A REPLY TO DIXON'S "A TREND IN SEMANTICS"[1]

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1. There is not, as Mr. Dixon's title and manner of presenting our views suggest, a single, essentially homogeneous, "trend in semantics", of which Ziff and we are cosponsors. Rather, there are two distinct approaches which differ as much from each other as both do from more traditional approaches to semantics. Ziff's conception of a semantic theory differs from ours with respect to explanatory and descriptive aims, conceptual machinery, empirical and methodological constraints, and relations to other theories.[2] Thus, in the present discussion, we shall not concern ourselves with Dixon's commentary and criticism of Ziff's work. Ziff is capable of defending his approach, and so we shall stick to defending ours.

Moreover, Dixon's use of the word "trend" is hyperbolical. Hopefully, our respective work may someday begin trends in semantics. But, as yet, Ziff has written a book and we have written an article. Neither has been in existence long enough to start a trend.

2. Dixon's article begins by stating flatly that theories in linguistics are simply descriptions of patterns abstracted from a corpus obtained by the linguist.[3] He evidently regards this view as so obviously true as to need no justification, for none is given. Yet it is just this view of the nature of theories in linguistics that Chomsky and we have been at pains to deny.[4] Thus, it is surprising to find that Dixon merely takes this view as axiomatic in his attack on Chomsky's and our approach. We shall see to what

[1] This work was supported in part by the U.S. Army Signal Corps, the Air Force Office of Scientific Research, and the Office of Naval Research; and in part by the National Science Foundation (Grant 16526), and the National Institutes of Health (Grant MH-04737-63). This work is a reply to Dixon, R. M. W., "A Trend in Semantics", *Linguistics*, 1, pp. 30-57.

[2] Katz, J. J., Review of *Semantic Analysis*, *Language* 38.52-69 (Jan.-March 62).

[3] Dixon, p. 30.

[4] "A Transformational Approach to Syntax", "The Logical Bases of Linguistic Theory", 9th Congress.

extent his failure to supply a justification for this view leads Dixon to beg significant questions.

Suffice here to reiterate the opposing conception of theories in linguistics. Chomsky and we hold that the speaker's ability to communicate with others in his native language rests on his knowledge of the rules which structure his language. The speaker puts these rules to use to produce utterances and to understand the utterances produced by other speakers. Thus, correctly formulating the goals of a theory in linguistics requires a distinction between the linguistic rules that the speaker has in his possession and his actual verbal behavior which is the product not only of the speaker's applications of the linguistic rules but also of his misapplications of them, his memory and perceptual limitations, fluctuations in his psychological state, etc. In terms of such a distinction between *epistemic competence* and *verbal performance*, Chomsky and we characterize the goal of a theory in linguistics as that of formulating rules which represent the linguistic knowledge that the speaker has. Hence, the aim of a theory in linguistics is explicitly *not* that of describing patterns abstracted from a corpus obtained by a linguist. For the corpus is merely a record of a finite number of verbal performances. The corpus is part of the evidence from which the linguist infers the character of the speaker's epistemic competence, i.e. the set of rules that represents the speaker's knowledge of his language.

3. The view of the goal of a theory in linguistics Dixon attributes to Chomsky and us is not, however, the one just set forth. In the first section of Dixon's article, he distinguishes between two approaches to the synchronic description of a natural language. The approach of which Dixon approves is the one which takes the goal of a theory to be describing what the speaker can be observed to do in the process of using language. The other approach, which he mistakenly attributes to Chomsky and us, seeks to formalize the informant's own ideas about his language, his home-spun linguistics. This approach proceeds by simply asking the informant what he thinks about the fundamental theoretical questions in linguistics and then by rendering his answers to these questions precise and systematic. Of these two approaches, the latter is regarded as bad because the informant's notions about language may rest on false, crudely unscientific theories and because they may be inconsistent with each other or with the notions of other informants. The former approach is regarded as good because it concentrates upon the facts of language use and confines its theorizing to them.

We can fully agree with Dixon that the latter approach is bad for just the reasons he cites. The formalization of the speaker's own notions about language would be a wholly misguided goal for linguists to adopt. This is immediately evident on the view Chomsky and we actually adhere to. Since the goal of theories in linguistics is to reconstruct the linguistic rules the speaker employs and since there is absolutely no reason to suppose that a speaker's home-spun notions correctly reflect these rules, it follows that the speaker's home-spun notions about language can have no more weight in determining the descriptive theory the linguist finally adopts than any other data he may encounter. It is certainly true that anyone who neglects the facts of verbal performance in favor of an exclusive concern with what speakers say about their language may end by offering only a sociological account of what every-day thinking has to say about language. Every-day thinking may be more or less adequate as a scientific theory about language. But, since determining the adequacy or inadequacy of theories about language is the linguist's scientific obligation, the linguist who codifies the speaker's home-spun notions still has his primary work to do after he finishes the codification. Where a theory comes from is no determinant of its truth, so the speaker's home-spun theories are on a par with those the linguist concocts.

Dixon should have been aware that it is unfair to attribute the view just criticized to Chomsky or any of his followers. For Chomsky has made it amply clear that he regards a syntactic theory of a natural language as an *explication*,[5] *viz.* the construction of a highly articulated, scientific theory to replace the "intuitive concept" of grammaticality in L. Thus, an explication of grammaticality in L differs from home-spun notions as to what grammaticality in L is in just the way that Newtonian physics differs from common sensical notions about the behavior of bodies in motion. Furthermore, although Chomsky has repeatedly asserted that the evidence for a theory in linguistics may include anything from observed linguistic behavior to linguistic intuitions of speakers expressed in their judgments of grammaticality, ambiguity, etc.,[6] he never suggested that the evidence for a linguistic theory include the speaker's home-spun notions. There is a difference between the speaker's judgments of the grammaticality, ambiguity, etc. of particular sentences and his theories about what grammaticality, ambiguity, etc. are. It is Dixon's failure to notice this difference that accounts for the radical disparity between the view he attributes to Chomsky and us and

[5] Chomsky, N., *Syntactic Structures* ('s-Gravenhage, Mouton & Co.), § 2.
[6] *Ibid.*

the view we actually hold. Perhaps the reason why he fails to notice this difference is that his unexamined conception of theories in linguistics, according to which the corpus is the only source of legitimate data, leads him to lump together all information about the language that is not in the corpus.

Moreover, it is this unexamined conception of theories in linguistics that leads him to misconstrue Chomsky's claim that the set of sentences of a natural language is infinite. Dixon's conception makes no provision for the distinction drawn above between epistemic competence and verbal performance. *Of course*, the set of sentences speakers have in fact uttered is a finite set, and, *a fortiori*, the set of sentences found in any linguist's corpus must be a finite set. The question that needs to be raised, however, is that of the generative power of the linguistic rules at the speaker's disposal. Once we have abstracted from such linguistically irrelevant facts as that the memory of speakers is limited, that life is short, etc., we see that there is no upper bound on the length of the sentences such rules are capable of producing. In short, the claim that the set of sentences the linguist seeks to describe is infinite can be understood only after one has first appreciated the distinction between epistemic competence and verbal performance. It is not surprising that Dixon, having failed to grasp the one should equally confuse the other.

4. Dixon says, "Katz and Fodor take 'meaning' and 'synonymity' as generally understood terms, which need no explanations: they appear to think of the meanings of terms, such as these two, as being quite discrete and constant, and common to all speakers of the language. They are trying to account for people's ideas regarding meaning and synonymity. But their entirely exemplicatory account makes one wonder whether what they say are ideas about synonymity are held by anyone at all except Katz and Fodor."[7] There is no need for Dixon to wonder further about this. We are, in fact, quite eager to have people believe that we were the only persons to have held the conception of meaning and synonymity put forth in our paper, since this is the basis on which we claim originality for that conception.

The question here at issue is whether the fact that our conception is idiosyncratic constitutes a criticism of that conception. Here we find Dixon making the old mistake of uncritically and falsely assuming that we are trying to systematize the home-spun ideas of native speakers. If we *were* trying to do this, then the fact that our conception is held by no

[7] Dixon, pp. 35-6.

one but us would fully refute the position we take. But, as pointed out above, our sole interest, throughout our work, has been to create a theory which provides a formal *explication* for the theoretical terms of the science of semantics.

5. Dixon regards our view that some sentences of a natural language are redundant and that some are ambiguous as somehow unscientific because this view seems to him to impugn the "efficiency of language as a form of behavior".[8] For reasons that are incomprehensible to us, Dixon appears to believe that it is "outside the scientist's terms of reference"[9] to contend that some feature of language is less efficient than it *might* be. Dixon says he believes this because there is no standard against which to measure the degree to which a feature of language is inefficient. But this is impossible to entertain as a justification for Dixon's belief since any acceptable explication of the concepts of redundancy and ambiguity would *ipso facto* provide the required standard. Once we know what redundancy and ambiguity are, we *ipso facto* know what it would be for a sentence to be non-redundant and unambiguous.

Nevertheless, Dixon asserts that it is "A prime criterion for a linguistic theory to be an adequate description ... that it should represent language as neither redundant nor inadequate with respect to the components of the theory – but instead as 'just right'".[10] Against this assertion, we can point out the following two facts. First, one need not be making a derogatory remark about language when one says that redundancy and ambiguity are features of sentences. There are good reasons why both of these features contribute to efficient communication: redundancy by facilitating communication in noisy situations, and ambiguity by facilitating economy and versatility of communication.

Second, the implementation of Dixon's "prime criterion" would lead linguists to make numerous false statements about languages. In several well-defined senses of the notion 'redundancy', many sentences of English, such as "Spinsters are unmarried", exhibit redundancies. Dixon's criterion forces us to deny the linguistic facts. Furthermore, Dixon's criterion forces us to deny the equally obvious fact that many English sentences are open to more than one interpretation. Thus, if language actually conformed to Dixon's criterion, all puns and many memorable lines in poetry would be impossible in principle!

[8] Dixon, p. 37.
[9] Dixon, p. 37.
[10] Dixon, p. 37.

6. Section 4 of Dixon's paper begins with the argument that because direct correspondences between features of situations and formal, lexiogrammatical patterns often cannot be formulated, "... we have to set up an interlevel – called context – which will provide the theoretical apparatus whereby we can establish form-situation correlations."[11] This is the only argument he gives for setting up such an interlevel in Section 4, and as it stands it begs the very question at issue. It is as though one were to argue that our inability to construct a perpetual motion machine with present techniques implies that we will have to provide theoretical apparatus whereby we can construct such a machine. Thus, the question begged by Dixon at this point is whether or not it is, in principle, possible to "provide the theoretical apparatus whereby we can establish form-situation correlations".[11]

However, in Section 8, Dixon tries to avoid the consequence of our argument that no general theory of context is possible. He does not deny the soundness of our argument, but holds rather that it can be circumvented by formulating a theory of context on a probabilistic model.[12] His idea is that we should weight situational features according to their probable influence in determining the way speakers understand sentences. Given such a weighting, a theory of context for a particular language may be elaborated to any designated degree of completeness by incorporating only situational features having a probability coefficient greater than some specified number. But this alternative does not escape our argument against the possibility of a general theory of context. The point of our argument was that no general theory could be given because giving such a theory presupposes satisfying the impossible condition of representing *all* information that any speaker could employ to interpret sentences contextually. But Dixon's idea of a probabilistic theory of context also presupposes this impossible condition, and so it does not really avoid the piont of our negative argument that no general theory of context is possible.

Dixon's proposal presupposes an assignment of probability values to every situational feature that is relevant to the way a speaker understands sentences. That is to say, if k is the number such that no situational feature whose probability value p is less than k is in the theory of context T_k (i.e. the value of k determines the "delicacy of focus" of the theory), then a theory T_k is complete and correct if and only if every situational feature F whose probability coefficient p is greater than or equal to k is in

[11] Dixon, p. 39.
[12] Dixon, pp. 51-2.

T_k and no other F is in T_k, for every triple of a situational feature, situation, and sentence. But, then, the claim that a certain theory of context is complete implies the claim that we can determine the proper p for every F (with respect to each situation and sentence). This clearly implies that we have a representation of *every F*, which representation is such that in it each F receives the correct probability coefficient. Unless we have such a representation we cannot effectively determine which F's have probability values that qualify them for T_k. This condition that every such F be represented is, however, just the condition which cannot be satisfied.

7. The entire discussion in Section 6 of Dixon's article is vitiated by his failure to comprehend the fundamental idea of the theory of recursive functions, the most significant branch of modern formal logic.

We argued that the rules of a semantic theory must be formal if the theory is to avoid the vacuity of having rules whose applicability cannot be determined without appealing to the very semantic relations that these rules are intended to explicate.[13] Thus, in formulating the constraints upon an adequate semantic theory of a natural language, we relied upon the generally accepted distinction between a formal theory and a non-formal theory. Dixon, however, refuses to accept our constraint because he cannot accept this distinction. He argues that "Being formal, being precise, is a matter of degree. The full natural language must come in at some stage [in the explanation of a logical [an abstractional] system], otherwise one could not understand the abstractional systems: the degree of 'formalness' of a system depends, in part, upon just where it does come in."[14] Dixon goes on to say, "Katz and Fodor, like very many philosophers and logicians, believe that there exists a dichotomy between, on the one hand, informal rules as expressed in a natural language, and formal rules, on the other. They do not recognize any cline of 'formalness'."[15]

But this failure to recognize a clear-cut distinction between a formal system and a non-formal one is simply a result of Dixon's failure to grasp the fundamental idea of the theory of recursive functions. This theory is concerned with the existence of *purely mechanical procedures* (*algorithms* or *effective computational procedures*) for solving problems. As Martin Davis writes, "What we have in mind are sets of instructions that provide mechanical procedures by which the answer to any one of a class

[13] Katz, J. J., and Fodor, J. A., *SST*, p. 192
[14] Dixon, p. 48.
[15] Dixon, p. 48.

of questions can be obtained. Such instructions are to be conceived of as requiring no 'creative' thought in their execution. In principle, it is always possible to construct a machine for carrying out such a set of instructions or to prepare a program by means of which a given large-scale digital computer will be enabled to carry them out."[16] The question of whether a rule is formal has nothing to do with how one explains such a rule to another person. To say that a rule is formal is to say that no creative thought is required to correctly apply the rule. The rule can be correctly applied by anyone or anything that can correctly identify the strings in its domain in terms of their form and perform mechanical operations on those strings. How much of a natural language may be needed to explain to someone what expressions like "print the symbol 'α'" or "erase the symbol 'β'" means is irrelevant to whether the rules containing such expressions are formal.

8. In Section 7 of his article, Dixon accuses us of mistakenly using non-probabilistic statements where probabilistic ones are required. He says, "Instead of statements like 'X occurs, but Z doesn't', which are found throughout both Ziff and Katz and Fodor, one should rather make statements of the form: 'people say X very often, Y less often and I have no recorded instances of Z, although all the patterns in it do occur separately and in every combination but this, and so Z might be very likely to occur just occasionally'; or else statements similar to this but ending 'Z is very unlikely to ever occur'."[17]

In the first place, Dixon misrepresents us when he says that we present the results of a semantic description in the form of statements about what does and what does not occur. Unlike Dixon, we do not construe a theory in linguistics as a description of patterns abstracted from what is found to occur in a corpus. Hence, we explicitly avoid talking about occurrences, since this locution invites equivocation between two senses of "occurrence", i.e. occurrence in speech, (in particular, occurrence in the linguist's corpus) and occurrence in the language L, that is, derivability from the rules of L.

In the second place, Dixon's claim that semantic results ought to be formulated probabilistically conflates the different functions of rule statements and probability statements. In linguistics the former are employed to describe the epistemic competence of speakers, while the latter may be employed to describe the degree to which speakers are found to conform

[16] Davis, M., *Computability and Unsolvability* (New York, McGraw-Hill, 1958), p. xv.
[17] Dixon, p. 50.

to such rules. Suppose, by way of analogy, that we were sociologists attempting to determine the traffic rules operative in New York from an investigation of the driving behavior of New Yorkers. As the result of such an investigation, we might arrive at the conclusion that one of the rules they follow is that of driving on the righthand side of the street. How, it would not do to object to our conclusion that occasionally New Yorkers drive on the left-hand side of the street, say as the result of intoxication, inattention, a road cave-in on the right side, or whatever. That is, we would not wish to reformulate our rule as 'people drive on the right hand very often, on the left hand less often...etc.' To accept that reformulation would be to confuse describing what the drivers do with describing the rules the drivers follow.

At the end of Section 7 Dixon remarks that "... we are repeatedly told in these and in many other books that 'bachelor' and 'unmarried man' are synonyms. Yet 'bachelor girl' is a quite common collocation: Katz and Fodor would be sure to classify this as anomalous. But the minority collocational instances of any item are as valid as its majority employment. Only by considering probabilities in a discussion of synonymity, and of most other relations, can we get adequate descriptions."[18] Now, we do not, in fact, consider 'bachelor girl' to be anomalous. Rather, we regard this expression as meaningful, but idiomatic.[19] But, in any case, anomaly has nothing to do with frequency of occurrence since, as we have seen, linguistic regularity is a function not of high probability of occurrence but of conformity to rules. Thus, conversely, linguistic irregularity is a function not of low probability of occurrence but of violation of rules.

Moreover, one wonders how Dixon knows that 'bachelor girl' *is* a 'minority collocation'. Assumptively he has not actually counted occurrences of 'bachelor' in some corpus of English. It seems more likely that his conflation of irregularity with low probability has led him to infer from his intuitive judgment that 'bachelor girl' is somehow irregular that it must also be infrequent.

9. Section 8 of Dixon's article is an attempt to give a detailed account and criticism of our conception of a semantic theory. But except for Dixon's counter-proposal of a probabilistic theory of context, which we have already rebutted, every one of Dixon's criticisms in this section is no more

[18] Dixon, p. 50.

[19] Cf. Katz, J. J., and Postal, P., "Semantic Interpretation of Idioms and Sentences Containing Them", *Quarterly Progress Report*, Research Laboratory of Electronics, Massachusetts Institute of Technology April 15, 1963, # 70, pp. 257-282.

than a dogmatic denial. There is not even a consideration of our supporting arguments.

Dixon criticizes us for using "dictionary entry methods" which he says are "... unsuitable for dealing with 'range of meaning' statements".[20] But we are not told what the conception of 'range of meaning' is, nor why dictionary entry methods are inadequate for dealing with the linguistic analysis of lexical meaning. Thus, Dixon's claim that our "... Semantic theory accomplishes little, and its results are surprisingly unrevealing"[21] is entirely gratuitous.

Dixon's contention that "A dictionary is a device to help people in their everyday use of language"[22] is certainly right for the sort of dictionary one finds on reference shelves. But to deny, as Dixon does, on this basis that a dictionary of our type can be a component in a linguistic theory is precisely to ignore the important differences between these two types of dictionaries. One might equally argue that a grammar cannot be a component in a linguistic theory since the grammars that are found on reference shelves are 'devices to help people in their everyday use of language'.

Though Dixon never considers our discussion of how a dictionary in our sense is a formal reconstruction of the everyday type of dictionary, we clearly show how both types of dictionary are essentially equivalent in function. Typically, an every day dictionary is employed by a speaker who understands all of a sentence excepting one word. The information the dictionary gives for that word, plus the speaker's understanding of the rest of the sentence, plus the speaker's mastery of the general rules of his language, must be adequate to determine the meaning of the entire sentence if the dictionary is to do its job. Precisely analogous considerations apply to the application of a formalized dictionary in a semantic theory, except that here the speaker's linguistic information is made explicit in the rules of the theory. The constraint that a formalized dictionary must satisfy is that, given the projection rules of a languages, and given asyntactic analysis of the sentence to which it is applied, it must contain adequate semantic material for each of the lexical items in the sentence to determine the meaning of the sentence.

It should be remarked that no everyday dictionary more than approximates this goal, just as no everyday grammar more than approximates the goal of being a scientifically accurate characterization of the forma-

[20] Dixon, p. 53.
[21] Dixon, p. 53.
[22] Dixon, p. 54.

tion rules of a language. There is not the slightest reason why conventional dictionaries *should* satisfy such constraints. Their application is not intended to be formally determined since they presuppose a user of considerable linguistic sophistication. Hence, to argue as Dixon appears to do that the validity of a dictionary approach to semantics is somehow impugned by the informal character and special features of conventional dictionaries is to argue irrelevantly.

Finally, Dixon says that we give no indication of how an entry is to be constructed, revised, or replaced by a better entry.[23] Dixon does not explain this remark, but since we discuss in great detail the form of dictionary entries, the theoretical vocabulary for writing entires in this form, and the evaluation of dictionary entries, we can only interpret Dixon to mean that we have not given a *discovery procedure* for constructing optimal dictionary entries. That we do not give such a discovery procedure is correct. But we do not attempt to formulate such a procedure because we hold, as we made explicit in our paper, that no such procedure is possible. Against this claim of ours, Dixon is curiously silent.

10. Dixon repeatedly criticizes Chomsky and us for adopting what he calls a "logical system" conception of linguistic theories. "A more valid scientific method," he contends, "is to begin with observational data and construct a theory out of the patterns recognized in observation."[24] But it is precisely Dixon's conception of linguistic theories which, as we have seen throughout this discussion, is the source of his difficulties. Thus, these difficulties are a *reductio ad absurdum* argument against Dixon's conception and in favor of Chomsky's and ours.

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[23] Dixon, pp. 54-5.
[24] Dixon, p. 52.