Looking back to 1952: the first MT conference

John Hutchins
(University of East Anglia, Norwich NR4 7TJ, UK)

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Abstract

In a review of the proceedings of the first MT conference, held at MIT in June 1952, it is found that the principal issues discussed have continued to be the focus of MT research to the present day, despite the substantial computational and linguistic advances since the early 1950s.

1. Introduction

Just five years after Warren Weaver first suggested the possibility of machine translation (MT), and no more than three years after his memorandum in July 1949, which effectively launched research in the field [Weaver 1949], the first conference devoted to the topic was convened at the Massachusetts Institute of Technology from 17 to 20 June 1952.

It was organised by Yehoshua Bar-Hillel, who had been appointed at MIT to the first full-time post in MT – not as a researcher, as he was later to stress, but in order to review the prospects and to make recommendations. In 1951 Bar-Hillel visited all the US sites which had embarked on some kind of MT activity, and wrote a 'state-of-the-art' paper, which was to form the background information for the conference [Bar-Hillel 1951].

At this date very little had been written on MT at all, and Bar-Hillel's paper was the first general overview. Weaver's memorandum had stimulated Erwin Reifler, a Chinese specialist at the University of Washington (Seattle), who circulated a couple of papers proposing the use of human editors before and after the translation process. It had also prompted Abraham Kaplan at the Rand Corporation to do some statistical investigations [Kaplan 1950], and Victor Oswald of UCLA and Stuart Fletcher of the National Bureau of Standards (NBS) Institute of Numerical Analysis to look at problems of syntax – which resulted in the first journal publication [Oswald and Fletcher 1951]. Finally, there had been the British research of Andrew Booth and Richard Richens, which pre-dated Weaver's paper but which was not written up until the 1952 conference, and not printed until later [Richens & Booth 1955].

At this time MT was generally known as 'mechanical translation'. It was probably a more apt description since no computer had actually been programmed yet to do any kind of translation. The ideas of Booth and Richens had been implemented only on punched card equipment. Kaplan's research had been a purely statistical background investigation on contextual disambiguation. Oswald and Fletcher's proposals on German syntax were designed for the NBS SWAC computer at Los Angeles, although no programs for translation were implemented. As for Reifler at Seattle, his were purely theoretical reflections.

Apart from Bar-Hillel, others from MIT at the conference included: Jerome B.Wiesner (Research Laboratory of Electronics), Jay W.Forrester (Digital Computing Laboratory), James W.Perry (Center of International Studies), William N.Locke (Dept. of Modern Languages), Vernon Tate (Director of Libraries) and Dudley A.Buck (Dept. of Electrical Engineering). Elsewhere from the US were Olaf Helmer (Rand Corporation), Harry D.Huskey (NBS Institute for Numerical Analysis), William E.Bull and Victor A.Oswald (both UCLA), Erwin Reifler and Stuart Dodd (both University of Washington, Seattle), Duncan Harkin (Department of Defense), A.Craig Reynolds (IBM Endicott Laboratories), Leon Dostert (Georgetown University) – invited for his experience in setting up simultaneous interpretation services at the Nuremburg trials and at the United Nations – and Victor H. Yngve (University of Chicago), who was later to succeed Bar-Hillel at MIT and lead its MT research group. The only non-American at the conference was Andrew D.Booth (Birkbeck College, London).

The proceedings of the conference were not published. It was intended that the collection edited by [Locke & Booth 1955] would contain all the papers, probably in revised form. In the event, only two of the papers correspond to those given at the conference. Fortunately some other papers are available from the MIT Library, and there are two reports by Craig Reynolds and by Erwin Reifler [Reynolds 1954; Reifler 1954]. (For fuller details of the background and for specific references see [Hutchins 1997].)

2. Pre-editing

The first proposals for pre- and post-editing were made by Erwin Reifler in early 1950. The reason was simple: mechanical processing was seen as exclusively one-for-one substitution, and therefore all that could be expected of a computer was word-for-word translation. To produce anything readable, someone would have to have available in the target language all the possible equivalents for each source text word and select the appropriate version. Reifler saw this as an intolerable burden on the 'post-editor' and so he proposed a 'pre-editor' working entirely in his own language. The goal of pre-editing [Reifler 1952a] was "a graphic supplementation of the conventional form of the foreign message which raises its graphic-semantic explicitness to the level necessary for a mechanical translation." The pre-editor would deal not only with "morphological and syntactical ambiguities" but also with "the rearrangement of the FL [i.e. source] text in accordance with a standard order in the TL following a set of instructions available to him in his own language."

However, Reifler thought that these solutions did not go far enough: "The burden on the supply side is too great, the extent of human intervention too large." Therefore, he put forward various ideas to support pre-editing. The first was a mechanism to assist pre-editors in the insertion of codes:

> When the pre-editor dials the conventional graphic form of the foreign message into the translation mechanism, it would first pass through the mechanical dictionary. Whenever in terms of the target language no multiple meanings are involved, the dictionary mechanism would not intervene and the dialled material would move on to the next stage in the translation process. Otherwise a device would call the attention of the pre-editor to the fact that multiple meanings are involved and the dictionary entry concerned would appear on a screen. The pre-editor would then select the meaning required by the context and dial the distinctive graphic symbol representative of this meaning and supplied by the dictionary. [Reifler 1952a]

The idea can be seen as an anticipation of procedures for human-computer interactive analysis in NLP, e.g. in the MIND system [Kay 1973], and in later MT systems.

His other ideas were more radical. First, he suggested the use of a regularised language which "people desirous of a MT" should or could write in. Second, he thought that authors could insert pre-editing codes themselves by referring to special monolingual dictionaries containing symbols which distinguished homographs. (As an aside, [Reynolds 1954] thought it most likely that this work would be done by secretaries.) As a further (even more eccentric) proposal he proposed a new orthography for all languages which would distinguish grammatical categories: "all nouns would have... a capital first letter..., all principal verbs ... a capital second letter and all attributive adjectives ... a capital third letter..."; so that, for example, the German *er hegt die fromme Hoffnung* would be written "er hEgt die frOmme Hoffnung"

It was perhaps the eccentricity of Reifler's pre-editing proposals which deflected researchers at the time and in the immediately following years from more serious consideration of pre-editing options. Otherwise, it would not have taken as long as it did before researchers proposed the interactive composition of source texts – or 'dialogue-based MT' as it is now often called – or the use of 'normalized' language for controlled input.

3. Post-editing

Bar-Hillel's view of pre-editing was that it was not realistic to expect anyone to be able to anticipate all problems of translating into another language. He did not think Reifler's proposals were feasible; indeed if the TL was unknown to a post-editor, he thought that the process as conceived by Reifler would be impossible. He believed that the functions of pre-editing could be virtually eliminated by mechanical analysis. Then, only a post-editor would be necessary.

During the conference, Bar-Hillel gave a paper entitled "MT with post-editor" [Bar-Hillel 1952b], which has unfortunately not been preserved. From the accounts by Reifler and Reynolds, however, it is clear that he repeated views which he had expressed the previous year [Bar-Hillel 1951]. His starting point was initial alarm at the storage implications if the post-editor was to be presented with all alternatives. "For a particular sentence, say of 10 words length, this can easily result in possible combinations of words in the target language extending to several thousands of more or less meaningful combinations" (as cited by [Reynolds 1954]).

However, he came to the conclusion that if source language ambiguities were resolvable by the statistical context technique proposed by Kaplan [Kaplan 1950] – essentially involving the examination of digrams and trigrams – and if his own proposals for syntactic analysis were to be found effective (see sect. 9 below), then the post-editing option was feasible. As he put it [Bar-Hillel 1951]:

> If one takes into account the fact that the post-editor will receive instructions, in his own language, for handling certain strange-looking combinations, that certain words with many possible translations might reoccur in the passage quite frequently in this same meaning so that time-consuming decisions will not have to be repeated, and so on, it should be clear that the burden on the post-editor will not be too heavy.

While stressing that the post-editor must know the target language and the topic, Bar-Hillel believed "he need not understand the source language!" [Bar-Hillel 1952a]. From our present perspective, this last assumption has been shown to be mistaken. We know now that if good quality is to be achieved by post-editing then the reviser must be able to check the original for the meaning intended by the author. Bar-Hillel was writing, of course, before any MT systems were producing output. All we can accuse him of is being excessively optimistic in the complete absence of concrete evidence.

4. Idiomatic expressions

Among the "strange-looking combinations" to be handled by the post-editor, Bar-Hillel would probably have included idiomatic expressions which had been rendered by literal translations. In another paper at the conference [Bar-Hillel 1952c], he considered various methods of dealing with an 'idiomatic' phrase such as the German *es gibt...* which should be treated as a unit with a single TL equivalent (*there is/are*), as well as being translated literally as *he gives*, *she gives* or *it gives* according to context.

In his first method, the dictionary would include *there* as an additional 'correlate' of *es*, and *is* and *are* as additional correlates of *gibt*. The problem was that, in his own words, "it works too well", generating not just the correct forms *there is/are* and *he/she/it gives* but also *he/she/it is*. As a result, the fully intelligible *she is a doll* would be given, incorrectly, as a possible translation of *es gibt eine Puppe*. As a second method, Bar-Hillel proposed therefore to supplement the word dictionary with a phrase dictionary which would include *there is (are)* as a translation of *es gibt.* For this method to work, he noted that certain grammatical rules would have to be applied before the phrase

dictionary were invoked, e.g. to deal with questions such as *gibt es einen Unterschied?* (These are the first suggestions that collocations might be treated as translation units, and that syntax and lexicon should interact in some kind of rule sequencing.)

He then remarks that the occurrence of an entry in the phrase dictionary should not exclude word for word translation. The 'idiomatic' version should be offered to the post-editor as an alternative. It would be the task of the post-editor to recognise the contexts when *he/she/it gives...* should be replaced by *there is* or *there are*. The major drawback Bar-Hillel saw was the increased sizes of dictionaries; he had no idea how many entries would have to be included in a phrase dictionary. In general, he preferred analytical approaches: arguing that, just because *fair play* could legitimately be regarded as an idiom does not mean that all collocations involving *fair* would have to be included.

As a third method, Bar-Hillel put the whole burden of recognising idiomatic usage onto the post-editor. There would be no phrase dictionary, and all 'idioms' would be translated literally word for word. The post-editor or reader would have to be told, or would have to learn, and would have to remember that the raw translation *it (he, she) gives* might be replaced in some contexts by *there is (are)*. The problem would be that a monolingual reader with no knowledge of the source or its context would not know whether the substitution could apply or not.

For Bar-Hillel the problems illustrated that the "task of finding a good combination of the mechanical methods (and perhaps others), either for human or machine translation, should prove to be interesting not only for [MT] but also for the theoretical linguist." In essence, he was correctly anticipating that the treatment of semantic collocations and contextual disambiguation would represent a major focus of future MT activity, with ramifications for linguistics and natural language processing in general.

However, his chief fear was that idiom dictionaries could never be large enough. Even with specialist vocabulary where one-to-one equivalents would be the norm, he thought that there would have to be 'idiomatic' entries:

> The main danger in not having sufficient idioms in a phrase-dictionary is not in the fact that some literal translations would be jibberish, it lies in the fact that some of these translations will make sense but the wrong sense and the post-editor will be unable to find this out.

Although the argument has often been repeated to the present day – usually by opponents of MT – it has been found, in post-editing practice, to be less of a danger than imagined.

5. Controlled language

Just as Reifler's ideas on pre-editing were looked at with scepticism, so too was his advocacy of 'model languages'. He saw these as having two possible roles in aiding MT. One was the idea of writing source texts in what would now be called a 'controlled language'. The other was the notion that output texts could be in a special kind of 'pidgin' language.

At the conference, Stuart Dodd (also from the University of Washington, Seattle) proposed a simplified form of English [Dodd 1952]. As Dodd put it, this was a "standardization of English syntax as a means of simplifying the use of English either as a source language or as a target language". It involved the regularising of verb forms, e.g. writing *She did be loved* instead of *She was loved*; the use of only nominative forms of pronouns, e.g. using *I will send he to she*; the standardisation of word order, e.g. adverbs always before verbs, direct objects before indirect; and, of course, the use of words in one (most common) meaning only, e.g. *tank* to mean only water tank, or the obligatory use of qualifiers, e.g. always *army tank*. Although extreme, Dodd's model English can be seen as a direct ancestor of the 'simplified English' used at Xerox in the 1970s [Elliston 1979], of the 'Perkins Approved Clear English' in the 1980s [Pym 1990], of the 'Caterpillar Technical English' in the 1990s [Hayes et al. 1996].

Reifler anticipated this use of regularised language in source texts. However, he advocated also the use of simplified language in MT output. Evidently, he wanted to reduce the burden on the post-editor who would have to rearrange and rephrase the word-for-word translations. If there were a simplified target language vocabulary with unique one-for-one equivalents to be generated, then it would be possible to "either restrict post-editorial interpretation to a minimum, or it may even make it completely superfluous." [Reifler 1952a]. The suggestion was made, in fact, in conjunction with his ideas on a 'universal grammar'.

6. Universals

In his 1951 paper Bar-Hillel stated that MT into more than one target language ('general MT', as distinct from translation involving just one SL and one TL: 'specific MT') would necessarily "require the establishment of a universal, or at least general, grammar, perhaps even the construction of a whole artificial language." Bar-Hillel knew of past failures at making universal languages, but he did believe that "mathematical logic, and modern structural linguistics" may lead to greater success [Bar-Hillel 1951].

Reifler saw the question as involving "one and the same preparation of the code text" for multiple target translation [Reifler 1952b]. Agreeing with Bar-Hillel that this meant some kind of 'universal grammar', he held that comparative-historical linguistics could help to identify real universals. But, in addition, he thought there may also be 'pseudo-universals' derived from grammatical analyses common to more than one language:

> by arbitrarily attributing grammatical meanings to linguistic forms which they, in fact, do not have, namely by changing the structure of a language, we may, for instance, within the limitations of intelligibility, so modify the grammar of a language as to bring it more in line with the grammar of other languages.

For example, the Mandarin Chinese version of English *he walks quickly* is *t'a*^{1} *tsou*^{3}*-ti k'uai*^{4}, where *k'uai*^{4} corresponds to something like "to be quickness" or "to be quick" and *tsou*^{3}*-ti* corresponds to "walk's" or "of walk"; so a literal translation might be "he is quickness of walk". But in other contexts *tsou*^{3}*-ti* can be freely translated as "walking"; therefore, Reifler argued, we could make an arbitrary equation of *-ti* and English *-ing*:

> We may therefore render the Mandarin sentence by "he walk-ing quick". This is bad English, but perfectly intelligible and, because it permits a word-to-word translation, has the great advantage of simplifying the mechanical correlation problem. [Reifler 1952b]

In fact, the idea of a 'pidgin' translation (which is effectively Reifler's proposal) had been made earlier in 1949 by Richens, and it was to receive more extensive consideration by the Cambridge Language Research Unit [Masterman & Kay 1960]. There are similarities in the avoidance of problems with articles and prepositions, the offering of single TL equivalents (no alternatives), the adherence to the word order of the original, and the inclusion of constructed symbols (the CLRU authors proposed 'pidgin variables' such as (W)THAT, and grammatical markers such as -ISH and -WARD for adjectives and adverbs). However, the CLRU ideas went further than Reifler in proposing an 'improvement' program to amend TL output (via a thesaurus), to insert articles, and to change word order.

Reifler's notion of aligning grammars was, in fact, close to the contemporary ideas of Zellig Harris on transfer grammar [Harris 1954]. Harris proposed rules for generating the phonology, morphology and syntactic structures of one language from the utterances of another language. The rules were derived from an examination of the differences between two languages after they have both been defined according to a common set of definitional categories and classes. The proposal was clearly related to his formalisation of grammatical transformations within the same language, although he stressed explicitly the differences between the two types of transformation.

The proposal can be related also to the much later ideas of Jan Landsbergen [Landsbergen 1987], who proposed isomorphic grammars in his interlingua-based approach to MT, where grammar rules for source and target languages were "not developed independently of each other but [were] attuned to each other" with respect to their equivalent meanings and functions.

7. Sublanguages

It is widely assumed that the notion of 'sublanguage' arose first in the MT context in relation to the research in the 1960s and 1970s at the University of Montreal, particularly for the Meteo system for translating weather forecasts from English into French.

In his 1951 survey, Bar-Hillel mentioned the use of restricted languages, such as those employed by aircraft pilots, as a potential fruitful area of application for MT – an idea which, of course, continues to the present time [e.g. Johnson 1996]. At the MIT conference, Victor Oswald and William Bull (both from UCLA) demonstrated that within a narrowly-definable subject area the semantic range of the vocabulary was restricted so that potentially ambiguous words were used in one sense only. Oswald referred to the 'microsemantics' of a subject; in his case, the vocabulary of brain surgery.

As a result of his research, Victor Oswald proposed micro-glossaries as one means of overcoming the need for pre-editors, post-editors and a battery of subject experts in the domains covered by translated material [Oswald 1952b]:

> an alternative arrangement is possible: to replace the battery of specialists by a series of permanent micro-glossaries, each of which would provide no more than two-to-one, and a preponderance of one-to-one, TL equivalents.

The 'sublanguage' vocabulary (as it would now be called) was to be identified by statistical analysis of corpora on the basis of the observation that

> the data of all frequency counts fall into the same pattern, which means that a frequency count of any micro-segment of any language – say the nouns in German contexts pertaining to brain surgery – should give a parabolic curve where high-frequency elements ought to dispose of eighty-percent of all running nouns.

Not only was this found to be true, and that familiarity with 80% of the technical words for any article was alone enough to make sense of the article, but Oswald found a similar frequency distribution for the non-technical words:

> In other words, brain surgeons writing on brain surgery are not only compelled to choose their technical nouns from a limited vocabulary, but their patterns of communication are so limited by practice and convention that even the range of non-technical nouns is predictable.

However, although encouraged by these findings, Oswald was cautious about micro-glossaries, since "[t]heir ultimate efficiency remains untested, however, and it is possible that it might be prohibitively expensive to produce them."

As we now know, it is indeed the case that sublanguages are not in themselves a solution to MT semantics. In very few subject areas are texts written strictly within a single sublanguage. In most cases, texts range across many domains, each with their own lexicon; but the identification of transitions from sublanguage to sublanguage has proved difficult. Consequently, most MT systems attempt to identify specific meanings from contextual clues.

8. Statistics

There was a suspicion that sublanguages would not be the complete answer to semantic problems. While agreeing with Oswald that micro-glossaries could reduce problems of ambiguity, William E.Bull was even more sceptical about the value of frequency analyses for constructing micro-glossaries [Bull 1952, quoted by Reifler 1954]:

> There exists no scientific method of establishing a limited vocabulary which will translate any predictable percentage of the content (not the volume) of heterogeneous material... A micro-vocabulary appears feasible only if one is

> dealing with a micro-subject, a field in which the number of objective entities and the number of possible actions are extremely limited. The number of such fields is, probably, insignificant...

And Bull went on to pinpoint a fundamental problem for all MT systems to the present day:

> The limitations of machine translation which we must face are, vocabularywise, the inadequacy of a closed and rigid system operating as the medium of translation with an ever-expanding, open continuum.

This inherent limitation of MT dictionaries was not to be widely recognised until they began to be used in actual operational situations in the middle of the next decade. Since then, dictionary maintenance is recognised as a major factor in the economics of MT usage.

Nevertheless, all participants were encouraged and wished for more statistical analyses of language. It was not only Oswald's work on micro-semantics but also Kaplan's findings on the potential of context for disambiguation that had demonstrated the value of the statistical approach. More particularly, traditional linguistics had clearly not provided MT researchers with what they needed for dealing with vocabulary and syntax; it was believed that statistical data from real texts could fill the gap. But Reynolds made an interesting comment reflecting the state of the technology: "A discussion of the means required ... showed clearly that the analysis could be facilitated by the use of punched cards" [Reynolds 1954]. It seems the possibility of using computers for statistical analysis of language did not occur to participants!

9. Grammatical analysis

Bar-Hillel was convinced that no advance beyond inadequate word-for-word renditions would be possible without syntactic analysis. He argued for the development of 'operational grammars' to identify and disambiguate grammatical categories, and to analyze syntactic structures [Bar-Hillel 1952d].

There were, he believed, good foundations to be found in methods used for the teaching of languages and in the already published studies of German syntax by Oswald and Fletcher [Oswald and Fletcher 1951]). At the conference, Oswald described how 'syntactic blocks', i.e. noun and verb phrases, could be identified on the basis of 'boundary markers': punctuation marks, articles and nouns, finite verb forms, participle forms, adverbs, relative pronouns, etc. [Oswald 1952a]. In essence, Oswald was putting forward a type of 'constituency analysis', which was beginning to become familiar to linguists through the work of Zellig Harris and Rulon Wells [Harris 1946; Wells 1947]. What was new, of course, was that the methods were formulated, as far as possible, in terms of 'instructions' for a computer, specifically for the SWAC computer at Los Angeles – although they were not in fact implemented. Bar-Hillel saw Oswald and Fletcher's formalisation as a valuable first step, but as lacking the specification of a programmable sequences of instructions.

Bar-Hillel envisaged a semi-automatic analysis of a text corpus for identifying grammatical categories and phrase structures (but gave no suggestions for computer processing.) He introduced [Bar-Hillel 1952d] what was later to be called categorial grammar, based on the work of the Polish logician Kazimierz Ajdukiewicz [Ajdukiewicz 1935] – the formalism had earlier been briefly described in [Bar-Hillel 1951]. Grammatical categories are combinations of basic categories *n* and *s*, defined in terms of potentiality to combine with other categories, e.g. an intransitive verb is defined as *s/(n)*, because it can combine with a noun (*n*) to its left to form a sentence (*s*); a transitive verb is defined as one combining with a noun to the left and either a noun or a phrase (sentence) to the right: *s/(n)[n]* or *s/(n)[s]*; etc. The approach could deal with the nominal and verbal 'blocks' in the analyses of Oswald and Fletcher. However, he admitted that the "word-category-list" would have "for English... some million and a half entries [and] The preparation of such a list is certainly not a simple task, since all possible occurrences of these words in all kinds of syntactic construction have to be envisaged."

In later years, categorial grammar was not as popular as more traditional constituency grammars and the application of transformations of the kind proposed by Harris [e.g. Harris 1957] – Chomsky's transformational grammar was much less popular. However, since the mid 1980s there has been a revival of interest in categorial grammar [Wood 1993], primarily in a 'unification grammar' framework, with potential application in MT research.

Bar-Hillel was confident that "a linguist with a staff of a few assistants and clerks should be able to provide [an operational system] for any language that has already been more or less exhaustively described – like English, German, or Russian – within a year or two." This was the kind of optimism to be found among all conference participants; the complexities of formalising language for computer applications were grossly underestimated.

10. Pivot language

At the end of the conference, Leon Dostert (Georgetown University, Washington, D.C.) suggested that "general MT (mechanical translation from one into *many* languages)... should be so developed that one translates first from the input language into one 'pivot' language (which in our case will, most likely, be English) and from that pivot language into any one of the output languages desired" (quoted by [Reifler 1954]). While Dostert thought a natural language could be the pivot (or interlingua), other possibilities mentioned briefly during the conference were international auxiliary languages such as Esperanto, and simplified languages such as Basic English and Dodd's model English.

At the time it is clear that no direct link was made between a 'pivot' language and a 'universal' language – i.e. it was not assumed that an interlingua must be language-independent, although Bar-Hillel and Reifler thought a 'universal language' (as suggested by [Weaver 1949]) might be necessary for 'general MT' (sect. 6 above). Discussion of universals in 1952 was in terms of elements which might assist analysis or transfer between languages of dissimilar structures. The source of such universals was, in the case

of Bar-Hillel, the work on logical syntax by Carnap, Ajdukiewicz, Reichenbach and others – it would appear that Bar-Hillel conceived his categorial grammar as simply a useful 'general' method of analysis; certainly there was no suggestion of a 'universal' syntactic representation and nothing corresponding to Chomsky's later idea of deep syntax. In the case of Reifler, the sources for his universals was the traditional work on comparative and historical linguistics, which suggested 'substantive' universals (such as the Chinese *ti* and English *-ing* correspondence) rather than any more abstract structural or 'formal' universals.

11. Modest aims

The main lessons to be learnt from the 1952 conference are not perhaps the various 'anticipations' of later ideas, but the down-to-earth pragmatism and realistic objectives of participants. In the introductory public session, Bar-Hillel stressed the limitations of MT. He was anxious not to raise expectations too highly:

> completely automatic and autonomous mechanical translation with unique correlates to the original text is, in general, practically excluded, even with respect to scientific texts... This being so, machine translation means no more than *mechanical aids to translation*. Only some kind of *brain-machine partnership* is envisaged. [Bar-Hillel 1952a]

But he stressed also the importance of even these relatively modest aims:

> Even if it should turn out that none of the possible machine-brain partnerships would be more effective than a human translator, in the sense that they will be neither quicker nor cheaper nor more exact than the human translator, under conditions existing today or in the near future, I would strongly advocate a continuation of this research. Electronic machines will doubtless become cheaper, human brains probably more expensive. A partnership that could not stand free competition today may well outbid its human competitors in some not too remote future.

These views were apparently shared by all other participants. None imagined fully automatic translation of near-human quality; they knew that MT output was going to remain poor into the distant future, that post-editing would be essential. On the other hand, they recognised that some problems could be eased by pre-editing or by the regularisation of input texts; that statistical data on language could be valuable; that disambiguation could be helped by the identification of sublanguages; and that there were regularities of syntax which could be usefully applied. But however encouraging, these hopeful signs did not constitute solutions.

At the end of the conference, the participants discussed the next steps. Leon Dostert proposed the "early creation of a pilot machine ... proving to the world not only the possibility but also the practicality of MT." On returning to Georgetown University, Dostert set up the collaboration with IBM that was to lead to the first MT demonstration in January 1954. Previously, MT had been only simulated manually or with punched

cards; the IBM-Georgetown system was a genuine implementation of MT on a computer, and despite its limitations, it was to be a major catalyst for MT research in the following decade.

In later years, partly as a result of the impressive output achieved by this 'pilot' system, much of the MT research in the United States and elsewhere was directed implicitly (and often explicitly) towards the construction of 'perfect' general-purpose systems with near-human quality output. The pragmatism and realism of the 1952 conference was lost sight of in an enthusiasm for powerful linguistic and computing techniques. During the 1960s there was much unfounded optimism and expectations of imminent breakthroughs, most of which came to naught [Hutchins 1986]. In a number of respects it was not until the mid 1980s that the majority of MT researchers began once more to see that computers should not be regarded as 'replacements' of human translators but should be used as tools to aid translators and others in a wide variety of contexts and practical tasks.

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[1] Since 1997 a number of the 1952 conference papers have been traced. They will be found on the Machine Translation Archive at http://www.mt-archive.info/MIT-1952-TOC.htm

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