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LOGICAL SYNTAX AND SEMANTICS

THEIR LINGUISTIC RELEVANCE

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The relation between linguistics and logic has been discussed in a recent paper by Bar-Hillel,[1] where it is argued that a disregard for work in logical syntax and semantics has caused linguists to limit themselves too narrowly in their inquiries, and to fall into several errors. In particular, Bar-Hillel asserts, they have attempted to derive relations of synonymy and so-called 'rules of transformation', such as the active-passive relation, from distributional studies alone, and they have hesitated to rely on considerations of meaning in linguistic analysis. No one can quarrel with the suggestion that linguists interest themselves in meaning or transformation rules, but the relevance of logical syntax and semantics[2] (at least as we now know them) to this study is very dubious. I think that a closer investigation of the assumptions and concerns of logical syntax and semantics will show that the hope of applying the results which have been achieved in these fields to the solution of linguistic problems is illusory.

Bar-Hillel sums up the major part of his argument in the following statement of a presumed relationship between linguistics and logical syntax (236–7):

> There exists a conception of syntax, due to Carnap, that is purely formal (structural) and adequate in a sense in which the conception prevalent among American structural linguists is not. This conception entails a certain fusion between grammar and logic, with grammar treating approximately the formational part of syntax and logic its transformational part. The relation of COMMUTABILITY may be sufficient as a basis for formational analysis, but other relations, such as that of formal CONSEQUENCE, must be added for transformational analysis.

Later he urges that linguists 'follow Carnap's lead' even further, introducing semantic notions for, presumably, an even more adequate theory.[3]

In particular, he claims that the prevalent conception of syntax among linguists cannot lead to the definition and fruitful analysis of such relations as that between *oculist* and *eye-doctor*, and that between the active and the passive,

[1] Logical syntax and semantics, *Lg.* 30.230–7 (1954).

[2] I want to make it clear that the remarks which follow are critical not of logical syntax and semantics as such, but of the claim that these disciplines furnish solutions to linguistic problems. I have borrowed freely from various critical accounts of the theory of meaning, including W. V. Quine, *From a logical point of view*, esp. Ch. 2, 7, 8 (Cambridge, Mass., 1953); M. G. White, The analytic and the synthetic: An untenable dualism, *John Dewey: Philosopher of science and freedom* 316–30 (New York, 1950), reprinted in Linsky, *Semantics and the philosophy of language* 272–86 (Urbana, 1952).

[3] I will not discuss these proposals separately, since, as I will try to show, they fail for the same reasons.

while the conceptions of logical syntax are adequate to these ends. What is the source of this 'greater adequacy' of the suggested approach? It lies in the fact that this approach adds certain primitive notions to the customary ones of linguistic theory. (The primitive notions of a theory are those that are not analyzed within the theory, but are taken for granted as a basis for constructing the theory.) But what in fact are these primitive notions? One is the notion of formal consequence, for instance, the relation which holds between the active and the passive. Now if we do take the consequence relation as an unanalyzed primitive relation holding in just the cases that we would like to consider to be 'grammatical transformations',[4] including for instance the case of active–passive, then it will hardly be surprising that the conception of syntax based on this primitive will be adequate to provide such relations as that of active–passive. In fact, this primitive alone will be sufficient with no further theoretical work at all. Similarly, if one of our primitives is 'synonymous', and if this holds in just the cases that we want, e.g. between *oculist* and *eye-doctor* (but not between *Washington* and *the capital of the United States*), then there will indeed be little difficulty in establishing the relation between *oculist* and *eye-doctor*. But it is at once clear that the reference to Carnap, logical syntax, and semantics is hardly to the point here. None of Carnap's results is being used, but only the fact that Carnap considered such notions as 'formal consequence' and 'synonymy' to be so clear that he could significantly construct various theories taking these notions as primitive terms not requiring analysis. Naturally, the terms themselves remain as much in need of explanation, clarification, and operational analysis after a system of semantics has been constructed upon them as primitives, as before. Bar-Hillel seems indeed to recognize this, since he continues the thesis quoted above as follows (237): 'Since modern techniques of elicitation have been developed mainly with distributional analysis in view, a new approach is required that will yield reliable techniques of elicitation for the establishment of synonymy and the like.' But this injunction to linguists to search for reliable elicitation techniques for synonymy leaves the problem exactly where it was, and has exactly as much or as little force as it would have if no work had ever been done in logical syntax or semantics.[5] The relation between *oculist* and *eye-doctor* would certainly be much clearer if we had an adequate operational, behavioral account of synonymy, but it is hardly necessary to study semantics to become convinced of this truism. Since the notion is taken for granted in semantics as an

[4] Thus, for Bar-Hillel no doubt the proper interpretation of this relation will be such that *Mary was seen by John* follows from *John saw Mary*, but not *This gas was at such-and-such a temperature* from *This gas was at such-and-such a pressure*, or *This dry twig will burn* from *This dry twig will be thrust into the fire*. Similarly, from *Socrates is a man* will follow *Socrates is a rational animal*, but perhaps not *Socrates is a biped* or *Socrates has a finite number of arms*. The transformation rules of logic are rules of valid inference; hence in this discussion the terms 'inference' and 'transformation' will be used interchangeably.

[5] Note that any behavioral definition of synonymy will probably yield paradoxical results with respect to such notions as logical truth, analyticity, and synonymy itself. It is not hard to imagine the result of asking 'ordinary people' what is the status of a sentence like *There are as many even integers as there are integers altogether*, or *There are uncountably more real numbers than rational numbers, but between any two real numbers there is a rational number*, or simply a tautology too complex to be readily grasped.

unanalyzed primitive, and since techniques of elicitation do not belong to its domain, it seems that reference to it here is irrelevant.

Although it is impossible to go into Carnap's ramified and carefully constructed systems in this paper, it will be revealing to mention certain characteristic features.[6] In various formulations of his semantic theories, Carnap takes various starting points for the development; but in each case some notion of 'cognitive synonymy' (i.e. the relation between *oculist* and *eye-doctor*) is assumed. Thus none of these theories provides analysis or understanding of this notion, if it did not exist before. In logical syntax, the formal character of the notion of analyticity (and thus of synonymy as well) lies essentially in the fact that the membership of the different classes of terms (logical and descriptive), and the particular formal (i.e. typographical) properties which will signify analyticity can be enumerated by listing.[7] But if the vocabulary of the language extends beyond logic, and its 'descriptive' part includes such terms as *red*, *blue*, and *colored*, then we must account for the validity of the inference from *This is red* to *This is not blue* or to *This is colored* (if we have decided somehow that this is a valid inference). Within the general framework of logical syntax, we might do this by listing the inferences in question as postulates (so-called meaning postulates[8]) of the language system in question. In each formulation of these theories we thus have a characterization of synonymy in terms of what amounts to a list, for the given language, of the synonyms of the language. This list is given either as a set of meaning postulates or by some other device—e.g. a list of 'semantical rules'.

At the outset of his paper, Bar-Hillel asserts that there are certain 'rigorous, structural procedures', over and above distributional procedures, which are much more powerful than the latter, and which linguists have been neglecting because of lack of acquaintance with logical syntax. It is important to realize that it is the procedure outlined above (listing of synonyms and listing of the formal properties that determine valid inference) that is the rigorous structural procedure which linguists are admonished to disregard no longer. If a linguist has qualms about establishing or using somehow the fact that *oculist* and *eye-doctor* are synonyms, he can avoid all fear of mentalism by the formal procedure of setting down this fact as a meaning postulate. And if he is concerned about the question why *An oculist is an eye-doctor* is a meaning postulate, while *Washington is the capital of the United States* is not, he can be assured that there is a rigorous procedure for making this distinction, namely, to list the former sentence under the heading 'meaning postulates' and not to list the latter. But it is clear that such an ad-hoc approach to the problem of classification and characterization of elements in particular languages will be of no help to linguists, who are interested

[6] See Quine, op.cit., for a discussion of Carnap's systems and a general discussion of synonymy and related notions.

[7] Thus Carnap points out that it is purely a matter of convention whether only 'logical laws' or also 'physical laws' are given among the transformation rules of a language. For any given language there is no general way of deciding how to differentiate between the two.

[8] Carnap, Meaning postulates, *Philosophical studies* 3.65 ff. (1952). Incidentally, Carnap here (66) explicitly states his present view that the explications which he is developing 'refer to semantical language-systems, not to natural languages ... the problems of explicating concepts of this kind for natural languages are of an entirely different nature.'

in the GENERAL GROUNDS by which these elements and relations are established in each particular case. If we have an operational account of synonymy or transformation, this formal and rigorous procedure of arbitrary listing is not needed; and if we have no operational account, this procedure, despite its literal formality, is obviously useless.

The word 'formal' has played a rather crucial role in this discussion. Thus Bar-Hillel assures linguists that logical syntax is formal, and that the active–passive relation is one of formal consequence. This is true, but in a sense which has turned out upon closer investigation to be trivial for linguistics. The word 'formal' has misleading connotations. To say that a relation is formal is to say nothing more than that it holds between linguistic expressions. In this sense active–passive is a formal relation, but so is the relation 'longer by three words', which holds for instance between the sentences *John did not come home* and *John came*. In view of the greater adequacy claimed for logical syntax, it may come as a surprise to realize that logical syntax gives us no way of determining which of these formal relations (active–passive, or three-words-longer) is to be considered a relation of formal consequence. Similarly, modern semantics gives us no way of determining whether synonymy (also a formal syntactic relation if you like, since it holds between linguistic expressions) holds between *oculist* and *eye-doctor*, or between *oculist* and *horse*. The point is that while these relations are formal in the sense that they hold between linguistic expressions, they do not have the further property, as far as we now know, that systematic investigation of linguistic expressions alone will suffice to determine the linguistic expressions of which they hold. Thus logical syntax and semantics provide no grounds for determining synonymy and consequence relations. The only assistance that these disciplines offer to linguistics is to point out that consequence is a relation between sentences, and synonymy a relation between words, and that if we knew the results of linguistic analysis before such analysis was undertaken, we could write down an immense list of synonyms and valid inferences. The word 'formal' disguises this triviality.

This observation is sufficient to establish my point, but one minor illusion should be dispelled at the same time. One could argue that the lists in question might not be unmanageably large—that in the case of formal consequence, at least, it would suffice to give rules only for certain 'logical particles' such as *and*, *not*, *all*.[9] Actually, the well-known discrepancy between the material conditional of logic and the 'if–then' of English (which seems to be true only if there is some kind of connection in content between antecedent and consequent) should be enough to warn anyone not to make a blind leap from mathematical systems to ordinary linguistic behavior. Furthermore, there are clear cases of types of inference which are apparently not statable in syntactic terms, except in the utterly trivial sense that any utterance, hence any inference, can be stated and thus put in the appropriate list; the inference from *This is red* to *This is colored* is a perfectly good instance. Or consider a case pointed out by Good-

[9] Even if true, this would be a minor point for linguistics; for the linguist's problem is not the size of his ad-hoc lists but their arbitrariness.

man.[10] From *All butter melts at 150°* we can infer *If that piece of butter had been heated to 150° it would have melted.* But from *All the coins in my pocket on V-E day were silver* we cannot infer *If this penny had been in my pocket on V-E day it would have been silver.* Yet the two inferences have the same 'logical structure'. If another example is needed, consider *The man is tall and thin,* from which we can infer *The man is tall,* as opposed to the 'syntactically identical' *The flag is black and white,* from which we cannot infer *The flag is black;* no one would say that a newspaper page is black.[11] Such examples can easily be multiplied.

In the light of this discussion we can return to the assertion by Bar-Hillel that 'Carnap has shown that even transformation aspects of syntax can be described without appeal to meanings.' It is clear that Carnap has 'shown' nothing about inference in ordinary language (except in a trivial sense), as he would no doubt be the first to state. Carnap has developed certain so-called artificial languages, in which what are called transformation rules can be stated in purely syntactic, i.e. typographical terms. This development was an interesting one for logic, since such syntactic treatment had been, and was to continue to be, the source of immense progress in logic and the foundations of mathematics. But from this we learn nothing about inference in ordinary linguistic behavior. It might well be that what passes as inference in the ordinary discourse of daily life can be stated exclusively in terms of meaning. The question as to the nature of inference in natural languages can scarcely be intelligibly put now, since we have almost no systematic knowledge about inference or meaning in ordinary linguistic behavior, and no study of new and deeper foundations for mathematics can be expected to tell us more about this.

Bar-Hillel suggests that confusion of meaning and reference may have led linguists to disregard MEANING, which is their proper concern, because questions of REFERENCE (truth etc.) do not fall within linguistics. Of the two branches of semantics, theory of reference and theory of meaning, only the latter, he argues, is of real interest for linguists. Furthermore, we read (235) that 'Bloomfield's strictures against semantics and the use of meaning for linguistic description, though valid against the state of that field at the time he wrote, do not hold against this revitalized science in the form given to it by Tarski, Carnap, Quine, and others.' This gives an incorrect impression of the field of semantics. The distinction between the theory of reference and the theory of meaning is an important one, but in fact only Carnap, among the three logicians named, has attempted to build a theory of meaning. This attempt we have discussed above, and have found its basic inadequacy for linguistics to result from the fact that the notions taken for granted are just those which the linguist would like to see analyzed. Tarski's work is completely in the theory of reference. Quine does discuss the theory of meaning, but only to develop his notion that it remains in pretty much the state that repelled Bloomfield. Bar-Hillel cites Quine's Notes on

[10] N. Goodman, The problem of counterfactual conditionals, *Journal of philosophy* 44.113 ff. (1947), reprinted in *Semantics and the philosophy of language.*

[11] The last case has some specific linguistic interest. Though in the cited case the expressions are 'syntactically identical', in other cases they will differ. Thus *tall* and *thin* are equally stressed in *a tall and thin man,* but *black* is more heavily stressed than *white* in *a black and white flag* (as if *black-and-white* were here a single word).

existence and necessity[12] as an example of the work in semantics which has so revitalized the science that linguists may now freely use meaning. Yet in this article, Quine is so far from holding this position that he can assert that 'The relation of synonymity ... calls for a definition or a criterion in psychological and linguistic terms. Such a definition, which up to the present has perhaps never even been sketched, would be a fundamental contribution at once to philology and philosophy.' This article, it is true, antedates Carnap's attempt in *Meaning and necessity* to construct a theory of meaning. To discover Quine's later position in the light of Carnap's more recent work, we need only turn to the other paper of Quine's that Bar-Hillel cites later on, Two dogmas of empiricism.[13] This is perhaps Quine's most trenchant attack on current formulations of the theory of meaning, Carnap's and others'. After pointing out the interdefinability of 'meaning', 'synonymy', and 'analyticity', Quine concludes by asserting: 'But, for all its a priori reasonableness, a boundary between analytic and synthetic statements has not been drawn. That there is such a distinction to be drawn at all is an unempirical dogma of empiricists, a metaphysical article of faith.' Just above we read: 'Appeal to hypothetical languages of an artificially simple kind could conceivably be useful in clarifying analyticity, if the mental or behavioral or cultural factors relevant to analyticity—whatever they may be—were somehow sketched into the simplified model. But a model which takes analyticity merely as an irreducible character is unlikely to throw light on the problem of explicating analyticity.' In view of the immediate interdefinability, we can substitute 'synonymy' for 'analyticity' in the quotations above. The citation of Quine and Tarski in support of the theory of meaning must itself be a confusion of meaning and reference, since both Tarski and Quine have done important work in the theory of reference. This is the branch where real progress has been made; but it is also the branch that has little interest for linguists.

Evidently logical syntax and semantics can bring the linguist no nearer to an adequate conception of synonymy or transformation. But there are other points in linguistic theory, it may be argued, where meaning is involved, and where problems can be solved by logical syntax and semantics. Bar-Hillel indeed asserts (230) that the more powerful techniques of logical syntax will actually enable linguists to avoid certain considerations of meaning, while distributional techniques will fail to do so. He explicitly takes issue with a statement of Harris's (*Methods in structural linguistics* 8 fn. 7) concerning the distributional basis for the morphemic division of such words as *boiling, princeling, sight;* but there is nothing in Carnap's approach that suggests a way to answer such questions. It is easy enough to construct artificial languages to exemplify some proposed solution to a problem in the linguistic analysis of a real language; but Carnap's conception does not indicate which of these artificial languages is a correct model for the language under analysis: we would know how to choose the right model only if we knew how to solve the problem. It is no use arguing that models seem

[12] *Journal of philosophy* 40.113 ff. (1943), reprinted in *Semantics and the philosophy of language.*

[13] *Philosophical review* 60.22 ff. (1951), reprinted almost without change as Ch. 2 of *From a logical point of view.*

to be useful in other fields. It must be demonstrated that artificial languages provide some illumination in the instance under consideration. It is conceivable that Carnapian constructions might some day provide some insight into these problems, but the burden of proof in such a case is on the model-builder. For reasons which I will go into presently, it seems to me questionable that this will ever be the case.

Much of the difficulty here is caused by two analogies, one drawn by Bar-Hillel, the other by Carnap himself. Bar-Hillel asserts (235) that linguists and logicians do essentially the same thing.

> Both are essentially attempting to construct language systems that stand in some correspondence to natural languages ... But whereas for the linguist the closeness of this correspondence is the criterion by which he will judge the adequacy of the language system he is setting up ... the logician will look primarily for other features of his system, such as simplicity of handling, fruitfulness for science, and ease of deduction and computation, with close correspondence to a natural language as only a secondary desideratum.

Thus the difference is one of degree rather than of kind, and this is apparently what suggests to Bar-Hillel the legitimacy of drawing conclusions about natural languages from systems constructed for the study of the foundations of mathematics or science. But one might as well argue that a science-fiction writer or an abstract artist is doing roughly the same thing as a physicist, and that we can make inferences from the creations of the one to the descriptions of the other. For a linguist can be said to construct artificial language systems only in the sense that a physicist describes the behavior of objects in an artificial world.

The false analogy is fostered, I think, by an unfortunate analogy of Carnap's. In the introduction to *The logical syntax of language* Carnap asserts that 'the syntactical property of a particular word-language, such as English ... is best represented and investigated by comparison with a constructed language which serves as a system of reference.' And further (8):

> The direct analysis of these [natural languages], which has been prevalent hitherto, must inevitably fail, just as a physicist would be frustrated were he from the outset to attempt to relate his laws to natural things—trees, stones, and so on. In the first place, the physicist relates his laws to the simplest of constructed forms; to a thin straight lever, to a simple pendulum, to punctiform masses, etc. Then with the help of the laws relating to these constructed forms, he is later in a position to analyze into suitable elements the complicated behaviour of real bodies, and thus to control them.

There are several senses in which a physicist can be said to be dealing with idealized situations. On the one hand, he deals with simple objects in the laboratory, such as balls rolling down an inclined plane, rather than with avalanches storming down a mountainside; he will attempt to explain the latter as a complex case of the laws established in terms of the behavior of the simpler objects. But he does this on the basis of the assumption, which he will attempt in every way to verify, that complex objects follow the same laws outside the laboratory as simple objects within it: all these objects are part of the same universe, and are

composed of matter with the same fundamental properties. This assumption is justified by its success in explaining natural phenomena, and in predicting and controlling the behavior of complex objects. On the other hand, when a psychologist observes the behavior of rats in a laboratory, we are less inclined to accept the assumption that the laws which govern this behavior also govern the behavior of rats in the outside world, or human beings in social situations. Yet even this assumption may often be reasonable, since after all the same organisms are involved; we are assured at least that we are learning something about animal behavior, even if only in unusual situations. But in the case of the artificial 'languages' investigated by Carnap in his logical laboratory, there is little if any antecedent reason for regarding these as in any way comparable to the actual languages of the outside world. The ball rolls down the inclined plane exactly as it does, the rats behave exactly as they do; but an artificial language has whatever properties its maker determines it to have.

In another sense, the physicist constructs his laws in terms of such ideal elements as rigid bodies and perfect vacuums, which are not found even in the laboratory. But he will construct experiments which come closer and closer to matching the ideal conditions, and will not be satisfied unless he can show that the laws for the ideal case are approached more and more closely as the conditions grow MEASURABLY closer to ideal. Similarly, he will attempt to account PRECISELY for deviations from the ideal in terms of physically significant notions such as the measurable and calculable effects of friction, air resistance, and limitations of his equipment—all notions which are part of physical theory. If the measured deviation from an idealized model cannot be accounted for precisely (within the limits of the equipment), the underlying theory is scrapped. Again there is no analogy here to the models which, it is asserted, are indispensable for the solution of problems in the analysis of 'word-languages'. We cannot measure the deviations in the behavior of actual languages from that of the artificial systems that we invent. We have no way of knowing whether an artificial language is closer to English if such a sentence as *A man is a rational animal* is listed as analytic, synthetic, or neither, since we can demonstrate nothing about the behavior of natural languages in the relevant respects. Thus we know as little about synonymy after constructing an artificial language as we did before.

It appears, then, that artificial languages are neither special cases nor idealized versions of natural languages. No analogies with physics will circumvent the necessity of demonstrating that a given model can be interpreted in an interesting and objective way, yielding effective and interesting solutions to current problems.[14]

Is it possible to provide answers to problems of synonymy and transformation by extending the methods of linguistics? We do not yet know. Harris has made

[14] I am not arguing that a model cannot be interesting unless it is a special case, or measurably a limiting case, of something real; but Carnap, by this analogy, implies that it is in this strong sense that artificial languages are necessary. Such a view, I have argued, is incorrect. A general argument 'for or against' models is pointless. A model must prove its worth: it must analyze, not assume, some interesting notions, and it must be applicable in a reasonably unambiguous way.

some suggestions for this kind of extension, but Bar-Hillel states (237) that his attempt 'to reduce the transformational part of syntax to its formational part is based on a series of equivocations in the terms *language*, *equivalent*, *commutable*, and their cognates, and so is without foundation.' But the argument supporting this contention contains a serious misrepresentation of Harris' proposals.

This argument makes the valid point that two interpretations of 'distribution' cannot be used to develop a concept of synonymy. That is, we cannot define '*X* is synonymous with *Y*' as '*X* and *Y* are mutually substitutable in the class of all grammatical sentences' or '... in the class of all sentences which constitute a given corpus'. This is quite correct, and should serve as a warning to anyone who attempts a too-facile identification of distribution with meaning, where 'distribution' is not carefully analyzed. We may now ask how the quoted conclusion follows from this correct argument.

Since in his *Methods of structural linguistics* Harris never discusses the 'transformational part of syntax', the argument cannot refer to any part of this book. Harris does discuss transformations in his article Discourse analysis, *Lg.* 28.1–30 (1952). Here he discusses two types of transformations: those concerned with large classes of morphemes (e.g. active–passive), and those concerned with single morphemes (e.g. the relation between *buy* and *sell*). But Bar-Hillel's point is irrelevant to the first kind of transformation, since no one suggests that this relation be defined in terms of synonymy. In his account of this construction, Harris considers certain formal relations between large classes of grammatical sentences. The notions involved are vague, and require much further study—in particular, the notion 'grammatical' must itself be carefully defined; but it seems to me that the central ideas are sound. No question of synonymy is involved, and the fallacy in question is not to be found.

The transformation that Bar-Hillel has in mind in his argument is apparently the second kind. But in the only section of this paper in which Harris treats this type (24–5), he explicitly avoids the fallacy cited by Bar-Hillel. In his very sketchy treatment he points out that this relation (e.g. between *buy* and *sell*) CANNOT be handled by considering substitutability within single sentences, and suggests rather that it is 'necessary to study the restrictions over more than one sentence at a time'. That is, we must investigate certain kinds of distributional relation within complete texts as units, not in single sentences. This is the whole point of discourse analysis. While the argument in Bar-Hillel's paper is correct, it has no bearing on the criticism of Harris's proposals that he infers from it.

One point incidental to this discussion deserves further comment, since it bears on the general problem of synonymy. Bar-Hillel asserts the common view that any two absolute synonyms are substitutable salva veritate in any context (apart from possible sporadic exceptions), implying indeed that their synonymy resides precisely in this fact. But in indirect discourse this is in general not true; thus, the sentence *Everyone* (*who is sane*, *who knows English*, etc.) *knows that an oculist is an oculist* is a true sentence, but *Everyone knows that an oculist is an eye-doctor* is surely false. Scheffler, who has made this point in a quite general

form,[15] goes on to develop the argument that any analysis of synonymy must fail to achieve one of its main purposes, that of describing substitution in indirect discourse.

Note that I am not criticizing the notion that artificial languages may be useful for philosophical purposes. Nor am I arguing that the construction of artificial models cannot increase our awareness of the subtleties of actual language; there are examples of philosophical work which do just this.[16] Furthermore, I am not arguing that logic cannot be used in linguistics. At one point Bar-Hillel suggests that recursive definitions may be useful in linguistic theory; whether this turns out to be the case or not, I agree in this instance with the spirit of his remarks. The correct way to use the insights and techniques of logic is in formulating a general theory of linguistic structure. But this fact does not tell us what sort of systems form the subject matter for linguistics, or how the linguist may find it profitable to describe them. To apply logic in constructing a clear and rigorous linguistic theory is different from expecting logic or any other formal system to be a model for linguistic behavior.

I do object to the thesis that incorporating logical syntax and semantics into linguistic theory will solve certain of its problems, or that the theory of meaning in natural languages is in any way clarified by constructing artificial languages in terms of rules which contain the word 'synonymous'. The conception of syntax which Bar-Hillel advocates is formal, rigorous, and structural in the sense that any distinction which the linguist is interested in establishing for a particular language can be attained by listing the elements which are held to have the properties in question. In particular, we can solve the problems of synonymy and transformation in English by the formal procedure of listing synonymous pairs under the heading 'synonyms' in the grammar, and transformational pairs under the heading 'transformations'. On the other hand this conception is empirical in the sense that it urges linguists to search for ways to eliminate the need for arbitrary listing by finding operational tests to determine these relations. I have pointed out that if they find adequate tests, then logical syntax and semantics will scarcely interest them, since the tests alone will apparently solve exactly as many of their problems as the tests together with the whole edifice of logical syntax and semantics.

[15] I. Scheffler, On synonymy and indirect discourse, to appear in *Philosophy of science*. A special case of the same point was discussed by Benson Mates, Synonymity, *University of California publications in philosophy* 25.201–26 (1950), reprinted in *Semantics and the philosophy of language* 111–36.

[16] For instance, Goodman's account, in *The Structure of appearance* (Cambridge, 1951), of the notion 'all alike', or 'at a time at a place' as opposed to 'at a time and at a place'; or Austin's account of 'call', 'describe', and 'match', in How to talk: Some simple ways, *Proceedings of the Aristotelian Society* 53.227 ff. (1952/53). Although these and other particular semantic studies, whether in the form of models or not, may have some interest for linguists, they do not clarify the general notion of meaning or synonymy, and they provide no grounds for demonstrating that the conclusions at which they arrive are correct descriptions of the way in which language is used. Hence they have no direct bearing on the program of developing an adequate linguistic theory.