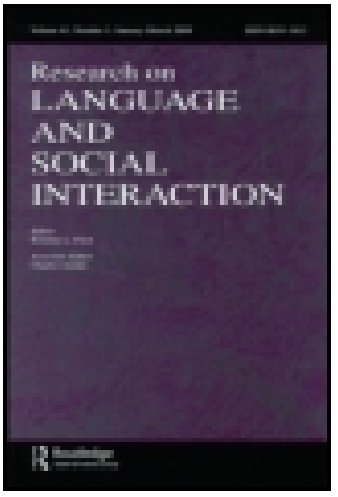

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On the empirical content of the theory of lexical insertion

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On the Empirical Content of the Theory Of Lexical Insertion*

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I. Recently, I have taken up the study of words, elusive things which we use all the time without quite knowing what they are. The first edition of the Encyclopedia Britannica claimed that they are the minimal units of speech which bear meaning, which is the same as our modern definition of the "morpheme." The major fact about words is that sentences (the things we linguists like to talk about since we don't really care about language very much) are (in some sense) composed of them. Those who propose that sentences are ultimately composed not of words, but of morphemes, should ponder the fact that while one could easily answer the question "What does detain mean?", one cannot so easily answer either "What does tain mean?" or "What does de mean?"

Interest in words, in the lexicon as the word inventory has come to be called, is relatively recent. In early transformational linguistics words were treated as ordinary formatives to be stuck unto nodes much as category labels are, and there was not much interest in them per se. When attempts were at long last made to formulate what we might term serious

theories of semantics, the lexicon had to be treated as a separate subcomponent of the grammar, and, specifically, of the semantic component of the grammar. It is obvious that the "meaning" of a sentence depends in some arcane way on the "meanings" of the words comprising it: sentences with different words in them tend to mean different things. It is of course true that sometimes sentences with different words in them nonetheless mean the same thing, and equally perversely, sometimes sentences with the same words in them mean different things. When these facts were first seriously considered, they seemed very confusing and necessitated changes in the theory of transformational linguistics which were by no means simple ones.

Presently there are at least two approaches to the problem of semantics (and particularly lexical semantics) in transformational linguistics: the older, interpretive semantics, initiated in 1963 by Katz and Fodor's paper in Language, has developed at M.I.T. into a more powerful theory under the rubric of "lexicalism"; the newer, which came out of Jeffery Gruber's 1965 M.I.T. dissertation, and which has further been developed by Fillmore, Postal, Ross, Lakoff, McCawley, and Perlmutter, among others, is called generative semantics, although I prefer Gruber's name deriva-

tional semantics if only because "generative" semantics has little to do with generation.

The characterization of the lexicon, and hence of words, is, in each of these theories, obviously dependent on the view that each takes of the nature of meaning in general. The systematic characterization of the vocabulary of a natural language or of Natural Language in general has accordingly been tied up closely with the requirements and results of the specific linguistic theoretical context in which it is embedded. As a result this characterization has become an issue in the current lexicalism-derivational semantics debate, especially since the derivational semanticists have attempted to integrate syntax and semantics into a single "semantax", and, further, have tried to integrate the derivation of lexical items with that of sentences.

This great debate has at times been acerbic, sending off much heat but little light; the lexicalists have drawn conclusions from the existence of deep structure which the derivationalists have objected to in the name of "linguistic reality;" at the same time the derivationalists have drawn equally powerful conclusions from its absence which the lexicalists object to likewise in the name of "linguistic reality." Each side marshals evidence which the other claims to be un-

acceptable, and each counters the other's conclusive arguments with equally conclusive counter-arguments which serve, however, to convince no one.

Some linguists have concluded that no conclusive evidence either way can be found. Drawing from Ritchie and Peters' proof that transformational grammars are equivalent in power to Turing machines, that is, unrestricted re-writing systems, Emmon Bach has concluded (according to some recent presentations of his) that transformational grammar is, at present, too vague in formal terms, too weakly constrained, for actual data to be brought to bear crucially in deciding controversies formulated in transformational terms. It is not clear that we must conclude from this that the current debate is doomed to futility. Indeed this debate has spurred on the discovery of many new facts (which can be used as ammunition by one side or the other). Nonetheless, one is uncomfortable with a potentially undecidable issue, since no research can be carried on in a theoretical vacuum, and sooner or later, whatever one's doubts, a stand must be made by each of us.

There is then a practical problem for the lexicologist, who ultimately must decide whether his view of what words are like fits better into the views of the

one school of thought or the other. There is in addition the problem of whether the theory of lexical insertion, or more generally, that of the lexicon, interacts seriously with the issues in the lexicalist-derivationalist debate. The relationship of much lexical research to the debate, or its relevance to the debate, is by no means clear. Even when one poses a question in the framework of a certain theory the question need not be an artifact of that theory (in which case it can hardly be a crucial point), nor need it be a crucial empirical point in terms of the debate. It may merely be irrelevant or neutral to the argument.

II. For there are some important questions about the lexicon upon which we have useful intuitions. These fall roughly into three groups. The first is concerned with our intuitions about the possible words of our language or indeed of any human language; the second concerns our intuitions about the intertranslatibility of words in different languages or indeed in the same language (by which I refer to synonymy); and the third concerns our intuitions about the relationship of semantics and syntax, and in particular about the category membership of lexical items. It often sounds as if lexical semantics is concerned only with

the formal content of the lexical entry or with the search for the optimal lexical component of a grammar, but in fact there are substantive empirical questions which can be raised, even if (as is likely) they are neutral to the debate. While it may appear that we know a great deal about words, this is not true; we have barely been able to utilize our intuitive store of knowledge about language in this area.

Our innate knowledge of the possible words of our language is a case in point. Superficially, languages differ most clearly in their vocabularies. While it is possible to find substantive universals in the areas of phonology and syntax, as Greenberg and others have done, non-trival substantive universals in the area of lexicology have not been forthcoming. Between any two languages we find a tremendous number of apparently random differences in vocabulary. I am not just talking of the absence of equivalent terms, but also of differences in the relationship between terms. The English speaker is bewildered by the existence of two verbs in Spanish for '<u>be</u>' and two for '<u>have</u>'; many languages do without our copular verb; we do without any regular causative affixation; negatives of verbs in Japanese are morphologically adjectives; many Latin verbs must be translated into English as adjectives; and so on.

One is hard put to see how lexical universals can be found. And yet we have intuitions not only about what the words of our language can be, but what the words of any language could be. From the nature of the lexicon it follows that these intuitions fall into two groups: if morphemes are the minimal units bearing "meaning," then the set of possible "meanings" conveyed by words is a function first of the possible meanings of morphemes, and secondly, of the possible combinations of morphemes into words.

It has long been noted that there are constraints on the combination of morphemes, and thence that there are constraints on the word-set of a language defined in morphological terms. But it has not been so obvious that there are constraints on the meanings of morphemes and hence on those of words, or on what a word of a given morphological shape could mean. The relationship between the meaning of words and those of the morphemes comprising them is not well understood. To say that there is no relationship is, I think, not only to deny our obvious intuitions, but also to reject all of past morphological research. For we can recognize the morphological construction of a word only because (and precisely because) of our intuitions about the meanings of morphemes and of words composed of those

morphemes.

The meanings of morphemes are by no means atomic elements, semantic primes: this is clear because of the synonmy of some morphemes with words which are morphologically complex. If there is any regularity to the meaning of morphemes, it is on the level of semantic primes and not of morphological entities. Such semantic primes may have little relationship to the lexical items used to express them: for example, even though it has been claimed that be and not represent two primes, it is false that the verb be has a unique meaning, and also false that negation is only expressed in English by the word not. The derivation of morphemes from structured sets of semantic primes is a complex process.

The derivation of words from morphemes is constrained not only by restrictions on co-occurence of morphemes, but on co-occurence of the meanings they represent. We know a great deal informally about the cryptotypes (semantic primes) and phenotypes (morpheme-borne semantic complexes) of a language and how they combine to form greater units on larger levels. But one question arises which we have no direct intuitions about: traditionally the derivation of words from morphemes was thought to be a separate process from the derivation of sentences from words. But if

the structures from which the sentences are derived are semantic representations, then it may be the case that words are automatic consequences of the derivation of morphemes, which itself is the automatic consequence of the derivation of sentences.

Moreover, until we have investigated the morpheme and the word further, not only can we not have a serious theory of the lexicon, but many interesting problems of intertranslatibility cannot be considered. Nor can the most interesting of our intuitions about words be used; we know a great deal about what syntactic properties an item will have, once we know what the item means, but using these intuitions involves having a far better theory of lexical semantics than we currently have.

A further question arises in regard to these considerations, namely what a word is, and why there should be such things. What function do they serve that could not be met by morphemes alone? Why are words composed of morphemes, instead of, for example, partly morphemic and partly phonemic material? We are so familiar with the word that we never stop to think that the word is actually a very strange animal indeed, just like the sentence. (For after all, why should there be sentences? What function do <u>they</u> serve? Why

should they be composed of words and why should they have the form they do?) One can imagine languages wildly different from either natural languages or the artificial languages mathematicians create, yet such languages are felt to be very strange indeed: why?

It may seem that these are metaphysical questions, vague, unformalizable, outside the ken of mortal linguist. Or it may be felt that they more properly belong in the domain of the psychologist or philosopher than in that of the grammarian. But this is wrong. For all lexical research to date which attempts to comprehend our intuitive knowledge of lexicology and not merely to fit the vocabulary into a neat little formalism is nothing more than an attempt to, if perhaps not answer, at least raise and investigate these questions. In particular the theory of lexical insertion which has arisen within the paradigm of derivational semantics, is nothing more, when we strip the ideological trappings and other artifactual imposition, than the attempt to answer just two questions: first, How can you tell a word from a non-word?, and second, How are words generated in the generative process?

III. By the first of these two questions I

really mean a cluster of questions, one of which has been discussed by Green (1969). She was concerned there with (p.76) "the difficulties of determining whether two phonologically indistinguishable forms were 'the same word' or 'the same morpheme'", that is, she asked (p.85), "...what are the constraints on how two or more meanings realized in the same lexical item can differ, or more simply, just what constitutes relatedness between lexical entries?"

Lexical items and lexical entries are distinguished in the paper, the former being phonological forms with morphological properties, the latter being subtrees which the former replace in lexical insertion. Lexical items are conceived of as having at least one phonological shape, certain morphological properties, and certain syntactic properties; and it is stated (p.79) that she views "all relations between meanings (related or not) and forms as entirely arbitrary." If I read correctly, what is being rejected here is a morphological level, a level between that of syntax and that of phonology.

Perhaps the strongest evidence for this rejection is the existence of exceptions. In all languages not just those with what Green calls (p.74) "thoroughly bastardized, irregular, and unsystematic derivational morphology" like English, but too in those "with much

more regular and systematic derivational morphology like Japanese," there are many major exceptions to almost any attempt at generalizing on the relationship of form and content.

It would be wrong to draw from this rejection either the conclusion that she is accepting a lexicon of the type devised by Katz, which she specifically rejects in the paper, or that there is no way left in which to define lexical relationships if one agrees to her Saussurian stand on the arbitrariness of form-meaning relations. Actually, a suggestion is made (p.82) "that lexical entries which differ merely in the order or configuration of their semantic constituents can be realized by the same lexical item", so that not likely, as in It's not likely that anybody will come., and likely not as in It's likely that nobody will come., can both be paraphrased by unlikely.

While it is true that exceptions exist in all languages, one can, generally, group languages into those with more or less regular morphology, like Japanese and the Altaic languages, and those with more or less irregular morphology, like English and the Hamitic languages. But we must distinguish between productive and non-productive derivation. In English most derivation is non-productive, whereas in Japanese the preponderence is productive. It is characteristic of non-

productive derivation that the meaning of the derived stem is not predictable from that of the root and that of the derivational affix alone, whereas it is, on the other hand, the hallmark of productive derivation that this predictability should be the case. In English, for example, while causation is relatively constant as a semantic derivation, there is no way of predicting the morphological shape of a derived causative verb. In most of the Hamitic and Semitic languages the morphology of causation is constant; for example in Ancient Egyptian causative verbs are formed uniformly with a prefixed s-, but the semantic result is rarely predictable. But in the Mongolian languages, for example, given any verb, the causative verb associated with it is very nearly predictable as to both shape and meaning, and I believe this is the case with Japanese as well.

So it would be false to conclude that there are no regularities worth noting or that there can be no universals. Admittedly, even productive derivation presents great problems; but even in the case of unproductive derivation there must be constraints in both directions: on what a derived form can mean, given the meaning of its root or stem, and on what the shape of associated forms can be. In the case of productive affixation we usually have almost complete predictability,

but even in the case of unproductive affixation it is not the case that no generalizations can be made. We must simply learn to live with facts that refuse to fit into neat categories or theories, and must temper our absolutes with an occasional modal.

Miss Green's concern with relatedness is restricted to a specific sense of relatedness: she wants to know how many entries there needs to be in a lexicon. It has been known for quite awhile that not only need not every one of the Katzian "readings" be a separate lexical entry, but that not even every Katzian lexical item need be a separate entry. But to solve this problem it is not enough to ask how lexical items are related, one also has to approach the second question I posed, namely how lexical items are generated. This is not to say that answering the second question will automatically provide an answer for the problem of distinguishing between homophones and polysemous lexical items, between pairs like kill 'cause to die' and kill 'stream', or die 'a device for casting or molding' and die 'cease to live', on the one hand, and comb 'device with teeth, etc.' and comb 'use a comb on', or construction 'act of constructing' and construction 'something constructed', on the other. Such a means of distinction should, however, be one of the most serious

goals of lexical semantics.

Part of the solution of this problem undoubtedly lies in the direction of the number-of-entries problem, in the kind of configurational analysis Miss Green has suggested, as well as in an investigation of the derivation of lexical items. There will also have to be involved some notion of the system of language, so that accidental resemblances can be distinguished from systematic ones.

IV. The second question which I have raised was how lexical items are derived. Let us assume that they are derived by a process something like lexical insertion in which abstract, semantic entities are replaced by morphological material in a series of transformations. There seem a priori to be only four ways in which this could happen: first, it may be the case that whole subtrees are replaced by single units which are words; second, whole sub-trees may be replaced by single units that are morphemes, in which case words are derived either as a consequential by-product of the replacement process or by some subsequent "verbalization" (or word-formation) process; third, trees may be replaced node by node, with semantic and syntactic (by which I mean morphological) units co-existing at various stages

of the replacement process; or, lastly, some combination of these possibilities may obtain. Considerably complicating this question is the general problem of computation versus storage in language use in terms of our theories of competence. None of the four possibilities I have just mentioned particularly leans heavily on the storage of lexical items, and all certainly imply a single way of deriving items. It should be kept in mind that there is no reason to rule out in advance the possibility that not all lexical items are derived by the same process; that is, it is quite possible that some words replace entire subtrees whereas others undergo a more complex process of derivation through replacement of semantic entities by morphological ones.

Let me now consider these possibilities in order. So long as there is any productive derivation then the replacement of a whole subtree by a word entails needless redundancy as well as loss of generality. Even if the subtree underlying "having no money" were replaced as a unit by moneyless, we would still need to specify elsewhere that the less part is equivalent to "having no ...." or "lacking ....". Thus, although we can specify a subtree for each word like moneyless, friendless, coatless, etc., we will also need to specify in some way what the function of the suffix less is; and if

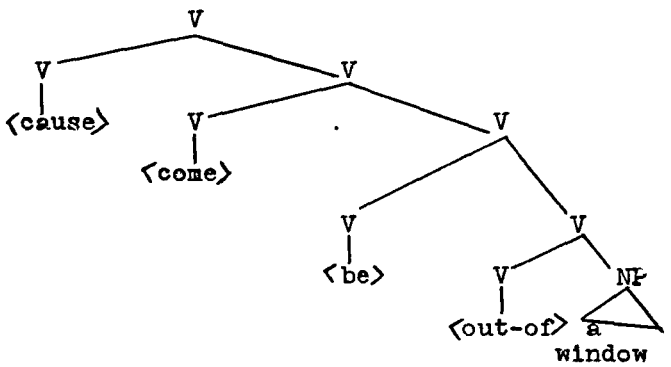
that specification is going to be a single statement about the properties of less, then why not just have a rule that maps "having no ...." or "lacking ...." into less and then incorporates it into an adjective along with the associated noun phrase? This incorporation could be accomplished by using something like auxiliary hopping and the ancillary morphophonemic rules which we will need for affixes anyhow. By adding a few rules we would save other rules and a great many entries in the lexicon, with the bonus of a generalization about words containing the suffix less. This is not to say that we need be unmindful of the problems posed by words like reckless, penniless, and feckless; on the contrary, it would be dishonest to do so, but we should keep in mind not only that the adjectives incorporating less in a productive, regular way far outnumber the irregularities, but also that the relationship between what we would expect a word like penniless to mean and what it does mean is not only clearly constrained, but actually rather close, and further, that there probably are some speakers for whom reckless and feckless are not opaque. Certainly, historically they are not, and there may be some justification for setting up abstract underlying forms as their roots. In any case it may be possible as well to relate less to antonymous forms

such as ful and thereby gain further insights, although ful presents problems of its own.

The second possibility has great problems too. So long as there is non-productive, it is hard to see how lexical insertion can proceed morpheme-wise. In terms of the mapping obtaining between the semantic and surface syntactic levels, if that mapping is always "regular," it is just not possible that a word composed of a string of morphemes should not mean what we would expect such a word to mean on the basis of the meanings of those morphemes. Yet this is the case, for example, with feckless, which is opaque for most speakers, who are unaware of what feck might mean (and who, by crossing the words dreck and feces, would probably guess that it was a dirty word). It can hardly be interpreted as "lacking feck". (Whether this less is the same one in guileless is something else again.) Any such possibility would at least contradict the doctrine that the derivational process is merely a mapping which doesn't change meaning.

Also, there may be a wide variation in the morphological systems of individual speakers. In the case of Latin and Greek borrowings it is likely that some speakers have the entire system whereas others have none of it. For some speakers, for example, ad-

judicate will be morphologically and semantically opaque, but to others it will be transparent. For those speakers for whom defenestrate is simply a unit with no internal structure, its denotation consists in a subtree such as



being replaced in toto by the word defenestrate.

This problem is even more difficult in regard to old compounds one part of which has ceased to have an independent function. It seems fair enough that no one recognizes the word for "man" in either werwolf, which is often spelled werewolf and occasionally punned on *willwolf, *wouldwolf, etc., or in world, which is an old compound of wer 'man' and eald 'age'; many English speakers who know French have never realized what the mer of mermaid means. There are many such words in English. Not all speakers connect the Quakers with the quake of earthquake; back formations make clear how few speakers ever connected hamburgers with Hamburg.

tree. It may sound like I am moving toward the lexicalist position, but actually to accept this is to accept a more radical conception of grammar and grammatical process, in which the whole notion of generation or derivation is abandoned. (I will not attempt to argue for or against such a position at present.)

For the fact that some words are for some speakers unanalyzable units, for others synthetic complexes, places some strain on the concept of competence as idealization, or at least on the concept of an ideal "grammar of L", for if it is indeed a fact of English that defenestrate is a complex rather than an atom, then every speaker of English who properly uses the word should have internalized this information, and correspondingly, if the speaker has internalized the knowledge that Sprachgefühl, for example, consists of several parts, then it ought to be the case for all speakers of English who properly use Sprachgefühl that they have internalized this knowledge. In fact this is not the case, and doubt is thereby thrown on some of the idealization involved in dealing on the one hand with the speaker's competence and on the other hand with what it means to speak some language, say English. For if the morphology of a word like defenestrate is a part of a grammar of English, then being speakers

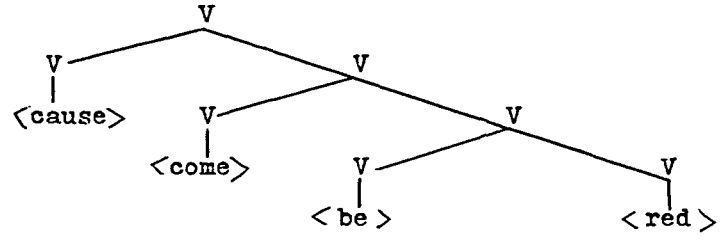
Clearly there are marked differences in morphological competence from speaker to speaker.

Thus there is a certain tension between unitwise and partwise derivation. Whereas any sentence has a low probability of occurence, even the most outrageous compound of late Sanskrit may have extremely high probability, at least under limited circumstances, so that the feeling is inescapable that compounds like (German) wissenschaftstheoretisch (linguistic-theoretical) and Altertumsforschung (archaeology) are just as much frozen forms as defenestrate or antidisestablishmentarianism are for many. One can argue that defenestrate is stored as a unit in the speaker's lexicon, and that the fact that it contains several morphemes plays no role in the derivation of sentences containing the word. We can argue that in certain cases a word is simply drawn from the memory store and not regenerated each time it is used. This is a very traditional approach. It may well be the case that the most-underlying (or "deepest") level actually contains some lexical entities, so that when one decides to talk about horseshoes, he need not have an image of horses in his mind; that is, not every sentence containing a word like horseshoe need have the definition of that word in its most-underlying structure in the form of a

of English cannot consist in having the same grammar as every other speaker of English, and if this is so, then what, if anything, does it mean to talk of "a grammar of L"? The synchronic facts of a language must define something totally different from what competence defines, in that case.

This brings us back to the second pair of solutions.

By the third solution I mean a process in which nodes are considered one at a time. Ever greater subtrees are cyclically considered until a subtree matches one underlying some morpheme or word. For this to work semantic and morphological entities would have to coexist in the tree and the derivation of words would have to be an integral part of the derivation of morphemes, although not a consequent of it. Confronted with the tree



on the first cycle the rules merely replace the semantic element <red> with the morpheme red. On the second cycle <be> is replaced with be. On the third cycle

⟨come⟩ and be are incorporated into become. Optionally, become and red are incorporated into redden. On the last cycle ⟨cause⟩ is either morphemicized as cause or incorporated into redden. This solution shares some features with the theory of lexical insertion familiar from McCawley (1968), which employs the rule of pre-lexical predicate raising, studied critically by de Rijk (1968). It differs principally in that in the present theory, semantic and morphological material may co-exist in a tree.

The three solutions I have sketched are not actually all that different from one another. Certainly they all have grave faults as well as many advantages. But to fairly judge these we have not only to confront the problem of storage vs. computation, but we have also to consider three subsidiary questions, for which no solid answers have been forthcoming: first, can semantic and morphological entities co-exist in a tree? While some linguists reject this out of hand, there would seem to be no real reason to do so, since we would be mixing what are, after all, abstract entities of only nominally different kinds. Second, is the derivation of words an integral part of, or necessary consequent to, the derivation of morphemes, the result of a seond sort of process, or not a part

of the derivation of sentences at all? And finally, how much of a tree is considered in each pre-lexical transformation? Does lexical insertion apply node-wise, constituent-wise, or how?

From what little we know about the lexicon, it would seem that these questions and others like them demand far more complicated and messy answers than we have been willing to give them in the past, and it is altogether likely that some combination of the three theories, that is, the fourth solution, is correct.

V. In addition to studying the relationship of lexical items in the same language, our intuitions allow us to study the relationship of items across languages, and specifically, that of synonymy or intertranslatibility. In addition to the theoretically trival problems of translating items with no equivalent in other languages, like English comfy, Spanish duende, and myriads of cultural items like German Schadenfreude or Yiddish yenta, there is a real issue involved in deciding, for example, whether unravel is in fact equatable with Spanish deshilar or deshilarse, first, because the English verb has two syntactic functions which are fufilled by two different words in Spanish, and secondly, because unravel is morphologically quite

different from deshilar, which is derivative from hilo, 'thread', formed by prefixing des-. There is a very large set of verbs in Spanish which are similarly formed, and all have a meaning something like 'remove the ....'. If there is any such systematic affixation evidenced by unravel at all, it is far more limited, restricted to undo, unloose, untie, unchain, unhook, unbutton, unfence, etc., which form a somewhat different class from the des- verbs in Spanish.

The problem is very similar to that of sentences: sometimes similar morphology means similar meaning, sometimes not, and sometimes dissimilar morphology means dissimilar meaning, but again, sometimes not. English and the Romance languages, for example, have two sets of present participles for Latin verbs. The old participles, in ant(e), ent(e), and ient(e), are, for the most part, stative adjectives, and take very for their adverb: Shirley is very dependent on her children. They also serve as nominals: president, resident, recipient, patient, etc. The newer participles, in Spanish in ando and iendo, in English in ing, are real verbal forms, take very much as their adverbs, rarely function as nominals and often function as prepositions or conjunctions: Shirley is very much depending on her children. Compare presiding, re-

siding, receiving, etc. (Patient has no such participle, unless it be suffering, which is medically realistic, but linguistically dubious.) During, pending, and including are only some of the participial prepositions.

The problem of setting up rules for intertranslatibility of derived forms is that much more is involved than just questions of semantic primes or the formal calculus of morphology, since the same form, or forms with similar roots and derivation, will have markedly different semantic values in different syntactic or semantic contexts. Furthermore, forms in two languages which are seemingly synonymous, when further affixed, even in a similar way, may have different meanings. That is, different semantic tendencies are latent in superficially similar forms. It is a very unusual situation where, as with English and several other unrelated languages, the notions of being 'alive' and being 'lively' are completely tied up in such ambiguous forms as quick and quicken. Whereas some languages have no notion of speed latent in their word for being alive, others do. The various roots in English for the earth have remarkably different latencies, as evidenced by the adjectives earthy, chthonian, and terrestrial.

Determining the latent meaning may be of further use in deciding what the root of a derived form is. Is the noun shoulder derived from the verb or vice-versa? The latent meaning plus some notion of the general patterns of the lexicon should provide an answer. Once we have rules for determining the direction for such derivations, we can go on to investigate systematic and non-systematic gaps in the lexicon. So the various problems I have mentioned are interrelated and the investigation of all of them must proceed simultaneously if the solution to any of them is to found.

These questions in turn bring us to the last area I wanted to consider in which our naive native speaker's intuitions are a fruitful source of information, namely that concerning the relationship of syntactic properties to semantic features. It is easy to demonstrate that many syntactic properties are functions of semantic ones. For example there is a class of verbs in English, called stative verbs, which are typified by not having a progressive present tense, by not allowing imperatives, and so on. Such verbs are be, have, seem, appear, etc. But some verbs of this class, like appear and have, have some senses in which they act like the majority of verbs, the active verbs, they take progressives, they have imperatives, etc. Thus appear when it means "seem"

has no progressive:

(1) John appears ill.

(2) *John is appearing ill.

and no imperative:

(3) *Appear ill, John.

(Cf. Look ill, John! as a stage direction.) but when it means "come out" it has both:

(4) John is appearing on stage from behind the curtains.

(5) John, appear at the hearings next week!

(Cf. ?*John has just appeared ill. John has just appeared on stage.) Similarly, have usually has no progressive or imperative when it means "possess, own, or be characterized by",

(6) *John is having a broken arm.

(7) *John, have one leg shorter than the other!

nor as the passive verb have:

(8) *John was having his dear, rich uncle die on him at an inconvenient moment.

(9) *John, have your dear, rich uncle die on you at an inconvenient moment.

but it does when it means "eat or consume", as in

(10) John is having a banana split.

(11) John, have some more chicken soup, it's good for you!

or when it is causative:

(12) John is having his dear, rich uncle put in a corner stone next week.

(13) John, have your dear, rich uncle take a one-way swim to Jersey in cement life-jacket.

When a verb has a stative meaning it takes on the syntactic properties typical of a stative verb, and when it has an active meaning, it takes on the properties of an active verb.

Generally, the same word will have different co-occurence properties in regard to adverbs, subjects, objects, etc. depending on its particular sense. When words are systematically ambiguous, as is the case with verbs like surround, which is either stative or inchoative, or roll, which is either intransitive or transitive (if causative), they will in each meaning have a set of syntactic properties determined by their membership in that class of verbs and their having that meaning.

It has been known for quite awhile that syntactic features for selectional restrictions all have semantic cogeners, and that no syntactic feature plays a role in selectional restriction if it has no such cogener. Traditionally, too, subcategorization has often been treated as a reflex of semantics. There remains how-

ever a great deal of work before we can be sure that all of our intuitions are real ones and not artifactual. It is very difficult, as a prime case, to relate the set of transitive verbs to any specific semantic property or set of properties. It may be that there are many more kinds of syntactic feature than we have thought.

VI. In effect I have been sketching here a program of future research aimed at resolving some of the grey fog surrounding many of the issues I have mentioned. Even the tasks of data collection and organization lie mostly before us. It is perhaps too early therefore to venture a guess as to what effect (if any) these researches will have on the theory of transformational linguistics, and whether any forthcoming results will tend to be crucial for the solution of the grand debate over Chomsky's lexical hypothesis. At first glance I am pessimistic that they will. I fail to see anything which would come out of lexicology which will transcend the difficulties every proposed argument has met with in the debate. Any fact that can be integrated into the one scheme can, with a few exceptions, be accepted into the other. It is far more likely that these new results will erode the basis on

which the debate is waged. Already the consideration of lexical problems has led me to the conviction that some basic premises of transformational grammar are wrong, or at least need revision rather badly. In particular the treatment of lexicology will almost certainly lead to difficult paradoxes unless we change some of the postulates which might be in question are (1) that performance is not rule-governed or in unhandleable by the linguist, (2) that the linguist's task is to provide a mapping between abstract entities on various levels, and (3) that grammar is an ideal process (or at least describable as an ideal process) rather than an actual, algorithmic process operating in real time and involving strong interactions between stored and computed entities. It is precisely in the areas of lexical semantics that I have been describing that some of the commonest assumptions of our science are most put into question. Continuing research is needed before this hypothesis can be fruitfully discussed, though.

VII. The program of research I have sketched here is particularly interesting because so much of "the great debate" devolves on matters lexical; recently a well-known linguist left our discipline on the grounds

that it was "too much like knitting": if lexical semantics is indeed that important, then I think it is precisely in this area that it is most likely to be proven false that linguistics is too much like knitting.

FOOTNOTE

*The author was at the University of Massachusetts when this was written.

A very slightly different version of this paper was read before the Brown University Linguistics Club in early April, 1970. The author would like to thank Martha Laferriere and the other members of the club for inviting him to speak to them and for their many comments which he has as yet to fully ponder.

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