phonologically equivalent, linguists uniformly consider allophonic variants to be token instantiations of the same abstract word-type. Even "neo-Bloomfieldian" linguists committed to a strictly non-semantic account of phonemic classification still relied on the linguistic terminology of the phoneme—even though the phenomenon of phonemic categorization could, according to them, be accounted for in more basic physicalistic terms, be they acoustic, phonetic or perhaps behavioral. Everyone relied on at least the terminology of the phoneme, as the issue was not whether or not phonemes existed (at least until the advent of generative phonology, see notes 20, 50, and 58), but whether the "phoneme" was physicalistically reducible, semantically irreducible, or just some sort of linguistically convenient "theoretical fiction," as Twaddell (1935) once put it. Independent of how the phoneme should be defined, both semantic and non-semantic accounts of basic phonological equivalence consider phonetically distinct tokens as "instantiating" or "exemplifying" one and the same phonemic type.

But this intuitive practice of regarding word-tokens as realizing an abstract linguistic type sets up the challenge of actually devising some way of referring to these abstract phonemic entities—whatever their allophonic equivalence might ultimately be based upon. The purpose of this chapter, then, is not so much to review the philosophical significance or metaphysical implications of this common practice of viewing different word-tokens as instantiating the same abstract type. Rather the purpose of this chapter is

to explore how linguists actually manage to refer to abstract word-types. It is the notation phonologists use to refer to abstract word-types and the notation devised by Wilfrid Sellars to refer to the abstract linguistic type synonymous word-types instantiate which is of interest: phonologists use phonemic slash bars, e.g., /mat/, to refer to the abstract entity both [mat] and [mat^{h}] exemplify; Sellars uses "dot quotes", e.g., •attorney•, to refer to the abstract entity /attorney/ and /lawyer/ exemplify.

To be sure, philosophers prefer to be much more explicit in their metaphysical commitments. For philosophers, concerns related to the classification of linguistic expressions into abstract linguistic types tend to provoke general considerations regarding the ontological status of linguistic types as platonistic objects or "universals" of some sort. More often then not, however, contemporary philosophical discussions concerning linguistic substitutivity invoke the proprietary terminology of proposition, concept, analyticity, reference, meaning, and content—"semantic content" being that which can group even the most radically diverse word-tokens into a single equivalence class. It is not form that either synonyms or co-referring names might have in common, but clearly something else.

So whereas philosophers are asked to explain what "content" is, where it is to be found and how it is to be individuated, phonologists are inevitably asked to explain what "phonemes" (in contrast to individual phones) really are, where they are to be found, and how they are individuated. In both cases, however, linguistic-like objects seemingly, if not deliberately, distinct from those which are actually heard (or seen, etc.) during the course of a linguistic event are introduced. For whereas phonologists remind us that no one has actually ever heard or uttered a phoneme, philosophers are quick to observe that

we do not converse in linguistically disembodied content either.

Such observations quite naturally raise fundamental questions regarding the status of such abstract entities as semantic content and phonemes. In both cases, the "reality" of the phoneme and similarly, the reality of content, has played an important role in shaping more than a century of phonological theory (Bloomfield 1933; Hymes and Fought 1981; Jones 1957; Koerner 1978) and of course contemporary philosophy of language.

Yet linguists and philosophers are by no means alone in confronting some of the perplexities provoked by attempts to compare, categorize, and classify language which lead academic professionals to either detect or invent phonemic types or semantic content. Independent of debates surrounding how phonemes and semantic content are to be defined and accounted for, is the practical problem of realizing some technique by which to actually refer to such linguistic-like objects—whatever they may ultimately prove to be. And this concern is not merely academic, for even the most routine linguistic chore of reporting what someone has said inevitably raises issues which are not easily resolved without some way of actually referring to abstract linguistic objects.

When faced, for instance, with Galileo's famous declaration *Eppur si muove* and asked to state what was said, it would seem only two options, both deeply problematic, present themselves. One may attempt to state, *verbatim et literatim,* exactly what Galileo said, aiming to repeat *Eppur si muove* with maximum fidelity. Or equally, one may attempt to report Galileo said that the earth moves. And whereas the former may be credited with all the accuracy of a mechanical recording, such a response may very well prove to be more than just useless, but impertinent.

By contrast, the latter response pays little respect to the particular noises which happen to emanate from Galileo's mouth, but instead, offers an interpretation of Galileo's speech with the intent of specifying what Galileo meant. And there appears to be no stable compromise resolution to this rather mundane problem. Either what Galileo actually said is sacrificed in an attempt to convey his belief, namely that the earth does indeed move, or what he meant is potentially sacrificed in favor of merely parroting what was uttered. Worse, neither option is easily accomplished. For in order to accurately recreate Galileo's speech utterance, one must know exactly which features of the utterance are critical and which are merely incidental. Is it necessary to say it as loud as Galileo did in order to recreate what he said? What about his intonation or speed? The fundamental directive of Davidson's (1984) demonstrative account of quotation—"the expression with the shape here pictured" (90)—leaves such matters ill-defined. What counts as the same shape? If this strikes one as trivial, try recreating a totally foreign phonology or orthography, perhaps the Arabic: سلام . Like the hapless tailor who is asked to make another copy of a suit and reproduces the holes in the collar, anything less than absolutely perfect replication represents a potential liability to say something radically different, or even nothing at all. As Bennett (1988) reminds us,

> Where English is concerned, linguistic significance includes the respects that differentiate 'can' from 'con', but not those that differentiate a clumsily printed 'can' from an elegantly written one. There could be languages, however, where the difference between shapes like our printed 'a' and 'o' was a mere matter of handwriting, with no linguistic significance, while a shape like our handwritten 'a' represented a completely different letter. (404)

With respect to the diversity of natural language phonology, such cross-linguistic variation is commonplace. Language-specific allophonic variation occurs in the

phonology of all languages. Aspiration is phonemically distinctive in Hindi and Thai, for example, but not in English phonology.

Opting out of such misplaced precision, one may simply refer (via a "that-clause" in some preferred language), to the propositional content expressed by Galileo's statement, namely the proposition that the earth moves. But again, it is unclear exactly what qualifies as a re-expression of Galileo's words. For once the security of perfect duplication has been abandoned, the precise boundaries of interpretive discretion are notoriously elusive. Did Galileo mean that the third planet from the sun moves? Or that the earth moves very fast? Or that the earth spins on its axis along an elliptical path? Once again, our linguistic tailor is confronted with not knowing exactly what "counts" as the "same," for both the tasks of so-called direct quotation and reasonable interpretation demand that certain samenesses be preserved, while others may be ignored. To make matters worse, language-to-language translations, while seemingly a viable compromise solution between the needless specificity of "direct quotation" and the curiosity of "language-independent" propositional content, inherits both sorts of problems: first, content-equivalent words, phrases or sentences in the translating language must be chosen, then these words must be concretely realized in such a way as not to incidentally misrepresent those very words. Translation, like Davidson's notion of "samesaying," is a way to acknowledge an equivalence relationship between different words, phrases or sentences, but does not explain what the basis of this practice is. According to Davidson (1984), "samesaying" is a relationship between words uttered such that, for example, "'Galileo uttered a sentence that meant in his mouth what "The earth moves" means now in mine'" (104). Continuing, Davidson notes that

> We are indeed asked to make sense of a judgment of synonymy between utterances, but not as the foundation of a theory of language, merely as an unanalyzed part of the content of the familiar idiom of indirect discourse. The idea that underlies our awkward paraphrase is that of *samesaying*: when I say that Galileo said that the earth moves, I represent Galileo and myself as samesayers. (104)

(In a footnote added in 1982, Davidson claims that "The fact that an informal paraphrase of the predicate appeals to a relation of sameness of content as between utterances introduces no intentional entities or semantics," though he does acknowledge that "Some have regarded this as a form of cheating, but the policy is deliberate and principled" (104).)

In any event, it is the basis of our varied practices of somehow "recreating" what someone said—whether this be in the same language with the same words (via "direct quotation"), into another language with different words (via "translation" or Davidsonian "samesaying") or even into no language at all (but just pure propositional thought)—that is of interest. And while no problems typically arise when attempting to repeat, translate, interpret, or "samesay" speech, such conversational ease should not be mistaken as an indication that the principles for making these intuitive moves are well understood. Perhaps effective communication does not require more than an intuitive grasp of these constraints and perhaps, as Quine suggests, it is philosophically naïve to demand more. For even when direct quotation, translation, and propositional that-clause interpretation do succeed in samesaying, the first may still be criticized for the dumb-witted repetition of mere parroting, the second for reliance on a "translation manual," of one sort or another, and the third for leaving us slightly bewildered as to exactly what propositional content is. As Carnap (1947) once remarked in connection with indirect quotation and the propositional attitudes, "Although sentences of this kind seem to be quite clear and

unproblematic at first glance and are, indeed, used and understood in everyday life without any difficulty, they have proved very puzzling to logicians who have tried to analyze them. . . ." (53).

A Philosophical Response: Nominalism

Indeed, philosophers of language have long struggled to more fully understand our practices of indirect discourse and translation and in the process, clarify what it is that makes two different expressions "the same," and square a tight semantic circle of the closely inter-related notions of analyticity, meaning, synonymy and propositional content. But at least for those who are, as a matter of philosophical principle, nominalistically reluctant to admit abstract objects such as propositions and meaning into their ontology, the choice is clear: Better to say nothing about the putative equivalence between *Eppur si muove* and *The earth moves*, and synonyms and translations generally, than something misleadingly unscientific. Postulating propositions and semantic content as the glue which somehow binds the apparent equivalence between translations and synonyms is a philosophical solution that is, as Quine argued, suspiciously over-designed for the problem. At best, "propositions" and "meanings" are merely labels which succeed only in papering over a problem. At worst, talk of "propositions" and "meanings" perpetuates a non-existent pseudo-problem: "Pending a satisfactory explanation of the notion of meaning, linguists in semantic fields are" quips Quine (1953a, 47), "in the situation of not knowing what they are talking about."

Such is our Quinean legacy. Skeptical of abstract entities, but equally confident that no comparison of the actual physical appearance of synonymous expressions yields a

single "natural kind" category that subsumes them both, categorizing them as instances of a single, but inherently unspeakable, abstract object is scientifically unjustifiable and arguably incoherent. Asking synonymy, or even the more technical notions of "intensional isomorphism" (Carnap 1947) or "synonymous isomorphism" (Church 1954) to prop up intuitions of linguistic equivalence are, according to skeptics, misguided attempts which merely substitute, as Quine would say, one "creature of darkness" with still others.

The Linguistic Response: The Phoneme

But questions of linguistic equivalence attract more than just philosophers. Specifying how two phonetically distinct word-tokens may differ but still exemplify the same word-type is among the professional pursuits of phonologists. After all, from a strictly phonetic point of view, it is no more technically accurate to identify two token pronunciations of the same word—one produced with aspiration and one without—as the "same" than to identify synonymous expressions as the "same." Yet there is also no denying that aspiration is phonemically insignificant in English anymore than it is possible to deny that such "incidental" aspiration is phonemically distinctive in other languages like Hindi and Thai. Indeed, what English speakers take to be an obviously undeniable truth, namely that aspiration does not change one word into another word is just as confidently, and correctly, denied by many others.

For that matter, any attempt to equate two linguistic forms which differ merely in aspiration would immediately be greeted by any competent speaker of Hindi or Thai with as much skepticism as that which has philosophically plagued the analytic identification

of synonyms and translations. Given the nature of their language-specific phonological constraints, an effort to convince competent monolingual speakers of Hindi that one word-token produced with aspiration and another produced without aspiration were some how substitutionally equivalent mimics the skeptical challenge confronted by philosophers who wish to treat tokens of synonyms as substitutionally equivalent. Both represent attempts to equate word-tokens which have already been implicitly recognized as phonemically distinct: in Hindi, aspiration is phonemically distinctive distinguishing one word-type from another; in English (as in other languages), synonymous and co-referring words are phonemically distinct word-types.

Fortunately, phonologists have a solution to this paradox of how two different things can really be the same—how phonetically distinct linguistic specimen can nevertheless still be strictly equivalent. But it is a solution that is also applicable to synonymous word-types (and co-referring names too). If phonologists can somehow represent distinct linguistic specimen as the same, then maybe there is a way to represent how synonymous (and even co-referring names) can be distinct word-types, but no less substitutionally equivalent. In fact, the generality of this solution is matched only by its simplicity.

The linguist's solution to this problem depends upon recognizing two distinct levels of phonological analysis: the phonetic and the phonemic. An aspirated stop consonant and an non-aspirated stop consonant may very well be the "same" when it comes to their phonological status as the same phoneme in some language, but they are no less phonetically distinct. Yet in order to more clearly express how one 't' is not the same as another 't' even though they are both 't's—and avoid at least the appearance of

contradiction—some way of referring to an aspirated 't' which differs from a non-aspirated 't' is sorely needed.

This is why phonologists resort to phonetic brackets and the special symbols of the International Phonetic Alphabet to refer to distinct allophones of a single phoneme. In this way, the aspirated phone 't' is uniquely referred to as [t^h] while the non-aspirated phone is uniquely referred to as [t]. Now it can be clearly stated without fear of contradiction that [t^h] and [t] are distinct, for [t^h] and [t] are, by force of notational forethought, distinct. Yet what about the equally important objective of representing the phonemic equivalence of [t^h] and [t] in a language like English? Now that [t^h] and [t] have been distinguished, the very possibility of their equivalence seems to have been blocked, for [t^h] and [t] are, as intended, now distinct and obviously not the same.

Of course the solution to this problem is to contrast phonetic brackets with phonemic slash bars. 't' aspirated and 't' non-aspirated are distinct when uniquely represented as [t^h] and [t], respectively, but clearly the same when uniformly represented as /t/, for /t/ is, as intended, identical to /t/.[89] Equating /t/ with /t/ is trivially true. (The choice of /t/ rather than /t^h/ to represent the abstract phoneme both aspirated [t^h] and non-aspirated [t] instantiate is notationally arbitrary. The phoneme could have equally been represented as /t^h/, though linguists generally choose the most frequent allophonic variant. In cases of "fully" free allophonic variation, one phone is arbitrarily chosen to canonically represent the phonemic norm, see Pike 1947a.) In this way, the claim that aspirated 't' and non-aspirated 't' are the same for some natural language is expressing by saying that [t^h] and [t] are both /t/: [t^h] and [t] are "tokens" of the "type" /t/.

There is, however, an alternative way of notating phonemic word-types which preserves their derivational history. For instance, the phonemic representation of the aspirated phone could be notationally expressed as /[t^h]/ and the non-aspirated variant as /[t]/. In this way, [t] and [t^h] would be phonemically equated in terms of the equation /[t]/ = /[t^h]/. Linguists do not use this alternative technique (nor does Sellars when constructing "dot quote"-types, see below). The purpose of phonemic slash bars is to represent phonetic tokens as incidental variants of the same abstract phonemic type. This multiply embedded notation, while preserving derivational history, fails to depict how variants at the level of word-tokens are exemplars of one and the same abstract type. If the phonetically distinct [mat] and [mat^h] were to be phonemically represented as /[mat]/ and /[mat^h]/, respectively, rather than uniformly as /mat/ (or /mat^h/), the claim that /[mat]/ and /[mat^h]/ really are the same (e.g., /mat/ or /mat^h /) is no longer notationally obvious (as their phonetic differences have been retained within the phonemic notation). But the purpose of phonemic slash bars (as well as Wilfrid Sellars' "dot quotes") is to notationally suppress these differences so as to express or "reveal" how the variants are more than just equivalent, but actually one and the same.

Either way, the introduction of notational devices like phonetic brackets and phonemic slash bars allows for linguists to express how distinct linguistic specimen can be, at one and the same time, both the same and different. For that matter, the clarity of any theoretical move towards multiple levels of linguistic description critically depends on the introduction of multiple devices by which to refer to speech, new devices in fact, by which to quote speech at various levels of detail, whether that be the micro-phonetic

devices of dialecticians, the phonetic brackets of phoneticians, or the phonemic slash bars of the phonemicist, or, as we shall see, the stars and dots used by Wilfrid Sellars.

In a sense, phonetic brackets and phonemic slash bars are really specialized *quotation marks*: phonetic brackets serve to "narrowly" quote speech in terms of a language-general inventory of phones, while phonemic slash bars serve to "broadly" quote speech in terms of only those language-specific phonetic distinctions necessary to distinguish one word from another. With the assistance of these specialized quotation marks, the question of how two occurrences of exactly the *same* word can be different can at least be stated without contradiction, and the question of how *two* distinct linguistic forms can nevertheless be strictly equivalent can be asked without precluding an answer. Indeed, without the leverage afforded by a notational system structurally comparable to phonetic brackets and phonemic slash bars, it is unclear how the admittedly paradoxical claim that *two distinct* word-types (be they synonyms or co-referring names) are substitutionally equivalent—the crux, I think, of Frege's puzzle, Mates's argument, as well as the "paradox of analysis" (see Sellars 1950b, and below)—might be satisfactorily stated, no less resolved.

The practical advantages of a notational system allowing for multiple levels of quotation should not be underestimated. For someone, a native speaker of Hindi for instance, trying to appreciate how two distinct words obviously differing in aspiration could be equated, phonemic slash bars provide the notational resources to indicate the possibility of their equality, namely [mat^{h}] aspirated and [mat] non-aspirated are both /mat/. Without someway of uniformly referring to both [mat^{h}] aspirated and [mat] non-aspirated, the possibility of expressing their identity is moot.

Nevertheless, the actual deployment of such specialized quotation marks will inevitably impose a classification of linguistic forms which will be profoundly counter-intuitive. For recall that while phonologists detail a variety of foreign contrasts strangely native to our own language—explaining how the [t^h] in /top/ is actually distinct from the [t] in /stop/—phonologists are also busy challenging other seemingly obvious truths by dissolving contrasts which appear to be indubitably genuine. According to the phonologist, [lot] and [rot], incredible as it may seem (to non-Japanese speakers), are one and the same word-type.

In short, brackets and slash bars allow for the expression of true but counter-intuitive and otherwise inexpressible facts, for example that 't' is not equivalent to 't' (i.e., [t] is not the same as [t^h]) and that 'l' is the same as 'r' (i.e., [l] and [r] are phonemically equivalent in some languages). By denying the equivalence of almost imperceptibly distinct items, or by endorsing highly unusual equivalencies, brackets allow overlooked phonetic details to be signified, while slash bars allow insignificant phonetic details to be overlooked. Indeed the violence foreign phonemic categories can inflict upon native phonological confidence is matched only by the violence traditional analytic equivalencies exact upon those very same phonological intuitions.

Consider that while linguists might be naïvely accused of phonological perversion for distinguishing between /mat/ aspirated and /mat/ non-aspirated, or for equating [lot] and [rot], philosophers have been brought to task for equating tokens of /attorney/ with /lawyer/ or tokens of /Cicero/ with /Tully/. But if tokens of /mat/ and tokens of /mat/ can be different, and tokens like [lot] and [rot] can be the same, then perhaps /attorney/ can, for all that, be strictly equivalent to /lawyer/, and /Cicero/ with /Tully/, notwithstanding

the fact that synonyms are different words and co-referring names different names. If phonetically distinct tokens can be phonemically equivalent and notationally represented as identical with the help of slash bars, perhaps phonemically distinct word-types can be notationally represented as identical.

If only there were some way of uniquely referring to two phonemically distinct word-types—some way of uniformly "quoting" synonymous pairs—in the way incidental variation among [mat^{h}] and [mat] is impartially quoted as /mat/.[90] In this way, the phonemic difference between synonymous words would be no more relevant to their status as exemplifying the "same abstract type" than the phonetic differences between [mat^{h}] and [mat] block their identification as the same abstract phonemic type. As Hutton (1990) observes, there is a structural parallel that can be constructed between allophonic word-token variation and synonymous word-type variation:

> We can draw an analogy between phonetic variation on the level of word as corresponding to 'stylistic' variation on the level of sentence and between synonymy on the level of word and the fact that two sentences can each express the same proposition. (36)

The problem with this analogy is that while [mat] and [mat^{h}] are both exemplars of /mat/, what is it that tokens of both /attorney/ and /lawyer/ are "incidental" variants of?

But in addition to phonetic brackets and phonemic slash bars by which to narrowly and widely "quote" speech, we also have Sellarsian "stars" and "dots." Sellars' "dot quotation" affords precisely the kind of notational resources to represent the identity of phonemically distinct word-types. Indeed, Sellars' notationally-inspired solution to the problem of equating synonyms, and an analysis of abstract linguistic types generally, is structurally identical to that devised by phonologists.

The Sellarsian Response: Dot Quotes

Sellars' contribution to the analysis and regimentation of linguistic expressions comes in the form of his "star" and better known "dot" quotes. On the one hand, Sellars' star quotes are intended to classify word-tokens purely in terms of their objective physical similarities—what Sellars (1950b) calls their intrinsic "design features." For while "two linguistic events resemble one another," Sellars realizes that they will typically not "resemble one another perfectly":

> That is to say, suppose two linguistic events differ in some respect other than their space-time position as written by a schoolboy and the 'same' word written by a professor of penmanship. (1950b, 25)

For cases like these, Sellars introduces his notation of "star quotes" to indicate that even perceptually distinct word-tokens can be represented as exemplars of the same "token-class" denoted with the help of "star quotes". So while [bachelor] and [BACHELOR] are distinct word-tokens, they may be identified as token variants of one and the same ("star quote") *bachelor*. (As with phonemic slash bars, one word-token variant is arbitrarily chosen to represent the "star quote" token-class. Indeed, Sellasian star quotes are phonemic slash bars by a different name: star quotes are designed to capture the basic orthographic/phonological equivalencies of a language.)

But while star quote token classes are—like neo-Bloomfieldian phonemic word-types—ideally constructed exclusively through the comparison of perceptually definable features, Sellars (1950b) seems to acknowledge that even these basic "token-class" categories are unlikely to be entirely free of interpretive considerations. In fact, when turning his attention toward a spoken word and its visual equivalent, Sellars (1950b) asks: "Can we take care of this sameness by extending the scope of the resemblance families

we have called token-classes so that they can include auditory as well as visual configurations?" (25). But rather than responding in the affirmative or negative, Sellars moves to reconsider his initial reliance on the notion of similarity, pointing out that "similarities" and "family resemblances"—far from being necessary for visual and auditory equivalence—are not even an essential feature for establishing basic "star quote" token-class (phonemic) equivalence:

> Over and above these modes of resemblance there is the similarity of the roles played in the linguistic economy by the visual token-class and the auditory token-class for the 'same' word. In short, that which is common to a word as written and the same word as spoken is a linguistic role, a role which can be performed by members of more than one token-class. *As a matter of fact we have really known all along that a linguistic token is not linguistic merely by virtue of the intrinsic characteristics (color, shape, melody) which it is observed to have. It must belong to a class of events which is associated with a linguistic role. Thus the concept of a linguistic role, which we have been adumbrating, is presupposed by the concept of token-class, although the latter came more easily to our attention.* (25, italics added)

So while one might naively hope that at least basic "token-class" orthographic/phonemic equivalencies are underwritten by intrinsic perceptual features, it is obvious this will not work for the "same word" as pronounced in speech and written down on paper. But Sellars seems to realize that "intrinsic characteristics" say, of shape, do not even define "token-classes" in the same modality. And it is this realization that helps to propel Sellars to devise dot quotes, for perceptual analysis of will yield a well-formed category among variants of the same "token-class."

In any event, perceptually-based "token-class" categories built from intrinsic "design features" of shape and sound are not, according to Sellars, the only classificatory scheme for linguistic specimen. Sellars also has "dot quotes." Insofar as abstract linguistic types may also be individuated by the purely functional, as opposed to the merely perceptual, ("star quote") *bachelor* and *unmarried man* could be members of

a single even more abstract linguistic type, a type Sellars refers to as a "dot quote-type. In this way, just as 'bachelor' and 'BACHELOR' are exemplars of *bachelor*, *bachelor* and *unmarried man* would be exemplars of •bachelor•. (Using brackets and slash bars, instead of single quotes and star quotes, [bachelor] and [BACHELOR] would be exemplars of /bachelor/ and /bachelor/ and /unmarried man/ would be exemplars of •bachelor•.)

In "The Identity of Linguistic Expressions" Sellars (1950b) illustrates this use of star and dot quotation by explaining that "while the claim *or* is a token-class of •or• would be a tautology . . . since one and the same type can be embodied by several token-classes, *V* [the logical symbol for disjunction] is a token-class of •or• could also be true" (29). In this passage, Sellars is pointing out how more than just *or* is a member of •or•; any number of token-classes can be members of the "dot quote"-type represented by •or•. *or * and *V* are just two ways of realizing the •or•.

In this way, *bachelor* and *unmarried man* may not look or sound alike (any more than *or* and the symbol *V* do) but that is irrelevant to their status as the same linguistic "dot quote"-type. What is important, argues Sellars—echoing the phonemic functionalism of the Prague Linguistic Circle (Vachek 1966)—is how they are (properly) used, and in this respect, *bachelor* and *unmarried man* are strictly equivalent. As Rosenthal (1968) explains, "To specify that a certain linguistic expression is an instance of the expression type '---' is to specify it, at least in part, as a certain sign design in terms of some determinate factual character, that is in terms of its sign-design-characteristics. To specify a linguistic expression as an instance of a •---• however, is to specify it in terms of its intentional properties alone" (152).

Here is how Sellars (1964a) in "The Paradox of Analysis: A Neo-Fregean Approach" explains his notation of dot quotes:

> The convention for dot quotes differs from that for ordinary quotes in that whereas "father" is a common noun which is used to refer to instances of the design *father* as playing in English a role which could be played by instances of other designs, •father• is a common noun which is applicable to items which play this role whatever their design may be and to whatever language they may belong. (302)

And in the same article, Sellars writes:

> Now concepts, as I see, it, are linguistic objects in the sense in which the various pieces involved in the game of chess (e.g., the pawns) are chess objects. Thus, just as the pawn can be realized in material objects of different shapes, sizes and composition, so the concept *Father* can be realized in different linguistic materials or sign features. From this point of view, the German sign design *Vater*, the French sign design *Pere*, and the English sign design *Father* are all realizations of the concept *Father* in different materials. (301)

And yet while detailing a version of linguistic typing which transcends natural language phonology, Sellars is not, strictly speaking, alone. Much the same had already been philosophically outlined by MacIver (1937a), (1937b) and (1937c) during an extended dialogue with MacDonald (1936), (1937a), (1937b) in the pages of *Analysis.* Moreover, Uldall (1944)—a Danish linguist associated with the Copenhagen Linguistic Circle of Glossematics pioneered by Louis Hjelmslev—linguistically proposed such a devise. He called in a "cenia."[91]

This is how MacIver (1937a) explains his position on the phonemic typing of phonetically distinct word-tokens, quoting at some length:

> I think that, to get a clear account of what is meant by "identity of type," we must draw a distinction between what I shall call "*formal* type" and "*semantic* type." Tokens are "of the same formal type," if they diverge qualitatively by not more than a certain amount from a certain qualitative norm. (The norm and the permitted limits of divergence both depend on the particular language in question, so that tokens which are of the same formal type in one language may be of different formal types in another: this is recognized in what philologists know as "the theory of the phoneme.") . . . Tokens are not "of the same *semantic* type" unless they have the same *semantic value.* By the "semantic

value" of a token-sentence I mean its *meaning*—by the semantic value of a token-word (or sound or letter), the contribution which it makes to the meaning of the sentence in which it occurs. (61)

. . . I suggest that, when we say that "two tokens are the same type," we mean that two token-words are *both* of the same formal [phonological] type *and* of the same semantic type. . . . Where we have tokens of the same formal type but not of the same semantic type, we have homonyms; where we have tokens of the same semantic type but not of the same formal type, we have synonyms; but neither a pair of homonyms nor a pair of synonyms are (according to ordinary usage) "the same word." But where we have tokens that are both of the same formal type and of the same semantic type (that are, so to speak, at once homonyms and synonyms), there we do say that we have "the same word twice over." (62)

"But I am ready to go further," declares MacIver, and this is where the structural parallel between phonemic word-typing and Sellarsian dot quote typing (what MacIver calls the *archetype* of "semantic typing") is revealed most clearly:

. . . and suggest that all that is directly important for the philosophical theory of language is identity of semantic type; or, on other words, that, if "type" is to be considered purely as a technical term of linguistic philosophy (which, after all, it is), it would really be most convenient to define it in such a way that all synonyms would be called "of the same type" (e.g., *quickly* and *rapidly*). This would be to turn the tables on Miss MacDonald and Mr. Hardie completely. It is by no means as paradoxical as it sounds. The philosophical theory of language is concerned with the way in which language functions as a symbolism—that is (in I. A. Richards' terminology) with the "scientific" use of language, not with its "emotive" use. But the difference in use between synonyms (if they are genuine synonyms) is a difference purely in "emotive" use—it is stylistic, not logical; as far as the "scientific" use of language is concerned, we might equally well use either—they are exactly equivalent. In other words, synonyms are identical for the purposes with which the philosophical theory of language is concerned. But the fact that two tokens are identical in the uses with which we are concerned is just what we are trying to express by saying that they are "of the same type. (63-64)[92]

Identical in structure then, to the claim that, despite their more or less obvious phonetic differences, [mat^{h}] and [mat] are both exemplars of /mat/, *bachelor* and *unmarried man* are both exemplars of •bachelor•—despite their obvious phonological differences. So while it would be inaccurate to quote someone who said *male parent* as

saying *father*, it is perfectly accurate to report •father• was said, in precisely the same way it is accurate to record a [mat^{h}] as /mat/.

In this way, Sellars' dot quotes do for different phonological word-types what his star quotes (and slash bars) have been doing for different phonetic word-tokens: /attorney/ and /lawyer/ are substitutionally equivalent as both are simply word-type variants of the same dot quote type •attorney•. Equipped with the notational leverage afforded by slash bars and dot quotes, neither the phonologist nor the semanticist must suffer the perpetual criticism that no one actually speaks in phonemes or linguistically disembodied meanings. With the appropriate notation, /mat/ and •attorney• are concretely realizable transcriptions of speech.

The following chart may be useful. There are three levels of linguistic analysis, a "phonetic" representation, a "phonemic" representation, and a "dot quote" representation:

PHONETIC REPRESENTATION (4 word-tokens)	**PHONEMIC REPRESENTATION (2 word-types)**	**DOT QUOTE (1 dot quote type)**
a. [ATTORNEY]		
	e. /attorney/	
b. [*attorney*]		
		g. •attorney•
c. [LAWYER]		
	f. /lawyer/	
d. [*lawyer*]		

Phonemic representations abstract away from allophonic/allographic variation; dot quote representations abstract away from synonymous variation. So while slash bars are blind to allophonic variation, dot quotes are blind to synonymous variation. From the perspective of dot quotation it does not matter which of any of the *four* word-tokens are actually used; they will all instantiate the same dot quote type. Another way of putting

this is: given that •s• is a dot quote sentence-type, word-type /w/ is intersubstitutable with word-type /w'/ iff, for any sentence-type /s/ of •s•, the result of replacing word-token [*w*] of word-type /w/ by word-token [*w'*] of word-type /w'/ yields another token of •s•. That is to say, the word-type /attorney/ is intersubstitutable with word-type /lawyer/ iff, for any sentence-type /s/ of •s•, the result of replacing word-token [*attorney*] of word-type /attorney/ by word-token [*lawyer*] of word-type /lawyer/ yields another token of •s•.

Dot quotes are for distinct word-types what slash bars (and star quotes) are for distinct word-tokens. And whereas the phoneticist may be heard complaining that lay people are generally guilty, as it were, of not listening closely enough to the actual phonetics of speech, looking at this chart, Sellars can be understood as complaining that philosophers of language have been over-sensitive to "star quote" token-class phonological/orthographic word-type equivalencies, when what is really important is their dot quote form.

In a sense, Sellars argues that philosophers of language should not be exclusively concerned with the phonemic word-type differences between synonyms anymore than phonologists should be exclusively concerned with word-token allophonic differences. As Bloch and Trager (1942) warn us, no matter how phonetically accurate, strictly phonetic transcriptions can be severely misleading:

> granting that [the transcriber is] not guilty of such omissions, and assuming that his transcription reflects every detail of the actual utterances as faithfully as anything can, short of a mechanical reproduction, a purely phonetic description of the language is still inferior to a description in phonemic terms. It may or may not err in telling us too little; but it is quite certain to err in telling us too much. Instead of giving us a clear picture of the language, it complicates the vocabulary and obscures the grammar with a profusion of incidental and irrelevant particulars, significant of nothing but the acuteness of writers' ear. (39)

Similarly, Daniel Jones (1957)—a prominent exponent of the phoneme in early twentieth-century linguistics—would argue:

> The phonetic analysis of a language does not end with an accurate description of the sounds and a notation of them by a narrow system of transcription. It is necessary further to discover how the sounds are grouped into phonemes, and in consequence how to convert the 'narrow' transcription into a 'broad' one showing the true linguistic structure of the language" (reprinted in Jones & Laver 1973, 104-105).

One could not ask for a better description of what I think Sellars was trying to impress upon his philosophical colleagues. Synonymous word pairs (but by extension co-referring names, see below and chapter 10) are "narrowly", that is phonemically, distinct but "broadly," that is dot quote equivalent. In fact, in virtue of being "star quote" exemplars of the single dot quote, *bachelor* and *unmarried man* are substitutionally equivalent, no less than [bachelor] and [*bachelor*] are substitutionally equivalent, an insight which eventually leads Sellars (1950b) to remark in "The Identity of Linguistic Expressions and the Paradox of Analysis" that "It is along these lines that the so-called paradox of analysis is to be resolved" (31): when represented in their dot quote form, the terms flanking the identity sign are the same, stalling the paradox before it can even get started.

Equipped with the notation of dot quotation, asking how equating *bachelor* and *unmarried man* can be informative is like asking how equating [bachelor] and [*bachelor*] can be informative. But when [bachelor] is represented as a token exemplar of /bachelor/, and [*bachelor*] also represented as another token exemplar of /bachelor/, there is no paradox to resolve: [bachelor] and [*bachelor*] are the same word-type. [bachelor] and [*bachelor*] are both exemplars of /bachelor/, so their equivalence—at the level of word-types—is entirely trivial: /bachelor/ and /bachelor/ are obviously the same

word-type. Similarly, when *bachelor* is represented as an exemplar of •bachelor•, and *unmarried man* represented as another exemplar of •bachelor•, there is no paradox to resolve: *bachelor* and *unmarried man* are the same "dot quote"-type. *bachelor* and *unmarried man* are both exemplars of •bachelor•, so their equivalence—at the level of "dot quotation"—is entirely trivial: •bachelor• and •bachelor• are obviously the same "dot quote"-type.

Note, however, that Sellars' dot quote notation, does not prove the intersubstitutability of synonyms anymore than the phonologist's slash bar notation proves the intersubstitutability of allophones. The purpose of such specialized notation is different. These specialized ways of representing words—special ways of quoting speech—allow the assertion that synonyms and allophones are the "same" to be expressed without tripping over the obviously false claim that 'attorney' and 'lawyer' are tokens of the same word-type, or that 'attorney' and 'ATTORNEY' are the same word-token. 'attorney' and 'lawyer' are not tokens of the same word-type and 'attorney' and 'ATTORNEY' are not the same word-token.

Instead, the notation of dot quotes, like that of slash bars, merely allows for the concretized expression of linguistic identities otherwise inexpressible. Slash bars provide the resources to notationally capitalize on the claim that allophones are really "the same" thing. Similarly, dot quotation provides the resources to notationally capitalize on the claim that synonyms are really "the same" too. In fact synonyms become the same "dot quote" like allophonic variants are the same "slash bar." But neither the notation of Sellarsian dot quotes nor the phonological notation of phonemic slash bars prove anything. Nor is such notation capable of further substantiating the claim that allophonic

variants are phonologically equivalent or synonyms (and co-referring names) are dot quote equivalent. Both dot quotes and slash bars are notational maneuvers designed to highlight how word-tokens can be viewed as incidental variants of each other at multiple levels of abstraction: allophonic variants are "slash bar" equivalent and synonyms are "dot quote" equivalent. In particular, dot quotes are useful in defusing the claim that merely because synonyms (and by extension, co-referring names, see below and chapter 10) are different word-types that synonyms (and co-referring names) do not, or cannot, substitute freely. [bachelor] and [BACHELOR] are different—different word-tokens—but they still substitute freely. Phonemic slash bars notate how—not why—this is possible. Similarly, dot quotes can be used to notate how—but do not explain why—even distinct word-types can freely substitute. Phonological slash bars and Sellarsian "dot quotation" merely give notational expression to an equivalence relationship among allophones on the one hand and synonymous expressions on the other. Phonological slash bars allow for various word-tokens to be categorized as incidental variants of the same "slash bar" and Sellarsian "dot quotation" allow for synonyms to be categorized as incidental variants of the same "dot quote."

But the real question is: why should various word-tokens be categorized as incidental variants of the same "slash bar"? And why should various word-types be categorized as incidental variants of the same "dot quote"? Given the failure of phonemic reductionism, semantic equivalence is at least part of the "slash bar" equivalence story. And given that Sellars' dot quotes are designed to highlight an equivalence relation among synonyms, "dot quotation" is almost transparently conditioned by semantics. Neither slash bars nor dot quotes advance any argument: they

only demonstrate how certain substitutions can be notationally guaranteed. Backed up by an account of what actually underwrites basic phonological equivalence, however, the counter-intuitive status of "dot quotation" can be assimilated to the intuitive natural language status of basic phonemic "slash bar" equivalence. Dot quotes are (or at least appear) to be an abstract construction obviously conditioned by semantic content. Basic word-types are also an abstract construction conditioned by semantic content (though this is less obvious). Dot quotes complete this semantic-notational cycle. Dot quotes and slash bars notationally highlight the structural parallels between semantic-based phonology and semantic-based synonymy.

To be sure, Sellars tell us that his classificatory standard for dot quotation is "functional," not semantic in nature, and at that, only applied to synonyms and translations, not co-referring names. But whereas in chapter 7, perceptual and behavioral versions of phonemic reductionism were criticized, the provision of a *functional* proxy for synonymy is critically assessed in chapter 10. In particular, Sellars' functional account of synonymy is challenged by asking whether this functional account is "individualistically" descriptive or "anti-individualistically" prescriptive in nature. Is Sellars' dot quote "functional equivalence" sensitive to normative concerns or is it purely descriptive? On the one hand, if Sellars' account of dot quote synonymy is to be understood in descriptive terms, it is by no means obvious that synonyms will turn out to be "functionally" equivalent given that there are at least some people (particularly if Mates 1950 is to be believed) who will inevitably fail to treat synonyms as "functionally" equivalent. Descriptive functional equivalence is too easy to falsify to be philosophically compelling.

On the other hand, if Sellars' account of dot quote synonymy is to be understood in normative terms governing *proper* usage, than it is by no means obvious that co-referring names will not also count as "functionally" equivalent, even though there will almost inevitably be those who do not correctly treat, say, /Cicero/ and /Tully/, as "functionally" equivalent either. If /attorney/ and /lawyer/ can both be •attorney•—independent of deviations in actual usage—then it's unclear why co-referring names are not also be dot quote equivalent, again independent of deviations in actual usage.

But you can't have it both ways: either the criterion for dot quotation is individualistically descriptive in which case even synonyms will not come out as "functionally" equivalent or the criterion is anti-individualistically prescriptive in which case even co-referring names will come out as "functionally" equivalent. Such is the dilemma addressed in the next chapter. Insofar as Sellars' functional account of dot quotation aims to portray how expressions should be used (rather than how they may actually be incorrectly used), the fact that some substitutions fail for some people is not relevant to their status as substitutionally equivalent. But co-referring names are not to be distinguished from synonyms in this regard. Names are also subject to prescriptive norms just as much as the proper use of predicates are.

So much for the end game. But whether or not dot quotation should be enlarged to cover co-referring names, the significance of Sellars' "dot quote"-types should be recognized to be on a par with the linguistic innovation of phonemic slash bars. Both involve linguistic abstractions by which to categorize a diversity of linguistic forms: slash bars (and Sellars' star quotes) for word-token equivalencies and dot quotes for word-type equivalencies. As Love (1998) points out with respect to some five variants of the single

word-type /butter/ (identified variously as British 'received pronunciation,' Canadian and New York upper-middle-class, London working class, west-country British, and New York working class):

> Now a crucial fact about the abstraction underlying these five variants of butter is that in spoken English it has no name. . . . The only way to identify a word (or any other linguistic unit) in speech is to produce a particular phonetic realization of it. *There is no superordinate pronunciation which is the pronunciation of the word itself, as distinct from one of its phonetic variants.* What ties the five forms together—indeed, what makes it possible to see them as different versions of a single entity—is the fact that written English contains the invariant butter. A stable and consistent analysis of utterance-tokens in terms of the types that they instantiate would be impossible without the assistance afforded by writing in the form of a phonetically neutral notation with which to identify types. (108, italics added) [93]

I suspect Sellars perceived the value of his dot quote types in a similar vein. Dot quotes allow synonyms to be seen as different versions of a single invariant dot quote entity: /attorney/ and /lawyer/ (or using Sellars' star notation *attorney* and *lawyer*) are incidental variants of one and the same "dot quote"-type •attorney•, structurally analogous to the way [mat] and [mat^{h}] are phonologically equivalent token variants of one and the same word-type /mat/.

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[89] Daniel Jones (1957) credits Bernard Bloch with inventing phonemic slash bars to represent "broad" transcriptions of speech. They first appear in print in Trager & Bloch (1941).

[90] For speculation as to why there is no such single conventional linguistic form, see Sellars (1967). After imaging a giant game of chess played "Texas style" with vehicles for pieces, Sellars asks "The challenge and the question can be summed up by asking: Granted that *dreieckig*s (in German) and *triangular*s (in English) stand for triangularity can be compared to Volkwagens (in Texas Chess) and the familiarly shaped pieces of material (in conventional chess) embody the pawn. Why is there no common noun in actual usage such that, representing it by 'R,' we can say *dreieckig*s (in German) and *triangular*s (in English) are Rs as we can say Volkwagens (in Texas Chess) and the familiarly shaped pieces of material (in conventional chess) are pawns?" (240).

[91] Uldall (1944) coins the term "cenia" to refer to functionally defined categories of inter-language linguistic structure: "Units of expression belonging to different systems unite into groups analogous to the groups of sounds which we call phonemes, and defined by all the members of one group being functions of the same unit of content. A group of this kind I shall call a *cenia*. Thus the speech-chain *kat* and the written chain 'cat' belong to the same cenia, because they can be exchanged without a change in meaning, being functions of the same unit of content. But to the same cenia will belong also, it will be seen, any other unit from any other system of expression, if it is a function of the same unit of content" (150; as reprinted in Hamp et al. 1966).

[92] These paragraphs were intended to clarify earlier remarks which led an exasperated MacDonald (1937a) to assert that (1) "no ordinary person would think of saying that *quick* or *rapidly* are 'the same word'"; (2) "whether tokens are of the same word or not is determined by comparing tokens and not by reference to meanings. . . . A type-word may perhaps be not too misleadingly described as a logical construction out of token words related by physical similarity . . ."; and (3) "Mr. MacIver has not shown that the type-token distinction involves reference to meaning." (78-79).

In response to MacDonald, MacIver (1937b) explains: (1') "I agree, of course, as to the first (I never said that *quickly* and *rapidly* were 'the same word' but only that it might be convenient to describe them as 'of the same type')" (83); (2') reiterates, again, that similarity is too weak to determine natural language word-types; and (3') "if we now ask what determines this degree of similarity, the answer involves reference to meaning. Two tokens are 'of the same type' in a given language (for as pointed out (p. 61), tokens may be of the same formal type in one language which would be of different formal types in another), if they are alike to a degree such that any tokens within that degree of similarity would, in the same circumstances, have the same semantic value (contribute in the same way to the meaning of the sentence in which they occurred) in that language" (84-85).

Hutton (1990) provides an excellent summary of the MacDonald-MacIver *Analysis* debate, but fails to give MacIver full credit for debunking "phonemic reductionism": "MacIver had argued from the beginning that formal [that is "physical" similarity] criteria alone were insufficient. His problems arose from the difficulty of defining meaning, *and from a failure to grasp that semantic notions underlie not only the synonym class (the 'archetype') but also the establishment of the equivalence-class for a type in the conventional [phonological] sense, upon which the notion of archetype is built*" (43; italics added). But MacIver (1937b) clearly appreciates that *basic* phonological equivalence depends upon semantics; see pages 61, 84-85 reproduced as (3') in the previous paragraph.

[93] Nigel Love has suggested that written representations of language are more than just a linguistic convenience affording durable speech. Love detects a significant effect on our conceptualization of language itself, arguing that an independent (e.g., visual) vehicle for representing speech permits (forces?) speech to be conceived as essentially abstract and allows for "meta-linguistic" discourse to proceed more efficiently:

> So long as language is purely spoken language, there is no possibility of [neutral community-wide abstraction], partly because purely spoken language is purely context-bound language, and partly because purely spoken language cannot yield the consistently reliable metalinguistic discourse required for articulating it. The crucial difficulty when generalizing orally about linguistic experience is concerned is that we have nothing to do it in except utterances themselves. So how do we make clear that some of our utterances are to be taken as utterances, whereas others are to be taken as the names of something more abstract than utterances? . . . But systematic talk about [utterances] requires a radically new development. That new development was the use of writing as a linguistic medium. . . . The notion that a non-phonic medium might be used to set down language already implies the idea of linguistic units as something more abstract than phonic utterances themselves. . . . Developing a written counterpart to spoken language removes the difficulties attaching to a purely oral practice of metalinguistic discourse. For although type-token ambiguities may arise for writing as for speech, writing provides a firm anchorage for at least one dimension of type-token distinctions, by providing a medium for displaying types which is different from the medium in which the corresponding tokens are produced. It introduces a new level of clarity into attempts to show what the abstractions are, by providing a system of types in terms of which, in literate societies, utterances will henceforth be interpreted. (1990, 109-110)

FROM A PHONO-LOGICAL POINT OF VIEW

Chapter 10
A Cyclically Integrated Account of Linguistic Categorization

The study of such phonological problems is of central importance to cognitive psychology. Failure to come to grips with them has led two main branches of contemporary philosophy—phenomenology and the logical analysis of language that stems from Frege—into seriously simplistic proposals about concept formation. The standard position is that the problem of how to categorize objects is solved for the child by the names he hears applies to them. That presupposes that the problem of categorizing those phonological entities that we call names is psychologically simpler than that of categorizing nonlinguistic objects. To examine this presupposition will require a detailed analysis of phonological learning.

John Macnamara (1982, 85)

The failure of "phonemic reductionism" entails that natural language phonology cannot be reconstructed without appeal to semantic cues external to the speech stream. In addition to a variety of phonetic-based cues intrinsic to the language, the semantic significance of various word tokens—whether objects for names and common nouns, or properties for various predicates—must also be consulted. Both linguists and infants are in the same boat in this regard. For while linguists are merely charged with detailing the phonemic contours of natural languages, infants are actually charged with developmentally internalizing these language-specific phonological constraints.

As chapter 8 detailed, the developmental course of semantically guided infant speech perception can be experimentally explored by manipulating the interpretive significance of various speech stimuli. Even if the claims of phonemic reductionism were true and language-specific phonemic equivalencies could be recognized merely on the basis of non-interpretive information, one suspects, as some neo-Bloomfieldian linguists themselves periodically conceded, that actual learners would not (be so "crazy" they said, as to) needlessly neglect this source of information. Rather, learners would opportunistically exploit all available sources of information, with Hockett's "levels" of linguistic structure and Harris's methodological principles taking a back seat to the more

pressing goal of acquiring the language as efficiently as possible, by hook or by crook.[94] As Martin Braine (1971) once pointed out, "The human discovery procedure obviously differs in many respects from the kinds of procedures envisioned by Harris (1951), and others. For one thing, it is sensitive to semantic and pragmatic categories and relations" (181; quoted in Ohlander 1976, 203).

This is an important point to stress. The goal of acquiring phonological structure apparently cannot, and more importantly need not, be conducted in a semantic vacuum. Instead, language learners rely on information external to the speech stream itself. By leveraging an appreciation of what is actually being referred to by various linguistic stimuli, the linguistic stimuli can themselves be categorized in accordance with the target language.

But this dynamic is only half the story; only one indication of the integrated relationship between speech and referent categorization. As Roger Brown suggested (chapter 8), the relationship between linguistic and non-linguistic categorization can be put to use in both directions. Moreover, the relationship can also be cyclically deployed in both directions. Not only can semantic reference to objects and properties inform linguistic categorization in late infancy, and not only can linguistic word labels inform the categorization of objects and properties for children as well as adults (see below), reference to objects and properties can re-inform linguistic categorization. In fact, Sellars' classification of synonyms as the same abstract "dot quote" is an example of, say, the non-linguistic category of fatherhood guiding the re-categorization of the phonemically distinct words /male parent/ and /father/ as exemplars of the single "dot quote"-type •father•.

To see how categories of language and categories of objects and properties are developmentally intertwined, consider the infant subjects from chapter 8. In order for these subjects to leverage objects or properties in categorizing bits of language, distinct objects and properties themselves must first be categorized. Unless various, say, cats (e.g., Lollypop and Snowball) have been antecedently categorized as members of some more general, say "basic-level" (Rosch et al. 1976) category of cat-type objects, phonological learners could conceivably infer that two phonetically distinct speech-tokens [cat^{h}] and [cat] might very well be phonemically distinct—one for each individual cat (or perhaps one for each of two different subordinate types of cats): Lollypop is scrawny, small and black; Snowball big, white and puffy. (This example is similar to the [mat^{h}] and [mat] example discussed in chapter 2.)

Insofar as objects or properties are to serve as a guide to phonemic categorization, objects and properties must themselves be individuated into type-equivalent object and property categories. The use of semantic information for phonological purposes presupposes that at least some objects and properties are categorized as the "same" and others as "different" so that these categories can then be used to guide the phonological typing of word-tokens. For only if allophonically related variants are uniformly interpreted to refer to the same non-linguistic category is the language learner positioned to infer that two phonetically distinct variants are phonemically equivalent. Without some prior—even if provisional—individuation of a world of objects and properties into types of objects and types of properties, semantic-based phonological development has no traction by which to start. Everything would be a categorically unique particular. Here is how the linguist Nigel Love makes note of this issue:

> In order to make general statements at all, one must indulge in abstraction. Spatio-temporally distinct objects and events must be envisioned as 'the same' in some respect relevant to the purposes of the discourse. Something like a type-token analysis is required. If one is to talk about say, cats, distinct individual organisms have to be seen as tokens of a type cat. This is true for language no less for cats. (1990, 97)

Of course, both the philosophical and developmental literature is replete with discussions of human object categorization and its developmental course. What is relevant for our present purposes, however, is the increasing recognition among both developmental psychologists and philosophers that such categorization is not conducted in a linguistic vacuum. Instead, linguistic information provided in the form of words is instrumental in refining our object and property categorization, both for developmentally practical and philosophically compelling reasons. Complementing the claim that linguistic, that is phonological categorization is reliant upon external semantic information is the claim that objects and properties themselves are classified into categories, at least in part, on the basis of linguistic information.

In developmental psychology, this dynamic of relying on linguistic information to guide classification has been recognized as the effect (adult) "word labeling" can have on children's object categorization, and is an area of increasing experimentation and theorizing (see, for instance, Xu et al. 1999 and Gelman et al. 2000). These studies demonstrate that children are deferentially sensitive to the way objects are labeled, revising the way they sort various objects so as to conform to the template provided by their language. In philosophy, the effect of "expert" linguistic labeling on categorization has been explored by Hilary Putnam (1975) and goes by the name of the "division of linguistic labor" and by Tyler Burge (1979) in the form of linguistic deference. Putnam's examples of /beech/ and /elm/ are designed to demonstrate that it is not only children, but

of course also adults, who are sensitive to the way things are labeled, keeping those objects which are labeled differently categorically distinct, and those labeled with the same word categorically the same (like labeling both a bat and a whale with /mammal/). Similarly, Burge emphasizes that while we may habitually use the term /arthritis/ to speak about a pain in our thigh, we are more likely to concede that we have been using the term incorrectly than "defend" some sort of idiolectical interpretation. Rather than argue that we have somehow been misunderstood, we tend to agree with the experts that arthritis cannot occur outside the joints.

In this way, both adults and children demonstrate a willingness to deferentially re-classify, for instance, whales as "mammals," and in this way indicate that we use our words with semantic parity—even though some of us are more or less susceptible to over- and under-extending our words.[95] Despite such mistakes, both what children and what adults mean by their "own" words is typically constrained by normative considerations of proper use. Indeed, without such constraints, such under- or over-extended applications would not constitute mistakes at all. It is not merely that we re-classify seemingly dissimilar objects because they are called by the same name or that we may re-classify similar objects because they are called by different names. Rather, our willingness to yield to linguistically guided re-classification indicates that we wish to use our words in conformity with their proper use. In this respect, the phenomenon of linguistic deference identified by Putnam and Burge extends equally to children as well as adults. Our words are interpreted modulo the meaning attached to them by others, independently of our behavior individualistically construed—independently of what is going on "in our own heads" as Putnam might say.

As Kaplan (1989) observes, there are "producers" of language and there are "consumers" of language. And "we are," says Kaplan, "for the most part, language consumers":

> Words come to us prepackaged with a semantic value. If we are to use *those words*, the words we have received, the words of our linguistic community, then we must defer to their meaning. Otherwise we play the role of language *creators*. In our culture, the role of language creation is largely reserved to parents, scientists, and headline writers for *Variety*; it is by no means the typical use of language as the subjectivist semanticists believe. To use language as language, to express something, requires an intentional act. But the intention that is required involves the typical consumer's attitude of compliance, not the producer's assertiveness. (602)

And quoting from Putnam (1975):

> What this illustrates is that it may not be the typical speakers' dispositions to assent and dissent that the linguist must seek to discover; because of the division of linguistic labor, it is frequently necessary for the linguist to assess who are the experts . . . before he can make a guess at the socially determined extension of a word. (236)

But it is not only an expert you may have to consult in order to figure out what someone really meant to say. Experts may also have to be consulted in order to figure out whether two linguistic expressions are or are not synonymous or whether they are or are not even co-referring names. Given the social dynamic of a division of linguistic labor and patterns of linguistic deference, individual behavior no more unilaterally determines substitutional equivalence than individual behavior unilaterally determines semantic content. Rewording Putnam's remarks:

> it may not be the typical speakers' dispositions to assent and dissent [to questions of substitutivity] that the linguist must seek to discover; because of the division of linguistic labor, it is frequently necessary for the linguist to assess who are the experts . . . before he can make a guess at the [substutional relationship between among words].

According to the experts /elm/ and /beech/ name different types of tress, and /Cicero/ and /Tully/ name the same thing. These words may be used in all sorts of ways, but this in itself does not make /elm/ and /beech/ intersubstitutable synonymous

expressions, nor /Cicero/ and /Tully/ non-intersubstitutable names—even in belief contexts. Just because many cannot distinguish elms from beeches does not make the words /elm/ and /beech/ synonymous. Even those of us, like Putnam, who realize we cannot provide much justification for *not* treating /elm/ and /beech/ as synonymous—and can hardly prevent ourselves from using them interchangeably—realize that the limits our own epistemic horizon are not a linguistic trump card. Not, at least, if we wish to remain part of a wider linguistic community.

But similar considerations govern the status of co-referring names. Just as neither the inability to distinguish elms from beeches does not make /elm/ and /beech/ intersubstitutable (nor the apparent Matesian ability to distinguish attorneys from lawyers makes /attorney/ and /lawyer/ non-intersubstitutable), the ability to seemingly distinguish Cicero from Tully does not make /Cicero/ and /Tully/ non-intersubstitutable. From a sufficiently normative perspective, it is not really possible to believe that /Cicero/ and /Tully/ are not freely substitutable co-referring names anymore than it is possible for Putnam to have really believed that /elm/ and /beech/ refer to the same tree. A commitment to an interpersonally stable language constrains not only reference but also co-reference. As Owens (1988) observes, "Once one takes seriously the claim that the content of belief is determined (in part) by social and conventional factors one approaches the whole issue of synonymy interchange in a new light. As these conventions determine content, so they will determine what expressions, if any, express the same content" (370).

There is, then, more than one way for linguistic categories and non-linguistic categories to interact. One way is for the categorization of objects and properties to play

a role in the individuation of language. Both phonemic word-types and Sellar's "dot quote"-types are premised upon exploiting non-linguistic information to guide the construction of linguistic equivalencies. The other way is for language itself to play a role in informing the individuation of objects and properties. Our deferential sensitivity to the way things are labeled with words is premised upon our willingness to leverage linguistic information in guiding the classification and re-classification of object and property categorization.

Beyond Infancy: Synonymous Substitutions

The individuation of linguistic categories, then, does not stop in late infancy anymore than the individuation of objects and properties stops in early childhood. Object categorization is subject to revision—and for most of us—is the result of tracking the way things are "expertly" labeled. Most of us come to know that whales and dolphins are mammals not by actually observing how they give birth to live young, but more by being told that they, like dogs and people, are also /mammals/. But it can also work the other way around too: linguistic categorization is itself also subject to revision. When two word-types are "expertly" used to refer to the same object or property, our own beliefs with respect to how these words have been used in the past can be over-ridden by considerations of how the words should have been used.

A good example of this is provided by Burge (1978). In this article, Burge argues that personal intuitions regarding meaning and substitutivity typically do not trump conventional standards. For "on such an interpretation," says Burge, "the speaker would have never made a mistake in his use of the word 'fortnight'; for the utterance 'A

fortnight is a period of ten days' would be analytically true" (131) by his lights, rather than a mistake. Indeed, in a series of "anti-individualistic" articles, Burge has consistently argued that something like linguistic deference, rather than linguistic defiance characterizes our linguistic relationships:

> In the normal case, if someone were to say ['A fortnight is a period of ten days'], he would be perfectly clear about how to confirm or disconfirm his statement or belief in the face of disagreement: he need only find a dictionary or someone whose linguistic intuitions both he and his opponent trust. . . . Of course, if the speaker refused to take the dictionary as authority and blandly (and credibly) explained that where he came from 'fortnight' is normally used to denote a period of ten days, *then* we would surely reconstrue his words. . . . The willingness of the speaker to submit his statement to the arbitration of a dictionary indicates a commitment to having his words taken in their conventional sense, whatever that sense is. (1978, 130)

Suppose, then, that someone thought, mistakenly, that /fortnight/ meant ten days. This person takes himself to believe that /fortnight/ refers to ten days, and ten days is what he means by /fortnight/. For him, /fortnight/ and /ten days/ are "synonymous." Taking him at his word, if he really wishes to mean—to "defiantly" mean—ten days by /fortnight/ then what he says and believes is unproblematic, but completely irrelevant to what /fortnight/ conventionally refers to in English. On the other hand, if what he means—deferentially means—by /fortnight/ is what /fortnight/ conventionally refers to (i.e., fourteen days), then not only is he mistaken in believing a fortnight is ten days, when he says a fortnight is not fourteen days (but ten days), he contradicts himself by saying that fourteen days is not fourteen days. The belief that a fortnight is not fourteen days is more than just false. The belief that a fortnight is not fourteen days is a contradiction. (Two words need not be phonologically equivalent in order to create the conditions for a contradiction; they need only be the same "dot quote." Similar considerations hold for Kripke's Pierre, see below: Either Pierre wishes to "defiantly" use

/London/ to refer to only the city's ugly parts (and */Londres/* only for the pretty parts)—but then his idiolect usage is clearly non-standard. Or Pierre wishes to deferentially use both /London/ and */Londres/* to refer to all of London. But then he contradicts himself when he says London is not *Londres*.)

Consider that if mere doubt, no less resistance, to the substitutivity of, say, /attorney/ and /lawyer/ is sufficient to prove that the terms are non-synonymous, then it would seem that, as Mates (1950) suggested, synonymy and substitutivity are by their very nature illusory. But as Feyerabend (1956) complains in connection with the related "paradox of analysis":

> There would be no point in saying that X has just learned that 'P' is synonymous with 'Q' if the different attitude of X toward 'P' and 'Q' *before* the end of his P-Q lessons would suffice for calling 'P' and 'Q' non-synonymous. . . . One can of course always introduce a concept of synonymy which has just this consequence. But if we want at all to distinguish between the relatively stable semantical relations within a certain language L and the changing attitude of people who by degrees become acquainted with L we must not allow pragmatic changes to become parts of our criteria of synonymy. (92-93)

According to Feyerabend, "the paradox of analysis as well as the usual suggestions as to its solution are both based on the assumption *that a difference of the triviality value of otherwise synonymous expressions indicates a difference of meaning*" (1956, 93, italics original).[96] Indeed, why should the mere fact that two words are recognized to be two different words (rather than two word-tokens of the same word-type) guarantee that the terms are not freely substitutable? After all, there are phonetic differences between word-tokens which could conceivably provoke substitutional failures. But we do not allow even belief contexts to take advantage of allophonic discrepancies.

To be sure, there is no way that synonymous (or co-naming) word pairs are ever going to be perceived as "similar" in the same way that allophonic word-token variants

are perceived as the same word. Nor is it likely as Fiengo and May (1998) suggest (though only in passing) "that a speaker might somehow take 'Cicero' and 'Tully' to be dialectical phonetic variants" (385) of each other. But we do have the philosophical resources afforded by Sellars' dot quotation to subsume synonyms so that mere differences in their linguistic form are not themselves an obstacle to substitutional equivalence. Dot quotes allow synonyms (and co-names) to be represented as superficially distinct notational variants of each other.

Though there are differences between allophonic substitutions and synonymous substitutions, the situation is one of philosophical parity. Linguistic redundancy occurs both below and above the phonemic level: below the phonemic level it is allophonic variation; above the phonemic level it is synonymy. And just as the substitutions characteristic of allophony are diagnostic of one's linguistic competence, substitutions characteristic of synonymy may also be presented as diagnostic of one's linguistic competence. In fact Kripke (1979) explicitly argues that the substitution of synonymous terms is a litmus test for linguistic competence. According to Kripke, synonymy cannot be doubted without thereby indicating some sort of linguistic deficiency or deviancy. Synonymy is immune to doubt. But what about co-referring names?

Indeed there would appear to be an insurmountable barrier to claiming that co-referring names substitute freely in belief contexts given that they are often used in ways that clearly belie this substitutional equivalence. But synonyms may also be misused. The present argument, then, is designed to counter descriptive observations with normative guidelines for the use of both synonyms and co-referring names. It proceeds by first isolating the basic rationale, epistemological in nature, and individualistic in

orientation, that supports a traditional Fregean interpretation of the substitutional failure of co-referring names in belief contexts. So while at least some philosophers like Church (1954) and Kripke (1979) claim that the substitution of synonymous terms cannot be doubted without jeopardizing one's linguistic competence, co-referring names are held to easily fail substitution in belief contexts with no such repercussions. Yet this traditional Fregean claim sits uncomfortably with Kripke's own position regarding the linguistics of synonymy and the metaphysics of rigid designation, perhaps commonsense too. It is not clear whether those who do not—or, at a minimum, are not deferentially prepared to—interchangeably refer to, say, Cicero with both his names should be regarded as "fully" competent. Nor is it so obvious we intuitively regard ourselves as "fully" competent with co-referring names if we are unaware of their co-referentiality. Under-extending the name 'Tully' to refer to Cicero only under selective circumstances is not a robust indication of the full use of his name.

Synonymous and Co-Naming Competence Demands Substitutivity

First, consider how Wettstein (1991) sets up Frege's puzzle:

> Let us begin with the reflection that the more epistemology one builds into linguistic competence with names, that is, the more of a cognitive fix one requires, the more it will seem that Frege's data present not merely interesting and important phenomena to be explained but a prima facie problem, a *puzzle*. Why is this? The thesis that linguistic competence with names requires mental apprehension of their referents induces a tension between two names' co-referring and their being cognitively in equivalent. If in using each of the two names one must be in cognitive touch with their single referent, how can this identity of reference have escaped notice? (155)

Indeed, how is this possible? "How is it that," Taylor (1995) questions, "even someone who is maximally cooperative in attempting to inform us about his beliefs and who is fully competent in the language will, when asked 'Do you believe that Hesperus is

Phosphorus?' respond that Hesperus is distinct from Phosphorus. No difference in the meanings of the two sentences, nor the contents of the two beliefs is relevant to the explanation" (143). If the two terms are, in fact, being correctly applied to one and the same object, how could their co-referentiality not be trivially, "directly" as it were, apparent? But a closer look at Fregean cases of double naming reveals that such individuals are not "fully" competent with their words. Instead, their words are restrictively *under-extended* relative to their full denotation.

To see this under-extension, it is helpful to recall how Church and Kripke respond to Mates's argument. In their responses to Mates's (1950) "Synonymy" article, Church (1954) and Kripke (1979) suggest that those who take themselves to be doubting the equivalence of synonyms succeed only in forfeiting their claim to be fully competent. How could one credibly claim to fully appreciate what, say, /physician/ and /doctor/ *mean*, wonder Church and Kripke, without being prepared to apply them interchangeably? After all, /doctor/ and /physician/ refer to, or express, the same property. /Doctor/ applies to doctors and /physician/ applies to physicians, and insofar as the property of being a doctor is the same property of being a physician, then all doctors are physicians and all physicians doctors. One wonders what else—leaving Quinean eliminativism to the side—such substitutional skepticism could ultimately amount to (but see Rieber 1992[97]). For if /doctor/ is not also applied to what /physician/ is (and vice versa), the uniform application of /physician/ and /doctor/ slips—an indication of some sort of linguistic confusion or linguistic deviancy. According to Kripke (1979), if two words are synonymous, then "a sufficiently reflective speaker subject to no linguistic inadequacies or conceptual confusions who sincerely assents to a simple sentence

containing one will also (sincerely) assent to the corresponding sentence with the other in its place" (140).

For that matter, incomplete synonymous competence can be "tested" by observing whether the terms are applied interchangeably, whether /physician/ is uniformly applied to everything /doctor/ is applied to (and vice versa). If not, then the terms are obviously not being used in a way consistent with their synonymy. Of course, this simple predicate application test is overly broad. It does not succeed in distinguishing the intensional synonymy of /doctor/ and /physician/ from the extensional convergence of /renate/ and /cordate/—an indication that extensional interchangeability does not, in and of itself, guarantee predicate synonymy. The issue that concerns us here, however, is not how to account for the *convergent* application of /doctor/ and /physician/, but rather how to account for the possibility of their *divergent* application. Such a discrepancy is not to be explained, at least according to Kripke, in "hyper-intensional" terms sufficient to distinguish co-meaning predicates. Kripke does not appear to endorse the proliferation of hyper-intensional entities. Nor does Kripke account for the substitutivity of synonymous predicates in terms of an *a posteriori* discovery of a metaphysically necessary truth, like he does with co-referring names.

Instead, Kripke tenders a different sort of explanation. Any discrepancies between the use /doctor/ and /physician/ are not to be explained, but as it were, explained away—in terms of some sort of semantic or linguistic confusion. According to Kripke (1979), "if someone were to assent to 'Doctors are happy', but refuse assent to 'Physicians are happy', prima facie disquotation does not apply to him since he is under a linguistic or conceptual confusion" (147). Recapitulating Church's (1954) response to

Mates's (1950) argument, Kripke concludes, "So there are as yet no grounds, merely because this has happened, to doubt that all who believe that doctors are happy believe that physicians are happy" (147). Synonymy is regarded as a linguistic requirement. So rather than invoke sense-like entities to preserve the consistency of someone who might doubt a synonymous substitution, the rationality of such doubting individuals is preserved by undermining their linguistic competence.

This pattern of this reasoning, however, suggests an analogous solution to standard Fregean puzzles, as well as Kripke's own puzzle about belief. Indeed, a structurally identical argument can be applied to the co-naming substitutivity of /Cicero/ and /Tully/ and Kripke's */Londres/* and /London/—with precisely the same dismissive results. (Some of Kripke's (1979) remarks on page 133 regarding various "proofs" and "disproofs" of substituvity, and in footnote 42, page 146, indicate an awareness of this dynamic.)

As Church and Kripke argue, those who might doubt the equivalence of synonyms forfeit their claim to be fully competent with the words. By parity of reasoning, however, how could one credibly claim to "fully" appreciate what /Cicero/ and /Tully/ refer to without also being prepared to treat them interchangeably? Paraphrasing Church, it should be understood that those who are supposed to have doubted whether Tully is Cicero without doubting whether Cicero is Cicero are supposed also to have had a sufficient knowledge of the English language so that their doubt was not, for example, a doubt about what /Cicero/ or /Tully/ *refer* to in English. But it is not so clear their co-naming substitution can be doubted with linguistic impunity. For in order to genuinely doubt the substitution of /Cicero/ and /Tully/, it would seem one must be prepared to

apply one, but withhold the other from one and the same object. (For similar considerations, see what Biro (1995, 196) calls his "re-alignment test.") But this would entail that at least one, but typically both, of Cicero's names would be restrictively *under-extended* relative to their full denotation.

Co-Naming Competence

It is perhaps a truism to point out that we are, in some sense, linguistically responsible for naming Cicero with /Cicero/ (rather than with, say, 'Caesar'). Likewise, Tully with /Tully/. But if Tully is the same person as Cicero, then just as much as it is a "linguistic requirement" to refer to Cicero with /Cicero/, and Tully with /Tully/, it is also arguably incumbent upon language users to refer to Tully with /Cicero/ and vice versa. It is not as if, as Kripke and others have argued, Cicero's name is /Tully/ only when he wears a toga and /Cicero/ only when he addresses the Senate, or something like that. Nor is it the case that /Tully/ is only the name of Tully, and /Cicero/ only the name of Cicero because only /Tully/ meta-linguistically disquotes into Tully and /Cicero/ into Cicero. Nor, of course, is it wrong to refer to Tully with his other name /Cicero/ anymore that it is wrong or even pragmatically awkward—at least for those who are "linguistically competent"—to apply the predicate 'doctor' to a physician. As mentioned earlier, Kripke (1979) himself briefly raises this issue when he asks "If a parent, aware of the familiar identity, takes a child into the fields in the morning and says (pointing to the morning star), 'That is called 'Hesperus',' has the parent mistaught the language?" (146). (Kripke does not answer this question. He only concludes that "A parent who says 'Creatures with kidneys are called

'cordates', definitely has mistaught the language, even though the statement is extensionally correct" (146).)

What, then, are our linguistic responsibilities vis-à-vis names? Consider that there are many who do not refer to George W. Bush by all his "nick" names. Or consider those who might not interchangeably refer to Jerusalem with the Arabic /Al-Quds/. But these cases need not indicate an unfamiliarity with George W. Bush, Jerusalem, or the operation of unique senses associated with a variety of co-referring names. Nor must they be interpreted in a way that is so puzzling. In the first case, one may be far removed from a particular sociopolitical linguistic loop. Relative to those who do use the nick name to refer to George W. Bush, those who do not are unproblematically less competent.

As for the second case, though at least minimally competent with both /Jerusalem/ and /Al-Quds/—/Jerusalem/ is used to refer to most of Jerusalem and /Al-Quds/ for at least some of Jerusalem—the name /Jerusalem/ is never used to refer to one particular section or "quarter" of the city. Nor is /Al Quds/ ever used to refer to the city's remaining parts. Jerusalem may be a divided city, but not according to this confused individual. According to this Pierre, there are actually *two* cities: one called /Jerusalem/ and the other called /Al Quds/. And while this multicultural Pierre prays and shops in both, he lives only in one. In fact, this tri-lingual (let us suppose for ease of exposition) Pierre complains "Al Quds is pretty, but Jerusalem is not pretty." In fact, Pierre reasons out loud in English, "Jerusalem is not Al Quds."

My admittedly biased intuitions are clear. Insofar as Pierre is unable or unwilling to equitably refer to all of Jerusalem with both its names (and all of Al-Quds with both its

names), Pierre is not unquestionably competent with these two terms—anymore than Kripke's Pierre is with /London/ and /*Londres*/. After all, Kripke's (1979) Pierre "takes it for granted that the ugly city in which he is now stuck is DISTINCT from the enchanted city he heard about in France" (120, emphasis added). In virtue of an inability—however epistemically blameless in origin—to refer to their entire city by both its names, neither of these Pierres qualify as unqualifiably competent.

What Pierre tries to say with words he demonstrably under-extends need not—as Kripke urges with respect to misused synonyms—be an issue of philosophical puzzlement. Synonymous predicates "directly" express, as it were, the same property; individual epistemic factors are irrelevant. Synonymous substitutivity is a "linguistic requirement." But according to Kripke himself, co-referring names are also better viewed as "directly" referring to the same object, again independent of individual epistemic factors. Names, like synonyms, appear to be answerable to non-individualistic standards governing proper usage. Given that both /Cicero/ and /Tully/ are not correctly used to interchangeably name what both /Cicero/ and /Tully/ name, such individuals should not be construed as unquestionably competent with such words. After all, /Cicero/ and /Tully/ are referentially equivalent just as much as /physician/ and /doctor/ are synonymously equivalent. Full competence, then, requires that these terms be used correctly—or at least deferentially accommodated—neither over- nor under-inclusively.

To be sure, the requirements of "linguistic competence" cannot be so strict as to demand perfect predicative or denotative reliability. For if Pierre is guilty of linguistic incompetence simply because he is not able to always correctly identify the full extension of /*Londres*/ or attribute the property of being a nurse to a male nurse, then Pierre need

not really worry: most of us are no better off. From a sufficiently normative perspective, however, it does not matter how good we are "individualistically construed" at picking out referents, or applying our words (like /nurse/, /arthritis/ or /even prime/), for we may, as Putnam and Burge suggest, defer to those who are better. Deferential receptivity to continual linguistic guidance allows even errant users to coincide with Putnamian more "expert" and Burgean more "proper" usage.

What makes Kripke's Pierre potentially distinctive, then, is not his linguistic aptitude, but his social attitude. For not only does Kripke (1979) tell us that Pierre assumes the city he now lives in is *distinct* from the city he heard about in France, but Pierre "has no inclination to change his mind for a moment about the city he still calls '*Londres*'" (120). But that does not seem right. Surely Pierre has the inclination to try to properly refer to all of *Londres* by the name */Londres/*—even if he does make a lot of mistakes. Or perhaps Pierre prefers to willfully exclude part of *Londres* from the full denotation of */Londres/* so that he may continue to write his friends back home "*Londres est jolie.*"

Either way, however, it would appear that there *is* something linguistically peculiar about Pierre. On the one hand, either Pierre is not "fully" competent with the term */Londres/*, as he has a bad habit of excluding some districts from its denotation. Ask him if his apartment is in */Londres/* and he responds negatively. This does not, however, necessarily prohibit us from disquotationally interpreting Pierre's */Londres/* as referring to London. As Burge would argue, it is not as if Pierre is using a different word which just happens to be homophonously ambiguous with our word */Londres/*. Rather, he is guilty of misusing the word */Londres/*.

What Burge does not highlight, however, is that in virtue of a deferential commitment to abide by the standard interpretation of the words we may happen to misuse, partial understanding can land us in an unintended contradiction. If we allow Pierre to express by */Londres/* what we mean by */Londres/* (even though he makes mistakes), then we can interpret Pierre's */Londres/* as referring to London. But then it would seem that Pierre really is entangled in a direct contradiction when he assents, for example, to "London is not *Londres*," for this will be interpreted as London is not London. [98]

On the other hand, if Pierre wishes to deliberately name only certain parts of *Londres* with */Londres/* then he is perhaps "fully competent," but then our term */Londres/* is only ambiguously homophonous with Pierre's */Londres/*. In this latter case, it is not that Pierre attempts to conform to some standard, fails and as a result, unwittingly becomes entangled in some sort of linguistic confusion. In this case, Pierre has no intent (for whatever reason) to use the term to refer to what the term conventionally does refer to.

So if our words are used with the deferential intent of conforming to others, the possibility of incidentally contradicting ourselves arises in virtue of the possibility of lexical over- or under-extension (or what Burge calls incomplete or partial understanding). If we do not use our words with the deferential intent of conforming (either for deliberately idiosyncratic or with alternative conventions in mind), the speaker is not so much unfit but ineligible for disquotation. Either "deferential disquotation" can create contradictions (in virtue of less than full linguistic competence), or "defiant

quotation" undermines the very basis by which to infer speaker belief on the basis of speaker utterance in the first place.

Pierre, then—just like someone who doubts synonymy—is linguistically odd under either interpretation. He is "odd" either because he suffers from incomplete lexical understanding, relative to those who do not. Or he is "odd" because his linguistic allegiances simply lie elsewhere. There is parity among these cases, whether the equivalence is one of synonymy or co-referential naming. And this is why "it is far from clear," as Rieber (1992) points out "whether a person's sincere disinclination to assent to a sentence which he imperfectly understands implies that he does not believe what the sentence says" (227).

In "A Puzzle About Belief," Kripke (1979) maintains that while synonymous doubt is not a genuine possibility, co-naming doubt is. Other than citing standard Fregean intuitions, Kripke does not support this claim. Yet why there should be a different standard, no less an explicitly individualistic epistemic standard, for co-naming substitution in contrast to synonymous substitution appears to be inconsistent with Kripke's anti-Fregean sympathies—itself arguably symptomatic of a more pervasive "problem in the Frege-Church theory of sense and denotation" (see Salmon 1993, as discussed in note 13).

Kripke suggests that Pierre's problems (and perhaps Fregean cases generally) lie not in any specific fallacy, but rather in the nature of the realm being entered. But it is not difficult to describe this realm. It is a world where language users fail to properly extend their names to cover their full reference. It is not coincidental that Pierre's problems—like Fregean puzzles generally—proliferate where the full denotation of a

term is not respected. Note that even Kripke's (1979) Paderewski case is premised upon comparable denotational idiosyncrasies: Peter mistakenly takes /Paderewski/ (like the common noun /bat/) to be *ambiguous* between two referents. But /Paderewski/ is not homophonously ambiguous. The name /Paderewski/ is referentially unequivocal. Yet, because of this error, Peter is reluctant to refer to /Paderewski/ (the politician) as /Paderewski/ (the musician) and vice versa. But the name /Paderewski/ is, in fact, *unequivocal*: "both" /Paderewski/s refer to Paderewski. And Peter's refusal not to name Paderewski with /Paderewski/—no matter what the reason—reveals this lexical irregularity. (See Santambrogio (2002) for similar considerations.)

Nor is it, of course, coincidental that Burge's semantic thought experiments are premised upon there being some sort of linguistic misapplication—some sort of linguistic error—variously identified as "misconception", "linguistic error," "partial" or "incomplete understanding." Burge exploits the contrast between a linguistically confused individual and a counter-factual individual who makes the "same mistake" (individualistically construed) but who is not guilty of any confusion given the difference in socio-linguistic circumstances.

It is misleading, then, for Kripke (1979) to maintain that Pierre is not subject to *any* linguistic irregularities, irregularities which in fact encourage Pierre to confusedly assent to "*Londres* is pretty" but dissent from "London is pretty." Recapitulating Kripke's remarks on synonymy, we can no more conclude that Pierre does or does not believe London is pretty than we can conclude that someone does or does not believe that Jones is a doctor, when their assent is not linguistically "reflective." Pierre's confusion as to which airplane to take from *Londres* to London is just one of many indications that

Pierre himself would acknowledge as symptomatic of a linguistic eccentricity (see Marcus 1981, 1983).

In this way Kripke's Pierre is not completely free of the sorts of considerations that prompt Kripke to dismiss those who might question the substitutivity of /physician/ and /doctor/. One cannot claim to be fully competent with co-referring names but persist in believing or behaving as if they were not co-referential. And this is why it is open to question to what extent someone who sincerely claims that Cicero is not Tully can be taken—or would want others to maintain that they themselves intended—to believe Cicero was not Tully.[99] Marcus (1983) offers a helpful analogy with the belief-desire psychology of wanting in pressing this intuition:

> David Kaplan pointed out that an agent might, for example, have an aversion to mayonnaise. He says to the restaurant waiter that he wants the house dressing. But, on perceiving that the house dressing contains mayonnaise, the agent doesn't say that he formerly wanted the house dressing but no longer wants it. He says rather that he was mistaken in claiming that he wanted it. (331)

With respect to Kripke's Pierre, Marcus (1983) writes:

> Pierre may, for example, sincerely announce, 'I intend to move from ugly London to beautiful *Londres*.' But my intuition about my own language use is that in cases like Pierre's, once an impossibility had been disclosed, I would say that I had only claimed to believe that London was different from *Londres*. For that would be tantamount to believing that something was not the same as itself, and surely I could never believe *that*. So my belief claim was mistaken, just as, on disclosure that a possible state of affairs does not actually obtain, I say that my knowledge claim, if I made one, was mistaken. (330)

There appears to be much less room for doubt to operate asymmetrically over the substitution of co-referring names. And this is because the proper use of names is subject to semantic norms just as the proper use of predicates and words generally is subject to semantic norms. "Psychologizing" naming and co-naming substitution is no more philosophically unobjectionable than "psychologizing" meaning and synonymous

substitution. Moreover, it is not obvious that everyday intuitions would have it differently: we tend to be "anti-individualistically" deferential with respect to both the substitution of co-referring names and the substitution of co-meaning predicates. In fact, we seem to prefer deferentially excusing ourselves for misspeaking our own language than defiantly removing ourselves so as to be correctly speaking something else.

Deferential Substitutivity

Of course, there is the problem of what to say before it was "expertly" realized by *anyone* that /Hesperus/ and /Phosphorus/ or /Cicero/ and /Tully/ were co-naming pairs. Surely before any such discoveries, the fully competent use of each word did not demand their substitution. In fact their conventional use would have demanded that they be used to denote seemingly different objects. Even the "experts" did not condone calling Hesperus /Phosphorus/. In fact, the experts would have reprimanded those who did use such pairs interchangeably. But that just means that even our experts can be wrong, relative of course to others who are even more the expert. Insofar as there was, as of yet, no one on record actually resisting the categorization of pyrite as /gold/, or challenging the distinction between what /Hesperus/ refers to and what /Phosphorus/ refers to, there is simply no discrepancy between what the "experts" say and what we were all doing, albeit incorrectly by some other future standard.

But weren't the ancients "fully competent" with the use of /Hesperus/ and /Phosphorus/? And weren't medieval alchemists "fully competent" with the use of /gold/? Yet insofar as they are *our* words too, our practices are also relevant. "Of course," notes Kimbrough (1998), "we would be justifiably reluctant to say that a

relatively coherent and stable practice is mistaken just because it differs from ours. However, we shall sometimes have reason to believe that differences in usage between our linguistic community and some other community reflect mistakes (ours or theirs)" (478). And this is why, though perhaps counterintuitive, it is not so crazy to claim, pace Laurier (1986) that "before it was discovered that Hesperus is Phosphorus nobody [fully] understood these names" (45).[100] As Putnam (1975) remarks,

> Archimedes would have said that our hypothetical piece of metal *X* was gold, but he would have been *wrong*. But *who's to say* he would have been wrong? The obvious answer is: *we are* (using the best available theory today). For most people, either the question (*who's to say*) has bite, and our answer has no bite, or our answer has bite and the question has no bite. (235-236).

Insofar as we wish to consider ourselves "speaking the same language" as the ancient Babylonians and medieval scientists, our words are their words, and their words are our words. It is the difference between saying they were using different (but homonymous) words, as opposed to incorrectly using the same word. We can say that they were right and fully competent and from that, conclude that they spoke a different language. Or we may charge them with being mistaken so as to bring them into our linguistic community (or equally concede that it is we who may be mistaken relative to a common standard sponsored by them). Is Humpty Dumpty "right" because he is speaking his own language, or is Humpty Dumpty "wrong" because he is (trying to) speak our language? "Which of these considerations" asks Burge (1978) should "take precedence in determining how to interpret the speaker's statement and attitude. . .?" According to Burge, "The speaker's behavior, as we are imagining it, provides the key. His willingness to defer to a dictionary or the intuitions of others speakers and his and our

unmixed feelings, after consulting the relevant authority, that he made a mistake suggest that the latter consideration is crucial" (1978, 131).

In addition to past and even future lexical mistakes, there is also the problem of what to say about the usage of a predicate which has only one member in its extension. Consider the predicate 'is an even prime.' Can a predicate whose extension is a unit set be understood yet used incorrectly? Imagine someone were to think there are no even primes. The belief is not only false, but necessarily false. Is this an indication of incomplete mastery of the predicate 'is an even prime'? My intuitions, though clearly biased, favor withholding full competence. Though perhaps counter-intuitive, consider someone who thinks 9 is a prime number, or that there aren't any prime numbers. Here it would seem that these kinds of beliefs tend to undermine mastery of what a prime number is. But what if 2 is thought not to be a prime number because it is an even number. Here too, it would seem that this belief undermines being fully conversant with what a prime is. A prime is a number evenly divisible by only 1 and itself. And 2 satisfies this requirement. My intuition is that rather than "discovering" that there is an even prime number, experiences of this sort encourage a sharper understanding of what the definition of a prime number really is.

But deferentially accommodating a more precise understanding of our own words runs counter to both Fregean intuitions and their Mates progeny. Fregeans take individual belief and behavior to be the defining criteria of linguistic competence, not communal norms. But one does not ignore a pre-schooler's misguided attempt to apply and interpret the inscriptionally distinct [mat], [MAT], and [*mat*] differently. Nor does one ignore problems related to synonymy. But nor does one ignore problems related to

co-referring names by explaining them in virtue of Fregean senses. It is not the role of semantic theory to account for how the use of either synonymous terms or co-referring names can incorrectly split apart. Despite their differences [mat], [MAT], and [*mat*] are substitutionally equivalent. And despite their differences, the synonymous /attorney/ and /lawyer/, and the co-naming /Cicero/ and /Tully/, are also freely interchangeable, again independent of individualistic doubts.

For that matter, even Quine (1981) gestures toward these sorts of normative constraints governing the substitutivity of phonemically distinct word types—what he calls "recondite" words:

> This unanimity requirement works all right for our core language, Basic English so to say, which all English speakers command. However, when recondite words are admitted, a pair of occasion sentences may fail of cognitive equivalence for an ignorant speaker merely because of misunderstanding. If we still want to count those two sentences cognitively equivalent for the language, we may do so by relativizing the unanimity requirement to an elite subset of the population. (50)

But appeals to normativity do not come easily for Quine. After first explaining the difference between cognitive equivalence for an individual idiolect and cognitive equivalence for a language, and affirming that "It is the latter that we are interested in when we expound the semantics of a language" (50), Quine makes sure to qualify this prioritization. The passage from "Use and Its Place in Meaning" quoted above actually includes the following:

> Cognitive equivalence for the individual, however, is the prior notion conceptually, that is, in respect of criterion. Two occasion sentences are equivalent for him if he is disposed, on every occasion of query, to give them matching verdicts or, on doubtful occasions, no verdict. The summation over society comes afterward: the sentences are equivalent for the language if equivalent for each speaker taken separately. The unanimity requirement . . . (49-50)

But it is precisely this retreat to idiolecticism which is so objectionable. As Dummett (1974) argues against Quine with Burgean-like instincts, "an idiolect is not a language; there is no describing any individual's employment of his words without account being taken of his willingness to subordinate his use to that generally agreed as correct. That is, one cannot so much as explain what an idiolect is without invoking the notion of language considered as a social phenomenon" (528). Similarly, "The meaning of a word," says Sellars (1989), "is somehow a community fact. It is a community fact that 'surfboard' means something. To be sure, people can have a beetle bag, a private variation on it, but it would be parasitic on the domain of public language, the public meaning" (24). Continuing, Dummett (1974) stresses:

> Hence it is incorrect to view the problem of interpreting the words of another speaker of my language as analogous in principle to the problem of radical translation, as Quine sometimes does. In part of my paper which I did not read, I criticized Quine for first recognizing the social character of language (and recognizing it in a very vivid way, by distinguishing between the case where there is, and when there is not, an existing recognized scheme of translation), and then jettisoning this recognition by pretending that one may shrink the linguistic community as one chooses, even down to a unit set, and hence treat the problem of interpreting another speaker as the same in principle as that of interpreting a radically foreign language. (528)

Most importantly, however, are Burge's observations concerning the social constraints we impose upon ourselves:

> communal conventions about the meaning of a speaker's words tend to override what a speaker mistakenly associates with his words in determining what he says and even, sometimes, believes. . . The believer's own construal of the words does not determine content. For if it did, the believer's belief, in the relevant cases, would be true. Rather, in these cases the content partly depends on linguistic considerations of a broader community. (1978, 134-138)

The meaning of our "own words" derive their content from considerations external to ourselves, as it should be: no one really wants to be a Humpty Dumpty. In this way, Burge explicitly allows for individuals to not fully know or even believe what they

actually say: "Thus we may correctly see others as sometimes understanding our own idiolects better than we do. . . . The reason for their insight is not that they have made a study of us. It is also not that they are foisting some foreign, socially authorized standard on us" (Burge 1990, 126).

But in this regard, there is no difference between meaning and naming, between synonymous predicates and co-referring names. "On this model," emphasizes Owens (1986), "the contents of one's earlier beliefs are partly determined by the relevant conventions, and thus the appropriate characterizations of one's earlier beliefs is not determined simply by what one recognizes oneself as believing (then or now)" (371). In fact, the same kinds of considerations which prompt Putnam to distinguish the semantics of /water/ (as spoken on Earth) from the semantics of /water/ (as spoken on Twin Earth)—and Burge to distinguish the semantics of /arthritis/ here and over there—are relevant for *not* distinguishing some expressions. The Earth word /water/ and the Twin Earth word /water/, though almost identical in all respects, are not the same word. Instead, they are better viewed as semantically ambiguous, accidental homophones. Analogously, /Cicero/ and /Tully/ are better viewed not so much as different but semantically equivalent words, but as mere notational variants of one and the same "dot quote."

Here is how Owens (1986) expresses this point:

> The picture I am advancing is simply the other side of what is usually extracted from Twin Earth examples. They are usually taken to illustrate how contextual elements can provide grounds for attributing different beliefs to doppelgangers (despite the fact that they use the 'same words' etc. to fashion and express their respective beliefs)—grounds for distinguishing between the content of beliefs which are not 'introspectively distinguishable'. In like fashion I am arguing that contextual elements (conventions) may provide grounds for treating different expressions as expressions of the same content. (380)

But even Kripke (1979) acknowledges that some phonemically distinct word-types may be best approached almost as if they were incidental variants of each other. So while Kripke notes that the co-referring name */Londres/* just is the French notational version of /London/, "one cannot quite say that the same relation holds between [the Hebrew terms] 'Ashkenaz' and 'Germaniah.'" "Nevertheless," continues Kripke (much like Church 1954):

> (a) Our standard practice in such cases is to translate both names of the first language into the single name of the second.
> (b) Often no nuances of 'meaning' are discernible differentiating such names as 'Ashkenaz' and 'Germaniah', such that we would not say either that Hebrew would have been impoverished had it lacked one of them (or that English is impoverished because it only has one name for Germany), any more than a language is impoverished if it has only one word corresponding to 'doctor' and 'physician'. Given this, it seems hard to condemn our practice of translating both names as 'Germany' as 'loose'; in fact, it would seem that Hebrew just has two names for the same country where English gets by with one. (146)

Note that Kripke considers /Ashkenaz/ and /Germaniah/ to be co-referring names, not merely synonyms. And what he says about /Ashkenaz/ and /Germaniah/ can certainly be said about */Londres/* and /London/, and arguably /Cicero/ and /Tully/. For if it is indeed "standard linguistic practice" to substitute /Ashkenaz/ and /Germaniah/ for each other, then no Fregean-like puzzle can be created. In virtue of being "standard practice," their substitution would be standardly expected and deferentially accepted. Doubt regarding whether Ashkenaz and Germaniah are the same country would either be symptomatic of an unfamiliarity with the standard linguistic practice substituting /Ashkenaz/ and /Germaniah/, or perhaps an indication of some alternative linguistic practice. In either case, it is not genuine doubt that is expressed but some measure of linguistic deviance or linguistic defiance of one sort or another. But standard practices need not be widespread,

popular, obvious, or fully internalized in order to be "standard." That is what the "expert" in expert usage and the "proper" and proper usage are for. From this perspective, then, obligatory linguistic substitutions are not limited to synonymy and phonology. Instead, co-referring names represent another case of linguistic substitutions deferentially immune to substitutional doubt. Flanking co-naming substitutions on one side are phonological substitutions, and flanking co-naming substitutions on the other side are synonymous substitutions. The possibility of co-naming doubt is not something philosophers of language need to accommodate. The Fregean possibility of co-naming doubt is an artifact of an overly individualistic model of semantics insensitive to the willingness of language users to deferentially concede incomplete understanding of their own words.

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[94] Relevant here is Chomsky (1964). Chomsky considers what sorts of evidence external to phonology proper may have contributed to the one-directional "discovery procedures" of "taxonomic phonemics":

> In particular, it is important to ask whether reasonable requirements for a perceptual model . . . or a learning or discovery model . . . have any bearing on the validity of taxonomic phonemics. Considerations of this sort may actually have been at the core of some theoretical and methodological studies. One might try to justify the conditions of (32) [(i) linearity, (ii) invariance, (iii) biuniqueness and (iv) local determinacy] by arguing that speech perception involves two successive and entirely separate stages: the hearer first uses only local phonetic cues to identify the invariant criterial attributes that determine the successive taxonomic phonemes; and then goes on to determine the deeper structure of the utterance (in particular, its systematic phonemic representation and its syntactic structure). This clearly seems to be the view of Jakobson (cf. Jakobson, Fant and Halle, 1952) and of Joos (1957, 92) among others. However, there is no real basis for this account, and it is scarcely in accord with what little is known about complex perceptual processes, or, for that matter, about speech perception. Thus it is well-known that intelligibility is preserved under gross phonemic distortion, which may be completely unnoticed when grammatical constraints are met. . . . That is, one would naturally expect that, as in the case of other perceptual processes, the hearer's knowledge will provide a complex schema within which the actual signal is interpreted. To the extent that this is true, the 'atomistic' view of the taxonomic phonologists will be in error. It remains to consider the status of taxonomic phonemics with respect to a model of acquisition of language. There is, in fact, an approach to the question on these grounds.
>
> Suppose that we impose on the acquisition model the condition of separation of levels, which we can interpret as requiring that the level of systematic phonetic representation must be 'rationalized' and converted to a level of taxonomic phonemic representation without reference to any morphological or syntactic information. (99-101)

Chomsky continues in a footnote: "One or another form of this is implicit in all substantive discussions of linguistic procedures I have been able to locate. Some linguist (e.g., Pike and Harris) would allow

restricted use of certain higher level information in phonology, where this can be obtained by 'cyclic' or 'spiral' procedures (cf. Pike, 1947[b], 1952[a]; Harris, 1951), but many American linguists insist on strict separation." [Back to text]: "Observe that this condition is not to be confused with the conditions of biuniqueness and local determinacy. These (as all of the conditions (32)) pertain to the 'perceptual model'; they assert that the phonemic correspondent to a given phonetic sequence must be determinable by operations involving only neighboring sounds, once the phonemic system is fixed. But the condition of separation of levels is not a formal condition on a phonemic system and the rules that relate it to sound; it is a methodological condition on the information relevant to determining the correct choice of a phonemic system. It thus pertains to an acquisition model. . . rather than to a perceptual model. . ." (99-101).

[95] The relationship between current research in developmental psycholinguistics and Putnam's account of the "division of linguistic labor" is discussed in more detail in "*D*eferential Semantics: Referential continuity through ontogenetic and diachronic lexical change." (Unpublished manuscript presented at the meeting of The Society for Philosophy and Psychology, Columbia University, June 2000.)

[96] This article entitled "A Note on the Paradox of Analysis" (1956) is a short but powerful exercise in what we would call today "anti-individualism." It is peppered with comments which aim to de-psychologize semantics: "A difference of the pragmatic value ['pragmatic contexts' are defined by Feyerabend as those involving "expressions such as 'know that' and 'believe that,' i.e., expressions which refer to situations of human beings"] of otherwise synonymous expressions does not imply a difference in meaning. . . . (92-93).

[97] Rieber (1992) suggests that it is indeed possible to doubt the substitutivity of synonymous words without thereby jeopardizing one's competence with them. Rieber asks: "Suppose that Tom would not assent to 'All lawyers are attorneys' not because he misunderstands one of these synonyms, but rather because he suspects that this statement admits of at least counter-example—although he cannot think of one. Tom understands 'lawyer' and 'attorney'; he just doubts that they are synonymous, or even co-extensive" (227). The burden, however, is on Tom to "expertly" substantiate such non-synonymy. In fact, if he were to do so, then the rest of us would be guilty of synonymously misusing the terms.

[98] See Kimbrough (1998) for an examination of some of the inconsistencies in Burge's Fregean presentation of "anti-individualism."

[99] In reviewing various solutions to his puzzle about belief, Kripke (1979) rejects the move to retract Pierre's original belief that London was pretty: "Nor does it have any plausibility to suppose, because of his later situation *after* he learns English, that Pierre should *retroactively* be judged *never* to have believed that London is pretty. To allow such *ex post facto* legislation would, as long as the future is uncertain, endanger our attribution of belief to all monolingual Frenchman. We would be forced to say that Marie, a monolingual who firmly and sincerely asserts, "*Londres est jolie*," may or may not believe that London is pretty depending upon later vicissitudes of her career. . ." (120, italics original).

However, this is precisely the sort of "retroactive belief revision" philosophers should expect; a result which flows directly from the sort of normative considerations which Putnam (1975) encourages in "The Meaning of 'Meaning'": Those medieval scientists who sincerely asserted that pyrite was gold, or whales fish—depending upon their hypothetical willingness to defer—either did or did not believe pyrite was gold, or whales fish. If they are deferential, then though they might have *said* pyrite was gold, they did not mean pyrite was gold for they did not fully understand what they said, and upon correction, might deny that they ever really believed pyrite was gold (since they incompletely understood what 'gold' referred to). If they are not deferential, then they really did believe pyrite was gold, but—since they obviously attribute a different referent to their homophonous term 'gold'—should be represented not as believing pyrite is 'gold' but (as Putnam and Burge would say) pyrite is *thgold*.. Marcus (1993) also speaks of "retroactively revisable belief claims": "Just as norms of truth lead to retroactively revisable knowledge claims, norms of rationality should lead to retroactively revisable belief claims. What is being proposed is that, whatever the psychological dimension of the belief state, on disclosure of impossibility that belief claim should be viewed as mistaken" (252).

[100] McDowell (1977) seemingly also objects to this sort of reasoning: "Even if everyone knew that Hesperus is Phosphorus, that would not make the two names differ merely as [linguistic, notational] objects; or, if it did, the names would have changed in sense. If someone used the two names indifferently in talking about Venus, so that we could find no interesting correlation between utterances containing 'Hesperus' and (say) beliefs formed in response to evening appearance of the planet, and between utterances containing 'Phosphorus and beliefs formed in response to its morning appearance then he would not be displaying competence in our use of 'the Evening Star' and 'the Morning Star'. . . . Such connections, between the use of a name and the sort of situation which prompts the beliefs it helps to express, can be, not merely idiosyncratic facts about individuals, but partly constitutive of a shared language. (155)

Chapter 11
Conclusion

Considerations related to the basis of natural language phonemic equivalence provide a route by which to re-approach traditional issues fundamental to contemporary philosophy of language. Appreciating the semantic basis of phonological equivalence allows the status of non-phonemic substitutions between synonyms and co-referring names to be re-evaluated from a different perspective. Substitutions between synonyms and co-referring names are clearly not licensed merely in virtue of non-interpreted form. But allophonic word-token substitutions are not grounded merely in virtue of non-interpreted form either. Instead, interpretive considerations of semantic content are critical to the justification of both basic phonemic substitutions as well as synonymous and co-naming substitutions. Given that a grounding in semantic content is necessary for both TK-substitutions and TP-substitutions, the issue then revolves around exactly what kind of semantic content is required to license TK-substitutions and TP-substitutions.

Both Frege and Quine challenge the individuation of semantic content, though each in their own way. Quine challenges a naïve individuation of the content of predicates based upon meaning. Frege challenges a naïve individuation of the content of names based merely upon reference. These challenges, however, have phonological implications. If synonymous predication is not to be distinguished from co-extensive predication, then tokens of two different but co-extensive predicates are not to be phonologically distinguished from tokens of one and the same predicate. If co-naming reference is insufficient to guarantee substitutivity within belief contexts, then co-naming

reference is presumably insufficient to guarantee phonological substitutions based upon reference.

On the one hand, TK-substitutions among word-tokens of the same predicate require an individuation content sensitive to meaning if the co-extensional predication among tokens of phonemically distinct but co-extensive predicates (e.g., [dap] and [daf]—the "minimal pair" analogue of [renate] and [cordate]) is to be distinguished from the co-meaning predication among tokens of the same type of word (e.g., [dap] and [dap^{h}]—the allophonic analogue of [renate] and [RENATE]). Natural language phonology recognizes a distinction between tokens of the same word and tokens of two different words, even if this distinction is premised upon a difference in meaning. Mere predication extension is semantically too coarse to capture the phonemic categorization of natural languages for the same reason mere predicate extension is blind to word-type synonymy. So much the worse for both phonology and analyticity, Quine might say. But whereas philosophers might be content to skeptically eliminate analytic equivalencies among synonymous word-types, consistent meaning eliminativism imposes a form of phonological eliminativism. "Phonological eliminativism" is the result of applying Quinean meaning skepticism to the phonological typing of natural language predicates subject to free allophonic variation.

On the other hand, TK-substitutions among word-tokens of the same name or common noun do not require an individuation content sensitive to Fregean senses in order for tokens of the same word-type to be categorized as tokens of the same type. [mat] and [mat^{h}] substitute freely in English phonology not because they happen to have the same Fregean sense, but because they have the same referent. Natural language

phonology does not recognize a phonemic distinction between tokens which might have the same sense and co-referring tokens which have only their reference in common. Phonology distinguishes between word-tokens which have the same referent and word-tokens which have a different referent—not between word-tokens which only differ in sense. Unlike Quinean meaning eliminativism, Fregean senses are semantically too fine to capture the phonemic categorization of natural languages for the same reason Fregean senses block co-naming word-type substitutions within belief contexts.

According to Fregean reasoning, co-naming TP-substitutions fail within belief contexts because, in addition to co-reference, names also require an equivalence of Fregean sense in order to freely substitute. This distinction between sense and reference, however, suggests that insofar as basic phonological substitutions are semantically conditioned, phonological substitutions must also be substantiated in terms of sense if they are not also going to provoke analogous failures of substitutivity. Applied to allophonic word-tokens, two word-tokens would have to have the same Fregean sense in order to be protected from substitutional doubt. But epistemically relativizing phonological substitutions to individual doubt does not safeguard phonological substitutions anymore than epistemically relativizing co-naming reference safeguards co-naming substitutions. Fregean senses are not so much a substantive explanation for substitutivity, but the mechanism by which to suspend substitutional standards within belief contexts.

But whereas philosophers might be content to acknowledge the failure of co-referring names to freely substitute within belief contexts, a consistent invocation of Fregean sense theory encourages another form of phonological eliminativism. Co-

referring word-tokens of the same word-type would be subject to substitutional failures. This version of "phonological eliminativism" is the result of imposing Fregean senses on the phonological typing of natural language names subject to free allophonic variation. In this way, the semantic basis of natural language phonology places constraints on a Quinean rejection of synonymy and a Fregean introduction of senses.

Both of these complications, however, would be averted if the thesis of "phonemic reductionism" were true. Neither Quinean nor Fregean reasoning would be answerable to phonology as the typing of word-tokens into word-types would be independent of both the semantics of meaning and the semantics of reference. Substitutions among word-tokens of the same word-type and substitutions among word-tokens from different word-types might very well run parallel, but they would travel on two very different tracks. Synonymous and co-naming substitutions would be indebted to semantic considerations, but at least phonology would be free of such concerns. Commitment to the thesis of "phonemic reductionism" allows Quine to attack the semantics of analytic equivalence without compromising phonology, while "phonemic reductionism" allows Fregeans to steer clear of issues related to the substitutional status of phonetically distinct word-tokens.

If phonemic categorization were independent of semantics, then Quinean issues of synonymy and Fregean issues co-naming reference could be kept localized to the philosophy of meaning and co-naming reference. Both Quinean arguments attacking synonymy and Fregean intuitions regarding co-referring names would stand on their own. Neither the Quinean dismissal of meaning nor the Fregean postulation of senses would be encumbered by complications issuing from the semantic basis of phonological

categorization. A non-semantic basis for phonological categorization would distinguish the phonology of word-token TK-substitutions from the philosophy of word-type TP-substitutions. Phonological categorization would more or less take care of itself without triggering Quinean concerns regarding synonymy nor Fregean concerns regarding co-naming reference.

Yet "phonemic reductionism" also has a history of its own, not only pre-dating Quine's explicit endorsement but intuitively conforming to most philosophical instincts. After all, phonology is just about the sound of words, and there is no apparent connection between the way words might sound and anything of philosophical interest. The purpose of this dissertation was to identify the connection between phonology and philosophy, and explore the philosophical repercussions of a semantically irreducible basis for phonological categorization. Given a semantic basis for phonological categorization, appeals to semantic content—co-predicate meaning to substantiate synonymous substitutions and co-reference to enforce co-naming substitutions—cannot be so easily dismissed as question-begging against Quine and Frege. Co-predicate synonymy and co-naming reference have a phonological life of their own independent of Quinean scruples and Fregean intuitions. This way of looking at things, however, contrasts with the much of contemporary analytic philosophy of language.

Distinct Word-Types do not Substitute Freely

On the one hand, Fregeans argue that co-naming reference is insufficient to guarantee substitution within belief contexts (as distinct senses may intervene). On the other hand, Quineans consider predicate meaning, as distinct from extensional reference, to be overly

sufficient to secure synonymous substitution. For Frege, co-naming reference is not enough to guarantee substitution *salva veritate*, while for Quine, co-predicate meaning is, as it were, more than enough to guarantee synonymous substitution. Much of twentieth-century analytic philosophy of language is conditioned by these two theses. Yet these two considerations, when combined, create a situation whereby *no* two distinct words, whether co-referential or synonymous, are, philosophically speaking, unproblematically equivalent, either for Fregean sense or Quinean eliminative reasons. There is, here, a thesis common to both Fregean and Quinean reasoning: insofar as two word-forms are recognized to be distinct word-types, their substitutional status is not logically guaranteed. Tautological versus non-tautological substitutions are what distinguish substitutions between [a] and [a] from co-naming substitutions among [a] and [b] (e.g., [Cicero] and [Tully]) as well as synonymous substitutions among [a] and [b] (e.g., [attorney] and [lawyer]).

In and of itself, this Fregean/Quinean observation is truistic. It needs to be supplemented with a thesis of phonemic reductionism, however, if this observation is going to be put to philosophical use. This is because if the basic distinction between tautological and non-tautological substitutions is to be made philosophically compelling, the basis by which tautological substitutions are themselves recognized as tautological becomes of philosophical interest. If tautological TK-substitutions among tokens of the same word-type are themselves conditioned by semantic considerations, the observation that non-tautological TP-substitutions among synonyms or co-referring names also require some sort of semantics loses much of its force. But tautological substitutions are just a reflection of our natural language phonological categories: [Cicero] and [Cicero],

like [attorney] and [attorney] (i.e., [a] and [a]), count as the same word-type, but [Cicero] and [Tully], like [attorney] and [lawyer] (i.e., [a] and [b]), do not. Yet in order to count as the same word-type, word-tokens need not be perceptually indistinguishable (like [Cicero] and [Cicero] are). They only need to be of the same phonological type. [Cicero] and [CICERO], though clearly different, are nevertheless freely intersubstitutable in English orthography.

The observation that TP-substitutions—unlike TK-substitutions—are not tautologically required fails take into account what the "phono-logical" conditions of word-token tautological TK-substitutivity actually are. For unless there is a philosophically principled reason for why substitutions between word-tokens of the same word-type are not burdened by philosophical problems like substitutions between word-tokens of two different word-types, it is not at all clear why there should be this difference, or ultimately, that there really is a philosophically important difference. Of course, co-referring names and synonyms are different word-types. And two word-tokens constitute two different word-types if they are not composed of the same phonemes. But the phonemic typing of phones is itself dependent upon semantic considerations: two phones constitute the same phoneme, at least in cases of free allophonic variation, when the phonetic distinction is semantically neutral and are phonemically distinct when the phonetic distinction is semantically significant. None of free allophonic word-tokens, synonyms or co-referring names is substitutionally equivalent in virtue of what they sound or look like. At least with respect to phonemic reductionism, synonymous and co-naming substitutions are no different than allophonic substitutions.

The failure of phonemic reductionism suggests that the philosophical rationale guiding the evaluation of synonymous and co-naming substitutions is premised upon a misleading distinction. The distinction between non-phonemic TP-substitutions among co-referring names and synonyms on the one hand, and phonemic TK-substitutions among allophonic variants on the other, does not coincide with a distinction between substitutions indebted to semantic considerations and substitutions not indebted to semantic considerations. But if this is the case, the philosophical burden of introducing meaning to secure synonymy, or mere reference to secure co-naming substitutions, shifts. Once the semantic cat is out of the bag—co-meaning for synonymous word-tokens and co-reference for co-naming word-tokens—an additional argument is needed to selectively put it back in for synonymous and co-naming TP-substitutions. The argument that phonology is not dependent upon semantics is short-circuited by the failure of phonemic reductionism. The Quinean argument that co-extensive predication is predication enough is counter-indicated by phonemically distinct but co-extensive predicates. The Fregean argument that co-referentiality is not enough content to secure belief-context substitutivity is counter-indicated by distinct but allophonically related word-tokens.

The claim, then, that no two different words (whether co-naming or synonymous) are unproblematically equivalent is itself premised upon a false dichotomy. The claim assumes that, by contrast, at least the phonological equivalence of distinct word-tokens of the same word-type is unproblematic. But even basic phonological categories do not come for free. So while Quine's critique of analyticity is based upon a rejection of meaning, and Frege's invocation of sense is based upon the failure of co-referring names to freely substitute in belief contexts, both Quine and Frege adhere to the intuition that at

least some substitutions—the "phono-logical"—are licensed in virtue of non-interpretive form.

The observation that phonology is semantically conditioned does not, however, prove that co-referring names are freely substitutable within belief contexts in virtue of their co-referentiality. Nor does the observation that phonology is semantically conditioned prove that synonyms are synonymous in virtue of their co-meaning. Rather, the claim that phonology is dependent upon semantic considerations of co-predicate meaning and co-naming reference indicates that philosophers of language have overlooked an important parallel between substitutions involving the same word and substitutions involving different (but co-referring and synonymous) words.

The failure of phonemic reductionism serves to neutralize the philosophical claim that two distinct linguistic forms—merely in virtue of being distinct—do not freely substitute. Allophonically distinct linguistic forms substitute freely and the failure of phonemic reductionism guarantees that semantic considerations of co-naming reference and predicate meaning are implicated in the language-specific organization of natural language phonology.

The observation that phonological substitutions are semantically conditioned is a little like finding a small depression on the beaches of analytic philosophy of language. With respect to Quine, the philosophical landscape can be re-groomed to appear more smoothly consistent by challenging the semantic basis of natural language phonology. Given Quine's analytic eliminativism, phonemic reductionism becomes an internally generated thesis of Quinean consistency. There are even a variety of reductionist strategies to choose from: acoustic, phonetic, distributional, or behavioral data can be

collected to expose meaning as an unnecessary crutch rather than an integral component of phonemic analysis.

But a smoother beech can, at least in theory, be re-engineered by pursuing a different strategy. Rather than attempt to phonetically regularize or behavioristically neutralize the depression caused free allophonic variation, one may instead excavate the entire philosophical landscape down to the level of the depression itself. There are two ways to respond to an indentation on the surface of one's philosophy: attempt to fill up the hole and move on, or actually lower the surrounding landscape.

Planted at the bottom of this hole created by free allophonic variation, the purpose of the present dissertation was to argue that there is philosophical parity between TK-substitutions and TP-substitutions: either phonological substitutions are just as problematic as co-naming and synonymous substitutions, or co-naming and synonymous substitutions are just as unproblematic as phonological substitutions between allophonic realizations of one and the same word. Phonetically distinct allophonic word-tokens are substitutionally equivalent on the basis of semantic considerations; phonemically distinct synonyms or co-referring names are substitutionally equivalent on the basis of semantic considerations. Given that our everyday phonemic intuitions are ultimately dependent upon semantic considerations, semantic content plays a foundational role in justifying linguistic equivalence. Following Sellars, one may even hierarchically construct abstract "dot quote"-types by analogy to abstract phonemic word-types: [mat] and [mat^{h}] (and [hot] and [hot^{h}]) are phonemically categorized as "slash bar" /mat/ (and /hot/) and immune to substitutional skepticism or meaning eliminativism because they are one and the "same word"; /Tully/ and /Cicero/, like /attorney/ and /lawyer/, are impartially

represented as "dot quote" •attorney• and "dot quote" •Cicero•. Co-referring names and synonyms are notationaly immunized against substitutional skepticism or meaning eliminativism—they are the "same dot quote." For that matter, though the "dot quote" for /Tully/ and /Cicero/ may be arbitrarily represented as either •Cicero• or •Tully•, in some cases the most perspicuous dot quote representation may be neither. Taking a card from Church's (1954) deck, the most natural way to give "dot quote" expression to the co-naming /Hesperus/ and /Phosphorus/ would be not be •Hesperus• *or* •Phosphorus•, but •Venus• (see note 10).

It might be objected, however, that putting things this way is to beg two important questions: one dealing with the existence of abstract types (whether phonemic or "dot quote") and the other with a distinction between Fregean co-naming belief-context doubt and Quinean synonymous skepticism.

The Status of Abstract Linguistic Types

Taking up the first issue, what has concerned us is not the metaphysical status of linguistic types, but merely a normative accounting of our substititional practices. Abstract linguistic types are a symptom of linguistic substitutions; they are not the cause of linguistic substitutions. Allophones, for instance, are not phonemically equivalent because they are the same phonemic type, or exemplify the same phonemic type. Rather, allophonic word variants are categorized as the same phonemic type because they are substitutionally equivalent on the basis of their acoustic, phonetic or distributional structure—or as with free allophones—their semantic content. In any event, phonemic types are language-specific. Language-specificity does not sit well with metaphysical

claims. But two phonemically distinct but co-referring names or synonyms are not categorized as equivalent because they are the same "dot quote" either. Rather, they are the same "dot quote"-type because—when properly used—they are substitutionally equivalent.

So while the construction of a single "dot quote"-type spanning two phonemically distinct words serves to dramatize the claim that substitutions between co-referring names cannot be doubted or synonyms challenged (since they are notational variants of one and the same abstract "dot quote" type), one need not attribute any independent ontological status to abstract dot quotes—or, for that matter, to abstract phonemic types. One need not hypostatize abstract linguistic types in order to recognize that some phonetically distinct word-tokens are phonemically "slash bar" equivalent, nor to propose that co-referring names and synonymous word pairs are "dot quote" equivalent. Sellarsian "dot quote"-types—like phonemic "slash bar" word-types—are a notational convenience. Of course the convenience of "slash bar"-types intuitively reflects the phonological truism that phonetically distinct word-tokens group into natural language phonemic types. But this does not philosophically distinguish "slash bar"-types from "dot quote"-types.

Names versus Synonyms

Second, it may also be objected that there is an important distinction between Fregean considerations of co-naming doubt within belief contexts and Quinean reservations regarding synonymy, particularly when Sellars uses his "dot quotation" to equate only synonyms, not co-referring names.

Quine objects to synonymy because in order to distinguish co-meaning predicates from co-extensional predicates, a distinction between contingent co-predication and necessary co-predication must be made. The property of having a kidney and the property of having a heart may very well be different, but this distinction cannot be made without relying on the possibility that the objects that happen to be renates could be extensionally distinct from the objects that happen to be cordates. Everything which the term /cordate/ applies to, the term /renate/ also applies to. But everything which the term /attorney/ applies to, the term /lawyer/ also applies to. But unlike the former, synonyms are not contingently co-applying predicates; synonyms are necessarily co-applying predicates. But clearly this distinction is not made merely on the basis of observations regarding what is and what is not truly called an /attorney/ and a /lawyer/ versus what is and is not truly called a /renate/ and a /cordate/.

Instead, such differences require a distinction between linguistically redundant, or analytically related expressions, and expressions which are not linguistically redundant but synthetically related. The distinction requires that some terms be identified as linguistically superfluous, as with notational abbreviations where one expression is an abbreviation of the other, or stipulatively equivalent where neither expression is an abbreviation of the other. And to be sure Quine (1951) is not averse to this sort of weak synonymy: "Here the definiendum becomes synonymous with the definiens simply because it has been created expressly for the purpose of being synonymous with the definiens" (26). But Quine is surely right to point out that the "linguistic redundancy" of synonymous predication—as opposed to coincidental convergence predication—is not to be found merely through an analysis of true predication.

The Quinean dissolution of a traditional analytic/synthetic distinction challenges philosophers to explain such differences without appealing to the intensionalism of necessity and the possible worlds of counter-factual conditionals. But a philosophically less ambitious account of necessity merely preserves the possibility that language users are willing to deferentially accommodate semantic redundancies—either in the form of co-referring names or co-meaning predicates. Necessity is not a worldly phenomenon; necessity is a *wordly* phenomenon.

So while Quine concedes that truth is, in some sense, dependent upon both language and extra-linguistic fact, Quine does not want to allow some truths to be true merely in virtue of language. In "Two Dogmas," Quine writes:

> It is obvious that truth in general depends on both language and extralinguistic fact. The statement 'Brutus Killed Caesar' would be false if the world had been different in certain ways, but it would also be false if the word 'killed' happened rather to have the sense of 'begat'. Thus one is tempted to suppose in general that the truth of a statement is somehow analyzable into a linguistic component and a factual component. Given this supposition, it next seems reasonable that in some statements the factual component should be null; and these are the analytic statements. (36-37)

But if the word /Brutus/ can refer to Brutus and the word /kill/ to the act of killing, why can't some other predicate also be related to the act of killing in the same way that /kill/ is? If the word /Cicero/, for instance, can be linguistically associated with Cicero, and the predicate /attorney/ can be linguistically associated with the property of being an attorney, there is no reason in principle why Cicero can't also be linguistically associated with the word /Tully/, and the property of being an attorney can't be also be associated with the word /lawyer/. But redundancies of this sort do not need to be interpreted as metaphysically revealing. Kripke, for instance, does not interpret synonymous predication as another example of an *a posteriori* discovery of a necessary truth. But nor

do such redundancies issue from epistemic considerations of what seemingly competent speakers consider possible to doubt without appearing to contradict themselves. Instead, synonymy is interpreted as a linguistic requirement issuing from linguistic redundancies—typically not of our own making—but nonetheless linguistically obligatory. Both the double naming of properties and the double naming of objects should be divorced from both metaphysical and epistemic considerations. Names refer to objects and predicates express properties; co-referring names refer to the same object and co-meaning predicates express the same property.

But of course Quine is nor more keen on the linguistic redundancy of synonymy than the linguistic redundancy of co-naming. Here is how Quine (1966) paraphrases Ruth Barcan Marcus:

> Her paradigm of the assigning of proper names is tagging. We may tag the planet Venus, some fine evening, with the proper name 'Hesperus'. We may tag the same planet again, some day before sunset, with the proper name 'Phosphorus'. . . (181-182)[101]

Nor, of course, is Quine comfortable with considering synonyms to be "stipulatively" equivalent. "Who defined it thus, and when?" asks Quine (1951, 24). But other philosophers are. Church (1954), for instance, suggests that synonymy is perhaps better viewed as a dispensable "linguistic luxury"—the result of labeling the same property twice over. (In fact, parallel to the dispensable "linguistic luxury" of synonymy, Church (1989) has more recently considered the possibility of removing redundant co-naming "primitive" expressions: "Languages are of course possible within which no two primitive constants denote the same thing. For given any primitive constant we may delete from the language all but one of the class of primitive constants that are concurrent with it" (165).)

Following Church (1954), Sellars (1955) and Feyerabend (1956), Kripke (1979) defends the linguistic redundancy of synonymy. Unlike doubting whether all cordates are renates, doubting whether all attorneys are lawyers is not a genuine possibility. If one does not apply, or is unwilling to apply, the predicate /attorney/ to that which one applies the predicate /lawyer/ to, then one is not fully competent with the use of these predicates. According to these philosophers, synonymy is not subject to doubt. Synonymous doubt is not a genuine possibility because synonymous doubt succeeds in only revealing some measure of incomplete linguistic understanding or linguistic deviance. Resisting the Matesian temptation to "psychologize" synonymy, synonymy is enforced as a linguistic requirement. In this way, the alleged possibility of synonymous doubt is philosophically dismissed as reflecting a misguided conflation of epistemology and semantics.

Of course Quineans are not impressed by such reasoning. "Two Dogmas" is designed to blur the distinction between the semantics of so-called linguistic truth and the epistemology of empirical truth. Nor, however, are Fregeans impressed by such reasoning. Fregeans rely on the epistemic possibility of doubting whether Cicero is Tully to necessitate the existence of senses to semantically distinguish co-referring names. But this too represents a conflation between epistemology and the semantics of language. In fact, standards of linguistic competence can—and should—be used to dismiss the genuine possibility of co-naming doubt just as Kripke dismisses synonymous doubt. If the strict substitutivity of synonymy can survive doubt, the strict substitutivity co-referring names can also survive doubt. Not only would belief contexts be synonymously transparent, substitutions between co-referring names would be also be deferentially transparent. But Quine is no more interested in defending synonymy than he is making

belief contexts transparent to co-naming substitutions. This is because Quine is no more interested in securing synonymous substitution than he is interested in securing co-naming substitutions in belief or other opacity inducing contexts. For while synonymous substitutions are, as it were, intrinsically intensional, co-naming substitutions behave intensionally when embedded within contexts of belief. But whether intrinsically or extrinsically generated, Quine views all forms of intensionalism with eliminative suspicion. Indeed, this is why Quine turned his attention to phonology in the first place—to bring phonology back from the brink of meaning. And this is a consistent strategy. It's just that phonemic reductionism is unsuccessful.

What I find inconsistent, then, is not Quine's views on synonymy and belief, but Kripke's anti-Mates position regarding the belief-context transparency of synonymous substitution on the one hand versus his Fregean-friendly position regarding the belief context opacity of co-naming substitutions on the other. If synonymous doubt is indicative of linguistic deviancy because the proper use of synonyms is answerable to normative linguistic constraints, then the only way for co-naming doubt not to also be indicative of linguistic deviancy is for the proper use of names not to be answerable to normative linguistic constraints. But names, no less than predicates, are subject to norms. If the linguistics of synonymy can be divorced from the epistemology of doubt, so too can the dynamic of double naming. Fregean senses are no longer needed as the semantics of co-naming is freed from the epistemology of doubt.

Broader Philosophical Considerations

There are, then, a number of other philosophical considerations—independent of Quine—which point toward the free substitution of co-referring names. The substitutional status of co-referring names in belief contexts can be placed in broader philosophical context. On the one hand, philosophers like Church (1954) and Kripke (1979) argue that belief contexts are not barriers for the substitution of synonyms. On the other hand, Kripke (1980) has argued that modal contexts are not barriers for the substitution of co-referring names (nor presumably synonyms). Combined, these two considerations suggest that attempts to derive semantic conclusions from epistemic considerations may be as philosophically misguided as the attempt to derive metaphysical conclusions, as Kripke (1980) has argued, from epistemic considerations. The belief, for instance, that something that looks just like Tully might not be Cicero is not relevant to the so-called possibility that Cicero might not be Tully.

So whereas Kripke (1979) defends the strict substitutivity of synonyms in belief contexts, and Kripke (1980) focuses on the metaphysical repercussions of rigid designation, Kripke has avoided the confluence of his anti-Mates position on belief contexts and his anti-Fregean position on names. In fact, in "A Puzzle About Belief", Kripke (1979) reminds us that "*Naming and Necessity* never asserted a [co-naming] substitutivity principle for epistemic contexts" (139). But given that belief contexts need not block synonymous substitution and modal contexts need not block co-naming substitution, much of the present dissertation has focused on the apparent failure of co-referring names to freely substitute in belief contexts. Given (i) the substitution of synonyms is not blocked in belief contexts; (ii) the substitution of co-referring names and

synonyms is not blocked in modal contexts, and (iii) phonemically equivalent word-token substitutions are not blocked in any contexts (save perhaps hyper-quotational), the present dissertation has asked: Why should co-referring names be this odd man out? What is the argument that selectively makes co-referring names ineligible for substitution *salva veritate*?

Note that the argument cannot merely be that co-referring names are distinct types of words. Synonyms are phonemically distinct word-types, but this does not disable their substitution in belief contexts. Indeed, insofar as the failure of phonemic reductionism covers both synonyms and names, there must be some other reason to distinguish synonymous substitutions from co-naming substitutions. Since tokens of both co-referring names and synonyms are phonemically distinct word-types, the argument that synonymous and co-naming substitutions are not indubitable in virtue of being different word-types does not, in itself, provide the leverage by which to selectively save only synonyms from substitutional doubt. Yet there is a relevant difference between co-referring names and synonyms. One could argue that—unlike synonyms—full competence with co-referring names simply does not require that they be used interchangeably. In this way, the argument can be narrowed so that only synonymous substitution is a "linguistic requirement" issuing from linguistic meaning.

The second reason for permitting substitutional failures, then, is specifically tailored to accommodate the Fregean failure of co-referring names in belief contexts. Unlike the substitution of synonyms, the substitution of co-referring names can be doubted with linguistic impunity. But this is not so obviously true. One cannot convincingly claim to be "fully" competent with the name /Tully/ yet when confronted by

Cicero refuse to refer to him equally with either /Cicero/ or /Tully/. After all, /Cicero/ and /Tully/ are just different names for one and the same person. /Cicero/ cannot be Tully's "name" only when he is addressing the senate (or something like that), if the Fregean postulation of descriptive senses is not to be question begging. If we assume that /Cicero/ is Tully's name only under certain circumstances, then clearly /Cicero/ does not "directly" refer to Tully. But traditional Fregean puzzles are intended to motivate the existence of senses—not by assuming that names are not directly referring devices—but by tentatively assuming that /Cicero/ and /Tully/ really are just "Millian" labels, and then deriving some sort of counter-intuitive puzzle that can be resolved only by the postulation of senses.

But there is an alternative way of explaining—or, rather, explaining away—such Fregean puzzles. The thesis that reference exhausts the semantic content of names cannot be refuted by considering cases where the speaker restrictively under-extends the names. Instead, one may move to question the linguistic competence of those who do not, or more importantly are unwilling to, refer to Cicero with both /Tully/ and /Cicero/. Both parties to this debate, the Fregean and the non-Fregean, agree that Tully has been named twice. What divides them is the explanation of how this could have gone unnoticed. Fregeans introduce the psychology of senses to explain the informative realization that /Tully/ and /Cicero/ are co-referring. But the divergent application of co-referring names can be also be accounted for by questioning the linguistic competence of those who do not use /Tully/ to also refer to Cicero. Those who do not use /Cicero/ and /Tully/ interchangeably do not so easily pass as fully competent. In fact, this is precisely

the move Church (1954) and Kripke (1979) make in order to protect the synonymous substitution of co-meaning predicates within belief contexts.

The Philosophy of Phonemic Reductionism

Independent of these broader philosophical considerations, however, it is important not to lose sight of the classic attempt within linguistics proper to substantiate the thesis of phonemic reductionism. Guided by an empiricist philosophy realized by a descriptivist methodology, phonemic reductionism characterized a significant, though by no means uncontroversial, strain within twentieth-century American linguistics. In addition to examining the philosophical implications of the failure of phonemic reductionism vis-à-vis Quinean and Fregean philosophy of language, the semantic basis of phonemic categorization was historically surveyed in connection to the positivist-minded linguistic program of "neo-Bloomfieldian" linguistics.

The failure of phonemic reductionism was also developmentally charted with respect to infant speech perception in chapter 8, and notationally extrapolated to incorporate a discussion of Wilfrid Sellars' dot quotation in chapter 9. A concluding chapter explored the mutually constraining dynamic between words and their referents. Not only can semantic information external to the speech stream guide the categorization of language, word labels internal to the speech stream can itself guide the categorization of objects and properties. The semantic-basis of allophonic speech perception is an example at the phonemic level of non-linguistic categories influencing the classification of phonetically distinct speech-tokens. The word labeling basis of object categorization

is an example of phonemic word categories influencing the classification of token distinct objects into their type-equivalent object categories.

But these processes can be cyclically integrated to guide the re-categorization of language on the one hand, and the re-categorization of objects and properties on the other. Sellars' use of dot quotation indicates how "functional" information regarding idealized word usage can be exploited to guide the formation of abstract "dot quote" linguistic types transcending phonemic boundaries among synonyms and translations. But information in the form of proper or expert use of names can also be exploited to guide the formation of abstract "dot quote" linguistic types among co-referring names. So while Sellars originally designed dot quotation to capture only the meaning equivalence of synonyms, insofar as this equivalence is a prescriptive claim regarding proper usage (rather than a descriptive claim regarding actual usage), this restriction of dot quotation to synonyms is not followed. ("Dot quotes" are not applied, however, to co-referring descriptions, see note 15.) The proper or expert practice of freely substituting the names /Cicero/ for /Tully/, /Hesperus/ for /Phosphorus/, and */Londres/* for /London/, etc. can be deferentially exploited by the rest of us in the same way that the proper use of /attorney/ and /lawyer/ is used by Sellars to substantiate their dot quote equivalence.

It is clear we are, in some sense, linguistically responsible for labeling Tully with /Tully/ because that is his name. Yet if /Cicero/ is one of his other names, it would seem that we are also linguistically responsible for labeling Tully with /Cicero/. What is the philosophical motivation for drawing a distinction between /Tully/ labeling Tully and /Cicero/ labeling Tully? What's more, even granting that the sense of /Tully/ is different

than the sense of /Cicero/, how the unique sense associated with /Tully/ gets associated with /Tully/ rather than some other word either still demands an explanation, or is taken as primitive. In order to avoid an infinite regress, however, even Fregean senses must eventually be "directly" associated with the words that uniquely express these senses (insofar as it is senses names express). At some point, then, the sense of /Tully/ gets paired with /Tully/. But this kind of connection is just the sort of naïvely brute "tagging" connection that direct reference theorists are so often ridiculed for invoking.

So whether it is senses that get "directly" associated with names or referents that get "directly" associated with names, it is not the "direct" in direct reference that distinguishes Fregeans from non-Fregeans. Instead, it is what is directly referred to by our words: Fregeans would have it that what is referred to must be something intrinsically already within our epistemic grasp (something like what Russellian logically proper names refer to) while non-Fregeans would have it that what is referred to merely be within our epistemic reach construed very broadly. This extended semantic horizon allows us to refer to people, places, and things without knowing much about these objects. It even allows us to refer to people, places, and things without any transparent awareness that we have indeed successfully referred to such-and-such a thing. Sometimes these interconnected links of reference transmission are communicatively very helpful; other times they can land us in a tangled web of confusion as in when intending to refer to ten days we say /fortnight/. But this is, in principle, no different than over-extending the word /fourteen/ to refer to ten. And this is, in principle, no different than under-extending the word /Cicero/ or /London/ to apply to their referents only under certain circumstances. An interest in referring to things "beyond our own minds"

demands a deferential commitment to not always knowing what we have said—nor all the alternative ways of saying it. The discovery that /Cicero/ and /Tully/ are co-referring names need not be interpreted as an indication that names are not really names. The startling discovery that two names are in fact co-referring may instead be interpreted as an indication that we prefer the liability of misusing even our own words in lieu of continuously gerrymandering their meaning so as to preserve a perfected sense of rationality. [102] Pierre is not irrational, he's just confused about what /London/ and */Londres/* refer to. Pierre thinks that /London/ and */Londres/* do not name the same city. What we ask—and what we typically get—from someone like Pierre is deferential correction, not some sort of a "But I thought that /London/ only referred to *this* part of London" justification. What we get out of Pierre is the concession that he was incorrectly under-extending the use of /London/, and perhaps a promise to try to use /London/ and */Londres/* interchangeably. What we get out of Pierre is the confession that he never intended to cause so much confusion among philosophers simply by misusing a couple of names.

Like synonyms and allophonically related word-tokens, then, co-referring names can be viewed as just another example of the kind of linguistic redundancy found in natural language—instances where, as Kripke (1979) remarks, a language really "just has two names for the same" (146) thing. Linguistically obligatory substitutions occur not only with synonymous predicates and allophonic variants, but also with co-referring names. If [Cicero] and [CICERO], like [mat] and [mat^{h}], can be strictly equivalent (regardless of individual epistemic considerations), and synonymous terms can be strictly equivalent (again, regardless of individual epistemic considerations), then co-referring

names can also be viewed as strictly equivalent (indeed "dot quote" identical), independent of epistemic considerations regarding what we may or may not have taken ourselves to individualistically believe.

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[101] The original passage from Marcus (1961) reads:

> That any language must countenance some entities as things would appear to be a precondition for language. But this is not to say that experience is given to us as a collection of things, for it would appear that there are cultural variations and accompanying linguistic variations as to what sorts of entities are so singled out. It would also appear to be a precondition of language that the singling out of an entity as a thing is accompanied by many—and perhaps an indefinite or infinite number—of unique descriptions, for otherwise how could it be singled out? But to give a thing a proper name is different from giving a unique description. . . . Perhaps I should mention that I am not unaware of the awful simplicity of the tagging procedure. . . But my point is only to distinguish tagging from describing, proper names from descriptions. You may describe Venus as the evening star and I may describe Venus as the morning star, and we may both be surprised that as an empirical fact, the same thing is being described. But it is not an empirical fact that [Venus is identical to Venus] and if '*a*' is another proper name for Venus [Venus is identical to *a*]. (310)

[102] In arguing that belief cannot always be read directly from sincere assent, Santambrogio offers the following analogy between people and measuring devices:

> The analogy between people and measuring devices is, I think, quite close. Incidentally, it shows what is wrong with the idea, cherished by some philosophers, of accounting for . . . behavior in terms of modes of presentation and that belief is really a three-place relation, relating persons, propositions, and modes of presentation. In regarding belief as such, these philosophers represent a person's utterances as infallibly revealing what a person believes. Whenever Ralph, for example, both assents to 'Mount Everest is snowy' and puzzles over 'Mount Gaurisanker is snowy', they account for this behavior by describing Ralph as believing Mount Everest to be snowy under some mode of presentation (somehow related with the name 'Everest') and at the same time not believing that Mount Everest, that is, Gaurisanker, is snowy under some other mode of presentation (related with 'Gaurisanker'). Everything that Ralph says and does is thus taken into account and is given as much weight as possible. The price they must pay, however, is jettisoning the ordinary notion of belief and inadvertently substituting it with the quite different notion of belief-under-a-mode-of-presentation. The analogue for this move for measuring devices would be regarding a measuring device as being invariably reliable by tinkering with the magnitude it is supposed to be measuring. Consider a police speedometer. Instead of admitting that its reliability is subject to severe constraints, one could take it to be measuring the speed of a car under particular modes of presentation—at dark, at night, in heavy traffic, and so on. Surely, this would turn the speedometer into an infallible device, but one whose use was no longer clear. (645-646)

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