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The Present Status of Automatic Translation of Languages•

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1. Aims and Methods, Survey and Critique

1.1 Introduction

Machine translation (MT) has become a multimillion dollar affair. It has been estimated[1] that in the United States alone something like one and one-half million dollars were spent in 1958 upon research more or less closely connected with MT, with approximately one hundred and fifty people, among them eighty with M.A., M.Sc. or higher degrees, working in the field, full or part time. No comparable figures are available for Russia,[2] but it is generally assumed that the number of people engaged there in research on MT is higher than in the States. At a conference on MT that took place in Moscow in May 1958, 347 people from 79 institutions were reported to have participated. Not all participants need necessarily be actively involved in MT research. There exist two centers of research in MT in England, with a third in the process of formation, and one center in Italy. Outside these four countries, MT has been taken up only occasionally, and no additional permanent research groups seem to have been created. Altogether, I would estimate that the equivalent of between 200 and 250 people were

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[1] This estimate is not official. In addition, it is still rather difficult to evaluate available machine time. Some basis for the estimate is provided in Appendix I.

[2] Reitwiesner and Weik, in their report cited in reference [3], say on p. 34 that "Dr. Panov's group consists of approximately 500 mathematicians, linguists and clerical personnel, all working on machine translation of foreign languages into Russian and translations between foreign languages with Russian as an inter-language." No source for this figure is given, and it is likely that some mistake was made here.

working full-time on MT at the end of 1958, and that the equivalent of three million dollars were spent during this year on MT research. In comparison, let us notice that in June 1952, when the First Conference on Machine Translation convened at MIT, there was probably only one person in the world engaged more than half-time in work on MT, namely myself. Reduced to full-time workers, the number of people doing research on MT could not at that time have been much more than three, and the amount of money spent that year not much more than ten thousand dollars.

For the 1952 MT Conference I had prepared in mimeograph a survey of the state of the art [1]. That report was based upon a personal visit to the two or three places where research on MT was being conducted at the time, and seems to have been quite successful, so I was told, in presenting a clear picture of the state of MT research as well as an outline of the major problems and possibilities. Time has come to critically evaluate the progress made during the seven years that have since passed in order to arrive at a better view of these problems and possibilities. To my knowledge, no evaluation of this kind exists, at least not in English. True enough, there did appear during the last year two reviews of the state of MT, one prepared by the group working at the RAND Corporation [2], the other by Weik and Reitwiesner at the Ballistic Research Laboratories, Aberdeen Proving Ground, Maryland [3]. The first of these reviews was indeed well prepared and is excellent as far as .it goes. However, it is too short to go into a detailed discussion of all existing problems and, in addition, is not always critical to a sufficient degree. The second review seems to have been prepared in a hurry, relies far too heavily on information given by the research workers themselves, who by the nature of things will often be favorably biased towards their own approaches and tend to overestimate their own actual achievements, and does not even attempt to be critical. As a result, the picture presented in this review is somewhat unbalanced though it is still quite useful as a synopsis of certain factual bits of information. Some such factual information, based exclusively upon written communication from the research groups involved, is also contained in a recent booklet published by the National Science Foundation [4]. Brief histories of MT research are presented in the Introductory Comments by Professor Dostert to the Report of the Eighth Annual Round Table Conference on Linguistics and Language Study [5] as well as in the Historical Introduction to the recent book by Dr. Booth and associates [6].

The present survey is based upon personal visits during October and November 1958 to almost all major research centers on MT in the United States, the only serious exception being the center at the University of Washington, Seattle, upon talks with members of the two research groups in England, and upon replies to a circular letter sent to all research groups in the United States asking for as detailed information as possible concerning the number and names of people engaged in research within these groups, their background and qualifications, the budget, and a short statement of the plans for the near future, as well as, of course, upon a study of all available major publications including also, as much as possible, progress reports and memoranda; with regard to the USSR I had, unfortunately, to rely exclusively on available English translations of their publications and on reports which Professor Anthony G. Oettinger, of the Harvard Computation Laboratory, who had visited the major Russian research centers in MT in August 1958, was so kind to put at my disposal. Some of the purely technical information with regard to the composition of the various MT research groups, their addresses and budgets is presented in Appendix I in tabular form.

1.2 Unreasonableness of Aiming at Fully Automatic High Quality Translation

During the first years of the research in MT, a considerable amount of progress was made which sufficed to convince many people, who originally were highly skeptical, that MT was not just a wild idea. It did more than that. It created among many of the workers actively engaged in this field the strong feeling that a working system is just around the corner. Though it is understandable that such an illusion should have been formed at the time, it was an illusion. It was created, among other causes, also by the fact that a large number of problems were rather readily solved, and that the output of machine-simulated "translations" of various texts from Russian, German or French into English were often of a form which an intelligent and expert reader could make good sense and use of. It was not sufficiently realized that the gap between such an output, for which only with difficulty the term

"translation" could be used at, all, and high quality translation proper, i.e., a translation of the quality produced by an experienced human translator, was still enormous, and that the problems solved until then were indeed many but just the simplest ones, whereas the "few" remaining problems were the harder ones—very hard indeed.

Many groups engaged in MT research still regard fully automatic, high quality translation (FAHQT) as an aim towards which it is reasonable to work. Claims to the effect that FAHQT from Russian to English is attainable in the near future were recently made, for instance, by one of the four subgroups working on MT at Georgetown University (Section 2.1.3). I shall discuss these claims below. But let me state already at this point that I could not be persuaded of their validity. On the contrary, I am quite ready to commit myself to concoct Russian sentences or, should this for some reason be regarded as unfair, to exhibit actually printed Russian sentences for which a perusal of the proposed translation program of this group, or of any other group that would offer in the near future a method of fully automatic translation, would result either in gibberish or, what is even worse, in meaningful but wrong translations. I am so convinced of this because I believe to be in possession of an argument which amounts to an almost full-fledged demonstration of the unattainability of FAHQT, not only in the near future but altogether. This demonstration is given in Appendix III.

Most groups, however, seem to have realized, sometimes very reluctantly, that FAHQT will not be attained in the near future. Two consequences can be drawn from this realization. One can go on working with FAHQT in mind, in the hope that the pursuit of this aim will yield interesting theoretical insights which will justify this endeavor, whether or not these insights will ever be exploited for some practical purpose. Or one gives up the ideal of FAHQT in favor of some less ambitious aim with a better chance of attainability in the near future. Both consequences are equally reasonable but should lead to rather different approaches. Lack of clarity in this respect, vague hopes that somehow or other both aims can be attained simultaneously and by the use of the same methods, must lead to confusion and result in waste of effort, time and money. Those who are interested in MT as a primarily practical device must realize that full automation of the translation process is incompatible with high quality. There are two possible directions in which a compromise could be struck; one could sacrifice quality or one could reduce the self-sufficiency of the machine output. There are very many situations where less than high quality machine output is satisfactory. There is no need to present examples. If, however, high quality is mandatory—and I do not think, for instance, that scientists are prepared to be satisfied with less than the present average standard of human translation, while many regard this standard as too low for their purposes—then the machine output will have to be post-edited, thereby turning, strictly speaking, machine translation into *machine aids to translation*.

1.3 Commercial Partly Mechanized, High Quality Translation Attainable in the Near Future

In the remainder of this survey, I shall exclusively deal with those situations where translation involved has to be of high quality. It should be easy to see how the conclusions at which I arrive have to be modified in order to deal with situations in which lesser quality is satisfactory.

As soon as the aim of MT is lowered to that of high quality translation by a *machine-post-editor* partnership, the decisive problem becomes to determine the region of optimality in the continuum of possible divisions of labor. It is clear that the exact position of this region will be a function of, among other things, the state of linguistic analysis to which the languages involved have been submitted. It may be safely assumed that, with machine-time/efficiency becoming cheaper and human time becoming more expensive, continuous efforts will be made to push this region in the direction of reducing the human element. However, there is no good reason to assume that this region can be pushed to the end of the line, certainly not in the near future.

It seems that with the state of linguistic analysis achieved today, and with the kind of electronic computers already in existence or under construction, especially with the kind of large capacity, low cost and low-access-time internal memory devices that will be available within a few years, a point has been reached where commercial partly mechanized translation centers stand a serious chance of becoming a practical reality. However, various developments are still pending and certain decisions will have to be made.

First, a reliable and versatile mechanical print reader will have to become available. It has been estimated that the cost of retyping printed Russian material into a form and on a medium that could be processed by a machine would amount, under present conditions, to about one fourth of a cent per word [7]. This estimate is probably too low, as the quality of the retyping has to be exceptionally high, in order to avoid printing mistakes which would perhaps be quite harmless for a human reader, but could be rather disastrous for machines which so far are totally unable to deal with misprints. The original text might therefore have to be keypunched by two operators, verified, etc., or else to be keypunched once, but at highly reduced speed. Indeed, whereas the above estimate is based on a rate of 20 Russian words per minute, another report [8] gives the maximum rate of trained and experienced keypunch operators as half this number. In one place [9], it is estimated that an automatic print reader might be ten times cheaper than human retyping. The difference between one half of a cent per keypunched word and one twentieth of a cent per print-read word could make all the difference, as the present cost per word of human Russian-to-English translation in the United States is generally given as lying between one and three cents [10], apparently depending on the quality and urgency of the job, and perhaps also on the exact form of the output. The costs may be different, of course, for other language pairs and in other countries. An informative synopsis on the variation of rates of payment for scientific and technical translation is given in a recent UNESCO survey [11].

Secondly, a concerted effort will have to be made by a pretty large group in order to prepare the necessary dictionary or dictionaries in the most suitable form. That this is not such a straightforward affair as laymen are apt to think becomes clear in the work of the Harvard MT group [12, 13]. This group developed an interesting semiautomatic method for preparing dictionaries (Section 2.1.6).

Thirdly, a good amount of thinking accompanied by an equally large amount of experimenting will have to go into the determination of the location of the interval in the above-mentioned continuum within which the optimal point of the division of labor between machine and post-editor will have a good chance of being situated, as a function of the specific translation program and the specific qualities of the envisaged post-editor. Among other things, these studies would have to determine whether some minimal pre-editing, while requiring but very little knowledge of the source language by the pre-editor, could not be utilized in order to reduce the load of the machine by a considerable amount. At present, many of the experimental MT programs make use of such limited pre-editing (Section 2.1.4). As one illustration of an operation that is in almost all cases so ridiculously simple for a human pre-editor that it could be almost instantaneously performed by a keypunch operator with only the barest knowledge of the source language, let me mention the distinction between the functioning of a point as a period, hence as one of the all-important markers of end-of-sentence, and its various other functions. Having the machine make this decision—a vital one, indeed so vital that it is one of the first operations, if not the first, in many translation programs that shun the use of pre-editing altogether—might be a complex and costly affair, throwing some doubts on the soundness of the case presented above in favor of a mechanical print reader. For the time being, at least, so long as keypunching is being used for the input, it is doubtless profitable to introduce as much elementary pre-editing as the keypunch operator can take into stride without considerably slowing down.

Fourthly, an old question which has not been treated so far with sufficient incisiveness, mostly because the ideal of FAHQT diverted the interests of the research workers into other, less practical directions, namely the question whether MT dictionaries should contain as their source-language entries all letter sequences that may occur between spaces, sometimes called *inflected forms*, or rather so-called *canonical forms*, or perhaps something in between like *canonical stems* [14], has to be decided one way or other before mass production of translations is taken up. This question is clearly highly dependent, among other things, upon the exact type of internal and external memory devices available, and it is therefore mandatory to have a reliable estimate of this dependence. It is obvious that the speed of the machine part of the translation, and thereby the cost of the total translation process, will depend to a high degree on the organization of the dictionaries used. Most workers in the field of MT seem to have rather definite, though divergent, opinions in this respect. However, I am not aware of any serious comparative studies, though the outcome of such studies most surely will have a considerable impact upon the economics of MT.

In general, the intention of reducing the post-editor's part has absorbed so much of the time

and energy of most workers in MT, that there has not been sufficient discussion of the problem whether partially automatic translation, even with such a large amount of participation by the post-editor as would be required under present conditions, is not nevertheless a desirable and feasible achievement. I fully understand the feeling that such an achievement is not of very high intellectual caliber, that the real challenge has thereby not yet been taken up, but I do not think that those agencies for whom any reduction of the load imposed at the moment on the time of highly qualified expert translators is an important achievement, should necessarily wait with the installation of commercial man-machine translation outfits until the post-editor's part has become very small, whatever amount of satisfaction the MT research worker will get from such an achievement. It is gratifying to learn that this attitude coincides with that of the Harvard group (Section 2.1.6) and is probably now shared by many other groups in the USA, USSR, and England, though it would further the issue if clear-cut statements of policy could be obtained in this respect.

1.4 Compromising in the Wrong Direction

At this stage, it is probably proper to warn against a certain tendency which has been quite conspicuous in the approach of many MT groups: These groups, realizing that FAHQT is not really attainable in the near future so that a less ambitious aim is definitely indicated, had a tendency to compromise in the wrong direction for reasons which, though understandable, must nevertheless be combated and rejected. Their reasoning was something like the following: since we cannot have 100% automatic high quality translation, let us be satisfied with a machine output which is complete and unique, i.e., a smooth text of the kind you will get from a human translator (though perhaps not quite as polished and idiomatic), but which has a less than 100% chance of being correct. I shall use the expression "95%" for this purpose since it has become a kind of slogan in the trade, with the understanding that it should by no means be taken literally. Such an approach would be implemented by one of the two following procedures: the one procedure would require to print the most frequent target-language counterpart of a given source-language word whose ambiguity has not been resolved by the application of the syntactical and semantical routines, necessitating, among other things, large-scale statistical studies of the frequency of usage of the various target renderings of many, if not most, source-language words; the other would be ready to work with syntactical and semantical rules of analysis with a degree of validity of no more than 95%, so long as this degree is sufficient to insure uniqueness and smoothness of the translation. This approach seems wrong to me and even dangerous since the machine output of the corresponding program will be of low quality in a misleading and soothing disguise. Since so many sentences, "5%" of a given text, will have a good chance of being mistranslated by the machine, it is by no means clear whether the reader will always be able to detect these mistranslations, just because the machine output in so smooth and grammatical (so let us assume for the sake of the argument, though I doubt whether even this much can really be achieved at this stage of the game) that he might be able to find only few cues to warn him that something is wrong with it. It is not inconceivable that the machine translation would be so wrong at times as to lead its user to actions which he would not have taken when presented by a correct translation. (When I talk about "100%," I obviously have in mind not some heavenly ideal of perfection, but the product of an average qualified translator. I am aware that such a translator will on occasion make mistakes and that even machines of a general low quality output will avoid some of these mistakes. I am naturally comparing averages only.)

But there is really no need at all to compromise in the direction of reducing the reliability of the machine output. True enough, a smooth machine translation looks impressive, especially if the reader is unable to realize at first sight that this translation is faulty ever so often, but this esthetically appealing feature should not blind us to see the dangers inherent in this approach. It is much safer to compromise in the other direction. Let us be satisfied with a machine output which will ever so often be neither unique nor smooth, which ever so often will present the post-editor with a multiplicity of renderings among which he will have to take his choice, or with a text which, if it is unique, will not be grammatical. On the other hand, whenever the machine output is grammatical and unique it should be, to adopt a slogan current in the Harvard group, "failsafe" (to about the same degree, to make this qualification for the last time, as the average qualified human translator's output is fail-safe). Let the

machine by all means provide the post-editor with all possible help, present him with as many possible renderings as he can digest without becoming confused by the *embarras de richesse*—and here again we have quite a problem of finding an interval of optimality—but never let the machine make decisions by itself on purely frequential reasons even if these frequencies can be relied upon. If these frequency counts could be done cheaply—and I doubt very much whether this is feasible to such a high degree of reliability as would probably be required for our purposes—let this information too be given the post-editor, but by no means should practical MT wait until this information is obtained.

The only reasonable aim, then, for short-range research into MT seems to be that of finding some machine-post-editor partnership that would be commercially competitive with existing human translation, and then to try to improve the commercial effectiveness of this partnership by improving the programming in order to delegate to the machine more and more operations in the total translation process which it can perform more effectively than the human post-editor. These improvements will, of course, utilize not only developments in hardware, programming (especially automatic programming), and linguistic analysis, but also the experience gained by analyzing the machine output itself. Should it turn out that for the sake of competitiveness some use of a pro-editor, and perhaps even of a bilingual post-editor, would be at least temporarily required, then this fact should be accepted as such, in spite of the trivialization of the theoretical challenge of the MT problem which would be entailed by such a procedure.

1.5 A Critique of the Overestimation of Statistics and the "Empirical Approach"

Let me finish this part of the survey by warning in general against overestimating the impact of statistical information on the problem of MT and related questions. I believe that this overestimation is a remnant of the time, seven or eight years ago, when many people thought that the statistical theory of communication would solve many, if not all, of the problems of communication. Though it is often possible by a proper organization of the research effort to get a certain amount of statistical information at no great extra cost, it is my impression that much valuable time of MT workers has been spent on trying to obtain statistical information whose impact on MT is by no means evident. It is not true that every statistic on linguistic matter is automatically of importance for MT so that the gathering of any such statistics could be regarded as an integral part of MT research without any need for additional justification.

Gathering of statistics is regarded by many MT groups as being part of a more general methodological approach—the so-called "empirical approach" [15]. This term has already caused a lot of confusion. I am using it here in the sense in which it is employed by the RAND group [16]. This sense should become obvious from the following discussion. Adherents of this approach are distrustful of existing grammar books and dictionaries, and regard it as necessary to establish from scratch the grammatical rules by which the source-language text will be machine analyzed, through a human analysis of a large enough corpus of source-language material, constantly improving upon the formulation of these rules by constantly enlarging this corpus. With regard to dictionaries, a similar approach is often implemented and a dictionary compiled from translations performed by bilingual members of the group or by other human translators considered to be qualified by this group. This approach seems to me somewhat wasteful in practice and not sufficiently justified in theory. The underlying distrust seems to have been caused by the well-known fact that most existing grammars are of the normative type, hence often of no great help in the analysis of actual writing (and to an even higher degree, of actual speech), and that existing dictionaries are of such a nature that quite often none of the presented target-language counterparts of a source-language word are satisfactory within certain contexts, especially with regard to terms used .in recently developed scientific fields. However, even in view of these facts, I believe that the baby has far too often been thrown away with the bathwater. No justification has been given for the implicit belief of the "empiricists" that a grammar satisfactory for MT purposes will be compiled any quicker or more reliably by starting from scratch and "deriving" the rules of grammar from an analysis of a large corpus than by starting from some authoritative grammar and changing it, if necessary, in accordance with analysis of actual texts. The same holds *mutatis mutandis* with regard to the compilation of dictionaries. But grammars have in

general not wholly been dreamt up, nor have dictionaries been compiled by some random process. Existing grammars and dictionaries are already based, though admittedly not wholly, upon actual texts of incomparably larger extension than those that serve as a basis for the new compilers. Russian is not Kwakiutl, and with all due regard to the methods and techniques of structural linguistics and to the insights which this science has given us in respect to some deficiencies of traditional grammars, I do not think that it follows from its teachings that all existing codifications of languages with a highly developed literature should be totally disregarded. Let me add, without going here into details for lack of space, that the empiricalness of the derivations of grammar rules from actual texts is rather doubtful as such. For certain general methodological considerations one might as well be led to the conclusion that these rules incorporate a lot of subjective and highly biased and untested assumptions such that their degree of validity might very well, on the average, be lower than that of the well-established, often-tested and critically examined grammars, in spite of their normativity.

2. Critical Survey of the Achievements of the Particular MT Research Groups

After these far too short (and therefore occasionally rather dogmatic) general comments, it is now time for a more detailed survey of the approaches and achievements of the twenty or so groups which are at present actively engaged in research on MT or on linguistic topics believed to be of immediate relevance for MT. In one case a defunct group (Section 2.1.5) is being mentioned, first because it made significant contributions during its existence, and secondly because there is still some chance that it may be revived. This survey will deal exclusively with the more general aspects of the MT problem and especially with research methodology. Therefore, the innumerable specific advances of the various groups will) regard to coding, transliterating, keypunching, displaying of output, etc., will be mentioned only rarely. But the list of references should contain sufficient indications for the direction of the reader interested in these aspects.

The order in which these groups will be discussed is: USA, Great Britain, USSR, others, following, with one exception, the order of degree of my personal acquaintance. Within each subdivision, the order will in general be that of seniority.

2.1 The USA Groups

2.1.1 THE SEATTLE GROUP

Professor Erwin Reifler of the University of Washington, Seattle, started his investigations into MT in 1949, under the impact of the famous memorandum by Weaver [17], and has since been working almost continuously on MT problems. The group he created has been constantly increasing in size and is at present one of the largest in the States. In February 1959, it published a 600-page report describing in detail its total research effort. This report has not reached me at the time of writing this survey (April 1959) which is the more unfortunate as the latest publication stemming from this group is a talk presented by Reifler in August 1957 [18], and I was, due to a personal mishap, unable to visit Seattle during my stay in the States. It is not impossible that my present discussion is considerably behind the actual developments.

The efforts of this group seem to have concentrated during the last years on the preparation of a very large Russian- English automatic dictionary containing approximately 200,000 so-called "operational entries" whose Russian part is probably composed of what was termed above (Section 1.3) "inflected forms" (as against the million or so inflected forms corresponding to the total Russian vocabulary of one hundred thousand canonical forms). This dictionary was to be put on a photoscopic memory device, developed by Telemeter-Magnetics Inc. for the USA Air Force, which combines a very large storage capacity with very low access time and apparently is to be used in combination with one of the large electronic computers of the IBM 709 or UNIVAC 1105 types. The output of this system would then be one version of what is known as *word-by-word translation*, whose exact form would depend on the specific content of the operational entries and the translation program. Both are unknown to me though probably given in the above mentioned report. Word-by-word Russian-to-

English translation of scientific texts, if pushed to its limits, is known to enable an English reader who knows the respective field to understand, in general, at least the gist of the original text, though of course with an effort that is considerably larger than that required for reading a regular high quality translation, or else to enable an expert English post-editor to produce on its basis, with some very restricted use of the original text (in transliteration, if he does not know how to read Cyrillic characters), a translation which is of the same order of quality as that produced by a qualified human translator. However, no comparisons as to quality and cost between the Seattle MT system and human translation is given in the publications known to me. In any case, in view of the rather low quality of the machine output (word-by-word translation is theoretically a triviality, of course, though a lot of ingenuity is required to get the last drop out of it) the claim that the Seattle-Air Force system is "the most advanced translation system under construction" [19] is very misleading; even more misleading is the name given the photoscopic disc, "The USAF Automatic Language Translator Mark I" [20], which creates the impression of a special purpose device, which it is not.

The Seattle group started work towards getting better-than-word-by-word machine outputs in the customary direction of automatically changing the word order and reducing syntactical and lexical ambiguities (the Seattle group prefers to use the terms "grammatical" and "non-grammatical") but again little is known of actual achievements. One noticeable exception is Reifler's treatment of German compound words, which is an especially grave problem for MT with German as the source-language since this way of forming new German nouns is highly creative so that the machine will almost by necessity have to identify and analyze such compounds [21]. In the above mentioned 1957 talk, Reifler claimed to have "found moreover that only three matching procedures and four matching steps are necessary (sufficient?) to deal effectively with—that is, to machine translate correctly—any of these ten types of compounds of any [!] language in which they occur," [22]—a claim which sounds hardly believable, whose attempted substantiation is probably contained in the mentioned report. It is worthwhile to stress that this group does not adopt the "empirical approach" mentioned above, and is not going to be satisfied with so-called "representative samples," but is trying to keep in view the ascertainable totality of possible constructions of the source-language though representative samples are of course utilized during this process [23].

For reasons given above, I must strongly disagree with Reifler's "belief that it will not be very long before the remaining linguistic problems in machine translation will be solved for a number of important languages" [24]. How dangerous such prophecies are is illustrated by another prophecy of Reifler's, to the effect that "in about two years (from August 1957) we shall have a device which will at one glance read a whole page and feed what it has read into a tape recorder and thus remove all human cooperation on the input side of the translation machines" [25]. The best estimates I am aware of at present mention five years as the time after which we are likely to have a reliable and versatile print reader (Section 1.3) at the present rate of research and development.

2.1.2 THE MIT GROUP

I started work on MT at the Research Laboratory of Electronics of MIT in May 1951. In July 1953, when I returned to Israel, Victor H. Yngve took over, steadily recruiting new assistants for his research. During the last years, the MIT group has laid great stress on its adherence to the ideal of FAHQT. For this purpose they regard the complete syntactical and semantical analysis of both source- and target-language to be a necessary prerequisite. It is, therefore, to these processes that their research effort has been mostly directed. It seems that this group is aware of the formidableness of its self-imposed task, and is rather uncertain in its belief that this prerequisite will be attained in the near future. In one of his latest publications, Yngve says: "It is the belief of some in the field of MT that it will eventually be possible to design routines for translating mechanically from one language into another without human intervention" [26]. It is rather obvious from the context that Yngve includes himself among the "some." How remote "eventually" and "ultimately"—another qualifying adverb occurring in a similar context—are estimated to be is not indicated. On the other hand, the MIT group believes, and I think rightfully, that the insights into the workings of language obtained by its research are valuable as such, and could at least partly be utilized in practical lower aimed machine translation by whomever is interested in this latter aim. However, it will probably be admitted by this group that

some of the research undertaken by it might not be of any direct use for practical MT at all. The group employs to a high degree the methods of structural linguistics, and is strongly influenced by the recent achievements of Professor Noam Chomsky in this field [27].

The impact upon MT of Chomsky's recently attained insights into the structure of language is not quite clear. Since I presented my own views on this issue in a talk at the Colloque de Logique, Louvain, September 1958 [28], as well as in a talk given before the Second International Congress of Cybernetics, Namur, September 1958, a greatly revised version of which is reproduced in Appendix II, I shall mention here only one point. The MIT group believes, I think rightly, that Chomsky has succeeded in showing that the *phrase structure model* (certain variants of which are also known as *immediate constituent models*) which so far has served as the basic model with which structural linguists were working, in general as well as for MT purposes, and which, if adequate, would have allowed for a completely mechanical procedure for determining the syntactical structure of any sentence in any language for which a complete description in terms of this model could be provided—as I have shown for a weak variant of this model, already 6 years ago [29] by a method that was later improved by Lambek [30] (cf. Appendix II)—is not fully adequate and has to be supplemented by a so-called *transformational model*. This insight of Chomsky explains also, among other things, why most prior efforts at the mechanization of syntactical analysis could not possibly have been entirely successful. The MIT group now seems to believe that this insight can be given a positive twist and made to yield a more complex but still completely mechanical procedure for syntactical analysis. I myself am doubtful about this possibility, especially since the exact nature of the transformations required for an adequate description of the structure of English (or any other language) is at the moment still far from being satisfactorily determined. A great number of highly interesting but apparently also very difficult theoretical problems, connected with such highly sophisticated and rather recent theories as the theory of recursive functions, especially of primitive recursive functions, the theory of Post canonical systems, and the theory of automata (finite and Turing), are still waiting for their solution, and I doubt whether much can be said as to the exact impact of this new model on MT before at least some of these problems have been solved. I think that Chomsky himself cherishes similar doubts, and as a matter of fact my present evaluation derives directly from talks I had with him during my recent visit to the States.

The MIT group has, among other things, also developed a new program language called COMIT which, though specially adapted for MT purposes, is probably also of some more general importance [31], and whose use is envisaged also by other groups.[3] The fact that it was felt by this group that a program language is another more or less necessary prerequisite for MT is again the result of their realization of the enormous difficulties standing in the way of FAHQT. It is doubtful whether the development of a program language beyond some elementary limits is indeed necessary, or even helpful for more restricted goals. I would, however, agree that a program language is indeed necessary for the high aims of the MIT group, though I personally am convinced that even this is not sufficient, and that this group, if it continues to adhere to FAHQT, will by necessity be led in the direction of studying learning machines. I do not believe that machines whose programs do not enable them to learn, in a sophisticated sense of this word, will ever be able to consistently produce high-quality translations.

About the actual achievements of the MIT group with regard to MT proper little is known, apparently due to its reluctance to publish incomplete results. It is often felt that because of this reluctance other MT workers are wasting some of their time in treading over ground that might have already been adequately covered, though perhaps with negative results.

2.1.3 THE GU GROUP

The largest group working on MT in the States is that at Georgetown University, Washington, D.C., led by Professor Dostert. The GU group comprises four subgroups. One of these is headed by Professor Garvin and has been engaged during the last two years exclusively in programming the mechanization of the syntactical analysis of Russian. Their method seems to work rather satisfactorily

[3] This information was given to me in a letter from Yngve.

for the syntactical analysis of a large class of Russian sentences, though its exact reach has not yet been fully determined nor all the details of their program debugged. They have produced a very large number of publications, in addition to a multitude of Seminar Work Papers of the Machine Translation Project of Georgetown University, of which I shall mention only two of the more recent ones [32, 33].

The other three subgroups at GU are working on MT as a whole, two of them from Russian into English, the third from French into English. It is claimed that during the last few months the research done at GU has broadened and MT from additional languages into English has begun to be investigated. However, I am not aware of any publications reporting on these new activities and shall therefore not deal with them here. They seem to be at present in their preliminary stages only.

I already mentioned above (Section 1.2) that far-reaching claims were made by one of the GU subgroups. This is the group headed by Miss Ariadne W. Lukjanow and using the so-called *Code Matching Technique* for the translation of Russian chemical texts. I expressed then my conviction that this group could not possibly have developed a method that is as fully automatic and of high quality as claimed. There are in principle only two procedures by which such claims can be tested. The one consists in having a rather large body of varied material, chosen by some external agency from the field for which these claims are made, processed by the machine and carefully comparing its output with that of a qualified human translator. The other consists in having the whole program presented to the public. None of these procedures has been followed so far. During a recent demonstration mostly material which had been previously lexically abstracted and structurally programmed was translated. When a text, lexically abstracted but not structurally programmed, was given the machine for translation, the output was far from being of high quality and occasionally not even grammatical. True enough, this did not prevent the reader from understanding most of the time what was going on, but this would have been the case also for word-by-word translation, since the sample, perhaps due to its smallness, did not contain any of those constructions which would cause word-by-word translation to be very unsatisfactory. In contrast, however, with word-by-word translation which, if properly done, is hardly ever wrong, though mainly only because it is not real translation and leaves most of the responsibility to the post-editor, this translation contained one or two rather serious errors, as I was reliably told by someone who carefully went through the machine output and compared it with the Russian original. (I myself did not attend the demonstration, and my knowledge of Russian is rather restricted.)

The task of evaluating the claims and actual achievements of the Lukjanow subgroup is not made easier by the fact that there seems to exist only one semipublicly available document prepared by herself [34]. This document contains 13 pages and is not very revealing. The only peculiarity I could discover lies in the analysis of the source-text in a straight left-to-right fashion, in a single pass, exploiting each word as it comes, including the demands it makes on subsequent words or word blocks, whereas most other techniques of syntactical analysis I know go through the source-language sentences in many passes, usually trying to isolate certain units first. I shall return to Miss Lukjanow's approach below (Section 2.1.9).

The claim for uniqueness (and adequacy) of the translation of a chemical text is based upon an elaborate classification of all Russian words that occurred in the analysed corpus into some 300 so-called *semantical classes*. Though such a detailed classification should indeed be capable of reducing semantic ambiguity, I am convinced that no classification will reduce it to zero, as I show in Appendix III, and that therefore the claim of the Lukjanow group is definitely false. There should be no difficulty for anyone who wishes to take the trouble to exhibit a Russian sentence, occurring in a chemical text, which will be either not uniquely translated or else wrongly translated by the Lukjanow procedure, within a week after all the details of this procedure are in public possession.

On the other hand, I am quite ready to believe that this subgroup has been able to develop valid techniques for a partial mechanization of Russian-to-English high quality translation of chemical literature (or else for a full mechanization of low quality translation), but, unfortunately, this group seems to be extremely reluctant to make the details of its program publicly available. Should it turn out that they did make some real progress not achieved elsewhere, this reluctance will have caused a great waste of time and money in other MT research groups.

A third subgroup at GU, led by Dr. Michael Zarechnak, is proceeding in a somewhat different manner, using a so-called *General Analysis Technique*, and is making less far-reaching claims. Much

of its work which I was able to check seemed to me well-founded and to contain solid achievements. However, as this is not the place to go into technical details, it is not possible to present an exact evaluation of where this subgroup stands right now. The only fully official publication of this group [35]—the number of semiofficial Seminar Work Papers is rather large—is too short to tell the whole story. Dr. Zarechnak hopes to be ready with a demonstration within a few months, and I also understand that everybody is welcome to look over the program to the degree that it has already been written up. The group does envisage the utilization of a post-editor for high quality final output.

With regard to the fourth and last subgroup at GU, led by Dr. A. F. R. Brown, I shall say very little here since I was unable to talk with Brown personally. As already mentioned, he is mostly interested in translation from French into English. I understand from his numerous Seminar Work Papers that he is developing his program on a sentence-after-sentence basis, i.e., dealing with the translation problems as they come and, so I was told, solving them one after another with great ingenuity. I have already expressed my conviction that this approach is somewhat wasteful and am sorry indeed that I was unable to talk the issue over with one of the seemingly most successful adherents of the empirical approach.

Altogether, I think that among themselves the four subgroups at GU cover a good deal of the problems arising in connection with MT. Dostert's interest in this field stems from his participation in the first MT Conference in June 1952, and so does Garvin's who attended the public opening meeting of that conference. These two linguists have been spending much of their time since on scientific and organizational aspects of MT, and have succeeded in training a large number of other people now working on MT at GU. This is a good deal of experience, and it is therefore not surprising that the work done under their direction should indeed cover most aspects of the MT problem. I am stressing this point since, in spite of the fact that I do disagree with some of the views and approaches of Dostert and his collaborators, I believe that every newcomer to the field—and there have been many of those during the last year and more are in prospect—should make himself as thoroughly acquainted as possible with the work done at GU, and get as clear a picture as possible of their achievements and failures. Otherwise he will have a good chance of repeating work that has been done there, and perhaps repeating the many failures that undoubtedly must have occurred there during the years. There exists no other group in the United States, or in England for that matter, which has been working on such a broad front. This remark of mine is not to be interpreted as implying that the prospective newcomers will not have to get acquainted with anything done outside GU. On the contrary, I do not think that there is much done at GU in the field of MT which is not being done also elsewhere, sometimes in more than one place, and in some of these places perhaps even more effectively. But GU is still a good place to get a full view of the problem or rather could be so if each subgroup were equally willing to discuss in full detail its work with others.[4]

2.1.4 THE RAND GROUP

The RAND Corporation in Santa Monica, California, became interested in MT in 1949, and has dealt with MT off and on since. The well-known study by Professor Abraham Kaplan on the reduction of ambiguity through context [36] was done at RAND, and Dr. Olaf Helmer of RAND participated in the First MT Conference. However, it is only during the last years that RAND's interest in MT has greatly increased so that the present RAND MT group headed by Dr. David G. Hays, with Professor Kenneth E. Harper of UCLA serving as its chief consultant, is at the moment one of the larger ones. It is there that the empirical approach has found its perhaps strongest expression, probably because Harper is such a strong believer in its soundness. The method they advocate is to go over a

[4] A newspaper report—received after the manuscript was completed—on four talks presented on April 6, 1959, before the 135th National Meeting of the American Chemical Society by Dostert, Lukjanow, Brown and Zarechnak, respectively, contains nothing that should change the picture given here, except perhaps a quotation of Miss Lukjanow saying that "the results of the translation of ... a completely random article where there was absolutely no advance preparation, not even checking for presence of words in the dictionary, proved conclusively that machine translation is no longer a theory but an accomplished fact." It would be interesting to see the full report on this experiment. Figures given by Brown on the economy of MT are entirely consistent with those given below (Section 2.1.6).

certain sample of Russian texts, say of 30,000 words in length, "derive" from a human analysis of this corpus both a dictionary and a set of syntactical and semantical rules, test the derived dictionary and rules on a new sample of the same size, increase the dictionary and, if necessary, expand as well as improve upon the rules as a result of this test, go on to the next sample, etc. As a matter of fact, during the first six cycles-—by now, they might have finished the eighth─they have mostly tried to perfect the dictionary and solve some of the problems of polysemy,[5] for example, that bothersome problem of the unique rendering of Russian prepositions. It is only a few months ago that they started to attack the question of syntactical analysis. It is impossible to here go into a detailed description of their planned approach but, again, it is quite empirical and therefore rather slow, and not too promising in its details as they stand at the moment. So, for instance, it is planned to investigate hundreds of thousands of Russian consecutive word pairs in order to arrive at a revealing classification of such pairs for the purpose of reducing syntactical ambiguity, to be followed by an investigation of word triplets, etc. This procedure is, of course, rather natural and consciously, or unconsciously, based upon the immediate constituent model discussed above. I am not too much impressed by the claim that resolution of syntactical ambiguities by consideration of the immediate neighborhoods of the ambiguous expression has proved itself in practice. Not that I doubt that syntactical ambiguity as well as polysemy, for that matter, can quite often be completely resolved and even more often be considerably reduced through the exploitation of the immediate neighborhood of the ambiguous expression; it is only that I have what I regard to be good theoretical reasons for believing that this reduction will in general stop short of complete resolution. I am here talking of only those cases where the original sentence is not syntactically ambiguous as such; if it is ambiguous, then a technique that would "resolve" this ambiguity rather than display it would be no good. These theoretical reasons are, for syntactical ambiguity, that the immediate constituent model is not fully adequate so that trying to push a resolution technique based upon it beyond certain limits (which at present are admittedly by no means clear) must defeat itself. In addition, and this is probably of greater practical importance, I cannot persuade myself that "deriving" syntactical rules from a huge number of observations will yield better and quicker results than testing rules, whatever their "derivation," as to their ability to stand up against concocted counter-examples. This seems to me to be a methodological commonplace. In physics or chemistry, it is now probably generally agreed that it is a much better methodology to put "freely conceived" theoretical constructions to as sharp empirical tests as possible in order to refute these constructions than to arrive at theories that are only compatible with existing observations. I see no reason why linguistics should be different in this respect and why in this field observation and "derivation" is to be regarded as superior to empirical testing of "freely conceived" theories. A report which is probably inspired by Harper, if not actually written by him, contains a statement to the effect that its author is not very much impressed by the fact that counter-examples to his empirically derived rules can be concocted so long as these are concocted examples and not ones that occur in some actual text [37]. Final judgment on this issue must be left to the reader.

Most results of the RAND research are being published in a series of nine research memoranda called "Studies in Machine Translation," six of which had appeared between December 1957 and October 1958, with a possibility that the remaining three might have appeared in the meantime. I have already had an opportunity to mention one of these studies above (Section 1.1) and to praise its general reliability.[6] Another study contains a very clear statement of the RAND research methodology [38], the remainder being various manuals dealing with such matters as instructions for transliteration, coding, keypunching, pre-editing and post-editing of Russian scientific texts, all of

[5] This is the term preferred by the Russian authors; it is certainly more convenient than the terms "semantical ambiguity," "lexical ambiguity," "non-grammatical ambiguity," "multiple meanings" used in Anglo-Saxon countries.

[6] Among the exceptions should be mentioned the characterization (on p. 14) of the Polish logician Ajdukiewicz as a linguist and the similar mistake with regard to the Polish school of logicians. I myself am characterized on this occasion as the exponent of the Polish school in the United States, which is misleading in various ways. (It is true, however, that I acknowledged in a paper cited in reference [27] the impact of a certain article of Ajdukiewicz's which does not seem to have been read by the RAND group, though it appears in their bibliography.)

them of great practical interest but outside the scope of this survey, as stated above (Section 1.5). Special mention is deserved, however, of the memorandum [39] containing a list of 225 Russian articles in Physics and Mathematics, comprising 227,752 running words, that are available at RAND in punched cards for the use of system and procedure designers who might require textual material for their research. Part of the keypunching was done by the Ann Arbor group (Section 2.1.7}.

2.1.5 THE RAMO-WOOLDRIDGE GROUP

In the area of Greater Los Angeles there was another group working on MT at Ramo-Wooldridge Corporation. It started operating in 1955 and was directed in its last stage by Dr. Don R. Swanson. Harper acted as a consultant for this group at an earlier stage, and in 1957-58 there existed a close cooperation between the RAND group and the Ramo-Wooldridge one. Though there are some differences between their approaches, a description of these differences would require going into greater detail than I am prepared to do here. The Ramo-Wooldridge group published two highly interesting reports [7, 10], to both of which I have already had an opportunity to refer. A close study of these reports should be of great help to everybody in the field. I understand that work on MT at Ramo-Wooldridge has been discontinued at the end of 1958, though perhaps only temporarily so.•

2.1.6 THE HARVARD GROUP

The Harvard University group, headed by Professor Anthony G. Oettinger, stands in many respects quite apart from the others. First, it has busied itself for years almost exclusively with an exploration of the word-by-word translation method. Secondly, this preoccupation was accompanied by, and originated partly out of, a strong distrust of the achievements of other groups. Though it must be admitted that the possibilities of word-by-word translation from Russian into English have never before been so thoroughly explored as they were by this group, with many new insights gained, and that very valuable results were obtained as to the structure and construction of MT dictionaries, one may still wonder whether this group really struck the golden middle between utilizing other people's work in the field and distrusting their work, though there certainly were good reasons for the distrust on quite a few occasions.

The progress made by this group can be easily evaluated by comparing two doctoral theses submitted at Harvard University, the one—to my knowledge the first dissertation on MT—by Oettinger [40] in 1954, the other by Giuliano in January 1959 [41]. This second thesis seems to close an era and indicate the opening of a new one. The first five chapters describe the operation of the Harvard Automatic Dictionary, the methods for its compiling and updating, as well as a great variety of applications, in such thoroughness and detail that the impression is created that not much more is to be said on this subject. The last chapter, on the other hand, contains some interesting but tentative and almost untested remarks on what Giuliano calls a *Trial Translator* [42], i.e., an automatic programming system for the experimental production of better than word-by-word translations.

Out of the enormous amount of material contained in this thesis, let me dwell on those passages that are of immediate relevance to the question of the commercial feasibility of MT, The existing program at the Harvard Computation Laboratory can produce word-by-word Russian-to-English translations at a sustained rate of about 17 words per minute on a UNIVAC I, and about 25 words per minute on a UNIVAC II. This is 4-6 times more than an expert human translator can produce, but since UNIVAC II time is 100 times more expensive than a human translator's time, commercial MT is out of the question at present. Giuliano estimates that a combination of an IBM 700 (or UNIVAC 1105) with the photoscopic disc mentioned above (Section 2.1.1) would, after complete reprogramming─requiring some three programmer years—and a good amount of other development work, be able to produce translations at 20-40 times the present rate which, taking into account the increase in the cost of computer time, would still leave the cost of a word-by-word machine translation slightly above that of a high-quality human translation. The difference will, however, now be so slight that one may expect that any further improvement, in hardware and/or in programming, would reverse

• Note added in proof: In the meantime, continuation of this project has been decided upon.

the cost relationship. This does not yet mean that true word-by-word MT will be in business. The cost of post-editing the word-by-word output in order to turn it into a passable translation of the ordinary type would probably be not much less than producing a translation of this quality without machine aid. As a matter of fact, senior research scientists having excellent command of scientific Russian and English, and extensive experience in technical writing, would be hampered rather than assisted by the automatic dictionary outputs in their present form.[7] The number of these individuals is, on the other hand, rather small and few of them can take the time from their scientific work to do a significant amount of translating and would have to be remunerated several times the ordinary professional translator's fee to be induced to spend more time on translating.

Altogether, it does not seem very likely that a nonsubsidized, commercial translation service will, in the next five years or so, find use for an automatic dictionary as its only mechanical device. However, as the Harvard group is quick to point, out, an automatic dictionary is an extremely valuable research tool with a large number of possible applications, some of which have already proved their value. Let me add that in situations where speed is at a premium, high quality is not a necessary requisite, and human translators at a shortage for any price—such situations might arise, for instance, in military operations—automatic dictionaries would be useful as such for straight translation purposes.

The whole issue is, however, somewhat academic. There is no need to speculate what the commercial value of an automatic dictionary would be since the same computer-store combination that would put out a word-by-word translation can be programmed to put out better than word-by-word translations. This is, of course, the subject on which most MT groups, including the Harvard group itself as of this year, are working on right now. At what stage a winning machine-post-editor combination will be obtained, is not so easy to foresee. I personally believe that the combination of a computer of twice the efficiency/cost ratio of an IBM 709 and Photographic Disk Store, of a program resulting from the pooled present knowledge of all the groups working on Russian-to-English MT, and of 2-3 years concentrated effort on improving the input and output should turn the trick, but I admit that so many factors are involved which have not yet been adequately evaluated that this belief is somewhat irrational.

2.1.7 THE ANN ARBOR GROUP

There is probably no topic among those studied by the MT group working in the Willow Run Laboratories of the University of Michigan, Ann Arbor, and directed by Mr. Andreas Koutsoudas, which is not also studied elsewhere. This is by no means meant to be derogatory; the same remark applies to almost every other MT group as well. The particular achievements could still be unique.

The group adopted, in general, a methodology similar to the one prevalent at RAND (Section 2.1.4)—there has been close collaboration between these groups—though with much more attention to theoretical models [43]. In addition to keypunching a large corpus of Russian scientific articles with frequency counts, members of the group are working on a resolution of syntactical and lexical ambiguity by context, as well as on algebraic and automata theory models of language, following the lead of Chomsky (Section 2.1.2).

2.1.8 THE PHILADELPHIA GROUP

The group at the University of Pennsylvania, Philadelphia, headed by Professor Zellig S. Harris, is wholly concerned with developing programs for the syntactical analysis of English, without bothering at every step with the implications of its research for MT. They do, however, intimate that their research will lead to useful applications for MT, and even more so for information retrieval [44] and related problems. These intimations are based upon serious misinterpretations of the semantic impact of their own work and are to that degree unsubstantiated. Though the actual programs compiled by the Philadelphia group for the syntactic analysis of English embody solid achievements based upon

[7] This evaluation is taken from a paper by Giuliano and Oettinger, "Research on automatic translation at the Harvard Computation Laboratory," to be published.

valid intuitive insights as well as upon extremely painstaking and detailed observations, and are in this respect equal if not superior to parallel achievements obtained during the same period by other groups concerned with the same problem (or rather, in most cases, with the materially different but methodologically very similar problem of mechanically analyzing the structure of Russian, German, French, etc.), the theory behind these achievements seems to be of doubtful validity, if interpreted literally, and ill-formulated and misleading in any case. No detailed substantiation of this rather harsh judgment can, of course, be undertaken here. A few comments must do.

Harris introduced into linguistic theory the terms "transformation" and "kernel." These terms are, unfortunately, not at all well-defined and rely for their meaning on a farfetched and underdeveloped analogy with the use of these terms in modern abstract algebra. (It is worthwhile stressing here, in view of current misunderstandings, that Harris' use has to be carefully distinguished from that of his former pupil Chomsky with whom these terms have well-determined and clear meanings and are free of any pseudomathematical flavor. Chomsky is, of course, highly influenced by the views of Harris—and vice versa—but his formulations are certainly exempt from the shortcomings criticized here.) It is, however, obvious from the context that Harris regards a sentence and its negation, or a sentence and its passive, as mutual transforms of each other. So, for example, *Atoms emit electrons* and *Atoms do not emit electrons* are transforms of each other, as are *Atoms emit electrons* and *Electrons are emitted by atoms*, with *Atoms emit electrons* being the kernel in both cases. It is therefore surprising to read that "a sentence carries the same information as does its transform" and that "a sentence, or a text, transformed into a sequence of kernels carries approximately the same information as did the original" or even that "for scientific, factual, and original material, however, it seems that the relevant information is held constant under transformation, or is varied in a way that depends explicitly on the transformation used" [45]. As against these formulations—disregarding their internal inconsistency—it must be pointed out that not only do negations of kernel sentences not carry, in general, the same, or even approximately the same, information as the kernel sentences themselves, but that it is not even true that the information carried by kernel sentences is varied by negation, or by the passive transformation, in the same way. Though *Atoms emit electrons* and *Electrons are emitted by atoms* carry the same information, *Some number exceeds every number* and *Every number is exceeded by some number* do not. (As a matter of fact, the first sentence is false, and the second true.) Harris was apparently partly aware of this, since he later [40] treats *not* both as part of the kernel, rather confusingly and inconsistently with his definition—though due to its vagueness this cannot be stated very definitely—and as an operator on the kernel. (The question of applicability to MT of the analysis of a given sentence as the outcome of the performance of zero, one or more transformations, in a certain order, on one or more kernel sentences— these terms now taken in Chomsky's sense—I have dealt with in Appendix II.)

In order not to be misunderstood, let me stress that my criticism refers only to Harris' description of what the process he calls *kernalization* is apt to achieve and that part of his theory of transformations which lies behind it. From a short discussion with him, I gathered that some of his formulations are indeed not to be understood literally, but I was unable to determine what exactly was left. It would be of some importance to get more clarity on this issue.

On the other hand, the actual programming of the mechanization of the syntactical analysis of English, as produced by the Philadelphia group, is utterly independent of the transformational model and, so far at least, based exclusively on the immediate constituent model. Judging from its latest internal project reports,[8] amazingly great progress has been made. Similar to the Harvard group, however, the programming is hampered by the fact that the internal low-access-time memory of the UNIVAC I, with which it works, is too small for MT purposes.

2.1.9 THE NBS GROUP

Coming now to the four "young" groups that started their activities within the last two years, let me first briefly describe the work done by the group consisting of Mrs. Ida Rhodes and a couple of associates at the National Bureau of Standards, Washington, D. C. No publications exist so far. It is

[8] Some of these reports are listed in reference [4], pp. 38-39. At least seven more have been published since.

nevertheless my opinion, based upon a few talks during which I was able to go through her program in considerable detail, that her approach is promising and worth close study. Not that she has been able so far to achieve any new results, but she quite often reobtained old results by sufficiently new and occasionally quite ingenious methods. Mrs. Rhodes is one of the few people in the field who has had long experience with actual programming. Being a native Russian speaker, she succeeded in combining her linguistic intuitions with her thorough knowledge of computers and their programming into an MT program which, judging from its presently existing outline, should, when fully developed, be able to achieve much of what can be achieved in this field in one of the most efficient and economical ways of which I am aware. Mrs. Rhodes is a mathematician by training, and her knowledge of modern structural linguistics is very slight. It should furnish some grounds for thought to realize how much of the practical aims of MT can be attained with no little use of structural linguistics. It should, however, be taken into account that Mrs. Rhodes' aims are wholly practical, and that no attempt is made by her to obtain a FAHQ output.

Let me mention just one detail in her program. One of the major problems in the syntactical analysis of the given source-language sentence is the problem of where to start. Garvin, for example, instructs the machine to look first for participial constructions and relative clauses. Harris, working with English though, lets the machine look for nominal blocks beginning with the end of the sentence and working backwards. In both approaches it is necessary to go over the sentence a few times before its final analysis is obtained; as a matter of accident, three passes are envisaged by both Garvin and Harris. Mrs. Rhodes, perhaps because of her linguistic naïveté, starts the analysis always with the first word of the sentence and lets the machine go over the words one after another, each time rewriting part of its own program, recalling Miss Lukjanow's technique mentioned above (Section 2.1.3). Though Mrs. Rhodes' approach is then based, as it were, on a *finite-state model*—for the detailed description of which the reader is referred to Chomsky's booklet [47] — whereas Garvin and Harris are working in effect with a phrase structure model, which is demonstrably a more powerful one, it is interesting to note that this does not interfere, apparently, with its practical efficiency. As against the multipass technique of Garvin and Harris, it has the advantage of being much more easily transferable to the treatment of the translation from other languages whereas, I presume, Harris' and Garvin's approaches are very much more tailored to English and Russian, respectively. In this connection, the interesting question arises, which of these three procedures is closest to the one used by human translators, if human translators use one common procedure at all which seems to me to be at least highly doubtful. Not that this question is of any practical importance for MT at this moment; however, if and when the time comes when translations will be performed by machines with learning abilities and using, at least partly, rather general heuristic instructions instead of the fully spelled-out program which is customary at present, our question may become a practical one since we would then probably want to give the machine the same or similar heuristic instructions which are given today to human translators during their training or which they develop for themselves in time.

2.1.10 THE GROUPS IN DETROIT, BERKELEY AND AUSTIN

The group at Wayne State University, Detroit, headed by Professor Harry H. Josselson and Dr. Arvid W. Jacobson, a Slavic linguist and a computer mathematician, respectively, has been in operation for too short a time to have already achieved any noteworthy results. It expects to deal with the same problems treated elsewhere, but intends to make more use of modern statistical techniques. I am not quite sure what this is exactly supposed to mean, and I have already expressed my doubts as to the effectiveness of the analyzing of a huge corpus of text, to which alone statistical methods would be applicable, for MT purposes beyond certain obvious points.

The group at the Berkeley campus of the University of California, directed by Dr. Louis G. Henyey and Dr. Sydney M. Lamb, still more recently created, started off by advocating a strongly "empirically" minded approach, but has in the meantime somewhat changed its mind and is now trying to strike a middle way between the divergent philosophies. Though I do not believe that this group has yet found an optimum compromise, its program strikes me as quite reasonable and promising if only it will be able to avoid the ever existing danger of just doing once more, perhaps a little better, what other people have done before.

There exists another small group at the University of Texas, Austin, headed by Professor Winfred P. Lehmann. I know nothing of the activities of this group, except that it must have started its work rather recently, and that judging from a talk given by Lehmann at the GU Round Table Meeting on MT [48], they are working on German syntax.[9]

2.2 The British Groups

2.2.1 THE LONDON GROUP

One of the two British MT groups is operating at Birkbeck College in London and is directed by Dr. Andrew D. Booth. Booth is one of the very first persons who thought of the utilization of electronic computers for translation as early as 1946. In 1948 he wrote, together with Dr.R.H. Richens, presently a member of the Cambridge group, a pioneer paper on MT [51]. He has continued his research in this field almost uninterruptedly though always only part-time, and published last year, together with two associates, a book dealing mostly with MT [52]. He was also coeditor of the first book dealing with machine translation [53], which contained 14 monographic studies on various aspects of the MT problem, in addition to a foreword by Dr. Warren Weaver of the Rockefeller Foundation and a valuable historical introduction. His recent book contains a great wealth of insights into the syntactical structure of German, and to a lesser degree into that of French and Russian, but the approach suffers from an excessive adherence to the empirical method in so much as rules for resolving syntactical ambiguity are based, in principle, "on analysis of all the existing literature on the subject in question," and in practice, for the purposes of illustration, on the analysis of a very small amount of text. The same holds for the methods proposed in this book for the reduction of semantical ambiguities. The authors are aware of the limitations of this method but intend to leave the development of a method that would resolve ambiguities in all conceivable (scientific) texts to people with a high degree of acquaintance with the German language. Some of the statements made in this

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[9] Headers of the report of Reitwiesner-Weik (R-W) mentioned above (Section 1.1), written about a year ago, will notice that these three groups have not been mentioned by them. This is, of course, simply because they started their work after the completion of the R-W report. On the other hand R-W do mention activities which have not been treated by me. An explanation is in order.

The National Science Foundation (R-W, pp. 11-12) is not conducting MT research on its own, but is sponsoring it and organizing conferences on this and related topics. In addition, it is publishing very valuable reports on *Current Research and Development in Scientific Documentation*, part of which is dedicated to MT. The latest of these reports in my possession [49] deals with MT on pp. 31, 32, 38, 39, 42, 45-57. Another one was scheduled to appear in April 1959.

Similarly, the U. S. Air Force, Air Research and Development Command (R-W, p. 12) sponsors and supervises MT research but does not seem to be engaged itself in it. (The final report of the University of Washington group mentioned there as due for about May 1958 appeared in February 1959, as mentioned above (Section 2.1.1).) The U.S. Army (R-W, p. 13) likewise only supports research.

IBM (R-W, p. 14) joined forces in 1953/54 with the GU group in the preparation of the well-known GU demonstration. Dr. Gilbert W. King, formerly with Telemeter-Magnetics, Inc., Los Angeles, California, joined IBM in 1957 and is in the process of organizing an MT group at the IBM research center in Yorktown, N.Y. Research at Telemeter-Magnetics, Inc., itself (R-W, p. 17) has been discontinued, to my knowledge, after King left.

So was research at the California Institute of Technology, Pasadena (R-W, pp. 17-18), after Mr. Toma left in 1958 in order to join the GU group. He is now working with the subgroup headed by Zarechnak.

The University of California at Los Angeles (R-W, pp. 22-23) was one of the first centers of MT—Professors Kaplan, Victor A. Oswald, Jr., William E. Bull and Harper were, and still are, teaching there—but to my knowledge none of them is now working on MT at the University, though Harper is serving at the moment as a fulltime consultant to RAND Corporation and the others might still occasionally do some consulting on MT matters, too. Oswald gave a talk in the GU Round Table Meeting in 1957 [50].

Research at Bell Telephone Laboratories, Haskins Laboratories, Indiana University, University of Chicago, and Western Reserve University (R-W, pp. 14-15, 20, 23, 30-31) is either irrelevant or only remotely relevant to MT. I am not aware of any recent activities in MT at the State College of Washington, Pullman (R-W, p.22).

book, of either historical or systematic nature, are made in an offhand manner and could create a somewhat distorted picture, especially with regard to the relative importance of the insights gained by the London group itself. There is, however, no point, here of going into such details. The book contains, in addition, many technical details on the construction of programs for MT, a full account of which may be gained from a companion volume by Mrs. Booth [54].

It might be worth mentioning that this book also contains a refutation of one very frequent argument favoring the use of an artificial mediating language, an *interlingua* in short, for MT purposes [55]. This argument points out that translation from each of *n* natural languages into each other requires the establishment of *n(n-l)* programs (including dictionaries), whereas the use of an interlingua, into which and from which all translation exclusively proceeds, requires only *2n* such programs. (For ten languages, for example, this means a reduction from 90 to 20 programs.) The fallaciousness of this argument is immediately obvious, however, as soon as one realizes that using one, any one, of the original *n* languages as a mediating language would reduce the number of programs even more, namely to *2(n-l)* (in our illustration to 18). This counterargument does not, of course, prove that the idea of using an interlingua for MT purposes is wrong as such, since other arguments might be brought forward in its support, but the one refuted just now seems to have been one of the most potent ones, and with its elimination proponents of the interlingua idea should give it a second thought.

It should indeed be carefully tested, for independent reasons, to what degree the quality of a translation between two languages is impaired, if instead of a direct translation an indirect one is employed, based upon high-quality translation from the source-language into some intermediate language and from it into the target-language. So far there exist, to my knowledge, only more or less anecdotal results in this respect. Should it turn out that high quality translation is generally obtainable by going through some intermediate language, natural or artificial, this would be of enormous importance for multilingual MT of the future.

Whereas the mentioned argument "from n^2 to $2n$" for the use of an *artificial interlingua* in MT can definitely be proven fallacious, though it holds good as an argument for the use of *any* intermediate language, there are of course other arguments to support the use of an artificial interlingua *qua* artificial whether of the Esperanto type or of that of a symbolic language system. I admit that the idea of a "logical," unambiguous (in every respect, morphologically, syntactically, and semantically) interlingua has its appeal today as had the related idea of a *characteristica universalis* in the 17th and early 18th centuries. This appeal is bolstered by the great achievements of modern mathematical logic with its constant use of artificial language systems, and there is therefore some force in the claim that an idea that failed in the 17th century need not do so in the 20th. But the present argument is no less fallacious. Its fallacy lies in the assumption that "translation" from a natural language into a "logical" one is somehow simpler than translation from one natural language into another. This assumption, however, is totally unwarranted, whatever its appeal to someone with little direct experience with symbolic language systems. As a matter of fact, the transition from a sentence in a natural language to its counterpart in a language system deserves the name "translation" only in a somewhat Pickwickian sense. I shall not elaborate this point any further, but only mention that it has been discussed rather widely in recent methodological literature. The fallacy is probably another result of the customary loose use of the word "translation" which has already caused a lot of trouble on other occasions (such as in connection with information retrieval where the issue becomes constantly befuddled through an uncritical and still more metaphorical use of this word). Not only is the process of presenting a counterpart of some natural language sentence in some symbolic language system in general more difficult than its translation into some other natural language, even for a human being, as everyone who has ever taught a freshman course in symbolic logic will readily certify, but the mechanization of this kind of "translation" poses problems which are much more difficult than those posed by translation proper. It is no accident, again, that not only have linguists not attacked these problems in any serious sense, but that even hard-boiled logicians have shunned it in favor of dealing with "easier" ones (which ordinary linguists regard as lying beyond their comprehension). Altogether, the problems revolving around an interlingua as a device for MT are still in a highly speculative state, and it is probable that years will pass before any practical results can be expected.

2.2.2 THE CAMBRIDGE LANGUAGE RESEARCH UNIT

The second British group is located in Cambridge, England, and is directed by Miss Margaret Masterman.[10] In spite of its constant disclaimers, this group is a highly speculative one with many of the good and equally many of the less good connotations of the term. I find myself, again and again amazed by the prolificy of ideas emerging from it, almost all of which have some initial appeal, while also having the disturbing property of constantly changing their exact meaning or being quickly replaced by some other idea, for which the same process starts all over again after a very short time. I myself, in the early stages of my thinking on MT, played with many of these ideas and can therefore readily testify to their appeal. I did, for instance, repeatedly spend some time on the question of whether and to what degree *Combinatorial Logic* [56] could be applied to MT, and though I have failed so far to achieve any results in this connection, I am not convinced that I myself, or other people better equipped for this purpose, could not still do so if working very hard and uninterruptedly on this problem. In one of my publications I made a brief mention of this issue [57]. Miss Masterman wrote a long (unpublished) paper on this topic three years ago, but I had great trouble understanding its point, and the issue is no longer mentioned in more recent publications of the Cambridge group, having apparently been superseded by the idea of applying *Lattice Theory* [58, 59]. Now Lattice Theory is the theory of a structure which is so general that one should not be surprised to find it embodied in many actual situations. There can also be no doubt that lattice theory, and certain still more general branches of abstract algebra such as the theory of semilattices, trees, directed graphs and partially ordered systems, can be applied to linguistic investigations though I am not aware of any new insights gained so far by such applications. The applications made by the Cambridge group of their lattice-theoretical approach, inasmuch as they are valid, are only reformulations in a different symbolism of things that were said and done many times before.

A third idea emerging from this group, though not only from it, is that of using a *Thesaurus*-type dictionary in lieu of, or perhaps in addition to, ordinary dictionaries. I find here the greatest difficulties of understanding in spite of many attempts on my part to do so and many hours of talking with various members of the group. One cause of the troubles is the fact that the term "thesaurus" has not only been used by various groups in different, occasionally quite different, senses, but that members of the same group often use the term in different senses, and that its meaning keeps shifting even in the publications of one and the same person with no adequate warning given to the reader, perhaps without the writer being aware of such a shift. So we find that a thesaurus is sometimes meant to be rather similar to Roget's well-known *Thesaurus of the English Language*, and at other times to be rather different from it. Sometimes the thesaurus is supposed to contain after each entry so many expressions of the same language, at other times its equivalents in some interlingua [60-62]. Since I could not persuade myself that I really understood the Cambridge group's conception (or conceptions?) of the thesaurus (or thesaurus-lattice) approach to MT, I shall say nothing about it. Perhaps the reader will be luckier. The literature cited above and the further references contained there should suffice for this purpose.

By far the most important idea cherished by the Cambridge group is that of using an interlingua for MT purposes. In addition to what was already said above on this topic the following remarks might help to explain the attraction that this idea has exercised in the Cambridge group as well as in many of the Russian groups (though not, to any noticeable degree, in any of the American groups). It is undoubtedly true that human translators occasionally (and beginners quite often) use a procedure that might be described as consisting of three separate steps: coming across the German word *schreibt*, for instance, a translator sometimes explicitly (and may be said to always proceed this way implicitly, so long as this way of speaking is not pushed beyond certain limits) first analyzes this word as third person, singular, present, active of *schreiben*, then looks up *schreiben* in the German-English dictionary, finding *write* as its English rendering, and finally synthesizes the third person,

[10] It is perhaps not superfluous to point out, in view of such descriptions as given in R-W, p. 35, that Miss Margaret Masterman and Mrs. M. Braithwaite and even Miss Masterson [!] in Oswald's paper cited in [50], are one and the same person. Miss Masterman is married to Professor Richard B. Braithwaite of the University of Cambridge, England.

singular, present, active of *write* as *writes*. When attempting to render the same word in French, he may perform three completely analogous steps: he first analyzes *schreiben* as third person, singular, present, active of *schreiben*, then looks up *schreiben* in the German-French dictionary, finding *écrire* as its French rendering and finally synthesizes the third person, singular, present, active of *écrire* as *écrit*. If some book has to be machine translated into English and French at the same time, it seems, therefore, that it would be more economical to perform the respective first steps in common. Generalizing, one might come to the conclusion that it would be less economical to perform every machine translation in these three steps of *analysis*, *one-to-one transfer*, and *synthesis*, than to perform the first step which is purely a source-language affair independently of the target-language into which the text is going to be translated, and the third step independently of the source-language from which the text was translated. Generalizing still further, one might think of proceeding this way not only with regard to the treatment of single words (morphological analysis and synthesis) but also with regard to whole phrases and sentences (syntactical analysis and synthesis).

There can be no doubt as to the appeal of this idea, and I am quite sure that every MT worker must have thought of it sooner or later, probably sooner as I did, for instance, in the first week of my preoccupation with this topic. Would it not be an enormous saving indeed to have, when dealing with multiple mutual translation between *n* languages, instead of the required *n(n-1)* translation programs and *n(n-l)* unidirectional binary dictionaries—*n* analysis programs, *n* synthesis programs and *one* completely symmetrical *n*-ary dictionary? Hence, why waste the time on preparing one-way translation programs for one pair of languages at a time, having to start from scratch for each new pair of languages and even for the same pair translating in the opposite direction?

There is only one thing wrong with this idea, which relies, of course, on another, more complex variant of the argument "from n^2 to $2n$"; it is utterly chimerical and based upon a series of fallacies, and the only effect of its adoption would be that of postponing for an indefinite time, depending on *n*, the date of inaugurating the first commercial partly-mechanized translation center. First, the idea of a completely symmetrical *n*-ary dictionary, each entry consisting of exactly *n* words, one each for each of the *n* languages concerned, is wholly unrealistic. The Cambridge group is fully aware of this fact, but in its attempt to find a substitute, it has been led to the idea of an interlingual thesaurus of undetermined complexity. On the other hand, to the degree that a symmetrical *n*-ary idioglossary is practical—as it might conceivably be for some highly restricted technical field—its preparation would require exactly the same effort as the preparation of the *n-l* (two-way) binary idioglossaries from one of the *n* languages to all the others.

The situation is similar with regard to the analysis and synthesis part. The morphological and syntactical categories in terms of which an analysis of a given source-language is successful for translation into some given target-language might not be the best ones for some other target-language. Analyzing in terms of all possible categories that might be needed for translation into any of the *n-l* other languages is exactly of the same degree of complexity as the sum of the analyses for each language in turn, The illusion that this is not so is created by regarding the effort of preparing a "complete," "absolute" grammatical analysis of one language as being only slightly more complex than preparing such an analysis "relative" to some other language, and simultaneously regarding the preparation of *n-l* "relative" grammatical analyses as requiring only slightly less than *n-l* times the effort required for preparing one such analysis. The truth, of course, is that the effort is on the average exactly the same, almost by definition.

That the terms "interlingua," "intermediate language," "mediating language"—and their counterparts in Russian—are being used in many different senses should not come as any special surprise. Natural languages, artificial languages of the Esperanto type, symbolic language-systems of the type treated by logicians, "algebraic" languages of various denominations, all have been suggested at one time or other as candidates for mediating languages. I believe that my present criticisms hold equally against each of these interpretations. I find myself here in close agreement with the views of Booth [62a].

As already said above (Section 2.2.1), the only point in this connection that deserves serious consideration is the following: Assuming that translation programs already exist from language L_1 to L_2 and from L_2 to L_3, by how much would the output of a direct translation program from L_1 to L_3 be better than the output of a combination of the two existing programs, and would the difference pay for

the effort required to prepare the new program? It seems to me almost trivially true that to this question no general theoretical answer is possible. It just depends.

Altogether there exists so far no evidence that any of the ideas brought forward by the various members of the Cambridge group will ever contribute new effective methods for practical MT, and little evidence that they would result in new valid insights into the workings of language.

2.2.3 THE TEDDINGTON GROUP

The National Physical Laboratory, Teddington, England, is in the process of organizing an MT group. So far, however, I know of nothing more specific in this respect.

2.3 The USSR Groups

2.3.1 GENERAL

In Soviet Russia there are some ten groups active in MT research. In contradistinction to the United States, however, where each group has access to an electronic computer, some of the Russian groups have so far apparently not been able to put their theoretical schemes to an actual machine test. Whether this is because of a shortage of available machine time or of a relatively larger interest in theoretical analysis, I do not know. It may be due to both. Experimental testing of theories is quite often performed by having human beings simulate machines, as was often done in the States six or seven years ago.

In addition to more or less permanent MT groups there are many scientists who apparently do not belong to any of these groups, but spend much of their time on MT research, sometimes serving as consultants, occasionally to many groups simultaneously and taking an active part in the rather frequent conferences and Academy of Sciences meetings dedicated wholly or partly to MT.

Active research on MT in Russia started in January 1955 on a relatively large scale from the very beginning. It is likely that the interest of the government in MT was stirred up by the GU demonstration in January 1954. However, MT had a prehistory in Russia. It seems that the first serious attempt to mechanize translation was made in 1933 by an engineer, P. P. (Smirnov-) Troyansky who proposed in that year to construct "a machine for selecting and printing words by translation from one language into another or into several others simultaneously," and even got an author's certificate for his invention [63]. His proposal met with scepticism and derision on behalf of the Russian linguists and mathematicians of the time and fell into oblivion. It is hard to blame them in view of the fact that the advent of electronic digital computers was still a dozen years ahead. Troyansky seems to have died at the end of World War II. One may express the hope that more should become known of this Babbage of MT.

In general, it is rather safe to assume that the state of MT in Russia is not essentially different from that elsewhere, and that the direction which MT research is taking there is about the same as, say, in the United Slates, in spite of some statements to the contrary made by Russian MT workers in 1956 and later. If anything, Russian scientists may be somewhat ahead in the linguistic analysis, whereas they are probably somewhat behind in actual machine-testing. We find in Russia the same differences in policy as elsewhere, between the adherents of FAHQT and those who advocate more modest aims, between the theoretically-minded and the empirically-minded, etc.

Though all this is true in general, there are two points in which MT research in Russia seems to be different from that in the States by degree, though not qualitatively. First, and this point has gotten much publicity, probably more than it objectively deserves, the number of language pairs between which MT has been investigated is much larger in the USSR research. Morphological and syntactical analyses for MT, automatic dictionaries, and comparative studies of linguistic structure for MT have been carried out with varying depth for some twenty languages, and MT programs exist for perhaps thirty pairs though most of these programs are still rather rudimentary, with Russian always, and quite naturally, being either the target or the source language. Among these languages we find, as one would expect, the great world languages of science, English, French, and German, languages of countries with whom the USSR has political alliances, such as Chinese, Czech, Bulgarian and Albanian, but also quite a number of non-Indo-European languages (in addition to Chinese) with

totally different scripts, such as Japanese, Hindi, Arabic, Indonesian, Vietnamese, etc. It is somewhat surprising to see that MT from Norwegian, for instance, has also already been investigated. This is hardly to be explained by the existence of an urgent practical need in this direction. It is more likely that MT is often used as a pretext for the instigation of descriptive studies of languages from a structuralist point of view—the only one which makes sense for MT—a view which until a few years ago was rejected in Russia as "formalistic." In this respect, again, the situation is not much different from that prevailing in the USA where a good amount of solid linguistic research is carried out under the auspices, not to say disguise, of MT. I understand that all this is by no means typical for "applied" linguistics alone.

If we recall that in the USA and England, English is always the target language (with the exception of the GU group where English-to-Chinese and English -to-Japanese MT is in the first stages of investigation) and that the only source languages considered so far at any large scale were Russian, French and German exclusively, the impression on the general public created by the breadth of Russian MT is understandable. More important, however, is that Russian has been treated as both target and source language. I am not referring now to the political and cultural aspects of this fact, but rather to the fact, that this state seems to have been one of the strongest inducements for the second issue which distinguishes Russian MT, though again only in degree.

This second issue is the almost universal preoccupation with intermediate languages. The use of this term in Russia is in general no less equivocal and confusing than in England. One Russian group, the most practical and down-to earth of them (Section 2.3.2), would like to investigate multiple translation with Russian as the pivot language. I already expressed above (Sections 2.2.1, 2.2.2) my belief that this aspect of the intermediate language issue is indeed one, probably the only one, that deserves serious consideration.

Other groups, however, are using the term "intermediate language" in different senses, some of which are closely related to—though apparently conceived independently of—those in which it is understood by the Cambridge group. Though considerably less fantastic in the details than the Cambridge speculations, there is nothing in the Russian versions of the intermediate language idea to change my sceptical opinion as to its practical value.

Most Russian publications on MT have been translated into English by various American agencies, the most important of which is the U. S. Joint Publications Research Service. Photocopies of the reports issued by this service may be purchased from the Photoduplication Service, Library of Congress, Washington 25, D. C. Due to my insufficient knowledge of Russian, all my quotations refer to the English translations. These translations are in general quite good though they contain also, in addition to misprints and occasional ridiculous retransliterations of English proper names from Russian, a few rather bad mistakes and must therefore be used with some care. The references contain full indications to the Russian original publications. There exist also a few publications by Russian authors on MT written originally in either English or French [64-67].

Before I go into a relatively detailed discussion of the achievements of the particular MT groups, let me report briefly on some general organizational aspects of MT. I already mentioned in the first paragraph of this survey (Section 1.1) that in the May 1958 First All-Union MT Conference in Moscow, 79 institutions were represented. These included 21 institutes of the Academy of Sciences of the USSR, 8 institutes of the Academies of Sciences of the Union Republics, 11 universities and 19 other institutions of higher learning in the country. This does not mean, of course, as it would not for similar conferences in the USA, that there exist active research groups in all these institutions. Still, this conference was undoubtedly the largest of its kind held so far. Not less than 71 talks were given during its week-long meeting, though not all of them were of direct concern to MT.

A seminar on mathematical linguistics has been in operation since September 1956 in the Department of Philology of the Moscow State University. An Association for Machine Translation was established in December 1956 at the First Moscow State Pedagogical Institute of Foreign Languages and is publishing a *Bulletin of the Seminar on Problems of Machine Translation* (*Byulletin ob"edineniya po problemam mashinnogo perevoda*). A Committee on Applied Linguistics was established in June 1958, with its center apparently in Leningrad. It was decided to establish permanent liaison between the Committee and the Association.

The Russian biweekly, *Voprosy yazykoznaniya* (*Problems of Linguistics*), carried in the years

1956-58 a large number of papers dedicated to MT and continues to deal with MT as a regular feature. In 1958, *Problemy kibernetiki* (*Problems of Cybernetics*) started to appear, with A. A. Lyapunov as editor, and the first issue contained three papers on MT.

The best over-all description of Soviet work in MT was given in a talk presented by V. Yu. Rozentsveyg at the Fourth International Congress of Slavicists, Moscow, 1958 [68]. The second edition of a booklet by D. Yu. Panov on automatic translation [69] goes into much greater details than Rozentsveyg's talk but concerns itself, on the other hand, mostly with the achievements of the ITMVT group to be discussed in the next section. Much of the factual information given in the following sections, unless otherwise documented, is adapted from these two sources. I already mentioned that Oettinger was so kind as to let me see unpublished reports of his on the Russian MT literature and his visit to some Moscow MT centers in summer 1958. In addition, I heard and read a report by Professor John W. Carr, III (now at the University of North Carolina, Chapel Hill) on his visits to various Russian MT centers similarly undertaken in summer 1958. It may be hoped that all these reports will soon become publicly available.

2.3.2 THE ITMVT GROUP

The first Russian MT group was established in Moscow at the Institute of Precision Mechanics and Computer Engineering (Institut tochnoy mekhaniki i vychislitelnoy tekhniki) of the Academy of Sciences of the USSR. The institute is directed by Academician S. A. Lebedev. The MT group was originally led by D. Yu. Panov but is now under the direction of I. S. Mukhin who is also Assistant Director of the Institute. Its most prominent members are, at the moment, in addition to Mukhin, I. K. Belskaya and T. M. Nikolayeva. Research started in January 1955, and the first demonstration of translating a scientific text from English into Russian was made already at the end of 1955 on a BESM computer.

The group has since continued to concentrate on English-to-Russian translation. More than 3000 English sentences were hand-translated in machine simulation of the program developed by the group. Some 3000 English words (canonical forms) were included in the dictionary as of summer 1958, 600 of which were polysemantic. The group claims to have developed separate algorithms for resolving the semantic ambiguity of these words, but I take it that this was done relative to the analyzed texts rather than for every conceivable context.

In addition, dictionaries and algorithms have been compiled for MT into Russian from Chinese, Japanese, and German; these too were hand-tested. The aims of the group are mostly practical, and its efforts are concentrated toward a rapid achievement of a working method based upon detailed and careful investigations of actual texts, rather than toward a theoretical comprehension of the general problem of MT. Its approach is then rather "empirical" and occasionally even strongly opposed to too much theorizing. In distinction to almost all other groups, this group sees no good reasons for abandoning traditional methods of describing languages in favor of those of structural linguistics, and prefers to work, for instance, with the customary parts of speech rather than experimenting with new categories. It does not believe in the logico-mathematical approach practiced by the other groups and prefers methods of approach closer to those used in the experimental sciences. It does not bind its fate to some specific model, such as the immediate constituent model, and is ready to take its clues from wherever they come. Nikolayeva, for example, made a special study of the clues provided by punctuation marks [70], an aspect usually neglected in other approaches.

After its great initial successes, this group came to believe that FAHQT was a matter of the near future. Nikolayeva, in a publication of 1957, regarded it as premature to speak of the so-called "limits" of MT [71]. Panov, on the other hand, in the conclusion of his 1958 booklet, makes it quite clear that "the initial experiments in MT performed to date in the USSR and abroad still leave us far from a practical solution to the problem of automatizing translation on a large scale" but still goes on to anticipate "important achievements in the near future, at least as regards the translation of scientific and technical materials."

It is interesting that this group has come to the definite conclusion that "MT will have to be performed on a special computer," [72] a conclusion in which I tend to concur, whereas the American research workers have not quite made up their minds on this point.

As to multiple translation, this group uses the argument "from n^2 to $2n$" to advocate the use of intermediate languages, and more specifically Russian, for the USSR efforts. They are slightly critical of using artificial interlinguae such as Esperanto, but admit that this problem requires further study.

The ITMVT group is not represented in either the Association for Machine Translation or in the Committee for Applied Linguistics, for reasons which I do not know. It is possible, however, that this is an indication of the difference in approach indicated by the slogans "empirical" vs. "mathematical."

2.3.3 THE MIAN GROUP

Shortly after the establishment of the ITMVT group, a research group on MT was organized at the Steklov Mathematical Institute (Matematichesky Institut imeni Steklova) of the Academy of Sciences, USSR. This group is led by Professor A. A. Lyapunov and its most prominent members are O. S. Kulagina and T. N. Moloshnaya. It has access to a Strela computer.

Though this group too started off with an empirical approach and produced still in 1956 a very important paper on French-to-Russian MT written in this vein, it has thereafter adopted more and more a theoretical approach, partly reminiscent—though of course entirely independent— of the approaches of Harris (2.1.8) and Garvin (2.1.3), with a somewhat stronger mathematical tendency. In linguistic theory, they are following mostly the teachings of the Russian linguist F. F. Fortunatov (died 1916) which sound rather similar to those of the American structural linguists and especially to the immediate constituent model, in the form given it by Fries [73]. Moloshnaya arrived at 19 (syntactical) word classes for English, which is, by coincidence, the same number as Fries' [74].

Altogether, this group has so far completed three algorithms for French-to-Russian, English-to-Russian and Hungarian-to-Russian MT. The transition from the "empirical" treatment of the French-Russian algorithm to the "theoretical" treatment of the English-Russian algorithm was partly conditioned by the fact that morphological analysis alone yields much more information with regard to French than with regard to English, for which syntactical relations have to be exploited to a much higher degree.

Moloshnaya's structural-grammatical analysis of English proceeds independently of the target language into which a given English text is to be translated, but this is not—contrary to what she believed at the time—a peculiar feature of her work; this is the procedure of Garvin, of Harris (for English), of Yngve (for German), and of many others. Whether it is the wisest one in the long run remains to be seen. For immediate applications it seems unnecessarily wasteful; but this point was already discussed above (Section 2.2.2). So was its connection with the intermediate language issue.

Lyapunov might have been one of the first to call attention to the need for automatic programming techniques ("programming programs" is the literal translation of the Russian term) in order to obtain better quality automatic translations, and much work along this line is going on in the MIAN group.

Kulagina [75] calls her method "set-theoretical," but this term is unduly pretentious and therefore somewhat misleading as well as frightening to her less mathematically-minded linguistic colleagues. The use she makes of set theory is extremely elementary, and there should be no difficulty in translating her terminology into one with which linguists are more familiar, as requested by one of them, Rozentsveyg, who chaired a meeting in which Kulagina presented a paper.

Other Russian linguists had the feeling that the model of Kulagina, though perhaps well adapted to a closed language system, is of doubtful applicability to living, open languages. This is, of course, the standard complaint whenever a model is offered. This fact does not diminish its seriousness. How closely a model fits an actual situation cannot be determined by looking at the model alone. It would not surprise me to find out that Kulagina's model, like all the others so far proffered by MT workers, will be found wanting.

The more one gets away from empirical analysis, the farther recedes quick achievement of practical MT. The MIAN group seems to be aware of this and be ready to pay the price. Some outside Russian critics like Barkhudarov and Kolshansky [75] feel that this is not sufficiently stressed by the MIAN people and insist that automatic translation "will play only a subsidiary role in translation, moreover only of special texts. When it is necessary to attain a really good translation, a machine

translation can be only raw material for a human translator-editor." There is no need for me to stress at this point how much I agree with this view. A similar concern with the limits of MT is shown by Zhirkov who, in what seems to me to be a very sensible paper |77|, makes similar points though not always with adequate arguments: that a machine will be in trouble to decide whether *violet* is to be rendered in Russian as *lilovy* or rather as *fioletovy* is true but hardly impressive, since it is difficult to see the harm that would be done, for scientific texts, by arbitrarily choosing one of these renderings. None of them gives an argument to the same effect of that extremely simple form I present in Appendix III.

2.3.4 THE INSTITUTE OF LINGUISTICS GROUP

At the Institute of Linguistics (Institut yazykoznaniya), the division of applied linguistics headed by Professor A. A. Reformatsky is also working on MT problems. Its most active member is I.A. Melchuk who—as already mentioned above (Section 2.3.1)—has widely published and lectured on his version of interlingua which seems in general closely related to one of the Cambridge variants though it is much less fantastic and speculative. Through an analysis of Russian, English, Chinese, French, and Hungarian he hopes to arrive at a set of general basic operators— presumably expected to be sufficient for further languages though he would, of course, be ready to extend this set, if necessary—so that the syntactic function of every expression in every language will be describable by a suitable assignment of values to these operators, presumably with a lot of vacuous or don't-care assignments. Exactly as with regard to the Cambridge group method, I fail to see that this method could conceivably simplify things.

2.3.5 THE LE GROUP

Similarly to the Institute of Linguistics group, the group at the Laboratory of Electrical Modelling (Laboratoriya elektromodelirovaniya) of the All-Union Institute of Scientific and Technical Information (VINITI), directed by V. A. Uspensky, is strongly influenced by the MIAN group. As a matter of fact, Melchuk has been collaborating with both Kulagina of MIAN and with the LE group. This group has so far isolated some 200 different types of syntagmas, i.e., word-pairs in subordinate relationship, not necessarily contiguous, which are essential in both the analysis and the synthesis of Russian sentences. This type of work is closely reminiscent of part of the work done at RAND (Section 2.1.4).

2.3.6 THE ELMP GROUP

Though the group at the Leningrad State University Experimental Laboratory of Machine Translation (Eksperimentalnaya laboratoriya mashinnogo perevoda) started its work only in February 1958, it has since been incredibly productive under the leadership of N. D. Andreyev. Perhaps due to the fact that it has had so far no access to a computer— though it may by now have computer time available at the Ural machine —and that it felt that the English-to-Russian and French-to-Russian translations were already sufficiently taken care of, this group specialized in preparing translation programs for a large variety of language pairs according to a unified scheme. It also seems to have been the driving power for the establishment of the Committee for Applied Linguistics, a permanent committee of the Academy of Sciences intended to design and coordinate research in the total field of applied linguistics, of which MT is only a part. It was this committee which planned and helped organize the May 1958 conference and which decided on a whole series of conferences, the first of which, also lasting one week, took place in April 1959 in Leningrad.

The ELMP group too is strongly involved in the intermediate language idea, along lines equally different from those of the ITMVT group and of Melchuk's. Their intermediate language is planned to be a kind of common denominator between all languages of the world and to be derived from them by a formula weighting these languages according to the number of people who speak them [78]. Whatever the appeal of such an idea for an International Auxiliary Language—and similar schemes were often offered in this connection—it seems to me rather obvious that for MT purposes it

combines all the disadvantages of using either a natural language, like Russian, or an artificial language of the type envisaged by the Cambridge group and Melchuk, with hardly any advantages over them. It is my hope that Andreyev and his group will grow out of their conception of a *meta-language* as they call—somewhat misleadingly—their variety of an intermediate language and spend their energy and great abilities in a more promising direction.

Last year, this group published a first volume of 14 articles on MT [79], all but one written by its members, the exception being a long and well-documented paper by Ivanov of Moscow [80].

2.3.7 THE PEDAGOGICAL INSTITUTE GROUP

The group at the First Moscow State Pedagogical Institute of Foreign Languages (Pervy Moskovsky gosudarstvenny pedagogichesky institut inostrannykh yazykov), under the direction of I. I. Revzin and the active participation of Rozentsveyg, is working exclusively on theoretical problems of MT, especially with Russian as the source language and English, French and Spanish the target languages; the main interest of this institute is training in foreign languages. This group seems to be composed solely of linguists. It was already mentioned above (Section 2.3.1) that an Association for Machine Translation was created at this Institute which holds regular meetings and is publishing a Bulletin; the May 1958 conference was mainly organized by this Association, with the help of the Committee for Applied Linguistics.

2.3.8 THE DEPARTMENT OF PHILOLOGY GROUP

In 1956, a seminar on mathematical linguistics was instigated in the Department of Philology of the Moscow State University, where mathematicians, mathematical logicians, and linguists, under the direction of V.V. Ivanov, P. S. Kuznetsov, and V. A. Uspensky who combine training in the three mentioned fields, discussed application of mathematical logic and set theory to linguistics in general and to MT in particular, having realized that the standard methods of structural linguistics, as exposed, for example, in Fries' book [81] (which seems to have enjoyed great popularity among Russian linguists) are not fully adequate for a description of language as required for MT purposes. I presume that this means that they have come to realize that the immediate constituent model is not wholly adequate. It seems, however, that until summer 1958, at least, they were not aware of Chomsky's studies in this direction though Kulagina's formulations, for instance, show a considerable proximity of ideas.

2.3.9 OTHER RUSSIAN GROUPS

MT research is also being conducted in the State Universities of Kiev, Gorki, Tbilisi (Tiflis) and Yerevan as well as at the Institute for Mathematical Machines of the Academy of Sciences of the Armenian SSR in Yerevan on Armenian-to-Russian and Russian-to-Armenian MT, and the Institute of Electronics, Automation and Telemechanics of the Academy of Sciences of the Georgian SSR in Tbilisi on Georgian-to-Russian and Russian-to-Georgian MT.

2.4 Other Groups

Outside the USA, England, and the USSR, MT has been taken up, with few exceptions, only by individuals, usually spending only a small part of their time on this topic. The only groups of which I am aware are those in Milan, Italy, and Jerusalem, Israel. It is probable that there exist MT groups in China and Japan. A limited amount of work has been dedicated to MT also in France, Germany, Poland, Hungary, Czechoslovakia, Switzerland, India, and perhaps a few more countries. But I do not know of any original research in these countries that deserves to be mentioned here.

2.4.1 THE MILAN GROUP

A small MT group has recently been created at the University of Milan. It is directed by Professor Silvio Ceccato who had been working on this problem also prior to the establishment of the

group. The only publication by him on this topic [82] shows a good amount of originality, but is also extremely speculative and rather obscure. The Milan approach is based on such notions as *elements of thought* and seems to me therefore to be practically hopeless, at least for the near future.

2.4.2 THE JERUSALEM GROUP

At the Hebrew University of Jerusalem a small research group was organised in June 1958. It consists of myself and three part-time assistants. It tries to keep track of advances in MT, but the actual research work deals exclusively with such highly theoretical topics as language models and their connection with the theory of automata, Post canonical systems and recursive function theory, closely related to, and in exchange of ideas with, the present research of Chomsky.

The first technical report of this group [83] was extensively used in the preparation of the present survey.

3. Conclusion

Fully automatic, high quality translation is not a reasonable goal, not even for scientific texts. A human translator, in order to arrive at his high quality output, is often obliged to make intelligent use of extra-linguistic knowledge which sometimes has to be of considerable breadth and depth. Without this knowledge he would often be in no position to resolve semantical ambiguities. At present no way of constructing machines with such a knowledge is known, nor of writing programs which will ensure intelligent use of this knowledge.

Reasonable goals are then either fully automatic, low quality translation or partly automatic, high quality translation. Both are theoretically feasible and, for certain language pairs, attainable today though not yet on a commercial scale. Through a concentration of effort, pooling of knowledge, and planned division of labor it should be possible to establish within a period of three to five years translation centers which would be in a position, after a short period of subsidized operation, to make considerable use of electronic machinery in the translation process under competitive costs and with a substantial saving in expert bilingual manpower. It might perhaps be possible, after some additional time, to get along without expert bilingual post-editors altogether. For high quality output, however, the services of a human monolingual editor—in general, for practical reasons, a post-editor, though I can see no reasons why a pre-editor or even a pair of post- and pre-editors could not be equally effective, wherever available, and therefore deplore the almost total disregard of these possibilities—will remain indispensable. I regard it as unlikely that the cost of human editing can be pushed down in the near future below half of the cost of human translating of equal quality. This means that, for systems with post-editing, a mechanical print-reader will have to be used and most probably also a special purpose machine.

For the preparation of practical MT programs, great linguistic sophistication seems to be neither requisite nor even especially helpful at the present state of the art. Basic linguistic research is of great importance as such, and its support should preferably not be based on the pretense that it will lead to an improvement of MT techniques as is often done in the United States as well as in Russia. It is likely that far-reaching illumination of the human factor in translation will not be achieved without an enormous amount of such basic research, but this is a very long-range affair that should preferably be kept separate from immediate goals.

There has been a great amount of overlap in research among the various MT groups, not only among those in different countries but even within the same country. A certain amount of overlap is inevitable and even definitely helpful. It is my strong feeling, however, that the existing overlap is unnecessarily high and has led in many cases to costly repetitions of achievements as well as, and even more often so, of failures. This is not the place to offer recommendations for the improvement of this state of affairs.

Machine translation has not developed quite as speedily as its pioneers were hoping for seven or eight years ago, encouraged by spectacular initial successes. This is partly because the development of large-capacity and low-access-time memory devices has perhaps not quite fulfilled the high expectations of that time, and partly simply because of the vastness of the task which was not seen

clearly enough at that time, so that by sticking too long to the goal of FAHQT much effort was wasted, at least insofar as immediate results are concerned. Nevertheless, commercial utilization of electronic machinery as aids in translation is now a practical prospect which will materialize after a series of additional improvements in linguistic and computer techniques along lines well understood at present. Speculations as to a break-through which will be made by the advent of learning machines, exciting as they are in themselves, have been left aside in this survey.

4. Remark on Bibliography

No attempt was made to provide here a complete bibliography. The journal *Mechanical Translation*, edited by W. N. Locke and V. H. Yngve of MIT, contains in addition to articles and news items an annotated bibliography. The last item in the bibliography of the last issue in my possession, Vol. 5, No. 1, dated July 1958 (published in December 1958), has the ordinal number 152.

Other bibliographies are given on pp. 227-230 of reference [17] (46 annotated items), pp 82-95 of reference [7] (82 annotated items), pp. 22-51 of reference [2], and pp. 51-65 of reference [3] (contains some 170 items, including internal reports, work, papers, etc.); Appendix 7 of reference [13] contains a complete bibliography of work performed up to the end of December 1958 at the Harvard Computation Laboratory on automatic translation and mathematical linguistics (58 items, mostly unpublished seminar papers). Useful current references are given passim in reference [4], and further references are undoubtedly contained in issue No. 4, of that survey which was scheduled to appear in April 1959.

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Appendix I

MT STATISTICS AS OF APRIL 1, 1959

(No responsibility as to the accuracy of the figures is undertaken. They were obtained by personal communication, the author's impressions or *bona fide* guesses. In cases of pure guesses, a question-mark is appended.)

Institution	Year of start of research	Number of workers	Full-time equivalents	Current yearly budget ($)	Project leader(s)
University of Washington Department of Far Eastern and Slavic Languages and Literature Seattle, Washington	1949	10?	6?	?	Erwin Reifler
Massachusetts Institute of Technology Research Laboratory of Electronics and Department of Modern Languages Cambridge 39, Massachusetts	1951	10?	6?	?	Victor H. Yngve
Georgetown University The Institute of Languages and Linguistics Machine Translation Project , 1715 Massachusetts Avenue Washington, D.C.	1952	30?	15?	?	Leon E. Dostert Paul L. Garvin Ariadne W.Lukjanow Michael Zarechnak A.F.R.Brown
The RAND Corporation 1700 Main Street Santa Monica. California	(1950) 1957	15	9	?	David G.Hays Kenneth E. Harper
Harvard University The Computation Laboratory Machine Translation Project Cambridge 38, Massachusetts	1953	11	7?	?	Anthony G.Oettinger
University of Michigan Willow Run Laboratories Ann Arbor, Michigan	1955	11	7	?	Andreas Koutsoudas
University of Pennsylvania Department of Linguistics Philadelphia, Pennsylvania	1956 ?	10 ?	3?	?	Zellig S.Harris
National Bureau of Standards Washington. D.C.	1958	3	2	25,000	Ida Rhodes
Wayne Stale University Department of Slavic Languages and Computation Laboratory Detroit, Michigan	1958	10	6	40,000	Harry H.Josselson Arvid W.Jacobson
University of California Computer Center Berkeley, California	1958	8	5	40,500	Louis G.Henyey Sydney M.Lamb
University of Texas Department of Germanic Languages Austin 12, Texas	1958	?	?	?	Winfred P.Lehmann
Other American groups and individuals		50?	10?	?	
Total, USA		150?	80?	1,500,000?	
Birkbeck College Department of Numerical Automation London, England	(1947) 1955	6?	3?	?	Andrew D.Booth
Cambridge Language Research Unit 20 Millington Road Cambridge, England	1955 ?	20?	5?	?	Margaret Masterman

Institute of Precision Mechanics and Computer Engineering Academy of Sciences of the USSR Moscow	1955	?	?	?	I.S.Mukhin
Steklov Mathematical Institute Academy of Sciences of the USSR 1 Akademichesky Proezd, Dom No. 28 Moscow V-134	1955	?	?	?	A.A.Lyapunov
Division of Applied Linguistics Institute of Linguistics Academy of Sciences of the USSR Moscow	1956 ?	?	?	?	A.A.Reformatsky I.A.Melchuk
Laboratory of Electrical Modelling All-Union Institute of Scientific and Technical Information Moscow	19.56 ?	?	?	?	V.A.Uspensky
Experimental Laboratory of Machine Translation Leningrad State University Leningrad	1958	?	?	?	N.D.Andreyev
Association of Machine Translation Pedagogical Institute of Foreign Languages Moscow State University Moscow	1957 ?	?	?	?	I.I.Revzin V.Yu.Rozentsveyg V.V.Ivanov
Seminar on Mathematical Linguistics Department of Philology Moscow State University Moscow	1956	?	?	?	P.S.Kuznetsov
Other Russian groups and individuals		?	?	?	
Total, Russia		300?	120?	1,500,000?	
University of Milan Milan, Italy	19.53	?	?	?	Silvio Ceccato
Hebrew University Jerusalem, Israel	1958	4	1	4,000	Yehoshua Bar-Hillel
Other groups and individuals		?	?	?	
TOTAL		500?	220?	3,000,000	

Appendix II

Some Linguistic Obstacles to Machine Translation•

For certain pairs of languages it has been shown experimentally that word-by-word machine translation leads to an output which can often be transformed by an expert post-editor into a passable translation of the source text. However, if one is interested in reducing the burden of the post-editor, as one apparently has to be in order to make the use of machine aids in translation commercially profitable, or if one has to do with pairs of languages for which word-by-word translation is not by itself a satisfactory basis for post-editing, it is natural to think of mechanizing the determination of the syntactic structure of the source sentences. It is theoretically clear, and has again been experimentally verified, that knowledge of the syntactic structure of the sentences to be translated does considerably simplify the task of the post-editor. It is obvious, for instance, that this knowledge tends to reduce, and in the limit to eliminate, those syntactical ambiguities which are created by the word-by-word translation and which are nonexistent for the human translator who treats the sentences as wholes. The task of the post-editor would then consist solely in eliminating the semantical ambiguities and in polishing up the style of the machine output. Whether these steps, too, can be completely taken over by machines of today or of the foreseeable future is still controversial. I myself have strong reasons for regarding it as hopeless, in general, but this is not the point I would like to discuss here; it is the subject of Appendix III.

A few years ago, I proposed what I called a quasi-arithmetical notation for syntactic description [1] whose employment should allow, after some refinements, for a mechanical determination of the constituent structure of any given sentence. At that time, I actually demonstrated the effectiveness of the method for relatively simple sentences only, but cherished the hope that it might also work for more complex sentences, perhaps for all kinds of sentences. I am now quite convinced that this hope will not come true. As a consequence, the road to machine translation can be shown to contain more obstacles than was realized a few years ago. I think that this should be of sufficient interest to warrant some more detailed exhibition, especially since this insight is due to an important new, not to say revolutionary, view of the structure of language, recently outlined by the American linguist and logician Noam Chomsky [2], and could perhaps, in its turn and in due time, be turned into a new method of machine translation, which would be more complex than the known ones but also more effective.

Since I can not assume acquaintance with the paper in which I introduced the quasi-arithmetical syntactical notation mentioned above, let me present its main point here again very briefly, with some slight modifications in terminology and notation, partly under the impact of a recent article of Lambek [3]; for a full presentation, the paper should be consulted.

The basic idea, adopted from a paper of the Polish logician Ajdukiewicz [4], is to regard every sentence (of more than one word) as the result of the operation of one continuous part of it upon the remainder, these two parts being the *immediate constituents* of the sentence, such that these constituent parts which in general are not sentences themselves, but rather phrases, are again the product of the operation of some continuous part upon the remainder, etc., until one arrives at the final constituents, say words or morphemes. In accordance with this variant of the *immediate constituent model*, which is the standard model with which many modern linguists are working [5], all words of a given language are assigned to one or more, but always finitely many, *syntactic categories*.

For the purpose of illustration we shall try to get along, for English, with two fundamental categories, those of *nominals* and (declarative) *sentences*, to be denoted by *n* and *s*, respectively. The operator category of *intransitive verbals*, i.e., the category of those words that out of a nominal to their left form a sentence, will be denoted by $n\backslash s$ (read: *n* sub *s*), the category of *adjectivals*, i.e., of words that out of nominals to their right form nominals, will be denoted by n/n (read: *n* super *n*), the category of *intransitive verbal adverbals*, i.e., of words that out of intransitive verbals (to their left) form

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• This Appendix comprises greatly revised versions of parts of two talks, one given before the Colloque de Logique, Louvain, September 1958 and published in *Logique et Analyse* 7, 19-29 (1959), the other given before the Second International Congress of Cybernetics, Namur, September 1958, to be published in the Proceedings of this congress, 1960.

intransitive verbals, by (*n**s*)\(*n**s*)—for which we shall, by means of a self-explanatory convention, usually write *n**s**n**s*— etc, (Nominals, verbals, adjectivals, etc., in my present usage, are *syntactical categories*. They should not be confused with nouns, verbs, adjectives, etc., which are *morphological* (*paradigmatic*) *categories*, in my usage. The connection between these two classifications, as the choice of terms is intended to indicate, is that nouns usually, though by no means always, belong to the syntactical category of nominals, etc., and that most expressions belonging to the syntactical category of nominals, of course only if they are single words, are nouns.) In *Little John slept soundly*, for instance, we would regard *Little* to be an *n*/*n*, *John* an *n*, *slept* an *n**s*, and *soundly* an *n**s**n**s*.

Assuming then, that a category "dictionary" listing for each English word all its categories stands at our disposal, the task of finding out whether a given word sequence is a sentence or, more generally, a *well-formed* (or *connex*) expression and, if so, what, its *constituent, structure* is, could now be solved according to the following utterly mechanical procedure: We would write under each word of the given word sequence the symbols for all the categories to which it belongs, separated by commas, and then start *cancelling* in all possible ways, according to either of the two following rules:

$$\alpha,\ \alpha\backslash\beta \rightarrow \beta \qquad \text{and} \qquad \alpha/\beta,\ \beta \rightarrow \alpha$$

(The reading of these rules should be self-explanatory. The first, for instance, reads: Replace the sequence of two category symbols, the first of which is any category symbol whatsoever and the second of which consists of the first symbol followed by a left diagonal stroke followed by any category symbol whatsoever, by this last category symbol.) A series of such symbol sequences where each sequence results from its predecessor by one application of a cancellation rule; is called a *derivation*. The last line of a derivation is its *exponent*. If the exponent consists of a single symbol, *simple* when it consists of a single letter, *complex* when it contains at least one stroke, the word sequence with this exponent, and with the constituent structure given by the derivation, is well-formed; if the exponent of a certain derivation is, more specifically, *s*, the sequence is a sentence, relative to this derivation.

To illustrate, let us start with the last analyzed expression:

Little John slept soundly.

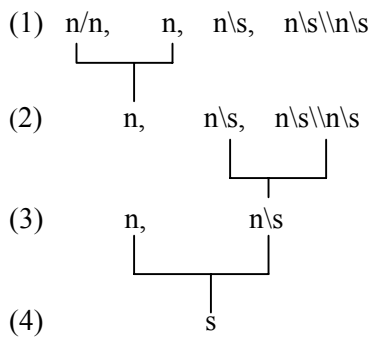
Let us assume (contrary to fact) that as a result of consulting the category dictionary we would have arrived at just the following category symbol sequence:

(1) *n*/*n*, *n*, *n**s*, *n**s**n**s*.

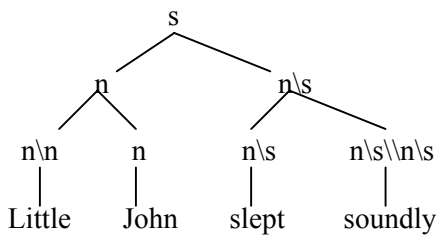
It is easy to see that there are exactly three different ways of performing the first cancellation, starting off three different derivations, viz.:

(2) *n*, *n**s*, *n**s**n**s*.
(2') *n*/*n*, *s*, *n**s**n**s*,
(2") *n*/*n*, *n*, *n**s*.

(2') leads into a blind alley. The other two lines, (2) and (2"), each allow for two continuations, of which one again leads into a blind alley, whereas the other allows for just one more derivation, with both exponents being *s*. Let me write down one of these derivations:



The other derivation differs from the one just presented only in that the two cancellation steps in (2) and (3) occur in the opposite order. These two derivations are therefore equivalent in an important sense; in fact, they correspond both to the same *tree expansion*:



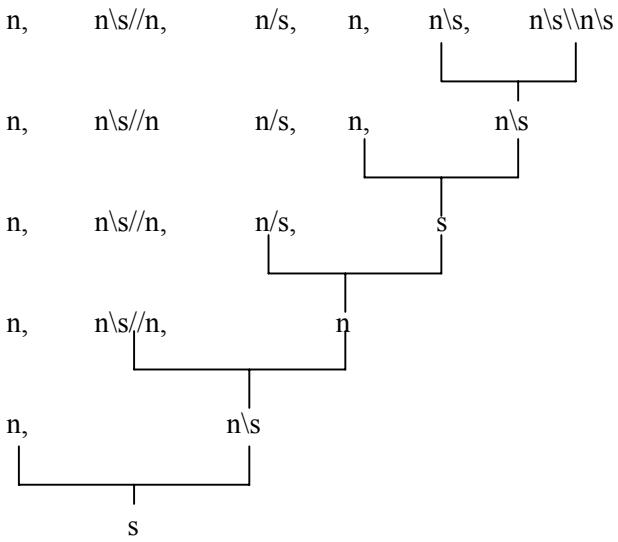
Our second and final example will be:

Paul thought that John slept soundly.

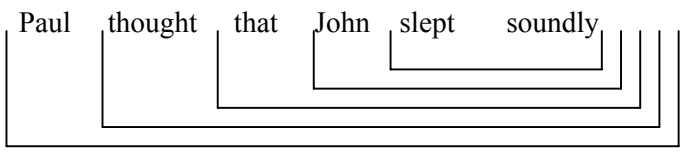
(I hope that the somewhat shaky English of this example will be forgiven; it simplifies making the point without falsifying it.) Copying only the first entry under each word in our fictitious category dictionary, we arrive at

Paul	thought	that	John	slept	soundly
n,	n\s/n,	n/s,	n,	n\s,	n\s\\n\s

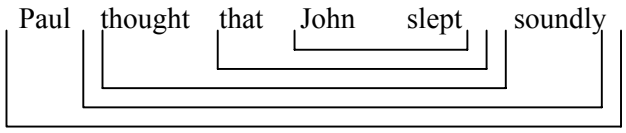
There are two nonequivalent derivations with a single exponent. I shall again write down only one of these derivations:



The constituent structure corresponding to this derivation can be pictured in the following parsing diagram:



The reader is invited to check that the parsing diagram corresponding to the other derivation is:



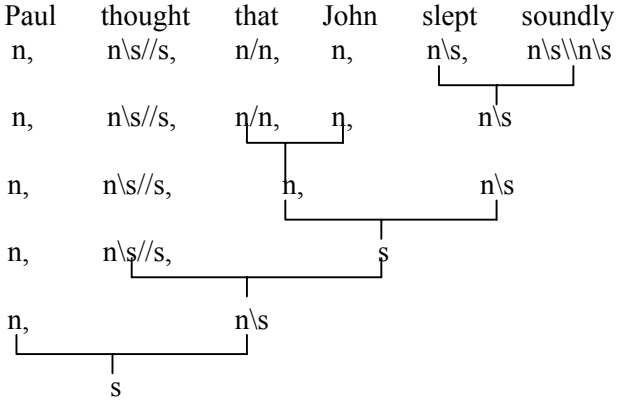
If this structure is regarded as unacceptable, this would prove that either the categorization assumed for this illustration is ill-chosen, or else that the whole model is inadequate for English. I shall not pursue this issue further here, since I shall later on proffer stronger reasons for questioning the adequacy of the model.

As said before, the situation actually is more complicated. An adequate category dictionary would contain in general more than one entry per word. *That*, e.g., is often a nominal, *n*, and even more often an adjectival, *n/n*; *soundly* could as well be an *n\s//n\s* or an *(n\.s)/n//(n\s)/n*, and *thought*, finally, belongs also to categories *n*, *n\s*, *n\s//s* (Paul thought John was asleep) and, as a participle, to still others.

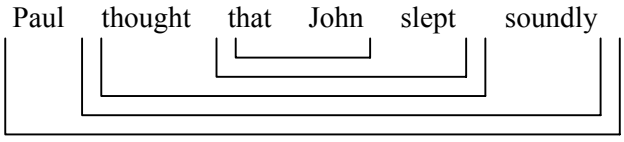
It can nevertheless readily be seen that our method is capable, at least in certain cases, to determine by purely mechanical operations the specific category to which a given word belongs in its given linguistic context. In our example, e.g., listing all the mentioned categories in column form yields the following scheme:

Paul	thought	that	John	slept	soundly
n	n\s//n	n/s	n	n\s	n\s\\n\s
	n	n			n\s//n\s
	n\s	n/n			(n\s)/n//(n\s)/n
	n\s//s				

It would be a tedious but wholly routine exercise to determine that out of the very many derivations corresponding to this word sequence— notice that there are 36 initial lines alone!—there exist only four essentially different ones with a single exponent, namely, in addition to the two above-mentioned derivations, just

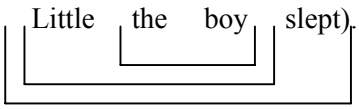


and the one whose parsing diagram would be



I still remember my surprise a few years ago when I discovered that the third constituent structure is doubtless grammatical, however wildly implausible the conditions under which it would be uttered. I was still less pleasantly surprised when I discovered recently that the second and fourth derivations yield constituent structures whose grammaticalness is highly doubtful. The sentence under discussion is *syntactically ambiguous* (or *constructionally homonymous*).

So far, so good then. But, unfortunately, the actual situation is still much more complicated. For categorizing English words, it would be necessary to increase the number of fundamental categories and distinguish various kinds of nominals, for instance, *singular* and *plural*, *animate* and *inanimate*. One would also have to distinguish *declarative sentences*, *question sentences*, etc., these being irreducible to each other under the present model. Some additional notational means would have to be found from which it will follow that *John slept*, *The boy slept*, *Boys slept*, *The boys slept* are well-formed but that *Boy slept*, *The John slept* are not, that *The little boy slept* is connex but not *Little the boy slept* (at least not with the parsing diagram



These and thousands of other additional refinements could perhaps still be introduced without blowing up the whole model. But there are many features which make it highly doubtful whether English grammar—or that of any other natural language, for that matter—can at all be forced into the straitjacket of the immediate constituent model and remain workable and revealing. Since the arguments against such a possibility have already been presented by Chomsky [6], I shall not repeat them here in all their generality but restrict myself to the point of view of machine translation.

It takes but little to realize that the four categories mentioned above for *thought* are far from being exhaustive. In addition to its being a participle, which has already been mentioned, there are such phrases as *thought processes*, *thought thirsty* (not common but definitely grammatical), *thought provoking*, etc. In order to take care of the first two contexts, for example, we would have to assign *thought* also to the categories *n*/*n* and *n*/*n*//*n*/*n*. ("In these contexts, *thought* occurs in the function of an adjective or an adverb, respectively" would have been one traditional way of putting the issue.) The third context would have raised the notoriously difficult problem of the status of the participle present, in addition. The task of preparing a category list that would work for all these and many other contexts is certainly much harder than the first successful analyses caused us to believe. Would not the required list become so long that the mechanical determination of the constituent structure of, say, a 30-word sentence might well require trillions of machine operations, hence be totally impractical for machines of today as well us of tomorrow?

It is likely, for instance, that every assignment to a category of the form α\β//γ (such as the assignment of thought to *n**s*//*s*) will have to be accompanied by an assignment to the category α\\β/γ; an attempt to avoid this through a unique assignment to α\β/γ would amount to a change in the notational framework and would require considerable changes in the cancellation rules.

Still worse, it is not clear whether assigning each word to a finite number of categories only would do at all. Should it be requisite to assign some words to an infinity of categories, then the simple mechanical procedure described above of determining the syntactic structure of a given word sequence breaks down, since there is no longer any assurance that the number of derivations is finite at all.

And what about a sentence such as *Playing cards is fun*? On first sight, it seems that one has to arrive at the category *n* for the phrase *playing cards*. However, it is intuitively clear that this should not be derived from *cards* being an *n* and *playing* being an *n*/*n* (and not only intuitively so: notice that the next word is *is* and not *are*; *playing cards* in our context is a singular nominal). There are, of

course, many other ways of enforcing an assignment of *n* to playing cards, but none of these, to my knowledge, is such that it would not introduce unwarranted and counter-intuitive syntactical resolutions of other sentences. "Hocus-pocus" linguistics—as certain linguistic methods were called whose only purpose was to save certain phenomena, without regard to any intuitive (or psychological) realities—would in our case definitely refute itself by saving also phenomena that are nonexisting.

And what about a sentence like *He looked it up*? We all feel that *looked* and *up* belong together and that in the context *He looked up the table*, at any rate, *up* is an operator that out of an intransitive verbal to its left forms a transitive verbal, hence belongs to the category $n\backslash s\backslash\backslash(n\backslash s)/n$. This assignment indeed works well for *He looked up the table*, but would obviously not do for He looked it up, since there exists no derivation from

$$n, \quad n\backslash s, \quad n, \quad n\backslash s//(n\backslash s)/n$$

with an exponent *s*,

Finally, what about a sentence like *John, unfortunately, was asleep*? *Unfortunately*, in the context *Unfortunately, John was asleep*, is clearly an *s*/*s* but with this category assignment, *John, unfortunately, was asleep* would turn out not to be connex.

If now the present variant of an immediate constituent model is not good enough to serve as a general model for the whole syntax of a given language, the method of mechanical structure determination outlined above can no longer be assumed to be of general validity, either. As a matter of fact, I had already noticed six years ago that the model did not work too well for complex sentences, but had rather hoped that this was due only to lack of refinement that could be partly remedied by increasing the number of fundamental categories, partly by using additional rules. I have now come to realize that its failure in the more complex cases has a much deeper cause: the linguistic model on which this method was based is just not good enough.

The situation is apparently not changed very much by using a more complex model which has recently been proposed by Lambek [7]. Though he uses in addition to the cancellation rules other rules of a different character which may perhaps allow for a reduction in the number of the machine operations required for a test of sentencehood, it is not clear whether Lambek's model is really more powerful than the one outlined above.

Another model, or rather a whole set of models, for linguistic structure has recently been developed, in outline, by Chomsky [8, 9,10]. (A similar conception has been developed also by Harris [11], but since Chomsky's formulations seem to me much clearer, I prefer to refer to his work in the sequel.) They are incomparably more powerful than the phrase-structure models, in all their variants. These so-called *transformational models* do not discard the immediate constituent model but rather supplement it. The former model remains intact for a certain kind of simple sentences, the so-called *kernel sentences* (or rather for their underlying *terminal strings*)—and our method of mechanical structure determination remains therefore valid for these sentences—but has to be supplemented by additional procedures, the so-called *transformations*, in order to account for the synthesis of *all* sentences.

Each sentence, according to the transformational models, is the result of a series of one or more transformations performed one after the other on one or more terminal strings—unless, of course, it is a terminal string itself. A complete analysis, mechanical or otherwise, of a given sentence has to tell us what its basic terminal strings are, together with their constituent structure, and what transformations, and in what order, were performed upon them. Assuming that a complete transformational grammar for some given language has been prepared, the preparation of a corresponding analytical (or operational) grammar is a formidable, though perhaps not necessarily impossible task. There exist here a large number of unsolved problems, partly due to the fact that the nature of the transformations involved have so far been left rather vague, partly to the fact that we find ourselves here within the confines of new and extremely complicated disciplines like recursive function theory, Post canonical systems and the like, the exploration of which has only started. So far, of course, no transformational grammar exists for any language, to any serious degree of completeness.

The recognition that immediate constituent grammars have to be supplemented by

transformational grammars makes the task of mechanizing translation look much harder, but the resulting picture is not at all uniformly black. On the contrary, there are reasons to suppose that the additional insight we get on the basis of this model will not only be of decisive importance for theoretical linguistics, but may well turn out to facilitate the mechanization of translation from new angles.

One gain of the transformational model is similar to, but still more effective and more intuitive than the one obtained by Lambek's model: a reduction in the number of categories to which the words will have to be assigned. No longer will *thought* have to be assigned to the categories *n*/*n* and *n*/*n*//*n*/*n* in order to take care of the connexity of *thought processes* and *thought thirsty*, because sentences containing these phrases are not terminal strings but result from transformations. In addition, the assignment of *thought* to *n**s*//*s* is no longer required, since the sentencehood of *Paul thought John slept soundly* will now be taken care of by our regarding it as the result, of a *that*-omitting transformation on *Paul thought that John slept soundly*, which itself is the result of a certain fusing transformation on the two terminal strings *Paul thought this*. *John slept soundly*, (This description is oversimplified and to that degree misleading. A better description is given in Chomsky's publications. A sufficiently sophisticated treatment would require too much space here.) As a result, the noun *thought* will (perhaps) always be assigned to the syntactical category of nominals, the finite verb *thought* to the syntactic category of transitive verbals, and the participle *thought* to an appropriate syntactical category (with which we shall not bother here), the multiplicity of category assignments to the word *thought* now being considered as exclusively the result of homonymity or homography, as the case may be.

One interesting result of all this will now be that the number of categories of many words will be reduced to—zero. This will happen if no sentence containing these words is regarded as a terminal string. To give an example: *sleeping* will not be assigned to any category, any sentence containing this word being considered as the result of a transformation. (*Interesting*, however, will be assigned to the category *n*/*n*, the difference being—to give only a hint—that *very interesting* is connex but not *very sleeping*.} That there might be words which do not belong to any syntactic category will strike many linguists as rather queer, but I am convinced that on second sight they will realize the enormous advantages of such an attitude; innumerable pseudo-problems have in the past been created by the search for the syntactic category (the traditional term is, of course, "part of speech") of certain words or phrases which — under the new model — just do not belong to any category. This is— if I may be allowed one generalization — just one more instance of the very common class of situations where the attempt of applying a model which is very useful within certain limits lends, when pushed beyond these limits, to pseudo-problems and their pseudo-solutions.

The second gain is somewhat more speculative: it seems likely, but has so far not been seriously tested, that languages will be much more similar with regard to their terminal string structure than with regard to the structure of the totality of their sentences. Word-by-word translation of terminal strings, with some occasional permuting, seems to yield satisfactory results for many pairs of languages, including those for which this kind of translation does not work at all with regard to more complex sentences.

The most remarkable gain, however, would be achieved when it turned out that between the sets of transformations of two languages there existed a close semantic relationship. Should it happen that for certain two languages, L_1 and L_2, there exist two transformations, say t_1 and t_2, such that for any semantically equivalent terminal strings of these languages, k_1 and k_2 , $t_1(k_1)$ is semantically equivalent to $t_2(k_2)$, this would allow for a relatively simple mechanization of the translation, provided, of course, that the syntactic analysis of L_1 has been mechanized, whereas a word-by-word translation of $t_1(k_1)$ into L_2 might be highly unsatisfactory.

Of course, there is but little hope that the sets of transformations of two languages which do not stand in any close genetical relationship will do us the favor of exhibiting isomorphism or near-isomorphism with regard to semantic equivalence. So far, there exists to my knowledge no *general* theory of machine translation which would ensure that, if only the precepts of this theory are followed, the target-language counterpart (or counterparts) of any sentence of a given source-language will be no more and no less syntactically ambiguous than the original sentence itself. Current statements to the contrary seem to me palpably false, and any hope for an imminent establishment of such a theory —

unsubstantiated. Great progress has been made in this respect with regard to certain ordered pairs of languages, such as French-English, German-English, Russian-English, English-Russian, German-Russian, and French-Russian, partly prior to the appearance of the transformational model and without any conscious use of its methods, and more progress may be expected in the future through a conscious use of these methods. As one almost necessary condition for future success I regard the recognition on behalf of the workers on machine translation that the model with which they were working, consciously or unconsciously, during the first decade of their endeavors was too crude and has to be replaced by a much more complex but also much better fitting model of linguistic structure.

References for Appendix II

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Appendix III

A Demonstration of the Nonfeasibility of Fully Automatic High Quality Translation

One of the reasons why we do not as yet have any translation centers, not even in the planning stage, in which electronic computers, general or special purpose, are used to automate certain parts of the translation process, in spite of the fact that such centers would fulfill a vital function in saving a considerable amount of qualified human translator time per document translated, and thereby facilitate more, quicker and, after some time, cheaper translation, is the reluctance of many MT workers to recognize that the idea of inventing a method for fully automatic high quality translation (FAHQT) is just a dream which will not come true in the foreseeable future. By not realizing the practical futility of this aim, whatever its motivational importance for certain types of basic research, they have misled themselves and the agencies which sponsored their research into not being satisfied with a partly automated translation system whose principles are well understood today, and instead to wait for the real thing which was believed, and made to believe, to be just around the corner.

During the past year I have repeatedly tried to point out the illusory character of the FAHQT ideal even in respect to mechanical determination of the syntactical structure of a given source-language sentence (see Appendix II). Here I shall show that there exist extremely simple sentences in English—and the same holds, I am sure, for any other natural language—which, within certain linguistic contexts, would be uniquely (up to plain synonymy) and unambiguously translated into any other language by anyone with a sufficient knowledge of the two languages involved, though I know of no program that would enable a machine to come up with this unique rendering unless by a completely arbitrary and *ad hoc* procedure whose futility would show itself in the next example.

A sentence of this kind is the following:

The box was in the pen.

The linguistic context from which this sentence is taken is, say, the following:

> Little John was looking for his toy box. Finally he found it. The box was in the pen. John was very happy.

Assume, for simplicity's sake, that *pen* in English has only the following two meanings: (1) a certain writing utensil, (2) an enclosure where small children can play. I now claim that no existing or imaginable program will enable an electronic computer to determine that the word *pen* in the given sentence within the given context has the second of the above meanings, whereas every reader with a sufficient knowledge of English will do this "automatically." Incidentally, we realize that the issue is not one that concerns translation proper, i.e., the transition from one language to another, but a preliminary stage of this process, or, the determination of the specific meaning in context of a word which, in isolation, is semantically ambiguous (relative to a given target-language, if one wants to guard oneself against the conceivable though extremely unlikely case that the target-language contains a word denoting both the same writing utensil and an enclosure where children can play).

It is an old prejudice, but nevertheless a prejudice, that taking into consideration a sufficiently large linguistic environment as such will suffice to reduce the semantical ambiguity of a given word. Let me quote from the memorandum which Warren Weaver sent on July 15, 1949 to some two hundred of his acquaintances and which became one of the prime movers of MT research in general and directly initiated the well-known researches of Reifler and Kaplan [1]: "... if ... one can see not only the central word in question, but also say N words on either side, then, if N is large enough one can *unambiguously* [my italics] decide the meaning of the central word. The formal truth of this statement becomes clear when one mentions that the middle word of a whole article or a whole book is unambiguous if one has read the whole article or book, providing of course that the article or book is sufficiently well written to communicate at all." Weaver then goes on to pose the practical question: "What minimum value of N will, at least in a tolerable fraction of cases, lead to the correct choice of meaning for the central word," a question which was, we recall, so successfully answered by Kaplan. But Weaver's seemingly lucid argument is riddled with a fateful fallacy: the argument is doubtless

valid (fortified, as it is, by the escape clause beginning with "providing") but only for *intelligent* readers, for whom the article or book was written to begin with. Weaver himself thought at that time that the argument is valid also for an electronic computer, though he did not say so explicitly in the quoted passage, and on the contrary, used the word "one"; that this is so will be clear to anyone who reads with care the whole section headed "Meaning and Context." In this fallacious transfer Weaver has been followed by almost every author on MT problems, including many Russian ones.

Now, what exactly is going on here? Why is it that a machine, with a memory capacity sufficient to deal with a whole paragraph at a time, and a syntactico-semantic program that goes, if necessary, beyond the boundaries of single sentences up to a whole paragraph (and, for the sake of the argument, up to a whole book)—something which has so far not gotten beyond the barest and vaguest outlines—is still powerless to determine the meaning of *pen* in our sample sentence within the given paragraph? The explanation is extremely simple, and it is nothing short of amazing that, to my knowledge, this point has never been made before, in the context of MT, though it must surely have been made many times in other contexts. What makes an intelligent human reader grasp this meaning so unhesitatingly is, in addition to all the other features that have been discussed by MT workers (Dostert [2], e.g., lists no less than seven of what he calls areas of meaning determination, none of which, however, takes care of our simple example), his *knowledge* that the relative sizes of pens, in the sense of writing implements, toy boxes, and pens, in the sense of playpens, are such that when someone writes under ordinary circumstances and in something like the given context, "The box was in the pen," he almost certainly refers to a playpen and most certainly not to a writing pen. (The occurrence of this sentence in the mentioned paragraph tends to increase the confidence of the reader that the circumstances are ordinary, though the whole paragraph could, of course, still have formed part of a larger fairy tale, or of some dream story, etc.) This knowledge is not at the disposal of the electronic computer and none of the dictionaries or programs for the elimination of polysemy puts this knowledge at its disposal.

Whenever I offered this argument to one of my colleagues working on MT, their first reaction was: "But why not envisage a system which will put this knowledge at the disposal of the translation machine?" Understandable as this reaction is, it is very easy to show its futility. What such a suggestion amounts to, if taken seriously, is the requirement that a translation machine should not only be supplied with a dictionary but also with a universal encyclopedia. This is surely utterly chimerical and hardly deserves any further discussion. Since, however, the idea of a machine with encyclopedic knowledge has popped up also on other occasions, let me add a few words on this topic. The number of facts we human beings know is, in a certain very pregnant sense, infinite. Knowing, for instance, that at a certain moment there are exactly eight chairs in a certain room, we also know that there are more than five chairs, less than 9, 10, 11, 12, and so on *ad infinitum*, chairs in that room. We know all these additional facts by inferences which we are able to perform, at least in this particular case, instantaneously, and it is clear that they are not, in any serious sense, stored in our memory. Though one could envisage that a machine would be capable of performing the same inferences, there exists so far no serious proposal for a scheme that would make a machine perform such inferences in the same or similar circumstances under which an intelligent human being would perform them. Though a lot of thought should surely be given to the problems which could only be touched slightly here, it would very definitely mean putting the horse before the cart if practical MT would have to wait for their solution. These problems are clearly many orders of magnitude more difficult than the problem of establishing practical machine aids to translation. I believe that it is of decisive importance to get a clear view of this whole issue and hope that my remarks will contribute to its clarification.

I have, no idea how often sentences of the mentioned kind, whose ambiguity is resolvable only on the basis of extra-linguistic knowledge which cannot be presumed to be at the disposal of a computer, occur on the average in the various types of documents in whose translation one might be interested. I am quite ready to assume that they would occur rather infrequently in certain scientific texts. I am ready to admit that none might occur on a whole page or even in some whole article. But so long as they will occur *sometimes*, a translation outfit that will claim that its output is of a quality comparable to that of a qualified human translator will have to use a post-editor, and this not only for polishing up purposes, contrary to what even so acute and impartial an observer as Warren Weaver was still hoping for in 1955 [3]. As soon as this is granted, the greatest obstacle to practical MT has

been overcome, and the way in free for an unprejudiced discussion of the best human use of the human partner in the translation outfit.

Having shown, I hope, that FAHQT is out of the question for the foreseeable future because of the existence of a large number of sentences the determination of whose meaning, unambiguous for a human reader, is beyond the reach of machines, let me now discuss this issue of reduction of semantical ambiguity a little further. There exist in the main two methods of reducing semantical ambiguity. One is the use of idioglossaries, the other is the already mentioned method of utilizing the immediate linguistic environment of the word which is ambiguous in isolation. Though some doubts have been raised on occasion as to the validity of the first of these methods, I do not know of any serious attempt to put its validity to test. At this point I would only like to stress the vital necessity of performing such tests before an MT method based upon the utilization of idioglossaries is claimed to yield high quality translations, even in collaboration with a post-editor. It is just the great effectiveness of the use of idioglossaries in general which is apt to yield disastrously wrong translations on occasion without giving the post-editor even a chance to correct these mistakes. It is just because a certain Russian word in a chemical paper will *almost always* have a certain specific English rendering that the danger is so great that in those exceptional cases where this word, for some reason or other, will have a different meaning, this exception will not be taken into account, yielding a meaningful but wrong translation.

In regard to the second method, the situation is even worse, and has lately become even more confused through the use of certain slogan terms like "thesaurus" in this connection. (Notice, e.g., that the very same— fictitious!—thesaurus approach for English-to-French translation that would correctly render *pen* by "plume" in the sentence *The pen was in the inkstand* would incorrectly render *pen* by "plume" in the sentence *The inkstand was in the pen*.) It is undoubtedly true that consideration of the immediate linguistic neighborhood of a given ambiguous word is a very powerful method, but it is again necessary to realize its limitations. I am referring no longer to those limitations which I pointed out through the use of my sample sentence, but rather to the fact that many MT workers seem to underestimate the importance of those cases of reduction of polysemy which cannot be obtained by looking at the immediate neighborhood, and even more so about the fact that partial successes in this direction have led many people to underestimate the depth of the remaining gap. Let me state rather dogmatically that there exists at this moment no method of reducing the polysemy of the, say, twenty words of an average Russian sentence in a scientific article below a remainder of, I would estimate, at least five or six words with multiple English renderings, which would not seriously endanger the quality of the machine output. It is looking at the quantities involved which creates a distorted picture with many people. Many tend to believe that by reducing the number of initially possible renderings of a twenty word Russian sentence from a few tens of thousands (which is the approximate number resulting from the assumption that each of the twenty Russian words has two renderings on the average, while seven or eight of them have only one rendering) to some eighty (which would be the number of renderings on the assumption that sixteen words are uniquely rendered and four have three renderings apiece, forgetting now about all the other aspects such as change of word order, etc.) the main bulk of this kind of work has been achieved, the remainder requiring only some slight additional effort. We have before us another case of what, in a superficially different but intrinsically very similar situation, has been called the "80% fallacy" [4]. The remaining 20% will require not one quarter of the effort spent for the first 80%, but many, many times this effort, with a few percent remaining beyond the reach of every conceivable effort.

References for Appendix III

1. This memorandum is reprinted as Chapter 1 of *Machine Translation of Languages* (W. N. Locke and A. D. Booth, eds.), Wiley, New York, 1955. The quoted passage appears there on page 21. For Reifler's and Kaplan's studies, see p. 227 of the same volume.
2. Dostert, L. E., The Georgetown-IBM experiment, in *Machine Translation of Languages* (W. N. Locke and A. D. Booth, eds.), Chapter 8, especially pp. 129ff.
3. In *Machine Translation of Languages* (W. N. Locke and A. D. Booth, eds.), p. vii.
4. Bull, W. E., Africa, C., and Teichroew, D., Some Problems of the "word," in *Machine Translation of Languages* (W. N. Locke and A. D. Booth, eds.), Chapter 5, p. 98.