FICTITIOUS SENTENCES IN LANGUAGE

by

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The purpose of this paper is to refute the argument contained in a paper entitled A Demonstration pf the Non-Feasibility of Fully-Automatic High-Quality Machine Translation, by Y. Bar-Hillel, which is included as Appendix IV, in his Report on the State of Machine Translation in the United States and Great Britain, (February, 1959), prepared for the United States Office of Naval Research, Information Systems Branch.

I

At a superficial level (that is, at a particular level), this argument is so far from being a "demonstration" that it is all but self-refuting, since experience shows that English speakers do not understand the test sentence anyway (see Appendix I), which is perhaps why it is difficult to persuade Mechanical Translation research workers to take Bar-Hillel's "demonstration" seriously.(1) Moreover, on the very curious linguistic presuppositions which Bar-Hillel has to make in order to preserve his test sentence from being gibberish, it shows up a thesaurus-procedure, or interlingual translation programme of any kind, not badly, but well (see Appendix II); Bar-Hillel, in transposing a common sentence to make a trick one, has forgotten the possibility of there being a thesaurus-entry for the word in, or a clause-bracketing programme to govern the thesaurus-procedure.

But if these superficial answers be discarded, as well as the more superficial aspects of the paper - including Bar-Hillel's highly Pickwickian use of the term "demonstration" - and, moreover, if this paper, which is short, is taken, not in isolation, but in conjunction with some of Bar-Hillel's other writings,(2) then, as it seems to me, issues of a fundamental philosophical and logical nature are indeed raised by it, and it is these implicit underlying presuppositions of his papers which I shall endeavour to bring out and to answer.

I shall take three sentences of Bar-Hillel's in conjunction with one another, giving their provenance; since to my knowledge he has already constructed at least three such sentences to show the unfeasibility of F.A.H.Q.M.T. (Fully-Automatic, High Quality Machine Translation), and clearly he could construct many more. The obvious policy, therefore, to adopt - in contending that F.A.R.I.M.T. (Fully-Automatised Reasonably Idiomatic Machine Translation) is indeed feasible - is to extract the common features, and deal with those.

The first sentence, S^1, is the one given in the paper itself. It is:

S^1 The box was in the pen.

The paragraph context given for it, p^m (S^1) is as follows:

P^m (S^1)* Little John was looking for his toy box. Finally he found it. THE BOX WAS IN THE PEN. Little John was very happy.

It is relevant to the argument which I shall try to develop that p^m (S^1), as stated, is in stilted English.

The second sentence, S^2, is contained in Note 6 of the same paper.(3) It is:

S^2 The inkstand was in the pen.

No linguistic context is given for this, so I will construct one. Since children and play-pens were clearly running in Bar-Hillel's mind at the time he wrote this paper, I will make the context follow on from p^m, and therefore call it P^{m+1} (S^2).

P^{m+1} (S^2) A few minutes...John was heard chortling with happiness. His mother, warned by this sign, hastened to see what he was up to.
"Good heavens!" she cried. "John is covered with ink. Where on earth did he get hold of it?"
She had not far to seek. The idea of putting

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* This notation is simplified, to enable me directly to compare the three sentences which I wish to compare. In fact, 'The box was in the pen' is S^3 in p^m, not S^1, and so throughout.

> things in play-pens had rooted itself in the mind of John. The hand-lamp was in the pen. THE INKSTAND WAS IN THE PEN. The living-room cushions, five books picked at random from the bookcase, his mother's apron, two screws, a tin of detergent and a half-used stick of shaving soap, all these, swimming in a sea of ink, were now in the pen.

S^3, unlike S^2 and S^1, cannot be given a context in English. Either it needs to be amplified by the addition at the end of ["drawer"] - the word in square brackets, - or it needs to be translated into a language in which i) "table" means both "large stool" and also "classification in tabular form", and ii) "in" - as in Latin - means both "in" and "on". It was the example which was discussed when, in the presence of about 11 other M.T. research workers, Bar-Hillel held a discussion with members of the Cambridge Language Research Unit at Washington, D, C.(4)

S^3 The book was in the table.

So, juxtaposing the three sentences, and forgetting for a minute about their context, we get:

S^1 The box was in the pen.
S^2 The inkstand was in the pen.
S^3 The book was in the table.

Now, at first sight, it looks as though S^1 , S^2 and S^3 were just three more of those trick sentences which your friends inflict upon you as soon as they hear that you are interested in Mechanical Translation. Some of these have now found their way into the Press, though they were circulating in Cambridge, England long before Mechanical Translation was ever heard of. Thus, "The whiskey was good, but the meat had gone bad" is now said to be a mechanical translation into the Russian of "The spirit is willing but the flesh is weak"; and "The invisible man is always insane" is now said to be a mechanical translation into Japanese of "Out of sight, out of mind".(5) And if it is grammar and syntax which are being alleged to be mechanically untranslatable, you are given, "Jones, where Brown had had had had had. Had had had the approval of the examiner".

Further examination, however, will show that it is a particular kind of trick sentence which is under consideration

here. And here we must use intellectual considerations to make up our minds which of the features of S^1, S^2 and S^3 are germane to Bar-Hillel's argument, and which are not. Thus, I do not think that it is, in fact, germane to Bar-Hillel's argument that S^1 , S^2 and S^3 all end with the form "in the X", though I think that such trick sentences in English will often tend to include some such prepositional phrase. I think it possible, that is, that either Bar-Hillel or I could construct a sentence which conforms to the specification which I am going to give, but which did not contain any such prepositional phrase. Neither do I think it necessarily characteristic of such sentences, as it is of S^1, S^2 and S^3 , that when subjected to the human translateability test described in Appendix I, they all fail. That is to say, in spite of the fact that S^1, S^2 and S^3 all fail under a human translateability test, (partly because the man who constructed them is having to build up a complicated argument in a language other than his native tongue), in that it can be shown that an ordinary American or English speaker fails to understand them, I do not think it out of the question that either Bar-Hillel or I could construct a sentence which conforms to the specification which I am going to give, but which would be readily understood by a British, English or American-English speaker if it was said to him.

I will now proceed to the actual specification. As I said earlier, I believe that what distinguishes S^1, S^2 and S^3, that is to say, Bar-Hillel's trick sentences, from other trick sentences which are adduced as counter-examples to the possibility of M.T., is the fact that <u>all of them are transpositions</u>. That is to say, all of these three sentences are, formally speaking, transpositions of frequently occurrent sentences in English; and, of course, semantically speaking, they are sentences capable of carrying a considerably different sense from the sentences from which they have been transformed. Thus, if we use the arrow,—>to mean "transforms into", we get the following:

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* I say, "capable of carrying" rather than "carrying" on the assumption that such sentences could be constructed which would pass a human translateability test. S^1, S^2 and S^3 cannot be described as in any direct sense "carrying meaning" because, when they are said to people, they are in fact not understood.

$(\exists S).(S \rightarrow S^1)$ "The pen is in the box" "The box is in the pen",

$(\exists S).(S \rightarrow S^2)$ "The pen is in the inkstand" "The inkstand is in the pen".

$(\exists S),(S \rightarrow S^3)$ "The table (classification in tabular form) is (printed) in the book"->"The book is [on] the table", or "The book is in the table [drawer]."

What we now have to discuss is whether this transposition characteristic of S^1, S^2 and S^3 is indeed the vital characteristic which would be possessed by all the indefinitely large set of sentences which Bar-Hillel says that he could find in texts or else himself construct(5) and which he alleges could be shown to fault any type of thesaurus M.T. programme in a demonstrable way; or whether it is only a special case of a more general characteristic which the total set of such Bar-Hillel sentences would possess. And here, even at the risk of seeming tedious, I am going to set out at length the full list of characteristics which, to satisfy me - that is, to serve, in the context of the present discussion, as M.T. counter-examples - any member of such a set of sentences would have to possess.

SET OF CONDITIONS REQUIRED OF A COUNTER-EXAMPLE TO THE POSSIBILITY OF HIGH-QUALITY M.T.

1) It must come out of an actually occurrent text. It must not be constructed by any logician, and particularly not by a logician writing and thinking not in his native language.

2) It must be shown accompanied by at least a page of that naturally occurrent context. This context must consist of, if possible, the paragraph in which the actual sentence under discussion occurs; ii) of the paragraph immediately before, and iii) of the paragraph immediately after.

3) It must pass a human translateability test, of the Appendix I type.

Actually this test, which is merely a rough and ready comprehensive test, is considerably weaker than a genuine translateability test; and if I knew how to make a general formulation of it (that is, not by reference to any particular pair of languages), I would require a genuine translateability test to be satisfied by any sentence alleged as a counter-example to M.T. For one of the great blemishes which runs through

Bar-Hillel's whole report is that, while several times claiming that he does not require of F.A.H.Q.M.T. (Fully Automatic High Quality Machine Translation) that it should achieve an output which a competent human translator could not achieve, he shows himself quite insensitive to the early points at which, for various reasons, competent human translation will break down(6).

Thus, he accuses M.T. of being able to set various traps for the unwary scientific reader: without mentioning that the average (non-scientific) translator is liable to set all the same traps (see, for instance, Savoury's instance of human mistranslation of a scientific text in his book on Translation(7). Moreover, with regard to any pair of languages, there will exist not a few but many simple, frequently-used, harmless-looking sentences in either of the languages which no translator, human or mechanical, can translate. (Try, for instance, translating "he shot the wrong woman" into French).

The sentence, then, which is to be a counter-example to M.T. must pass, with reference to the language into which it is desired to translate it, an agreed human translate-ability test.

4) It must demonstrate an inconsistency, not merely a deficiency, in any method of mechanically translating via a thesaurus, or an interlingua, or by setting up a system of semantic congruencies(8).

It seems to me evident that this is what Bar-Hillel is trying to do, with his reiterated transpositions, S^1, S^2 and S^3. He is not saying, merely, "No such method will translate S^1, S^2 and S^3. (cf. as an instance of such a method, Appendix II). What he is saying, by implication, is "Any method which will translate any S of which it is true that $(\exists S)$ $S \rightarrow S^1$, will not translate S^1; any method which will translate any S of which it is true that $(\exists S)$ $S \rightarrow S^2$ will not translate S^2; and any method which will translate any S of which it is true that $(\exists S)$ $S \rightarrow S^3$ will not translate S^2", Assuming, as he does, therefore, that a thesauric, or interlingual, translation-method will not combine with any syntactical or grammatic clause-bracketing programme sufficiently for the grammatico-

syntactic information given by transposition to become available to the thesaurus, (but see Appendix II) he thinks himself justified in further assuming that any permutation, transposition or other changed version of any common sentence in a language which affects the semantic content of the constituent words while leaving the semantic congruence procedure unaffected, will automatically fault the procedure. And assuming, as he further and finally does, that such permutations and/or transpositions are common in language, he thinks himself justified in drawing the unqualified conclusion that by the chain of arguments which he has just presented, Fully Automatic High Quality Machine Translation is "proved" to be impossible.

II

I do not think, for reasons which will become apparent as I build up my counter-argument, that such a type of alleged counter-example as Bar-Hillel has given is the best one for testing a thesaurus procedure for consistency(10): But I do think he brings vital issues up, namely the underlying differences between two conceptions of language; and therefore I think his alleged "demonstration" deserves a general and not merely a particular answer.

Before I proceed to try to establish this, however, there is one last particular remark which it seems to me relevant to make. That is, that with regard to all extant mechanical-translation programmes, whether fully automatic or man-assisted, the existence of naturally occurrent sense-carrying deviations of primer sentence patterns will have to receive a great deal more attention from M.T. workers in the future than they have had in the past. I cannot find any naturally-occurrent case of an actual transpositions: that is, I cannot find a naturally-occurrent analogue of S^1, S^2 and S^3 ; but I can find and have found naturally-occurrent sense-carrying ellipses, stylistic inversions and other deviations from common sentence-forms in language which, in my view, would fault any grammar- or syntax-finding programmes at present being used in experimental M.T. (Including, let me be quick to add, the monolingual sentence-finding programme of the Richens Interlingual Translation Programme: that is, the sentence-finding M.T. programme at present being used for experiment by the Cambridge Language Research Unit.) And if I am right in my general analysis of Bar-Hillel's examples,

those also, and especially the stylistic inversions and ellipses*, would come under the general category of sentences which he wishes to allege as counter-examples of M.T., even though they are not the same special instance of it as S^1, S^2 and S^3. The subject-matter of my counter-example, which is to be found together with a man-made translation, in Appendix III, is the first paragraph of the first Book of Caesar's Gallic War - the nearest thing in classical Latin to a factual scientific text, I do not believe, as I say, that any extant M.T. programme will translate it, and for Bar-Hillel-like reasons; namely, that the very grammar-and-syntax-finding directions which correctly distinguish normal word-orders and non-elliptical forms in any language will prevent the same programme from correctly distinguishing transposed word-orders and elliptical or inverted grammatical forms in the same language. And this is serious. For though it could be said that in this particular first paragraph, J. Caesar was polishing up his style in a perverse manner to impress the Pentagon, so do contemporary scientists. The mechanical translation of this paragraph, therefore, unlike S^1 , S^2 and S^3, constitutes a genuine and legitimate challenge to all M.T. grammar- and syntax-finding programmes. But the discovery of the extent to which this paragraph constitutes a challenge to M.T. had the converse effect from that which the idea of constructing S^1, S^2 and S^3 appears to have had on Bar-Hillel. For it was the fact that this particular paragraph, having failed wholly to yield to treatment by any ordinary M.T. method, responded to a surprising extent to punched-card analysis made with an ad hoc thesaurus which settled me finally to thinking that only a thesaurus-using procedure, extended to cover what can be semanticized, and therefore, interlingualised, of grammar and syntax, has any chance at all of correctly translating the basic sense of a stylistically inverted and highly elliptical passage such as this.

Having now produced my own particular M.T. counter-example, as well as particular comments on S^1, S^2 and S^3, I now propose, using S^1, S^2 and S^3 as a starting point, to try

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* A semantic-sense-carrying ellipsis in a sentence, represents a worse case even than stylistic inversion or a complete transposition. For how, it might be said, can a thesaurus-procedure, or any other procedure, draw semantic consequences from an initial state of affairs which does not occur in the text?

and delineate, in a philosophic and therefore completely general manner, the two vitally-differing conceptions of language which underlie the differing research suggestions currently being made in the relevant fields. For attacks on the possibility of ever achieving any adequate machine translation, such as Bar-Hillel's attack, derive all their plausibility from an unstated initial assumption. This is that that conception of language which the attacking author himself develops in the course of his attack is the only one which any M.T. worker, from the very fact of being an M.T. worker, can possibly hold.

This assumption is untrue; and it has only to be dragged out into the light to be shown to be untrue. But the argument then becomes an argument between philosophers; between the only two philosophers, in fact, at present engaged in the field. For, for the last twenty-five years, a growing and developing trend in Western philosophy - one associated, not always very happily, with the late work of the philosopher, Wittgenstein [12], - has been subjecting to increasing corrosion by detailed criticism that older conception of language, which is usually associated - also inaccurately - with the publication of Logical Syntax of Language[13], by Carnap, and which Bar-Hillel still holds. And it is an attempt to "formalise" (in a weakened sense of "formalise") what can be formalised of the newer conception of language which has produced the Cambridge Language Research Unit's form of the thesaurus approach to Library Retrieval and to M.T.; whereas it is the attempt consistently to apply to actual languages the restrictive formal criteria of the older conception of language which has led Bar-Hillel to deny the possibility of M.T. And the vital issue which Bar-Hillel's "demonstration" brings up is that which is right of two M.T. workers, one alleging the possibility and the other the impossibility, of adequate M.T., depends in the end on which is right of two conflicting philosophies of language. And whereas, with regard to the first of these, the Carnap philosophy, Bar-Hillel seems to assume that it is the only possible philosophy of language for M.T.; with regard to the second, the Wittgenstein philosophy - together with all its literature - Bar-Hillel gives no hint that he is aware of it at all. And since he is now carrying this distortion of philosophic vision to the practical point of urging discontinuance of financial support for research on

high quality M.T ([14]), that is, of those who are intuitively following a rival philosophy of language to that which he himself holds, there seems nothing for it here and now but to have the general philosophical argument right out into the open, however long it takes to get an agreed and satisfactory statement of it. And this is a good thing. For on the one hand, the fundamentals underlying M.T. need much more, and much more simple, philosophic re-examination, and on the other hand, Western philosophers are purblind in ignoring the intellectually gigantic implications of M.T.; so that an attempted general rediscussion, at this point, of the philosophy of language, seems to me likely to render considerable service in the long run, both to philosophy, and to the whole enterprise of technical research into language.

Let us return to the fact of sense-carrying transposition, in language, as a point of start. I will begin by requoting what I said about S^1, S^2 and S^3, namely, that each of them can be described as a transposition of presumed frequently occurring, because truistic and banal, sentence in English. Thus, the frequently occurring sentence, "The pen is in the box"*, transforms into S^1, "The box is in the pen"; the presumed frequently occurring sentence, "The pen is in the inkstand", transforms into S^2, "The inkstand is in the pen"; and the (not so truistic, but still quite normal sentence), "The table (classification in tabular form) is (printed) in the book", transforms into S^3, "The book is in (on) the table", or "The book is in the table (drawer)".

Now, once we admit that S^1, S^2 and S^3 are transpositions, by that fact alone we presuppose that the three sentences of which they are transpositions are, in some, up to now, undefined sense of "normal order", in normal order. For the existence of a sentence which is a transposition of a sentence in normal order presupposes the existence of a sentence in normal order; it's as simple as that. Thus, generalising, we get the idea of "normal sentences in English", using these words in a sense part of the meaning

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* The really frequently occurring, in the sense of truistic, sentence (in British English) is, "My pencils are in my pencil-box, (or pencil-case); i.e., they're where they ought to be". But we will allow that this has variants.

of which is, "sentences in English which are in normal order".

But now, (meditatively, rather than thinking strenuously), transpose the adjective "normal". Instead of saying "normal sentences in English", say instead, "sentences in normal English". Between the sense in which "normal" is here used in the phrase "normal sentences in English," and the sense in which "normal" is here used in the phrase "sentences in normal English", there lies the gulf between two different conceptions of language. Because the first "normal", - using the phrase "sentences in normal English" as we have here been using it, presupposes a normal order for non-trick sentences in English, USING THE WORDS "NORMAL ORDER" IN THEIR MATHEMATICAL SENSE. That is to say, when I presuppose a normal order for non-trick sentences in English, the kind of order which I construct, in my mind, is the same kind of normal order which I construct, in my mind, when I am writing out the rules of a proposed Combinatory Logic, in which, when any formula occurs out of normal order, it is always written preceded by a combinator-formula consisting of a precisely interrelated string of primary combinators, which precisely defines, according to the rules of the system, the nature and extent of the deviation of the deviant formula from the corresponding primitive formula which is in normal order (15). Thus, in a Combinatory Logic, or in any "language" which uses the same exact mathematical conception of "normal order", there can be well-formed formulae, that is, meaningful sentences which are permutations, or bracketings, or re-operations, or repetitions of sentences in normal order. In "Combinatory English, which is what Bar-Hillel, according to me, has substituted in his mind for normal English, S^1, S^2 and S^3 would * be examples of these. Thus, we could precisely define, - within a quite ordinary Combinatory Logic, - the permutation of a formalised sentence in a pre-established normal order which was made when, <u>instead</u> of writing, in the formalised notation, "the pen was in the box", we wrote instead, "the box was in the pen", and so on for S^2 and S^3 . But all such operations presuppose that view of language which says that the ordering of the total set of all sentences within such a language (within Combinatory English, that is, but not within normal English) can be defined by using a calculus (either a Combinatory Logic, or some other calculus). And this is the view of language which, unless I am mistaken

Bar-Hillel holds, and which, unless I am mistaken again, Chomsky desires to hold. Bar-Hillel however, is unlike Chomsky in this, that he not only assumes without question that this is the correct view of language, but holds intuitively that there could not possibly be any other.

Now, however, in contrast to the mathematical use of "normal" made when we consider sentences in normal order, consider the quite different use of "normal" which we make when we use the phrase, "sentences in normal English". Behind the phrase "sentences in normal English", there lies a quite different set of implications from those which lie behind the phrase "sentences in normal order", or "normal English sentences", meaning the whole set of these. Here, in "sentences in normal English", "normal" is taken to mean "as actually spoken", so that the whole phrase now means, "sentences in English as English is normally spoken"; and this kind of normality is a social-scientific kind of normality, not a mathematical one. That is to say, on this second criterion of normality, you succeed in talking "normal English" when, and only when, you succeed in making yourself unselfconsciously and easily understood. Similarly, you act according to the normal customs of your country, (say, according to the normal excretory customs of your country) when you do, or don't do, what everybody else does, and, in this sense, one can ask of someone whom one suspects of having been taken ill, "Was his behaviour at the office normal?"

Now, this second view of language, (according to which, if we may trust the compiler of Appendix I, neither S^1, S^2 or S^3 would be "normal") presupposes in its turn that if language is to be theoretically examined at all, it will be theoretically examined in the same sorts of ways as those in which a social structure or an economic system are examined. In other words, you first take the language, (like the society) as it is and as a totality: and you then see how much mathematical or other* structure you can discern in it; instead of

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* An example of such "other" structure in language would be the classification of concepts made successfully and intuitively, and with the aid of adequate cross-references, by a good librarian. He can arrange semantic concepts in such a way that he can encode them as to provide a scale of their relevance for any given retrieval; when, if he listened to the mathematico-logical advice given him at I.C.S.I. (Area 6) by Bar-Hillel, he would correctly infer that the whole enterprise of making such a library classification, is impossible.

first taking some mathematical calculus, as you find it or as you construct it, as your totality[16], and then seeing how much of actual language you can fit into it.

Now, each of these two views of language is held by philosophers, and each, therefore, has a whole philosophic literature behind it. Since, however, it is notoriously difficult to arbitrate between philosophers, it is worth asking immediately, though as one criterion only for arbitration, which school of thought is favoured by structural and descriptive linguists. A priori, one would say at once, "of course, the second", since linguists are concerned with language as it really is, not with language in so far as it will fit into a logical system. And indeed an inkling that it is the second view of language which is ultimately going to count most, and that the later work of Wittgenstein, as well as his earlier work, has a contribution to make to a general language-theory, is to be found in the writings of at least one distinguished contemporary descriptive linguist.[17] But if the linguist is concerned with language as it is, he is not yet concerned with language as a whole; or rather, having been earlier concerned with it, in forming the older, evolutionary philology, he has now found the factual and empirical foundations of his inferences to have been unreliable, and is now devoting himself wholely to remaking those foundations with more scope and with more precision. For the moment then, linguists are not concerned with language; they are concerned with texts. If, therefore, hope can be given them that the transpositions, expansions and permutations of the presupposedly more primitive lexico-grammatical units which are found in the actual bounded piece of linguistic material which they are taking as their text - if all or any of these can be accurately predicted by the mathematical logician with his calculus, or even by a progressive series of mathematical logicians with their calculi (but remember the limiting case: one transformation, one calculus), then the descriptive linguist tends to adopt a mathematico-logical view of language, and to take Carnap and Reichenbach, not Wittgenstein, as his tutelary philosophers. That Harris and Chomsky should have done this is no cause for wonder; for Harris conceived linguistic investigation as, in effect, applied logic even when he was a pure descriptive linguist[18]. That Quine should also have done this, though

with caveats, is more serious; for Quine, as well as being a logician, is a philosopher. The clarity and candour with which Quine gets into every kind of depth-philosophical trouble as soon as questions arise which bear on the foundations of symbolism is only equalled by the 5th Century Greek virtuosity with which he maintains and develops a quasi-Ptolemaic view of language, which a moment's reflection should convince him cannot work. But this, though a fascinating topic for debate, does not come up for discussion here.

Philosophically speaking, then, as well as practically also, the type of mathematical linguistic[19] approach which I have outlined breaks down for two reasons. It breaks down firstly, because of its complexity; the slogan, "one transformation, one calculus" parodies the kind of proliferation which it easily gets into, and which it can ignore only by drastic linguistic amputation. It breaks down secondly, also, and more fundamentally, because it never crosses the theoretic barrier from texts, (however many texts) to language. The number of transformations formally discernible in a text, however many of these there may be, and however few instances of each there may be, and however many levels of analysis the linguist may allow himself, in order to interrelate them in the most complete possible manner, is still finite. The number of possible uses of words in a language is not. And so - this is very often said - the finite counting and distribution-finding procedures normally used by the linguist to establish his complementarities, - these, as soon as he extrapolates them from texts to language, all become infinite, and so can no longer be used. And the symbolic logician of the Reichenbach-Bar-Hillel-Chomsky-type, far from being able to help the poor linguist in this crisis of extrapolation, regresses from him ever further in the wrong direction, by taking only one typical factor in the linguist's textual analysis, such as the formation of verbal derived forms, or some other such phenomenon, and makes up an elegant calculus to analyse that, coupling his practical activity with Gödel-like proofs that any type of model the linguist will tend to think of making for himself, will be necessarily inadequate.

Now it is the fascination and challenge of Fully-Automatic Reasonably Idiomatic Machine Translation that it straightforwardly requires a general theory of language to underpin it; a proliferation of calculi each handling a single grammar-transformation, or a proof that, say, a stochastic language model is inadequate, or a series of discouraging prophetic remarks, none of these will do. It is not unreasonable, then, in this sharp theoretic crisis (and crisis it is, for there is a widespread, and probably correct intuition about that mechanical translation is coming, and yet a total inadequacy of logico-linguistic ideas to deal with it) to turn to the other philosophy of language, which has been developing and spreading over the Western world in the last twenty years - even if, at first sight, this second philosophy of language should seem to have nothing to contribute to M.T.

III.

It is worth noting, - by those who tend to underrate his genius, - that both views of language, the logico-linguistic view, and the ordinary language view, derive indirectly from the philosopher Wittgenstein. The first, the Carnap-Reichenbach-Bar-Hillel view, derives from the publication of Wittgenstein's Tractatus Logico-Philosophicus (1922) which first brought the work done in England, by the mathematical logicians Russell and Whitehead, to the notice of a great many German and Austrian philosophers of science, headed by Maurice Schlick. The second, the general "ordinary language" view, - which leads, in part, to the thesaurus view, - derives from Wittgenstein's Philosophical Investigations (1953), parts of which, having been dictated to students at intervals from 1933-4 onwards, were in circulation in note form for many years before their incorporation into final book form.

Having made this one historical remark, I intend to make no others; but to try first to give a short and sketchy account of this philosophy in such a way as to show its relevance to the establishment of new kinds of research into language, and then apply it to the problem of S^1, S^2 and S^3. It must be emphasised that the account which I shall give will be, inevitably, selective, subjective and tendentious. There is nothing more irritating to philosophers who share a common

field of interest rather than a common point of view, to have one of their fellow-philosophers, afflicted by sudden megalomania, saying, "Yes, there were indeed several quite good points in all those papers which you all wrote; they were in fact pre-indications that the Movement would in the end produce a real theory, - my theory". What the thesaurus analysis of language presents is by no means the Truth (still less the Whole Truth) behind the general philosophy of ordinary language; it does suggest, however, a practical application of some of its ideas, which is a quite different thing. And there is something to be said for an account being given of this type of philosophy which claims for it a possibility of practical usefulness; for it is generally so fiercely attacked for being dilettante and non-useful, for being anti-rationalist, and generally torpor-inducing to the mind[20], that it will come as a shock to its detractors as well as to its upholders to hear that one philosopher at least considers that it embodies fundamental and original logico-scientific insights which can be tested out on punched-card machines.

To cap all, this kind of philosophy is still in a seminal state; moreover, the manifestos which have been compiled about it, from time to time, have not been undertaken, in my view, by these authors who understood it most profoundly[21]. Having now underlined the difficulties of proceeding, let us proceed.

Putting it very extremely, and quite a bit tendentiously - the philosophers of "ordinary language" claim, following Wittgenstein, to have discovered a method of analysing different kinds of structures of thought by distinguishing from one another cognate, but differing contexts of words in language. This new method, according to them, will render obsolete all other general ways of investigating thought; though not the special activity of constructing formal systems and not all other empirical ways of investigating languages. Wittgenstein himself, according to G. E. Moore[22], claimed for the method which he thought he had discovered the same sort of originality which Newton showed in constructing his mechanics, namely, the act of taking an intellectual step forward, in thinking about thinking, which the whole intellectual world would finally see to be irrevocable: to be something

which could be henceforward corrected and developed, but not gone back on. It was also claimed for this method that it was psychoanalytically therapeutic[23]; when you saw - having sufficiently[24] compiled a linguistically full framework of actual concrete contexts for any given concept to see (without being told) that to do this was all you could validly ever do, when thinking, - then suddenly, a kind of Zen Buddhist Enlightenment would come upon you, and you would see the futility of ever engaging in further "philosophic" (i.e. non-concrete) thinking. (It is worth remarking that this theory wholly failed to work on the present author, who, in consequence, was expelled from Wittgenstein's class early in 1933, for "not understanding anything", i.e. for deviationism.) Thus, this philosophy of thinking stultified itself; you talked a very great deal in order to render yourself dumb.

Without making any further general attempt to investigate this paradox (to which so simply enumerative a conception of concept, - formation will inevitably lead), I will now state, almost non-philosophically, i.e., with complete baldness, what I think Wittgenstein's insight really was. I do not think he had developed a new method of investigating concept formation: for, as has later emerged, his way of establishing contextual distinction was, in essence, only a refinement of the traditional dictionary-makers' way. The intuition which he had, and which I believe to be indeed irrevocable, was 1) that the units of any logic which professed to throw light on language would have not to be the word (Aristotle's term), still less the sentence (i.e. Russell's proposition), but the context of a word - and that this was a possible unit, and 2) that such contexts, by their nature, could never be defined; the thing which it is sufficient to do - and, indeed - all that by the nature of the case, it is possible to do - is to distinguish them by providing each with an analogy[25]. Each analogy made you "see" the original concept - that is, the total imaginable set of distinguishable contexts of a word, - under a new "aspect"; thus, a "concept", thus envisaged, was like gestalt psychologists' figure[26].

Of course, this was only part of what Wittgenstein was trying to say in Philosophical Investigations; he talks about very many other things besides language. But in the case of these aphoristic, prophetic, intuitive, and yet mathematical, thinkers, like Wittgenstein and Whitehead, to make an actual application of part of what they are trying to say - "to think some of the same thoughts"(27)over again - much better than, by reiterated attempts at overdose imitation, unconsciously to distort or misunderstand the whole.

One more thing before we leave Wittgenstein: to say that he had got hold of a new unit, which must form the basis for any new systematisation of language, involves saying of him, by implication, that, in his later work, as in his earlier work, the mathematical logician latent within him was by no means asleep; that the Wittgenstein of the Philosophical Investigations was far more like the earlier Wittgenstein of the Tractatus Logico-Philosophicus than at first sight appears. This interpretation, I am prepared to stand by. Wittgenstein, all through his later work, was for ever searching for system-indications which he could not find. Thus he used to speak a great deal, at one time, about "rules of grammar"; but Moore always protested that he (Moore) could not find these, however hard he tried. At another time, Wittgenstein spoke of the difference between language-games, and between different notations (within language): but when he was pressed, only mathematical examples of notation could ever be given*. It is my view that by keeping Wittgenstein's vision, but by abandoning his method - or rather by abandoning that exclusive and unperceptive use of his method which is at present being made by most of the so-called "Oxford philosophers" - in favour of actually constructing and mathematically transforming collections of concepts - it may be possible, in the end, to interpret also, even if only partially, some or all of what he says about rules, and logical grammar, and, notations and language games. And this is the only way, I am convinced, we shall ever get to understand it. Wittgenstein was practical; his life shows that he was; he was trained as an engineer and designed a jet-engine in 1911; he invented truth-tables; he designed a house. It is practical application, with its element of daring in betting on the truth of what you are applying, rather than endless repetitions, hagiographic comment, and textual literary criticism, that his work cries out for.

* Ryle, also following Wittgenstein, talks of making category mistakes in using language; and yet, try as we will, one can never, in the sense required, find any categories.

This point is well made by Professor R.C.Cross (Category Differences, Proc. Arist.Soc.,1958-59) "The idiom of "categories", "category differences", "category-mistakes" is familiar in recent philosophy. There have, too, been signs of dissatisfaction with it. Mr.Hampshire in his review of The Concept of Mind, refers to "such notoriously obscure expressions as "logical category", and Mr.Warnock concludes that - "it is not ... unreasonably over-scrupulous to be ill at ease with an idiom which has not at all of the precise backing which it naturally implies."

To return now to our general survey of this new view of language: it is evident that if even in the work of the man who originated it, it is in such a very early and seminal state, that all it is likely to have developed into is a set of scattered intuitions about language. But they will be general and non-obvious intuitions, - that's the point; unconnected, difficult to interpret, rough-hewn, with untied ends, unfinished, undigested, almost dreamlike; but unlike the inductive work of Chomsky and Lambek and Harris, really telling us something about language which we did not know before. For, as I have hinted, it is important to see, when examining the formal systems constructed by Chomsky[28] or Lambek, or any other mathematical logician who builds on linguistics, what very conceptually obvious bases these systems have. Bar-Hillel speaks[29] of Chomsky's "deep insights into language"; he is wrongs linguistically speaking, Chomsky's insights are not deep, but shallow. That verbs have derived forms, which can be envisaged as transforms of their primary forms (Chomsky) - that prepositional and other clauses occurring in a subject-predicate sentence can be logically parsed as either extensions of the subject or extensions of the predicate (Lambek) - both of these things about language we knew before. And when we ask something we didn't know before, they can't tell us. For instance, when, they having declared that grammar and/or syntax in language consists of transforms of primary cores, or kernels, when we ask them, as a practical matter, to help us find these cores or kernels, (to find which would indeed need a deep insight into language) they say they can't: they are unable to give us any reply.

Now it can be correctly retorted, (in every context but this), that such criticism does not ever apply to strict applications of inductive method which is what Chomsky and Harris are making to language; that linguistics would be in a poor way were it left to the dreams of philosophers; that what descriptive linguists are trying to achieve is an exact natural history of languages, not a general theory of the foundations of symbolism, which is what the philosophers are after; that Lambek and Chomsky are not aiming at depth, but at mathematical rigour; and so on. But precisely the issue raised by Bar-Hillel in this report - the vital, fundamental theoretic issue which he rightly brings up - is that with our present conceptual apparatus we can't get a semantic theory of language of sufficient generality to underpin M.T. And it's quite

true, by using the Lambek-Chomsky-Bar-Hillel-Harris analytic method we can't. But this doesn't mean we can't get it at all. After all, in spite of the terrifying notational complexity generated by linguists, and infinity-ridden recursive proofs of mathematical logicians, ordinary people do succeed, quite often, and quite quickly, in talking with one another. What we want (and what the thesaurus method does) is to throw some light on how it can be that people do this; instead of just saying, though by implication, that the communication situation is so complicated, that two people can never talk at all. And this is why it is worth taking such a very great deal of trouble to try to see exactly what is meant when the thesaurus method, with its quite different presuppositions from the logico-linguists' method, is put under attack in its turn by Bar-Hillel for theoretical inadequacy.

Let us go back to the new philosophy of language. Attention must first be called to the work of Waismann, who was not only, at one stage, Wittgenstein's closest collaborator(30) but who also, being himself by training a mathematician, comes closest to finding a systematisation of what Wittgenstein seeks. But we shall not find it. Waismann has written papers on Verifiability(31), and on Alternative Logics(32). In these he has pointed out the fundamental effects, on all "thinking about thinking", of that "open texture" of all words; this "open texture" can be seen to exist as the result of taking as the unit not the word, but its never wholly definable individual context. He generalises this idea that meaning has openness; and (in another way, but like Lee-Whorf before him) he stresses the dissolvent effect of this openness on the logician's proposition - the traditional unit of thought. He foretells the theoretic displacement also of the traditional method of philosophical analysis; in considering concepts with open texture, what use is, for instance, the Laws of Contradiction? But he does not, himself, ever juxtapose any such concepts, or construct any alternative system to that of normal logics, or give any indication that he knows what a construction of semantic units with open texture would be like. His work is anecdotal, though not negligible: and so it is not any practical good for underpinning M.T.

I will now list, briefly and contentiously, the other insights about language, which the new school of philosophers have had. No other, besides Waismann is by inclination a mathematician; and none, alas, is at present, as Lee-Whorf was, prepared to face the full horror of linguistic complexity. Nevertheless, logical poets are not to be ignored*[1].

1) A concept in language is like a gestalt figure. (One can "see" it differently by looking at it in various aspects.) Each of these "aspects" can be represented as a context. This insight is from Wittgenstein[26,33].

2) Undefinable, and, by normal methods indistinguishable contexts of a word, (i.e. "aspects") can be distinguished from one another by giving the sentence in which each occurs an analogy.

"Matter does not really exist."
"Beauty does not really exist."

The second of these two sentences provides an analogy to the first[34], making more precise the sense in which the speaker of the first does not believe in matter.

"You can say, 'I had the sensation of having understood him, but when I got home I found I hadn't', but you can't say, 'I had the hot burning sensation of having understood him, but when I got home, I found I hadn't'".[35]

3) There are "games" which can be "played", and/or "rules of logical grammar" which can be ascertained and/or notations which can be found even by looking at perfectly ordinary language. In other words, a general complex of "Somethings" which remind one somehow of grammar and/or of syntax, as well as of game-playing, and of notation-making, exists and can be found; and this is interlingual*[2].

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*1 Ref. to remark in his Spinoza by Stuart Hampshire, whose general view of the philosophy of language philosophers is the same as mine is.

*2 Critics have repeatedly accused the "ordinary language" philosophers of identifying all language with conversational English. This is unfair. Rightly or wrongly, their faith is that any move in a "language-game" or manifestation of "logical grammar", as opposed to ordinary grammar, could be exhibited somehow in any language.

4) That the primary patterns of how we actually talk (and think) are simpler and more fundamental than, and different from, grammatical pattern.[36]

5) That there is something fundamental to be found out about the beginning of thinking from the study of children's language, and of pidgin languages[37].

6) That, by employing a stateable procedure, dictionaries can be caused to produce closed circles of semantically analogous, rather than synonymous, definitions.[38] In other words, they can be caused to generate thesaurus-like heads.)

7) That there is something special to be learnt about language and about thinking, from the study of metaphor and paradox.[39]

8) That there is a profound and generalised logical sense, as well as a grammatical sense, of the imperative.[40]

9) That there is a special, logical class of performatory statements; that is of quasi-legal statements such as, "I take thee to be my wedded wife".[41]

10) That there is more to be learnt about the structure of actual argument by studying legal argument than by studying formal logical inference; and especially, something to be learnt about analogy by studying legal analogy.[42]

Now, in this whole series of outbursts of logico-philosophical prophesy, there are obviously some contributions which are fundamental, and some which are merely peripheral; moreover, every philosopher who has ever participated in such a movement (which is, I repeat, a movement of the mind, rather than an organised gang of conference-attending and badge-wearing philosophers) has his own ideas about where the basic insights are. What is historically true, and also relevant to the present discussion, is that the philosophic climate of opinion out which the Cambridge Language Research Unit's thesaurus approach to mechanical translation came, in so far as this required and was based on an antecedent "thesauric" (or content-based) view of language, was this new philosophy of language, not the older one. It is

only the theory, of course, which came out of this philosophy - or indeed out of any kind of philosophy. The application - that is, the idea of putting Roget's Thesaurus, or any other thesaurus, on to a computer to solve problems of library retrieval or of translation - this device has a multiple provenance. Christopher Strachey[43], David Hays,[44] Gilbert King[45], Bernier [46], H.P. Luhn[47], Roderick Gould[48], as well as members of the Cambridge language Research Unit - these at least, and probably others, had a hand in it. It is quite possible, moreover, that these will in no way relish being told that, (like M. Jourdain in Moliere's famous comedy, who, all his life had been talking prose without knowing it) for the last three years they have been exponents of the new context-based philosophy of language, - without ever having heard of this philosophy, and so without knowing it. And this is important, in the context of the present argument, since Bar-Hillel is making a theoretic claim, not a practical one, when he says that the use of such a device for retrieval is impossible, (At I.C.S.I., he was on similar theoretic grounds, saying that to compile a library retrieval thesaurus device for mechanical translation is, for similar theoretic reasons, theoretically impossible.) And I think that the philosophic insights which provoked this practical application - whether they were explicitly stated or not - were, first, that a language is primarily a totality of contexts, not of sentences or words, and secondly, that contexts can only be distinguished from one another by using analogy.

Of these, it is the second insight, not the first, which -provides, I think, the clue to the solution of the problem of S^1, S^2 and S^3. But before I go on to state how I think it does this, I must point out a relevant theoretic snag which arises from the first; namely, from the thought that a language consists of a totality of contexts, not of words, This snag is that if you envisage a whole natural language as context-based, (rather than as grammar-based, or sentence-based, or word-based) there results an immediate philosophic ambiguity about your consequent use of the words, "You can say..." Many of the current works of the philosophers of ordinary language implicitly presuppose (while explicitly denying) that only extant contexts of words count as within

language; so that you only can (logically, i.e. because of the very nature of thinking and of language)[49] say, what you can (causally, because of the accidental current state of the English language) say [50]. This is a distortion, and a parody, of the new view of language. Its criterion, as I said earlier, of a communication being made, that is, of a non-fictitious sentence being asserted in language - is not that no contexts except extant contexts should be used in the communication, but that the communication itself should be instantly and unselfconsciously understood. This easy communicableness, be it immediately said, is the touchstone of the non-fictitious use of ordinary language; not the touchstone of the non-fictitious use of legitimate language.

Esoteric poetry (which needs a special initiation to understand it) and mathematics, which needs not only a special initiation, but a lot of practice as well, - both of these are perfectly legitimate forms of language. They are not however, forms of ordinary language; and it can be intuitively seen that it is not such languages, however, clever, but ordinary language which is basic in thinking about thinking, in that it is only by using ordinary language, and a great deal of it, that any of the special languages can be understood. But now, for this kind of philosophy, the question acutely presses, "If instant and easy communicatableness is to be the basic criterion of meaningfulness, how the devil can any new context in language ever be created? How, if you only can (sense 1) say what you can (sense 2) say, can you ever say anything which no one has said before?"

This, of course, is where the first insight leads onto the second, for the prophetic answer (which is also, curiously enough, the linguist's answer) obviously is "By analogy". We create new word-contexts, and new phrase and sentence forms, by analogy; that is, by exploiting the same general kind of procedure as that which enables us to distinguish already extant contexts from each other; but we don't use the same variant of this general procedure. It is imperative, - this can't be too much stressed - that, on the general presuppositions of the philosophy of ordinary language, ordinary-language machinery for the creation of new contexts, as well

for the distinguishing of extant contexts, should be provided in any general scheme. And the down-to-the-bone realisation that this is so, leads inevitably to the general formulation of yet one more philosophic insight; an insight emerging this time not from general contemplation of the language-universe, but from the practical demands of M.T.

10)Language contains within itself its own machinery for creating by analogy new sentence-forms and new contexts. This procedure is the converse of that which language normally uses in order to distinguish already extant contents from one another; namely, the dictionary-maker's procedure of keeping the discussed word the same while embedding it in successive different sentence-collocations. The procedure for the creation of new contexts (that is, for thinking new thoughts) by analogy, consists, on the contrary, of the rhetoricians' procedure of keeping a set of sentence-collocations the same, and embedding in them successively words in old and new contexts.

As soon as this initially vague suggestion is taken seriously, it becomes evident how it applies to S^1, S^2 and S^3. For S^1, S^2 and S^3 are not merely sentential transpositions; they also, for all practical purposes, embody new contexts. "The box is in the pen" may or may not have been asserted before in English (I doubt myself that it has); "The inkstand is in the pen" has almost certainly never been asserted before in English; and "The book is in the table" can't be asserted in English, since it requires for its assertion a language with a wider range of uses for the word "in" than English has. If now, therefore, we re-examine these three sentences with the social-scientific general perspective of the philosophy of ordinary language in our minds, instead of with the normative mathematical perspective of the philosophy of Combinatory English in our minds, two things become clear. The first is the reason why I have produced at intervals in this paper (in my insistence on tests for counter-examples to M.T. and complaints of stilted English) so many apparently fussy requirements as to the ordinary-languageness required for these three sentences' contexts. The second is the set of conditions which must be complied with in order to prevent S^1, S^2 and S^3 being fictitious sentences in language.

I will deal with this second point immediately, as it gives this paper its title. In Combinatory English, as I said earlier, S^1 , S^2 and S^3 are not fictitious; they are straightforwardly well-formed formulae within the system, though not primary well-formed formulae. In ordinary language, however, they are fictitious in so far as they are agreed to be "new" sentences (that is, sentences creating new contexts in the language), except when they have been duly created new sentences, in the language by being displayed as such, with all the proper analogy. Language is like religion; it is ritualistic. New sentences have to behave like neophytes; they have to show themselves, when they appear in the sacred edifice of Language for the first time, correctly enveloped in the correct set of analogical contextual garments and with at least two old and tried sentences walking in front of them as sponsors. In other words, they have to be baptised in due form before they can graduate as new sentences within the language, i.e. as being sentences which you <u>can</u> say. And my contention is that the machine, like an unobservant social scientist, should be able to pick up at least some of the signs of a linguistic baptism; it should be able, at any rate, to pick up some of the signs of an important linguistic baptism; such as the baptism of S^1 or S^2 or S^3.

With this "language is ritual" image well in mind, let us painfully go back, for positively the last time, and reexamine the contextual surrounds of S^1 , and S^2 (S^1 and S^2 can be ritualistically incorporated in language; S^3 is plain fictitious, and must therefore, be left, like a Catholic unbaptised baby, in Limbo. Giving myself great space, great time and great freedom in constructing context, I will now proceed, as a particular linguistic full-close to my general argument, to construct and then examine logico-sociologically a continuous narrative context for S^1 and S^2* .

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*I tried hard to get someone else to do this for me, so that construction and analysis should not be by the same person; but nobody would do it. They advised me on the contrary to pour myself out a stiff drink, and then sip it gently, taking quite a long time drinking it. The only other type of context which I think can be constructed for these sentences, - namely the context in which they occur in Bar-Hillel's report, - I am leaving on one side for the moment; since it is a context of <u>mention</u>, not use, which would need a special thesaurus-device to deal with it.

The writer is one of those pompous child-psychiatrists; everyone who reads this will know just who I mean. The style is colloquial (colloquial, that is, for a psychiatrist); the man is giving a popular lecture to parents; one of those parent-ulcer-giving lectures which well-meaning psychiatrists invariably give when they want to increase the general angst level of our culture.

It is extraordinary how easily and with what rapidly established stereotypy a new form of action, perhaps performed by one or other of his parents in a wholly heedless manner, can incorporate itself into the stock of fixed behaviour-patterns of the child. In this connection, a story told me recently by a young mother, may serve to make my point clearer to my hearers than it would become if I attempted further exposition.

This young mother was preparing lunch in the kitchen in a house in which kitchen and living-area were contiguous. In this living-area was playing her three-year old son John - who should have been, but actually was not, playing in his play-pen. As it was, he was running about on the floor. In order to attract his mother's attention, every time she passed, carrying dishes, between kitchen and living-area. John would put, just between her feet or actually under them, the largest of a nest of cardboard square boxes, pasted over with coloured alphabetic symbols, which happened to be at the time his favourite toy.

Partly because she was afraid of tripping up through the box getting between her feet while she was carrying hot food, and partly for fear of accidentally breaking her son's prized treasure, the mother eventually, making a special journey, picked up John's box, and put it in the play-pen. At the same time, more through absent-mindedness and from a desire to tidy up the room than for any other reason, she put two or three of his other toys also in the pen.

John instantly set up a howl. Equally suddenly, after a time, the howl was checked, and his mother forgot about him. What was happening during that period in his mind can perhaps be simulated by using childish words, though of course it could in no way be assumed that these would be the images which the child would actually use.

Little John was looking for his toy box. Finally he found it. The box was in the pen. Little John was very happy. "Things go in pen", he said. "Things go in pen".

The sequel is obvious. A few minutes later, John was heard chortling with happiness. His mother, warned by this sign, hastened in to see what he was up to.

"Good heavens", she cried, "John is covered with ink. Where on earth did he get hold of it?"

She had not far to seek. The idea of putting things in play-pens had rooted itself stereotypically in the mind of John. The handlamp was in the pen. The inkstand was in the pen, The living room cushions, five books picked at random from the bookcase, his mother's apron, two screws, a tin of detergent and a half-used stick of shaving soap, all these, swimming in a sea of ink, were now in the pen.

I think this incident sufficiently proves my contention. I will now proceed... (The psychiatrist's lecture now continues indefinitely)....

It only now remains for us, to top off our conclusion, to guess some of the ways in which the machine, scanning the collocations of this passage, can be made to mimic a not-too-unobservant sociologist. Nor is the guess difficult, for the techniques are there; they are those already being employed for research experiment in mechanical abstracting.

In the passage under question, a direction to the machine to search for the most frequently occurring words and phrases would certainly retrieve "pen" and "play-pen" among the first,

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***** Note that the insertion of this word is the only addition made in transplanting this context from its original position on page 2.

and "in the pen" and "was in the pen" (the sponsors of this baptism) among the second.

A second directive to the machine, to the effect that, a)all words on the frequently occurrent word list should be treated as always occurring in the frequently occurrent phrase list (i.e. that "in" and "pen" should always be in this text translated as when they occur in the frequently occurrent phrase "in the pen", and b) that "in the pen" should always be translated, in this text, whenever it occurs, in the sense in which (with the aid of any semantic congruence or thesaurus procedure),it will be correctly translated when it first occurs - a programme based on ; these directives will sufficiently scan the ritual of this passage (see Appendix IV).

Doubtless other texts will require other scanning devices; and doubtless the machine will be sometimes obtuse or un-observant. Nevertheless, according to me, the trick is: if you want to deal with problems raised by Bar-Hillel, turn your machine not into a mathematician, but into a sociologist.

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(1) It has, rather surprisingly, been refused by the Selection Committee of the International Conference on Information Processing, before which it was designed to be read.

(2) See especially "A Quasi-Arithmetical Notation for Syntactic Description"; Language, Vol 29, No 1., 1953

(3) Notice, for example, that the very same - fictitious - thesaurus approach which would correctly render "pen" by "plume" in the sentence "The pen was in the inkstand", would incorrectly give "plume" for "pen" in the sentence "The inkstand was in the pen".

(4) This discussion was held on the evening of November 16th 1958. It is inaccurately referred to in Bar-Hillel's report, p. 36, since Bar-Hillel himself declaimed against metaphysics and thesauri most of the time.

(5) It is worth remarking that the thesaurus method - any thesaurus method - picks these up in its stride. You have one cross-reference to all proverbs, one to all biblical quotations, and one to all formal greetings, with the help of which a unique translation of these comes out en bloc. Cf M.Masterman, Unpublished Notes, "Vivat Rex in Aeternam"; C.L.R.U.,1956-7.

(6) Bar-Hillel, Op. cit.

(7) Bar-Hillel, Op. cit., Section 4, p.8.

(8) R.H.Richens, "Interlingual Machine Translation"; The Computer Journal, Vol 1, No 3.,1958.

(9) On the hampering effect on M.T. research of the deficiencies in existing thesauri, see M. Masterman, "What is a Thesaurus?", the companion paper to this.

(10) The whole question of what "consistency" means when applied to a thesaurus is an awkward one. My own proposed test is as follows: if the translation of a text A, with a thesaurus, requires that any set of dictionary entries for input language words should have N heads, (for any N include N = 1), whereas translating text B, with the same thesaurus, requires that any or all of the same dictionary entries should have N heads, for the same N, then the thesaurus shall be held to be inconsistent.

(11) C.L.R.U. Workpaper : M. Masterman, A.F. Parker-Rhodes, R.M. Blackmore and K. Spärck Jones, "Description of the Tests carried out on Methods of Constructing Sentence Lattices";1958.

See also "What is a Thesaurus?", Section Y.

(12) L. Wittgenstein, "Philosophical Investigations", trans. Anscombe, 1953.

(13) R. Carnap, "Logical Syntax of Language", trans. von Zeppelin, 1937.

(14) Bar Hillel, Op. cit., Section 23, Proposal (3), p.38.

(15) C.L.R.U. Workpaper: "Annexe V"; 1956.

(16) See, especially in this context, the very elegant, but un-applioable, Bar-Hillel- Lambek Calculus. J. Lambek, "The Mathematics of Sentence Structure"; American Mathematical Monthly, Vol LXV, No 3., 1958.

(17) W.S. Allen, "The Linguistic Study of Language"; Inaugural Lecture, Cambridge University Press, 1956.

(18) Z. Harris, "Methods in Structural Linguistics", 1951.

(19) I am not speaking here of the two types of mathematical app-roach which seem to apply statistical techniques directly to the study of language. Cf J. Whatmough, "Language: a Modern Synthesis", 1956; G. Herdan, "Language as Choice and Chance", 1956.

(20) B. Russell, "The Cult of Common Usage"; British Journal for the Philosophy of Science, Vol III, No 12., 1953; "My Philosophical Development", 1959.

(21) G. Ryle, "Systematically Misleading Expressions", in "Logic and Language", Series 1, ed Flew, 1951. See also the Intro-duction by G.N. Flew to "Logic and Language", Series 1 and 2,

(22) G.E. Moore, "Wittgenstein's Lectures, 1930-3"; Mind, Vol LXIII, 1954.

(23) B.A. Farrell, "An Appraisal of Therapeutic Positivism"; Mind, Vol LV, 1946.

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