# FIRST STEPS IN MECHANICAL TRANSLATION

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**Abstract**

Although the first ideas for mechanical translation were made in the seventeenth century, it was not until this century that means became available for realization with the appearance of the electronic computer in the mid 1940s. Fifty years ago, in March 1947 Warren Weaver wrote to Norbert Wiener and met Andrew Booth, mentioning to both the use of computers for translation. The possibilities were investigated during the next seven years, until in January 1954 the first prototype program was demonstrated. This article is a brief chronicle of these early years of mechanizing translation processes.

## 1. The beginnings to 1947

The idea of mechanizing translation can be traced back to the seventeenth century, almost always in association with the idea of inventing a universal language. In the previous century Francis Bacon had stressed the inadequacies of natural languages for the description of scientific discoveries. By the early seventeenth century the need was more widely felt, and the rapid growth of international trade throughout the world and the decline of knowledge of Latin made the dream of a universal language even more desirable. As a result many proposals for 'universal characters' were put forward; the best known being that of John Wilkins in 1668 for the newly founded Royal Society of London. These schemes for universal characters or 'common writing' were invariably furnished with dictionaries from a vernacular language or from Latin, which were intended to be applied by users in a 'mechanical' fashion. This was indeed been the case with a scheme published in 1661 by a German chemist Johann Joachim Becher, which at the height of enthusiasm for MT in the 1960s was re-published as an form of pre-computer program for machine translation.

However, it was not until the twentieth century that the means became available to develop practical devices for mechanizing translation. In 1933 two inventions were patented independently. One was in France by Georges Artsrouni, the other in Russia by Petr Troyanskii. In both cases, the patents were essentially electrical devices for mechanical dictionaries (Hutchins 1993). Artsrouni's invention was exhibited in 1937 at the World Exhibition in Paris, and attracted much attention. The device comprised four components: a 'memory' of dictionary entries on a paper tape, a keyboard for entering words, a search mechanism, and a means of displaying results in a series of windows on the keyboard.

While Artsrouni's invention was no more than a mechanical dictionary, Troyanskii went further. His device consisted of a mechanically operated table containing a multilingual 'glossary field' by which words of the source language were presented through an aperture and equivalent words *of* a target language were printed out. The words appeared not in full forms but as stems, and Troyanskii devised a set of 'logical symbols' common to all languages for expressing grammatical relationships. Troyanskii envisaged translation as a

three-stage process. A human 'editor' would prepare the source text by reducing words to stems and by identifying the relevant 'logical' relationships. The prepared text would then be converted wholly mechanically by his device into an equivalent sequence of words in a target language, while retaining the logical symbols. A human 'editor' would then convert the string of stems and symbols into the target language.

In subsequent years Troyanskii worked on various technical improvements and at some point suggested that the processes of analysis might also be mechanized. However, he clearly underestimated the amount of work involved in the analysis of texts into stems and logical symbols, he minimized the problems of homonyms and lexical differences , and he seemed to be quite unaware of the need for syntactic restructuring. Nevertheless, there is no doubt that had he lived to see the first computers, Troyanskii would have been one of the pioneers of machine translation.

## 2. March 1947

These precursors were, however, quite unknown in the mid 1940s, when the first electronic computers were being developed and when the first suggestions were made to apply their formidable powers to the task of translating languages. (A fuller account of the MT pioneer years is given in Hutchins (1997), where details of all the sources cited here will also be found.)

We can therefore date the true beginning of machine translation (i.e. the application of computers to translation) as the 4th of March 1947. On this day, Warren Weaver, director of the Natural Sciences Division of the Rockefeller Foundation, wrote to the cyberneticist Norbert Wiener:

> Recognizing fully, even though necessarily vaguely, the semantic difficulties because of multiple meanings, etc., I have wondered if it were unthinkable to design a computer which would translate. Even if it would translate only scientific material (where the semantic difficulties are very notably less), and even if it did produce an inelegant (but intelligible) result, it would seem to me worth while.
>
> Also knowing nothing official about, but having guessed and inferred considerable about, powerful new mechanized methods in cryptography ... one naturally wonders if the problem of translation could conceivably be treated as a problem in cryptography. When I look at an article in Russian, I say 'This is really written in English, but it has been coded in some strange symbols. I will now proceed to decode.'

Wiener's reply was disappointing, arguing that the problems of semantics were too great for successful mechanization. Wiener did not think that Basic English (proposed by Ogden and Richards, and popular at the time) would be an answer; in a subsequent reply, Weaver begged to differ, and thought the problems could be overcome.

However, just two days after writing to Wiener (on 6th March 1947), Weaver met Andrew Booth, a British crystallographer, who was in the United States investigating the newly invented computers. He had come to Weaver to discuss possible American funding for a British computer at the University of London. Weaver did not think there would be any funds for numerical applications but he thought that there might be an interest in supporting new non-numerical applications of computers, and he suggested translation.

At a later date (in the historical introduction to Locke and Booth 1955), Booth stated that the discussion about MT took place in June 1946. Booth certainly met Weaver at that time and there was discussion of the

funding of a British computer, but there is no evidence that Weaver raised the idea of MT on this occasion; and indeed, Booth himself stated in other papers that the meeting had been in March 1947.

Weaver had himself been thinking of the possibilities for some two years. He may have mentioned it to Desmond Bernal, head of Booth's department at Birkbeck College London, and possibly to colleagues in the Rockefeller Foundation. However, the idea had also come to others independently. In 1946, Duncan Harkin had been inspired by studies of word counts to think of a system based on the most frequent vocabulary, and in collaboration with an electronics engineer, planned the outline of a machine for translation – although possibly not a computer. In September 1947 Alan Turing wrote a report for the National Physical Laboratory about his plans for constructing a computer, in which he mentioned MT as a possible demonstration of the 'intelligence' of computers. And there may well have been others. The new 'electronic brains' were reported almost daily in newspapers. They had caught the imagination of many people. Using them for translation seemed an obvious application - which was widely seen as a simple process.

## 3. Developments in 1948

Weaver's discussion with Booth bore fruit. Shortly after his return to England, Booth met Richard Richens, whose interest in mechanical translation arose from experiments with punched cards for storing information. He proposed the segmenting of words into their base forms (stems) and endings (e.g. inflections), both to reduce the size of dictionaries and to use grammatical annotations to augment a strictly word-by-word dictionary 'translation'. The validity of the method was tested by hand and by using punched card machinery on a variety of languages. The results were predictably crude, but sufficiently comprehensible, it was claimed, to scientists knowing the subject matter. From an account of the experiment written up during 1948 (although not published until much later, in a revised version: Richens and Booth 1955), the crudity of the output is obvious:

> (French) Il n'est pas étonn*ant de constat*er que les hormone*s de croissance ag*issent sur certain*es espèce*s, alors qu'elles sont in*opér*antes sur d'autre*s, si l'on song*e à la grand*e spécificité des ces substance*s.
>
> (English) *v* not is not/step astonish *v* of establish *v* that/which? *v* hormone *m* of growth act *m* on certain *m* species *m,* then that/which? *v* not operate *m* on of other *m* if *v* one dream/consider *z* to *v* great *v* specificity of those substance *m.*

(The asterisks in the French indicate automatic segmentations, in the English 'translation' *v* indicates a French word not translated, *m* "multiple, plural or dual", *z* "unspecific", and slashes alternative translations.) Although such morphological segmentation was only the very first stage of a true translation system, Richens is rightly credited as being the first person to propose a method of automatically identifying word-endings, albeit not using a computer but a punched card machine.

By this time there were signs of the start of research in the United States. In May 1948 the National Bureau of Standards (NBS) decided to build a computer in Los Angeles at its Institute of Numerical Analysis. The project was led by Harry D. Huskey, who had worked for a year in England at the National Physical Laboratory (NPL). Either independently or perhaps from conversations with Turing at NPL, Huskey proposed to use SWAC (Standards Western Automatic Computer) for translation. SWAC was demonstrated in May 1949, and a report appeared in the *New York Times* mentioning MT as one of its applications. What was proposed was clearly a "crude word-for-word translation", as the reporter recognized. However, despite its limitations, it had potential value as "scientists' translations of foreign

technical papers". An editorial the next day (one of the few ever devoted to MT) expressed doubts, in view of the problems of ambiguities and lexical differences between languages. Wisely, it commented that: "We are still far from the machine into which we will pour cards and pull out great poetry or great novels. In fact we shall never reach that stage." Nevertheless, the editor shared the common assumption that the awesome mathematical power of computers made them capable of almost anything.

## 4. Weaver's memorandum, July 1949

In July 1949, two years after writing to Wiener and meeting Booth, Weaver wrote the memorandum (Weaver 1949) which was to launch MT as a serious subject of research in the United States, and subsequently throughout the world.

Weaver began by recounting a story about decoding Turkish, which had succeeded without the decipherer having any knowledge of the original language. The success of cryptography lay, he believed, on "frequencies of letters, letter combinations, intervals between letters and letter combinations, letter patterns, etc., *which are to some significant degree independent of the language used"* [Weaver's italics]; and in support he cites the findings of logicians such as Hans Reichenbach and linguists such as Erwin Reifler. He then reproduced his letter to Norbert Wiener and Wiener's disappointing reply. As evidence that he was not alone, he reported on the work of Richens and Booth, mentioned the newspaper report from California, and reproduced an extract from a letter by Max Zeldner on the crudeness of word-for-word renditions. But, the purpose of the memorandum was to indicate how MT might go beyond the limitations of word-for-word translation, and he proposed, as he puts it, "at the risk of being foolishly naive... four types of attack".

The first approach was to tackle multiple meanings by examination of immediate context. How much context would be required could vary from subject to subject, and from one word to another, and he thought an investigation of the "statistical semantic character of language" would be useful. His second proposal assumed that there are (at least in non-literary works) logical elements in language. He believed that the mechanization of translation could be justified by a theorem proved in 1943 by McCulloch and Pitts on logical deduction of proofs by automata. This theorem suggested that "insofar as written language is an expression of logical character" the problem of translation by computer is formally solvable.

It was linked to his third proposal involving the recently developed 'information theory' of Claude Shannon, which was concerned with the basic statistical properties of communication. Shannon's research had arisen partly from his involvement in cryptography during the war, and Weaver remarked: "If we have useful methods for solving almost any cryptographic problem, may it not be that with proper interpretation we already have useful methods for translation?" Although enthusiastic for this approach, he did stress the fundamentally probabilistic nature of language, which meant the inevitable imperfection of any translation:

> "Perfect" translation is almost surely unattainable. Processes, which at stated confidence levels will produce a translation which contains only X per cent '"error," are almost surely attainable.

However, in his fourth proposal, he was more optimistic. The "most promising approach of all" was to investigate the universal structures of languages:

> Think, by analogy, of individuals living in a series of tall closed towers, all erected over a common foundation. When they try to communicate with one another, they shout back and forth, each from his own closed tower. It is difficult to make the sound penetrate

> even the nearest towers, and communication proceeds very poorly indeed. But, when an individual goes down his tower, he finds himself in a great open basement, common to all the towers. Here he establishes easy and useful communication with the persons who have also descended from their towers.
>
> Thus it may be true that the way to translate from Chinese to Arabic, or from Russian to Portuguese, is not to attempt the direct route, shouting from tower to tower. Perhaps the way is to descend, from each language, down to the common base of human communication – the real but as yet undiscovered universal language – and then re-emerge by whatever particular route is convenient.

He recognized that the determination of this "real but as yet undiscovered universal language" would involve a "tremendous amount of work in the logical structures of languages before one would be ready for any mechanization", although he continued to believe that Ogden and Richard's work on Basic English represented some steps towards it.

## 5. Reactions to the memorandum, 1950

Some recipients of Weaver's memorandum were inspired to take up the challenges of MT immediately. First on the scene was the Sinologist mentioned in the memorandum itself, Erwin Reifler. In early 1950, he put forward the first formulated conceptions of pre- and post-editing. Assuming that 'mechanical' processes could only be word for word one-to-one substitutions, there were obvious inadequacies. Reifler suggested that in order to remove ambiguities from the source text a human "pre-editor" could add extra symbols for grammatical and logical explicitness – theoretically without knowing the target language. The task of the post-editor was to render the machine output into a reasonably literate form – and, theoretically too, did not need not know the source language. Reifler argued that ambiguities could be more easily resolved by the pre-editor who would know the linguistic and cultural contexts, and he envisaged that the pre-editor would have access to a monolingual dictionary presenting all the alternative interpretations for words with more than one possible translation in the target language.

Interest had been stimulated also at the Rand Corporation in Santa Monica, California. Following up Weaver's suggested statistical approach to resolving problems of multiple meaning, Abraham Kaplan investigated the micro contexts of polysemy in mathematical texts. His study, completed in November 1950, concluded that the "most practical context is ... one word on each side, increased to two if one of the context words is a particle", i.e. an article, preposition or conjunction. Despite its limitations and tentativeness, this study was to encourage hopes that problems of ambiguity could be resolved, and in particular that statistical analyses could contribute useful data for MT systems.

Also in California, this year saw the beginning of research at UCLA, initiated by Huskey of the NBS. William E.Bull began his statistical studies of vocabulary, and Victor A.Oswald, Jr. researched syntactic problems in collaboration with Stuart L.Fletcher of the NBS. The authors proposed the coding of German sentences to identify 'noun blocks' and 'verb blocks' and to determine which blocks were candidates for rearrangement after translation into English. Their conclusion was that syntax "does not constitute, as had been thought by some, a barrier to mechanical translations." It was a rather optimistic conclusion, but, we should remember, it was not based on any test by a computer – although procedures had been formulated as 'instructions' for the SWAC, they were not implemented.

## 6. Bar-Hillel's survey, 1951

In May 1951. Yehoshua Bar-Hillel was appointed as a research assistant in the Research Laboratory for Electronics at the Massachusetts Institute of Technology (MIT). His task was to study the possibilities of MT and to plan future research, and in October 1951 he visited the few places in the US where MT research was going on. After the tour it was decided to hold a conference at MIT in June 1952, and in preparation Bar-Hillel wrote a state-of-the-art report (Bar-Hillel 1951).

Although he could envisage no hope of achieving "high-accuracy, fully automatic MT... in the foreseeable future", he stressed that this did not mean computers could not be applied to translation: "with a lowering of the target, there appear less ambitious aims the achievement of which is still theoretically and practically viable." Therefore, Bar-Hillel reviewed various options for "mixed MT, i.e. a translation process in which a human brain intervenes."

A post-editor was considered indispensable to eliminate "semantical ambiguities", but what was also clearly needed was the automatic resolution of grammatical ambiguities, and he made suggestions for an "operational syntax", based on his own ideas of categorial grammar. He did not agree with Oswald and Fletcher that syntactic analysis must await the availability of complete statistical data for morphological analysis in order to overcome the limitations of computer storage. He was profoundly sceptical of statistical approaches (a view which he was to repeat often in later years). Statistics might well identify the most frequent words and endings and enable 90 per cent of an average text to be translated, but the result would be unsatisfactory since the post-editor would be faced with translating the words which are "the least predictable and highly loaded with information".

He then briefly considered the role of a pre-editor as envisaged by Erwin Reifler. The problem with Reifler's proposals was the construction of a monolingual dictionary which the pre-editor could use to select appropriate translation equivalents in an unknown target language. He thought it might be possible where only two languages are concerned, but not for "*general* MT, where translation from any language into any other is considered."

Whereas, in Bar-Hillel's view, "*specific* MT" (i.e. translation into a single target language) could be pursued on a simple trial-and-error basis, "general MT will require establishment of a *universal,* or at least *general grammar,* perhaps even the construction of a whole artificial exchange-language." He was aware of failed attempts in previous centuries to construct universal languages, but believed that "mathematical logic, and modern structural linguistics" may prove a better foundation.

In the final pages of his survey, he mentioned the possibility of MT of texts with "a restricted vocabulary or a restricted number of sentence-patterns." Here he had in mind Basic English, artificial languages such as Esperanto, and the codes used by pilots and meteorologists. He thought it might be possible in such cases to translate whole units or sentences, since "the theoretical difficulties... are clearly less formidable". But he went further. There was also the "possibility of restricting, by voluntary convention, the richness of expression", i.e. by establishing what are now known as 'controlled languages'.

Bar-Hillel's paper was written before any MT research on even a reasonably modest scale had begun, in particular before any computer had been programmed to do even word-for-word translation, and it is therefore remarkable that he was able to identify so clearly the main problem areas and to outline many of

the basic strategies that continue to be valid to the present day.

## 7. The first MT conference, June 1952

The first conference on MT was organized by Bar-Hillel at the Massachusetts Institute of Technology from the 17th to the 20th June 1952. (Contemporary reports were given by Reynolds (1954) and Reifler (1954).) It should be noted that MT was invariably referred to as 'mechanical translation' at this period, and since the 'machines' (computers) were not yet available or adequate for the task, the appellation was more apt than the now accepted term 'machine translation'.

At its opening public session Bar-Hillel outlined the history of MT, from Weaver's first ideas to the establishment of the research groups represented at the conference. He reiterated many of the points made in his survey, stressing in particular the complexities of mechanizing translation processes and that

> "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."

All participants agreed on the need for post-editing. Bar-Hillel repeated what he had already said on the topic, and then in a further paper argued that post-editors should be given every possible version. His example was the German sentence *Es gibt einen Unterschied,* where the correct translation should be *There is a difference.* If *es gibt* were entered as a unit in a phrase dictionary with the automatic translation *there is/are* it would prevent a literal translation when required, e.g. *es* might refer to *Mädchen,* in which case *she gives...* would be correct. He argued therefore that post-editors should be presented with all alternatives. In fact, Bar-Hillel believed that if MT were limited to Western languages and to scientific publications, and if the dictionary were limited to 'non-overlapping synonyms', then multiple meanings could all be dealt with satisfactorily by a post-editor.

Many agreed with Reifler that some kind of pre-editing was also essential to minimize ambiguities and syntactic complexities of source texts. As an alternative, it was thought that authors could be trained to write with MT in mind, e.g. by using a 'controlled language'. At the conference, Stuart Dodd of the Washington Public Opinion Laboratory presented ideas for the "standardization of English syntax as a means of simplifying the use of English either as a source language or as a target language", e.g. the regularization of verb forms *(She did be loved* instead of *She was loved)* and of pronouns (using only nominative forms: *I will send he to she)* and, of course, the use of words in one meaning only (e.g. *tank to* mean only a water tank; the military meaning to require always the full form: *army tank.)*

Reifler described proposed pre-editing codes which were to be "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 coding would take into account not just the multiple meanings in the source language but also lexical and semantic differences in the target language. He thought the codes could be inserted by writers themselves who could use special monolingual dictionaries with symbols for distinguishing homographs. But he had also a more radical proposal: a new orthography for all languages which would distinguish grammatical categories: "all nouns would have... a capital first letter..., all principal verbs with a capital second letter and all attributive adjectives with a capital third letter..." The idea was that this would help to specify the context in which Kaplan's method of disambiguation could

operate. Furthermore, if the codes were combined with a regularized target language, it would "either restrict post-editorial interpretation to a minimum, or it may even make it completely superfluous."

For translation into many target languages Reifler agreed that this raised the question of a 'universal grammar'. He believed that comparative-historical linguistics could help to identify real universals. But he suggested also 'pseudo-universals' derived from the arbitrary alignment of source and target language grammatical categories. For example, he suggested the alignment of the Mandarin Chinese *-ti* and the English suffix *-ing,* so that *t'a*$^{1}$ *tsou*$^{3}$*-ti k'uai*$^{4}$ (He walks quickly) would be rendered "he walk-ing quick" – a kind of pidgin English.

It may be noted that Weaver's idea of investigating universal languages for MT was not discussed in the conference. Instead, for translation into many languages, where now we would expect proposals for some kind of interlingua, it was suggested (e.g. by Leon Dostert of Georgetown University) that translation into multiple target languages should be via a 'pivot' natural language such as English.

Problems of semantics were tackled also by imposing restrictions. The participants were attracted by the ideas of Victor Oswald for domain-specific dictionaries ('microglossaries' he called them) to minimize the problems of "multiple meanings". The 'sublanguage' vocabulary (as it would now be called) was to be identified by statistical analysis of corpora: "the data of all frequency counts fall into the same pattern, which means that a frequency count of any micro-segment of any language...should give a parabolic curve where high-frequency elements ought to dispose of eighty-percent of all running nouns." A study by Oswald on brain surgery texts showed this to be true, and that familiarity with 80% of the technical words in a subject was alone enough to make sense of an article. However, Oswald found that there was a similar frequency distribution for the non-technical word:

> 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, his UCLA colleague William E.Bull was more sceptical about the value of frequency analyses, believing that there could be "no scientific method of establishing a limited vocabulary which will translate any predictable percentage of the content (not the volume) of heterogeneous material." Indeed he went on to highlight a basic problem for all MT systems: "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."

Syntax was something quite new for many participants. It was a neglected area of contemporary linguistics, and few could conceive of any way of tackling it. Victor Oswald spoke about his analysis of German syntax: his 'blocks' approach was in essence a form of constituency analysis, which was becoming familiar at the time. More of a revelation was Bar-Hillel's 'operational syntax', based on his categorial grammar. There were two basic categories *n* (noun) and *s* (sentence); other categories were defined in terms of them, e.g. an intransitive verb with a subject noun to its left *s/(n),* a transitive verb with a noun to the left and either a noun or phrase to the right: *s/(n)[n],* an adjective *n/[n],* or a conjunction *n/[s].* By applying simple 'cancellation' rules – e.g. the index sequence *β a/(β)* is replaced by *a*, the sequence *a /[β] β* is replaced by *a,* etc. – a succession of syntactic categories could be reduced to a single 'grammatical' sentence *s.* The simplicity of the method was particularly attractive to the computer engineers present, but the linguists

were also inspired by the revelation of how syntactic analysis could be formalized in rules.

At the conference there was virtually no practical experience of programming to be reported, or indeed of using computers. A highlight for participants was a visit to the "Whirlwind" computer at MIT. Nevertheless, their was enthusiastic talk of future research plans. In particular, Leon Dostert of Georgetown University, who had come as a sceptic, advocated "the early creation of a pilot machine or of pilot machines proving to the world not only the possibility but also the practicality of MT". It was to lead to the collaboration with IBM of a pilot system demonstrated in January 1954, and the establishment of MT as a reality and not mere speculation.

## 8. Further progress in 1952 and 1953

Before this historic demonstration, however, articles appeared which brought MT to a wider audience. Bar-Hillel published three articles on various linguistic aspects of MT, and two articles appeared from the UCLA researchers in the same issue of *Modern Language Forum,* one by Victor Oswald and Richard Lawson on microglossaries, the other by Kenneth Harper on the morphological analysis of Russian. Booth published some popularizing articles, and featured in newspapers. And at Harvard University Anthony Oettinger produced a progress report on his doctorate study for the automation of a Russian-English dictionary - his thesis (the first in MT) was to be approved in 1954.

There continued to be newspaper reports of the SWAC computer implying that MT was close to realization, but in practice most research consisted on manual or punched-card simulations. From MIT came a report by James Perry on a simulation of word-for-word translation of a Russian text into English on slips of paper. One simulation produced:

> On/Onto/At Fig. 12 traced/mapped-out/drawn parabola according-to/along/in-accord-with which move thrown/deserted with/from velocity 10m/sec. under/below angle to/toward vertical line into/in/at 15°, 30°, 45°, 60°.

However, it was claimed that "the rough translations exhibited a high degree of intelligibility."

In July 1953 Bar-Hillel left MIT and returned to Jerusalem. He was replaced by Victor Yngve, who proceeded to set up the MT research project in the Research Laboratory of Electronics. The first progress report in October (cf. Yngve 1955) described a simulated word-for-word translation from German into English, in which articles and other function words were left untranslated. Yngve was surprised to find how much was comprehensible, and concluded that since "word-for-word translations are surprisingly good, it seems reasonable to accept a word-for-word translation as a first approximation and then see what can be done to improve it." The most obvious need was syntactic analysis, and this aspect of MT was to be the principal focus of research at MIT in subsequent years.

## 9. The Georgetown-IBM demonstration, January 1954

The most significant outcome of the MIT conference in June 1952 was undoubtedly the decision by Leon Dostert to start work on a computer program to demonstrate the feasibility of MT. At Georgetown University he appointed Paul Garvin and established links with Cuthbert Hurd and Peter Sheridan of the IBM Corporation. A system for translating some Russian sentences into English was developed and on 7th January 1954 a demonstration took place at the New York headquarters of IBM (Hutchins 1994).

The demonstration was reported the next day on the front page of the *New York Times.* It was declared to be "the cumulation of centuries of search by scholars for 'a mechanical translator'" and the reporter cited Dostert's optimism that a working system should be ready "within three or five years". Furthermore, it was thought that other languages would be easier than Russian, and the dictionary data (i.e. the punched cards) for German, French and "other Slavic, Germanic and Romance languages can be set up at will."

Reports appeared in many other newspapers. Nearly all stressed the speed of the operation and the fact that the punched card operator did not know a word of Russian. Spectators wore also impressed by the wide range of subject matter translated, and that the output was so fluent. Although the Georgetown researchers stressed its limitations (a 250-word vocabulary and six grammar rules) the general public was impressed: US government agencies were encouraged to support research on a large scale for the next decade, and MT groups were established in other countries, notably in the USSR. On the other hand, unrealistic expectations had been raised which did not materialize for many years to come.

The IBM-Georgetown demonstration was the first actual implementation of a translation system performing on the still primitive electronic computer. All previous work on MT had been theoretical in the sense that none of the proposals had actually been implemented as computer programs. The SWAC demonstrations of 1949 and 1952 had not produced translations - Huskey and his colleagues had merely asserted that MT was possible and shown the machine on which they believed it could be done. All other previous research in the 1950s had been either 'thought-experiments' or simulations on punched cards or paper slips. Before January 1954, the mechanization of translation had been largely speculative. From now on, real research began.

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