

PAPERS FROM THE ELEVENTH  
REGIONAL MEETING  
CHICAGO LINGUISTIC SOCIETY

8

APRIL 18-20, 1975

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ELLIPTICAL CONDITIONALS AND/OR HYPERBOLIC IMPERATIVES:  
SOME REMARKS ON THE INHERENT INADEQUACY OF DERIVATIONS<sup>0</sup>

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There is a curious asymmetry in the paradigm (1)-(4):

- (1) Open the window and I'll kill you.
- (2) Open the window and I'll kiss you.
- (3) Open the window or I'll kill you.
- (4) #Open the window or I'll kiss you.

(1) and (2), with and, convey opposite meanings---(1) clearly conveys an order, request, or suggestion (the distinction will be ignored for the moment, and the cover term "impositive" will be used for all of these speech acts) to the effect that the addressee not open the window, while (2) conveys an impositive to do so, at least under the respective assumptions (which we will respectively assume) that killing is undesirable for the addressee, while kissing is desirable. (3), with or, conveys an impositive like (2), not like (1)---the desire of the speaker is clearly that the addressee should open the window, rather than that he should not do so. Given the logical properties of or as opposed to and, this is perhaps not too surprising. (4), however, is the kicker. If it worked like the other three supposedly do, it would convey an impositive for the addressee not to open the window, with the promise of a kiss upon acquiescence. If this were the case, we would have another mildly interesting set of data about and and or, suitable for inclusion in elementary textbooks, but certainly nothing worth writing a paper about. Of course, it doesn't work out that way. (4) conveys, if anything, an impositive to open the window under threat of kissing; for it to make even that much sense, it is necessary to revise our assumptions that kissing is desirable. What's going on here?

There are a number of things one can say about (1)-(4), some of which can be extended to apply to the other data we will discuss, and others which cannot; but it will not be my primary purpose here to try to solve the problems represented by (1)-(4) and allied phenomena. Rather than march down to the morass of data and settle in for a long siege after some initial successes, I propose in this paper to fire off a few guns in random directions and then surrender---not so much from lack of ammunition as from defective equipment. For in working on this and other problems for the last few years, I have become convinced, slowly and unwillingly enough, that the theory I have been using (and defending) is fatally flawed, and that some more radical changes are in order. In particular, I believe now that any theory of language that employs centrally the concept of "derivation" is wrong. I intend to use the discussion of these phenomena to demonstrate this point, and then to make some demands of the (as yet unformulated) successor theory.

I will begin by making what remarks are possible in a strictly derivational model. There is evidence that the Ss in (1)-(4) represent two quite separate derivational processes; (3)-(4) are apparently derived as imperatives, while (1)-(2) are not. It is not quite so clear what underlying form should be posited for (1)-(2), but a reasonable proposal in line with the evidence is that they come from if-clauses or something like them.

The evidence: there are a number of resemblances among the Ss in (1)-(4)---first, they all have you understood as subject, as (5)-(8) demonstrate:

- (5)a Scratch yourself and I'll kill you.
- b \*Scratch himself and I'll kill him.
- (6)a Scratch yourself and I'll kiss you.
- b \*Scratch himself and I'll kiss him.
- (7)a Scratch yourself or I'll kill you.
- b \*Scratch himself or I'll kill him.
- (8)a #Scratch yourself or I'll kiss you.
- b \*Scratch himself or I'll kiss him.

Second, this you subject can be retained and show up in the S:

- (9) You open the window and I'll kill you.
- (10) You open the window and I'll kiss you.
- (11) You open the window or I'll kill you.
- (12) #You open the window or I'll kiss you.

Note that (8)a and (12) are odd in the same way as (4). So far, nothing has been said that would suggest that there are different derivational sources for (1)-(4)---all of the data so far seems to indicate that they are imperatives. The difference shows up in negatives.

The ordinary negative of an imperative is with don't. This is perfectly all right with (3)-(4), but there is some uneasiness about negating (1)-(2) in this way:

- (13) ?Don't open the window and I'll kill you.
- (14) ?Don't open the window and I'll kiss you.
- (15) Don't open the window or I'll kill you.
- (16) #Don't open the window or I'll kiss you.

The emphatic negative of an imperative has the you subject present, and the don't undergoes subject-verb inversion. Aside from questions and exclamations, both of which can be safely ignored here, no other English sentence type has this property; and such an emphatic negative is ungrammatical in (1)-(2), while it is fine in (3)-(4), showing that (1)-(2) cannot be derived as imperatives, whatever their other similarities.

- (17) \*Don't you open the window and I'll kill you.
- (18) \*Don't you open the window and I'll kiss you.
- (19) Don't you open the window or I'll kill you.
- (20) #Don't you open the window or I'll kiss you.

(Once again, (20) is weird, but not for any syntactic reason)

The evidence for an if-clause source of (1)-(2) that I will present here involves negative-polarity items (NPIs). NPIs are not grammatical in imperatives unless a negative is present to trigger them; however, they can be grammatical in an if-clause, since (to state it derivationally) if is a weak negative trigger. There is therefore a cline of acceptable NPIs in if-clauses:

- |   |   |                |
|---|---|----------------|
| <ul style="list-style-type: none"> <li>(21)a If you take any,</li> <li style="padding-left: 2em;">b If you budge,</li> <li style="padding-left: 2em;">c If you make a false move,</li> <li style="padding-left: 2em;">d ??If you take a red cent,</li> <li style="padding-left: 2em;">e ??If you take long,</li> <li style="padding-left: 2em;">f *If you leave until 9,</li> </ul> | } | I'll kill you. |
|---|---|----------------|

But this is precisely the pattern of grammaticality observed with sentences like (1):

- |       |                   |                      |
|-------|-------------------|----------------------|
| (22)a | Take any          | } and I'll kill you. |
| b     | Budge             |                      |
| c     | Make a false move |                      |
| d     | ??Take a red cent |                      |
| e     | ??Take long       |                      |
| f     | *Leave until 9    |                      |

Which demonstrates in a derivational model that there must be either if or some other negative trigger of precisely equivalent strength in the underlying structure of (1)-(2), since there must be a trigger present in such a structural account.

Syntactic, derivational arguments like these can be multiplied; I will take the ones given here as representative---further arguments are left as an exercise for the reader. There are, however, many questions that these arguments leave unanswered, and even more that they do not even ask. To begin with, why is the process that derives (1)-(2) restricted to Ss with you as subject? And just what is that process? Why does (4) behave the way it does in respect to the conveyed meanings of (1)-(3)? A strict derivational account can make some contributions to our knowledge, but the account of (1)-(4) we have seen, with (1)-(2) being derived from if-clauses with the if deleted (therefore elliptical conditionals) and (3)-(4) from imperatives with a conjoined threat (therefore hyperbolic imperatives), cannot deal with these questions, which are the ones that led us to study the phenomenon in the first place.

In order to even begin to ask the right questions, we must pass over into the trans-derivational realm, as more and more studies of really interesting linguistic phenomena are doing these days. Even here, however, we will not find complete satisfaction.

The evidence given above in (21)-(22) neglected one fact: NPIs are not grammatical in Ss like (2), despite the fact that we are proposing to derive them, like (1), from if-clauses:

- |       |                    |                      |
|-------|--------------------|----------------------|
| (23)a | *Take any          | } and I'll kiss you. |
| b     | *Budge             |                      |
| c     | *Make a false move |                      |
| d     | *Take a red cent   |                      |
| e     | *Take long         |                      |
| f     | *Leave until 9     |                      |

The reason that this was neglected is that a strict derivational account cannot deal with this fact, since it is apparent that it is the meaning of kiss as opposed to kill in the second clause that influences the negativity of the if-clause. This means that negative polarity, at least in this case, is not a structural relationship stutable in terms of command, etc, which is the most a strict derivational account can do in the way of treating a phenomenon. This is not a new result; it has been pointed out by R. Lakoff (1969), Borkin (1971), and Morgan (1975), among others, that such things as speakers' intentions are important and influence grammaticality. Given these facts, one could point out that a pattern identical to (23) emerges in if-clauses with kiss instead of kill in the second clause:

- |       |                            |                  |
|-------|----------------------------|------------------|
| (24)a | *If you take any,          | } I'll kiss you. |
| b     | *If you budge,             |                  |
| c     | *If you make a false move, |                  |
| d     | *If you take a red cent,   |                  |
| e     | *If you take long,         |                  |
| f     | *If you leave until 9,     |                  |

Such a pattern actually strengthens the argument for if-clauses as a derivational source for (1)-(2), since the same pattern occurs in all cases in both if-clauses and the "elliptical conditionals". Note, however, that in order to achieve this pleasant result, we have had to throw away the structural relationship between if and NPIS, since it has been shown to be insufficient to predict NPI occurrence. Instead we have had recourse to entities which are outside the strict derivational approach that, while real enough, intuitively, are hardly specified in a theory with anything approaching precision, let alone rigor.

Again, this is nothing new. One could cite dozens of good papers in the past five years which have not only adopted this approach, but have called for the revision of the theory to permit more of it. What I am arguing for is something different---namely, that the theory cannot be revised sufficiently to do what we need it to do.

A trans-derivational approach can give us the tools to formulate an explanation for the weirdness of (4). As we shall see, however, the fact that such an explanation relies crucially upon the notion of derivation makes it unacceptable. I will present it here in familiar terms, and then argue that it is at best incomplete, and at worst, simply wrong.

In Lawler (1974) I presented a trans-derivational constraint that I will call the "Ample Negatives Principle" (ANP) [(58) in Lawler (1974)]. Stripped of its formalism, it says that underlying negatives may be moved or deleted only if all elements commanded by a negative in underlying structure are commanded by a negative in surface structure---if this condition is not met, either a new negative must be supplied in the appropriate relationship or the derivation blocks. It was suggested that the ANP is the result of a perceptual restriction forbidding negatives to occur in an S when they do not command the elements they logically negate, since this forces the listener to reinterpret the S after processing it as an affirmative first. The ANP was used to explicate constructions like:

- (25) He hasn't hit any homers yet, I don't think.  
 (26) Not any homers, he hasn't.

In these cases the surface negatives no longer command the elements they did in the underlying structure, and new ones have been supplied.

Given the ANP and the analysis on which only (3) and (4) are derived as imperatives, we can explain why (4) does not convey a negative impositive. If it did, it would have had to come from a derivation containing a negative, since imperatives have a direct illocutionary force of impositive, not simply an indirectly conveyed one. But the ANP would throw out such a derivation, since a commanding negative would have been deleted without replacement. The situation in (1)-(2) is quite different, given that they are derived from if-clauses, since there is no direct performative responsible for the illocutionary force conveyed; the conditional nature of an if-clause leaves all options open quite explicitly, to be settled by the nature of the final clause in the manner we have seen. Since this is a function of the second clause, not of any negative in underlying structure, we must conclude that the ANP will not

block these derivations. We can then let the appropriate inferences be drawn, and the conversational postulates will predict that (1) will convey a negative impositive, and (2) a positive one.

So far, so good. This the sort of analysis which has been proposed for a number of phenomena lately. It has some virtues: it relates the derivational history of constructions to their pragmatic characteristics; it provides independent motivation for treating different phenomena in similar fashions; it takes into account, in some sense, the pragmatic data that clearly influence the syntax; it is simple and intuitively sound, and relates syntax and pragmatics to perception and communicative function. Unfortunately, it has some drawbacks, as well.

First, there is some data it does not account for. Consider (27) and (28):

- (27) Open the window or you'll suffer the consequences.
- (28) Open the window or suffer the consequences.

It is obvious that (27) and (28) are as close to synonymous as it is possible to get; they convey precisely the same message. (27) is parallel to (3), and our analysis would derive it from an imperative connected by or with a threat or warning. (28) is rather different, in that the second clause is also subjectless, and a you subject is also understood. One might attempt to derive (28) from conjoined imperatives, along the lines of (29):

- (29) Open the window or close the door.

but such an attempt would fail since it is clear that the second clause of (28) is in fact not an impositive of any sort. (28), unlike (29), cannot be reversed:

- (30) Close the door or open the window.
- (31) \*Suffer the consequences or open the window.

It can have stative constructions in the second conjunct that cannot occur in imperatives:

- (32) Open the window or be thought a coward.
- (33) \*Be thought a coward.

Finally, such an analysis would have no way of accounting for the synonymy of (27) and (28). A more likely way to deal with this phenomenon on our analysis is to derive (27) and (28) from the same underlying structure, applying an extra rule to delete the you subject in the second clause of (28). This way, the synonymy falls out. Note that the rule deleting you in the second clause of (28) would have to be a different rule from the one operating in the first clause, since that clause is an imperative on our analysis, and you is deleted by a rule (either Equi from the performative or IMP-you-Deletion, depending on the theoretical perspective, which is irrelevant here) peculiar to imperatives; since the second clause cannot be an imperative, a different rule will have to be invoked. This is not too much trouble for a derivational account, since there are a number of rules around that delete subjects, and this may turn out to be one of them. In any event, rules like this do not need the type of independent motivation that a rule like (say) Raising needs, since they are heavily restricted and relatively rare. On this analysis of (28), then, the impositive comes from the same source as does that of (27), namely the illocutionary force of the imperative in the first clause.

Unfortunately for this analysis, however, the major piece of independent evidence for considering (28) to be an imperative is lacking. While (27)

has an emphatic negative with you-retention and subject-verb inversion, (28) does not; (34)b is ungrammatical:

- (34)a Don't you open the window or you'll suffer the consequences.  
b \*Don't you open the window or suffer the consequences.

The generalization here is that when the first clause is negated, and the subject is present, the subject must also be present in the second clause. However, the trans-derivational account we have developed has no way of capturing that generalization. The rule which deletes the you in the second clause must be different from the one operating in the first clause, so we cannot appeal to across-the-board application to bail us out; since the two clauses are different in underlying structure, we cannot depend on conjunction reduction to do the job; in any case, conjunction reduction could not possibly block (34)b and still allow (34)c, which is grammatical:

- (34)c You open the window or suffer the consequences.

Obviously, the generalization has to do with negatives, not with presence or absence of subjects per se. The logical scope of the negative in (34)a provides a clue for the badness of (34)b. Unlike the negation of disjoined imperatives, the negative in (34)a has a scope consisting of only the first clause; contrast this with (35), which is a true disjoined imperative:

- (35) Don't you open the window or close the door.

DeMorgan's Law applies to (35), and it is equivalent to (36):

- (36) Don't you open the window and don't you close the door.

That is, the negative in (35) has a scope consisting of both clauses. In (34)a this is not the case, since the warning/threat is still in full effect. I believe the explanation for the ungrammaticality of (34)b is that, with the subject deleted in the second clause, the S resembles a disjoined imperative too much for the negative to have single-clause scope, but the negative cannot have double-clause scope if it is to leave the sense of the warning intact. In a derivational account, it is not possible to state this explanation, whether or not it is true. Since the rules deleting the subject in the second clause are different in (34)b and (35) (representing different underlying structures), it is not possible to claim, as I would like to, that a constraint belonging to one applies to the other in particular cases. Similarity of rules is not a factor in derivations, nor is similarity of input or output structures. As with everything in a derivation, equivalence is the only controlling relation: either an item is present or it is not; either the structures match or they do not; either a rule applies or it does not; nothing else can be stated, and therefore nothing else is significant. We find, then, that the derivational account of (28) as containing an imperative in the first clause, which we posited to explain the syntax and pragmatics, does not explain the syntax at all.

As it happens, it does not explain the pragmatics, either. The trans-derivational account proposed above for explaining the oddness of (4), and the fact that an imperative cannot convey a negative impositive without an overt negative, relies heavily on the notion of illocutionary force and the distinction between direct force of an impositive, such as that of a syntactic imperative, and conveyed force, such as that posited for the elliptical conditionals in (1)-(2). However, these are not derivational notions at all, and no claim that they are "trans-" derivational can save the proposed explanation from circularity. For the fact remains that there is no place in a derivational

theory where the simple, obvious, and necessary generalization that both (1) and (3) are in fact impositives can be stated, and the explanation is based on this fact. The illocutionary force of (3) on this account comes "directly" (whatever that means) from the performative (or, if you don't believe in performatives, from whatever source imperatives get it from), while that of (1) is the result of complex and lengthy inferences and applications of "conversational postulates".<sup>3</sup> Even if we are prepared to disregard the fact that these entities are completely ad hoc in a derivationally-based theory, we still cannot use them to do the job, since they are completely different and lead to an analysis in which there is no single storable notion of illocutionary force.

It is time, and past time, that we asked ourselves just what it means to explain variation in terms of derivations. An analysis of (1)-(4) in terms of different underlying structures might (although we have seen that this one does not) manage to account for syntactic variation, but such an accomplishment is at the expense of the pragmatic data, which cannot be directly handled. This amounts to saying that a theory must handle syntax, and then, if there is any time left over, we can devote some attention to pragmatics, draping it about the form of the derivation. The pragmatic accounts in such a framework will of necessity be indirect and circuitous, since they refer to entities developed in the first place to deal with syntax only. Such a form of theory construction, which has been the practice, though not always the claim, of those who work in the area called "generative semantics", reflects a set of values which might have been useful once, but which is not valid any more. Generative semantics has successfully abandoned most of the formal impedimenta of its precursors, but has clung tenaciously to the derivation as its fundamental descriptive apparatus. I think it is now time to go all the way and give that up, too, now that we have recognized that its use amounts to syntactic imperialism.

We need a new theory. And that theory, of which some harbingers have already been seen, will have to abandon the notion of a derivation as we know it today.

In a way, this will be something of a relief. There is already a significant body of work in the literature which is more or less inconsistent with any usable generative concept of derivation, and the strain of believing in derivations has become more and more pronounced in the face of such evidence; G. Lakoff and Thompson (1975b) claim grammar is a fiction; if so, this "convenient fiction" has become something less than convenient. However, the work alluded to above, while admired by all, has not produced the wholesale re-examination and discarding of outworn axioms and ideas that it should have. Perhaps the reason for this is that, while we may know that this is all wrong, we don't have anything better to work with---or at least we haven't had. This, I think, is no longer a good excuse (if it ever was), since there are several emerging theories (or traditions, or at least new ways of dealing with data) that give promise of handling data of all kinds without the need for derivational apparatus of the formidable kind required by generative grammar in all its redactions.

For example, there is the theory of "non-discrete linguistics" represented in Ross's work. In a number of papers, Ross has demonstrated extensively and convincingly the fact that many processes in syntax are not discrete in nature, and that non-discrete theories are necessary in order to deal with them. In the only thorough discussion of the theoretical implications of this fact so far (Ross (1974)), he maintains the notion of derivation, but couples it with concepts that implicitly reject some of the discrete aspects of derivations; for example, instead of using the notion "command" as an index of cer-



tain structural relationships, he proposes that we should be able to say that a given element partially commands another, and that such partial command can, in principle, be quantified, so that a statement like "A .73-commands B" would be meaningful and useful.<sup>4</sup> What Ross has not yet come to terms with (at least in print) is the fact that any notion of derivation has an unalterably discrete base; derivations can account for phenomena in only two ways, and when a derivation is proposed for some variation or correspondence that needs an account, one must make an a priori binary choice: either there is a difference in the rules, or there is a difference in the structures. No other options are available. Most of the recent history of linguistics, it seems to me, has been a search for those other options within the constraining framework of a derivational theory, and many strange entities have thus been foisted upon us, ranging from variable rules through squishes to trans-derivational constraints and amalgams.<sup>5</sup> If, however, there is a large (perhaps continuous) range of strength and applicability for the concepts investigated by Ross, which seems to be the result of his researches (and virtually every topic investigated by Ross seems to result in a new squish), then it is futile to rely on exactly two ways of stating variation and correspondences, especially when it is largely ad hoc as to which source to assign the phenomenon to. If a non-discrete theory is taken seriously, rather than as a way of covering up the sins of derivations, it has a reasonable chance of success; if it is tied to a derivational model, it is doomed to contradiction and failure.

This non-discrete tradition is a reaction to one of the two major problems with derivations: their discreteness. The other major problem is the one we have been discussing: the fact that a derivational theory is designed to handle syntax, and whatever its virtues in that regard, there are drawbacks when it comes to semantics and pragmatics. There is, however, an emerging tradition as a reaction to this failing, as well. Perhaps the key word here is "conspiracy". It has long been recognized that there are surface structure "targets" at which the language seems to aim, and that, in a derivational account, there is no easy way to describe the ways in which the separate rules work in concert to produce those structures and avoid others. This early recognition that there are forces at work that go beyond the strictly derivational has led a number of linguists to investigate the reasons for such behavior, and to formulate explanations that relate the phenomena to the functions they are supposed to fulfill; one has only to look at the Paravolume to this volume to see a number of such papers, not all of which, however, are cast outside a derivational mold. There are many such approaches, which make many different assumptions and claims; most of them will, no doubt, turn out to be wrong---still, a start has been made.

I would like to close with some remarks on what the job of the linguist is, and what that entails for a workable theory of linguistics. To my mind, the study of linguistics means, first, making a rigorous and thorough account of the data---all the data. Linguists of our persuasion have been criticized of late for failing to include in the material we study enough different types of empirical data, and of relying on our intuitions too much. While the specifications of the charge may or may not be valid, there is some truth in it, and occasional overreliance on intuition to the exclusion of all else has led to some regrettable excesses. In addition, it is certainly true that whole areas of linguistic behavior have been systematically excluded from consideration in the past, even though they are acknowledged by all to have central importance in human speech and perception. Perhaps the most egregious example of this neglect is the study of intonation in language behavior; outside of a

very few people, and those only recently, there is little serious work being done on intonation in generative linguistics. Most of us would like to be able to deal with intonation, but protest that it's too hard. That is certainly true---since intonation is largely a non-discrete phenomenon, it does not fit too well into a digitalized theory such as generative linguistics---but that is no excuse; it simply indicates that the theory is at best incomplete. If it cannot handle such an important topic, the obvious answer is to either change the theory so it can, or abandon it as insufficient. I think we can no longer afford to ignore it. Our first task, then, is to broaden our horizons considerably, and to extend our notion of what significant linguistic data is. If we thereby extend the definition of linguistics, so be it---it has been done before, and successfully; if linguistics is ever to have anything to offer to the analysis of human behavior (or indeed to any science other than mathematics), it will do so better in a framework in which all the data is being studied.

Second, I believe the job of the linguist should be acknowledged to be what good linguists have always known it to be---to describe and if possible explain all of the interrelationships that obtain in language. The study of syntax by itself is, as has long been claimed by generative semantics, a sterile exercise; but so is the study of phonology, or semantics, or pragmatics, or any other artificial division of the field, by itself---and so is the study of sentences devoid of context, of speaker, and of listener, even though all of the traditional linguistic disciplines are brought into play. If the descriptions and explanations can be made to show something about a given theory, so much the better; but even if not, it is important to describe how various aspects of linguistic behavior interact.

What does all this mean for the (or a) theory? We have been bombarded in the past few years with studies detailing data and generalizations that "the theory must account for". I suggest it is time to take a trial balance, to see just what the theory does, in fact, account for. I strongly suspect that such an examination of the books will disclose bankruptcy. But, as in all such cases, there are significant assets which should be conserved in a reorganization: first and foremost are the very generalizations that have pushed us to such a task; next are some of the methods of research which generative linguistics has developed, and which have enabled us to ask at least some of the right questions; finally, there is the trained intuition of the linguist, which is and will probably remain the primary investigative tool in the field. What we can dispense with are pseudo-generalizations that are artifacts of narrowly-bounded areas of inquiry, formalisms of all sorts, and the set of invalid priorities which has gotten us into this mess.

Finally, what is the ordinary working linguist to do when the theory collapses around him, leaving him with all these problems to solve and nothing to solve them with? Perhaps the best way of answering this question is to regress in the abstractness of our discussion and revert to the problem which occasioned it, to see if there is anything that can be said outside the derivational theory that may help.

G. Lakoff and Thompson (1975a, 1975b) have recently proposed what they call "cognitive grammar"; it is based on a notion of serial processing of items as they occur in the perceived string, with the listener making guesses as to what the meaning of the string is, and revising them as more information is presented. While there is much to complain about in their initial theory,<sup>6</sup> the concepts it is based upon seem sound, and are in touch with areas of human behavior that have been ignored by generative linguistics. In particular,

there is a chance in such a theory to account for perception in linguistic terms, and vice versa. The explanation I proposed for the badness of (34)b is easily statable in such a perceptually-based grammar. Recall that the major problem was that (28) resembled a disjointed imperative so much that its negation led to a dilemma, since it would have to be interpreted with both two-clause scope and one-clause scope; the stumbling block in a derivational theory is that resemblance is not a valid concept. In a cognitive theory, however, it is easy to see what goes wrong with (34)b.

Perceptually, English imperatives are (as noted above) unique, and a listener processing a subjectless S when the context calls for a 2nd-person subject will have as his first approximation that the S is in fact an imperative---and it will in fact be a first approximation, since the lack of subject will be noted immediately. If it must be further modified, at least the initial reaction need not be thrown out, since the general rule seems to be that, *ceteris paribus*, English Ss with no subject and with a you-subject understood convey impositives, if not always imperatives. This points to a principle of economy of reinterpretation. The feeling that (28) is an impositive would be strengthened by the lack of subject in the second clause, and when it is negated as in (34)b, the initial reaction will be at odds with the needs of the pragmatics to leave the second clause unnegated so as to keep the warning intact. The processing of negatives seems to follow rather strict rules, and cannot be played with lightly, as I demonstrated in Lawler (1974); however, this can be explained nicely in cognitive terms, and the badness of (34)b is another example of how contrasting cognitive/pragmatic goals are settled.

All this raises a number of interesting questions for further study. What is the exact nature of the "Principle of Economy of Reinterpretation" mentioned above? How does it interact with such things as recognition of illocutionary (or for that matter, perlocutionary) force of a S, or negation, or modals, or a number of other topics? Is there a hierarchy of words, forms, constructions, etc, which trigger particular guesses that, like negation, cannot be trifled with? This would lead to theory of invariance in linguistic cognition, something that is potentially very exciting. What, in fact, are the guesses that a listener makes about the meaning of an utterance (and what does "meaning" refer to in this context)? Do they have to do only with grammatical relations and illocutionary force, as G. Lakoff and Thompson seem to imply, or are they also concerned with such things as identification of participants, establishment of personal relationships, topic and focus, old and new information, communication of emotions, etc? New data can be gathered, and old data reanalyzed in the light of these questions. Finally, what are the interactions between speaker and listener in this kind of grammar? Does the speaker aim for the guesses that he wants the listener to make, as I believe? If so, how does that influence what he says? Are there independent mechanisms for speaking and listening, complementary ones, or the same one? Many of these questions cannot be answered with our present research methods, but starts can be made; it is to be expected that the future will bring new methods (and many more questions to answer). In any event, linguists will not run out of things to do in the near future.

NOTES

0. I wish to thank Alice Myers, and especially Ann Borkin, for helpful comments and criticisms of earlier versions; I regret that I did not take all of their advice. I am also much more than usually indebted to Charlie Pyle, who explained to me in words of one syllable just exactly what the problems were with derivations, until I understood. What virtues this paper has are largely due to him; the defects are my own.

I also wish to beg the indulgence of the reader for my inconsistency; although this paper is about just why derivations are wrong, and why underlying structures and rules are the wrong way to think about language, I have not yet gotten used to thinking about it in any other way, and I am suffering withdrawal pains. Therefore, I wish to ask that occasional slips in reference that allude to such incorrect theoretical entities be forgiven in advance.

1. In using the term "derivational" from here on in the paper, I mean to include not only the strict derivational approach discussed in the first part, but also generative semantics, even including global and trans-derivational rules, since the notion of "derivation" is central to this theory too. By "derivation" I understand any account of an utterance in which a distinction is made between an underlying structure and a surface structure, with rules of any kind mediating between them. The precise nature of the structures or the rules is irrelevant.
2. I might add that I have been restrained only by threats of physical violence from adding the phrase "circular reasoning" to the title. I have not yet discovered any linguistic significance for parabolas, but when I do, I will report it promptly.
3. See Gordon and G. Lakoff (1971) for discussion of conversational postulates.
4. This possibility actually vitiates any useful notion of non-discreteness in a derivational theory, since "command" in a structural derivational theory is nothing more than an index of certain structural relationships, and if it is variable, then it ceases to mean what it has formerly meant, and requires a completely new interpretation, independent of the structures it used to represent. Such an interpretation, which might be useful, must be made in a theory which does not employ derivations centrally.
5. The problem is even worse in phonology, as Pyle (to appear) makes quite clear; and the notion of derivation is going to have to be abandoned (or at least restricted to two or three lines) in phonological theory as well.
6. For example, the formalism, which is ridiculous, and the concern only with grammatical guesses, when it is clear that simultaneous parallel guesses are made about a host of other things that influence the grammar.
7. One is immediately reminded of Quang (1971) and the constructions analyzed there. This is not a counterexample, since what Quang demonstrated so well was not that they were not imperatives, but that they were not imperatives---it is quite clear that they convey emotional attitudes not unrelated to those involved in strong imperatives, and that the subjectless form is an asset to such a conveyance.
8. And the ANP would be a special case of this.

REFERENCES

- Borkin, A. (1971) 'Polarity Items in Questions' In CLS 7.
- Gordon, D. & G. Lakoff (1971) 'Conversational Postulates' In CLS 7.
- Lakoff, G. & H. Thompson (1975a) 'Introducing Cognitive Grammar' In BLS 1.  
 \_\_\_\_\_ (1975b) 'Dative Questions and Cognitive Grammar'  
In A Fool and His Explanations Are Soon Parted (working title), the  
 Paravolume to this volume.
- Lakoff, R. (1969) 'Some Reasons Why There Can't Be Any some-any Rule'  
Language 45:3.
- Lawler, J. (1974) 'Ample Negatives' In CLS 10.
- Morgan, J. (1975) 'Title to Be Announced' In the Paravolume to this volume.
- Pyle, C. (to appear) 'Phonology: The Last Five Years' In Annual Review of  
 Anthropology, Volume V.
- Ross, J. (1974) Nouniness. To have appeared.
- Quang P. D. (1971) 'On English Sentences Without Overt Grammatical Subjects'  
 In Binnick, Salus, Vanek, & Zwicky (eds), Studies Out In Left Field.  
 LRI, Urbana.