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YEHOShUA BAR-HILLEL
A PHILOSOPHER'S CONTRIBUTION TO MACHINE TRANSLATION

JOHN HUTCHINS

Yehoshua Bar-Hillel (1915-1975) made many important contributions in a wide range of topics in philosophy and logic (cf. Kasher 1976, Koppel and Shamir 1996). This article focuses exclusively on his activities in the field of machine translation, where, first as enthusiast then as skeptic, he was one of the most prominent and influential figures during the 1950s and 1960s.

1 *From Jerusalem to MIT*

Born 1915 in Vienna, Bar-Hillel went to Israel in 1933 from Berlin as one of the leaders of the religious youth movement Bnei Akiva, joining the Kibbutz Tirat-Zvi (Koppel & Shamir 1996: ix). His interests in language, philosophy and translation began early. As he writes in two autobiographical sketches (Bar-Hillel 1964b, 1965), he was fascinated by formal grammar and problems of translation at high school. While studying philosophy at the Hebrew University of Jerusalem (1935 and 1936/37) he came across the works of Rudolf Carnap and Hans Reichenbach, the 'spiritual leaders' of logical positivism. In particular, Carnap's *Logische Syntax der Sprache* (1934) became his 'bible' – he refers to it as “doubtless the most influential book I read in my life, and a good part of my work is directly or indirectly related to it.” (Bar-Hillel 1964b: 1). Then while working on his master's thesis in 1938 he came across an article by the Polish logician Kazimierz Ajdukiewicz (1935) which was to a major influence on his later work on algebraic linguistics, in particular for the development of categorial grammar.

After teaching mathematics at high schools in Jerusalem, he was called up for service in the Jewish Brigade Group, returning to teaching four years later in 1946, and to the completion of his doctoral thesis. He came now under the influence of the 'ordinary language philosophers' and, combined with the approach of Carnap and the “logical reconstructionists”, tackled the analysis of natural language – particularly the problem of syntactic categories (later published as Bar-Hillel 1950). He had barely finished his thesis when he was called up again for two years for service during the Israeli war of Independence.

At this time, his linguistics knowledge was, as he confessed (Bar-Hillel 1964b: 3), “deplorably naïve”. He assumed, along with many others (including some linguists), that grammars of unknown languages could be derived from statistical analysis of large corpora. He assumed also that all sentences of natural languages were parsible in terms of immediate constituents according to definite rules. In other words, he assumed that natural languages could be adequately represented by context-free phrase structure grammars, and that there could be ‘discovery procedures’ for arriving at these grammars. This belief was confirmed by discussions he had with Zellig S. Harris during the latter’s visit to Palestine in 1947, and by reading parts of Harris’s as yet unpublished manuscript for *Methods in structural linguistics* (Harris 1951). He became convinced that philosophers and logicians could not neglect linguistics.

In 1950, Bar-Hillel received a fellowship from the Hebrew University enabling him to visit the United States. He spent the winter of 1950/51 at Chicago, where he met Rudolf Carnap, who drew his attention to the statistical theory of information transmission or ‘information theory’, recently expounded by Claude Shannon and Warren Weaver (1949). Information theory and cybernetics – he had read Norbert Wiener’s book *Cybernetics* (1948) while still in Israel – were particularly attractive to Bar-Hillel, as they “fitted in rather well with my general utter distrust of metaphysics and speculative philosophy... and with my increasing interest in the use of mechanical methods in logic and linguistics” (Bar-Hillel 1964b: 5). He knew that Harris was becoming interested in the then newly invented electronic computers to develop a mechanical ‘discovery procedure’, and cybernetics itself presented a theoretical framework based on logic, information theory, and neurobiology which held out prospects for computer models of human behavior.

Wiener was one of the people Bar-Hillel most wanted to meet while in the United States. In March 1951, he left Chicago for the Massachusetts Institute of Technology (MIT). His contact was to be Walter Pitts, a former student of Carnap’s, who was writing his thesis with Wiener. A few years before, Pitts had written with Warren McCulloch a highly influential article on the logical basis of the nervous system, which had paved the way for automata theory, one of the foundation stones of cybernetics (McCulloch and Pitts 1943).

Bar-Hillel was not able to make the contact with Wiener which he had hoped for, but through Pitts he met Jerome Wiesner who offered him a research associateship in his Research Laboratory of Electronics (RLE) at MIT. Thus began a two-year appointment that he was later to characterize as “doubtless the most stimulating and creative” of his life (Bar-Hillel 1964b: 6).

2 *Research appointment at MIT, 1951-1953*

Bar-Hillel was appointed at RLE mainly for the study of the application of computers to linguistic work. The atmosphere there engendered huge optimism about the prospects:

There was an ubiquitous and overwhelming feeling around the Laboratory that with the new insights of cybernetics and the newly developed techniques of information theory the final breakthrough towards a full understanding of the complexities of communication “in the animal and in the machine” had been achieved. Linguists and psychologists, philosophers and sociologists alike hailed the entrance of the electrical engineer and the probability mathematician into the communication field (Bar-Hillel 1965: 294)

Claude Shannon was employed at the nearby Bell Telephone Laboratories; many at RLE, at MIT and at the closely associated Harvard University, were greatly influenced by his experiments in the statistics of English. They became convinced that speech in English or any other language was a Markov process; that the set of all sentences could be generated by a Markov source; as Bar-Hillel (1965: 295) saw it, the “only thing missing for a complete understanding of the communication process... was reliable statistics on the relative frequencies of digrams, trigrams, etc...”, and for this, of course, they now had the resources of the electronic computer.

However, Bar-Hillel soon began to learn that things were not quite so straightforward. For this he had to thank discussions with Noam Chomsky, whom he met first in the autumn of 1951, shortly after Chomsky moved to Harvard University as a Junior Fellow. At this time, Chomsky was beginning to develop ideas on grammatical theory which diverged from those of his mentor Zellig Harris at the University of Pennsylvania. In particular, he was beginning to clarify his conception of the basic goal of linguistics as the formulation of a theory of ‘competence’ as distinct from the concerns of communication scientists (and applied linguists) with ‘linguistic performance’ (speech). In this context, statistical analysis played no role. The close relationship between Chomsky’s approach and the algebraic formulations of the logicians were obvious to both.¹

Bar-Hillel concentrated on the investigation of machine translation (MT).² He was “intrigued by the problem of making computers work toward the solution of non-numerical problems, and I picked the translation problem,

¹ Over time, Bar-Hillel became convinced that cybernetics and information theory were “irrelevant” to theoretical linguistics. “There is no room for probability and statistics (beyond trivialities) in the study of language” and “If you look at the work of Harris and Chomsky, cybernetics might never have existed” (Bar-Hillel 1965:297).

² Although called “the first full-time paid research worker in the field” (Booth and Locke 1955: 5), he pointed out that “I never wrote a program for MT, never collaborated with a group that designed mechanical translators, and never induced a student to write a thesis on MT” (Bar-Hillel 1964b: 9)

because of my independent interests in the ‘mechanisms’ of language and in the existence of ‘mechanical’ decision procedures for sentencehood and syntactic analysis in general.” (Bar-Hillel 1965: 298). For him, therefore, MT was to be essentially a theoretical problem – which did not, however, mean that he was oblivious of the severe limitations of MT in practical application, as he showed in later years.

Within a few months, he was giving lectures on MT at a MIT Summer Course on Communication Theory, and in October 1951 Bar-Hillel visited the few places in the US where MT research was known to be going on. In Los Angeles he met Abraham Kaplan and Olaf Helmer at the Rand Corporation, and Victor Oswald and William Bull at UCLA; in Seattle, he met Erwin Reifler at the University of Washington; and he gave lectures on MT at the University of California (Berkeley) and at the University of Michigan (Ann Arbor).

At the conclusion of the tour it was decided to hold a conference on MT – the first – at MIT, and that Bar-Hillel should organize it. For this, he received a grant from the Rockefeller Foundation. In preparation, Bar-Hillel wrote a state-of-the-art review – published two years later, although still dated 1951 (Bar-Hillel 1951) – in which he clarified the major issues, identified the main problems and the main lines of inquiry.

It was already “obvious” to him that “fully automatic MT, i.e. one without human intervention... [was] achievable only at the price of inaccuracy.” The major obstacle to fully automatic translation was that there were no obvious methods “by which the machine would eliminate *semantical ambiguities*.” The only approach he could envisage at this time was a vast bank of statistical “knowledge of the relative frequencies of all word digrams... trigrams, etc. [and] equivalent conditional frequencies of the foreign language...”, or “a ‘learning’ organ, the construction of which is still in its rudimentary stage.”

With no prospect of achieving “high-accuracy, fully automatic MT... in the foreseeable future”, he stressed that “with a lowering of the target, there appear less ambitious aims the achievement of which is still theoretically and practically viable”. In particular, he advocated “mixed MT”, i.e. a translation process in which “the human partner will have to be placed either at the beginning of the translation process or the end, perhaps at both...” With human and machine collaboration, “high accuracy” could be achieved.

He considered Erwin Reifler’s ideas on pre-editing to be impractical, since there could be no way of anticipating all problems in target languages. On the other hand, the post-editing task could be made “rather easy, if one assumes that the machine has eliminated all the grammatical ambiguities... and has arranged the text in the [target language] order.” Rather over-optimistically, he concluded that a post-editor “should be able to produce out

of the raw output... a readable translation in a fraction of the time it would take a bilingual expert to produce a translation with the conventional procedure.”

As Bar-Hillel had already learned, current linguistics had little to offer early MT research; the application of Harris’ ideas was not yet clear, and he had yet to meet Chomsky. However, Bar-Hillel did have some proposals from his logician background, and he sketched ideas for an “operational syntax”, i.e. his categorial grammar, based on work by Ajdukiewicz (1935).

Following Reifler, he made a clear distinction between “specific MT” (translation between just one source and one target), and “general MT” (from any language into any other). Bar-Hillel believed that systems for “specific MT” could be developed on a simple trial-and-error basis, while “general MT” required the “establishment of a *universal*, or at least *general grammar*, perhaps even the construction of a whole artificial exchange-language.” The failure of past efforts to build universal languages was not encouraging – Bar-Hillel was, as to be expected, particularly scathing about the utopian efforts of metaphysically inclined philosophers of the past – nevertheless, again not surprisingly, he believed that “empirical open-mindedness, mathematical logic, and modern structural linguistics” could provide better foundations.

On the final page of his survey, Bar-Hillel foresaw the possibility of MT with “a restricted vocabulary or a restricted number of sentence-patterns or perhaps both.” He had in mind Basic English, artificial languages such as Esperanto, and the “codes” used by pilots and meteorologists. In the latter case, he believed that “sentence-pattern translation might be an effective method...The theoretical difficulties of such a type of MT are clearly less formidable.” Furthermore, Bar-Hillel put forward the idea of controlling vocabulary and grammar in order to assist automatic translation: “restricting, by voluntary convention, the richness of expression... to such a degree that sentence-pattern translation might easily and quickly be applied”.

In 1951, MT research had scarcely begun on even very modest scales. Yet, Bar-Hillel identified many of the problem areas (obviously, not all) that still concern MT researchers now, and he outlined many of the basic strategies for practical MT applications. In particular, his emphasis on the non-feasibility of aiming for fully automatic translation, the need for ‘man-machine symbiosis’, and his comments on the value of limited systems were commendably realistic, but they were largely ignored for many years.

3 *The first MT conference*

Bar-Hillel’s survey set the scene for the conference which he convened the next year at MIT (Hutchins 1997a, 1997b) At the opening public session on 17th June 1952 he outlined the history of MT so far, and gave a brief survey of its practical potential, stressing the complexities and difficulties and concluding:

... 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.

The conference was to be a great stimulant to MT research in the United States, with its first fruit being the public demonstration by IBM and the Georgetown group in January 1954. (Hutchins 1997b). Its success was largely due to his enthusiasm (Reifler 1954), and Bar-Hillel himself remembered it with pleasure: “It was one of the most exciting conferences I had ever participated in, and I shall never forget the pervasive feeling of euphoria which I felt and thought that everybody else shared.” (Bar-Hillel 1964b: 8)

During the conference, Bar-Hillel expanded on his ideas for an operational syntax, an outline of his theory of categorial grammar, i.e. “a quasi-arithmetical description of syntactic structure which could serve as a basis for the mechanization of syntactic analysis” (the paper was later expanded as Bar-Hillel 1953a). The method was attractive for the engineers, as one of the participants noted (Reynolds 1954), but not – as it transpired – for the linguists, since categorial grammar was not to be taken up until many years later.

Bar-Hillel was a very active participant in the discussions. But, in hindsight, he had some reservations about his contribution (Bar-Hillel 1964b: 9). “According to the stenographic notes, I used the phrase “if a human being can do it, a suitably programmed computer can do it too” more than a dozen times during the conference.” At this date, he was still basically a ‘cyberneticist’; he was enthusiastic about full automation of translation as an overall (if distant) research goal, while at the same time maintaining that the most practical solutions were “man-machine partnerships”.

In the following months, Bar-Hillel became a propagandist for MT. In September 1952 he spoke at the seventh International Congress of Linguists in London, in 1953 he wrote a substantial overview of the linguistic problems of MT (Bar-Hillel 1953c), and at the end of 1953 he contributed a ‘popular’ article for *American Scientist* (Bar-Hillel 1954), surveying the achievements and prospects, which was brought up to date by a brief mention of the IBM-Georgetown demonstration in January 1954.

Finally, in this year he wrote a paper on idioms for the Locke-Booth anthology, which had originally intended to be the proceedings of the MIT conference. Bar-Hillel’s contribution (1955) raised the danger of inappropriate use of idiomatic renditions when ‘literal’ versions could be safer:

It is not implausible to assume that one or more ... rough translations will not only be grammatical and make sense, even good sense, and therefore be accepted by some or all potential users as the correct translation, but still be dead wrong.

More generally, the complexities involved in obtaining truly idiomatic translations convinced Bar-Hillel of the impossibility of full automation and of the essential role of human intervention.

In July 1953, Bar-Hillel returned to the Hebrew University of Jerusalem and took up his teaching duties. There followed a nearly five-year gap in his MT activity, until in 1958 he received a research contract from the U.S. Office of Naval Research to investigate the mechanization of linguistic data processing, and MT in particular.

4 Survey of MT, 1958-60

In September 1958, he attended the Second International Congress on Cybernetics, held in Namur (Belgium). There he gave his first paper on MT for some four years (Bar-Hillel 1958). Entitled “Some linguistic obstacles to machine translation”, it asserted that in order to go beyond simple and unsatisfactory word-for-word translation the syntactic structures of sentences had to be fully determined mechanically – a view accepted at this date by few MT researchers. The article presents his own categorial grammar approach, and then the transformational-generative model of Chomsky (1957). Although confident that Chomsky’s approach “may well turn out to facilitate the mechanization of translation from new angles”, he remains sceptical about the quality of output: “... 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.” Nevertheless, he urges MT workers to recognize 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.”

In October 1958, Bar-Hillel visited the major centers of research in machine translation both in the United States and in Great Britain, hardly any of which had been engaged in full-scale research for more than two years (see Hutchins 1986 for details). The report prepared for the US Office of Naval Research was based on these visits and on information received during the following months and was published in February 1959 (Bar-Hillel 1959). One year later it received much wider circulation when it appeared in a revised form in the annual review journal *Advances in Computers* (Bar-Hillel 1960) – the main addition being a survey of MT in the Soviet Union, based on Rozentsveig (1958), Panov (1958), and Oettinger (1958).

It begins by drawing attention to the huge sums of money given by US governmental agencies for the support of MT research, estimating that half a million dollars were spent in 1958, with some 150 people (including higher degree students) working full or part time in the field. In the Soviet Union he

guessed that even more researchers were involved. He notes that despite the scale of operations, there had been no comprehensive survey since his own report for the 1952 MIT conference (section 3 above).

The main aim of Bar-Hillel was to argue that most MT research was on the wrong track. Researchers had been misled by their early ‘successes’; the most straightforward problems had been solved with little difficulty; unfortunately, “the ‘few’ remaining problems were the harder ones – very hard indeed.” They had been wrong to see the aim of MT research as the achievement of ‘fully automatic, high quality translation’ (FAHQT)³, and as ‘proof’ he attached an appendix demonstrating its unattainability “not only in the near future but altogether.” (see below)

As in 1951, he held that the only feasible objective in the short term, if the outcome was to be high quality translation, was:

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. (Bar-Hillel 1960: 99)

It was not, he conceded, an attractive option for MT researchers, but it would provide a valuable assistance to agencies short of expert translators; they should not have to wait “until the post-editor’s part has become very small”.

Since the FAHQT goal was unrealistic, it was also wrong to attempt to produce “a smooth text of the kind you will get from a human translator.” Such results could be achieved if lexical selection were based on frequency analyses of the target language, and if frequency measures were applied to syntactic and semantic rules. But he thought this approach was wrong “and even dangerous since the machine output ... will be of low quality in a misleading and soothing disguise... [and] it is by no means clear whether the reader will always be able to detect these mistranslations.” He warned against overestimating the impact of statistical information – a remnant, he believed, of a time (in the 1940s and early 1950s) when “many people thought that the statistical theory of communication would solve many, if not all, of the problems of communication.”

He was consequently critical of those groups (such as RAND at this time) that adopted the ‘empirical’ approach.

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

³ Originally (in Bar-Hillel 1959) referred to as ‘fully automatic high quality machine translation’ (FAHQMT); changed to the non-tautological FAHQT in Bar-Hillel 1960.

analysis of a large corpus than by starting from some authoritative grammar and changing it, if necessary, in accordance with analysis of actual texts. (Bar-Hillel 1960: 101)

In fact, scarcely any MT group escaped censure. He was critical of the ‘interlingual’ approach – while conceding that mathematical logic might reinforce age-old hopes of discovering a ‘universal’ language – for the fallacious assumption that translation via an artificial ‘interlingua’ might be simpler than via a natural language. The MIT group was criticized for its “unnecessary” work on developing the COMIT programming language, and for “reluctance to publish incomplete results”; the Harvard group was accused of “strong distrust of the achievements of other groups”; the Cambridge Language Research Unit’s work on lattice theory was dismissed as “only reformulations... of things that were said and done many times before”. Only one group received unreserved praise, the National Bureau of Standards, for its efficient parser and for making “no attempt ... to obtain FAHQT output.”

The report was furnished with three appendices. The first provided information about MT projects, dates of foundation, principal workers, and where known, statistics of numbers of researchers and their budgets – still a valuable source. The second reprinted his article on ‘linguistic obstacles’ (Bar-Hillel 1958), but best known and most cited has been the third appendix, devoted to “A demonstration of the non-feasibility of fully automatic, high quality translation”.

This short article has become one of the most influential papers ever written on MT. In it, he set out to prove that FAHQT was quite simply unattainable in principle. The argument is based on the short sentence *The box was in the pen*, in the following context:

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

On the assumption that *pen* can have two meanings, a ‘writing utensil’ and an ‘enclosure where small children can play’ – in fact, it probably has more than two senses, but this does not affect his argument – Bar-Hillel claimed 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.

This amounted to the very strong claim that in certain (and, he believed, not infrequent) cases, no amount of context would suffice to resolve this type of homonymy. The reason he gives is that knowledge of the relative sizes of the possible referents of *pen* is “not at the disposal of the electronic computer.” To put such information in a MT system would mean that “a translation

machine should not only be supplied with a dictionary but also with a universal encyclopedia". For him, such a requirement was "utterly chimerical and hardly deserves any further discussion", since human knowledge is not just a vast store of facts but also the infinite set of inferences which may be drawn from those facts.⁴

He had to concede, of course, that ambiguity can be resolved by the use of microglossaries and contextual clues, but he held to the view (already propounded in his 1951 review) that the use of microglossaries increased the risk of mistranslation – by producing meaningful versions that were erroneous, or could be misunderstood, in a particular context – and that contextual analysis can have only limited effectiveness.

5 *From 1961 to 1964*

In the year following his report, Bar-Hillel spent a further sabbatical year (1960-1961) in the United States, first at the University of California, Berkeley, and then once more at the MIT Research Laboratory of Electronics (Bar-Hillel 1964b: 14). He "became more and more convinced of the futility of most approaches to MT", and he had "finally become disenchanted with the thought that computers should be in a position to improve their output in translation... through learning".

In July 1962, Bar-Hillel was invited to Venice, with a number of other prominent experts in MT, computational linguistics and formal linguistics, to participate in the NATO Advanced Study Institute on Automatic Translation of Languages. The four lectures he gave (Bar-Hillel 1962b) covered formal linguistics (categorical grammar, transformational-generative grammar, dependency grammar), with particular stress on the inadequacies of context-free grammars (both in principle and in practice) for MT analysis, and the advantages of transformational grammars.⁵ In a discussion of 'syntactic complexity' (later expanded as Bar-Hillel et al. 1967), he argued against the Wittgensteinian thesis that anything that can be said can be expressed simply – "For most languages... there are things worth saying which cannot be expressed in sentences with a low degree of syntactic complexity, without a loss being incurred in other communicationally important respects" – and that complexity was not to be equated with formal transformations (following Yngve (1960), as he acknowledges). The last lecture took a further step in his argument about the non-feasibility of high-quality MT. Previously (Bar-Hillel 1960, Appendix III), he had left open the possibility that computers with learning (or 'self-organizing') capabilities might be able to improve their initial algorithms and thereby improve output. He dropped the last vestiges of his

⁴ The arguments for and against Bar-Hillel's thesis continue to the present (Nirenburg 1996).

⁵ It may be noted that he held fast to a Chomskyan conviction that "[a] formal grammar of English is an *empirical* theory of the English language."

cyberneticist past. He was now convinced that computers could never acquire full common-sense ‘knowledge’ and ‘understanding’ of the ‘real world’, and would never be capable of ‘learning’ language: “So long as we are unable to wire or program computers so that their initial state will be similar to that of a newborn infant, physically or at least functionally, let’s forget about teaching computers to construct grammars” (Bar-Hillel 1962b: 23).

He was also certain that computers would never be able to resolve ambiguities as well as human translators: “Though it is undoubtedly the case that some reduction of ambiguity can be obtained through better attention to certain formal clues... it should now be perfectly clear that there are limits to what these refinements can achieve, limits that definitely block the way to autonomous, high-quality machine translation.” Even if the grammatical models were adequate – which he did not believe they were – it would never be possible to acquire a full understanding of the workings of language, and, in any case, “the more we shall know about linguistic structure, the more complex the description of this structure will become” (Bar-Hillel 1962b: 23-24).

6 *Final thoughts on MT*

In 1964, Bar-Hillel published a collection of his articles on MT, information retrieval, algebraic linguistics, and related matters (Bar-Hillel 1964a) – containing many of the articles mentioned in this paper. The introduction gave an autobiographical account of how he became involved in these areas and how he came to the opinions he now held with such conviction (Bar-Hillel 1964b).

It is particularly his views on FAHQT – no doubt an influence (though unacknowledged) on the deliberations of the ALPAC (1966) – that have had most impact. It was for a study by the MT group at the University of Texas on the future ‘feasibility of fully automatic high quality translation’, i.e. whether the situation had changed or improved since 1959/1960, that he made his final contribution to the field (Bar-Hillel 1971). In it, he summarized the main options as “(1) Machine-aided human translation, (2) man-aided machine translation, (3) low-quality autonomous machine translation”, holding them all as “eminently practical”. Finally, pragmatic as ever, he acknowledged that he had been too dogmatic about ‘high quality’:

A translation which is of good quality for a certain user in a certain situation might be of lesser quality for the same user in a different situation or for a different user, whether in the same or in a different situation. What is satisfactory for one need not be satisfactory for another.

With these words, Bar-Hillel was again among the first to express succinctly what became in later years a truism of MT in practice.

7 *Conclusion*

Although Bar-Hillel died over two decades ago (in 1975) and his last substantial contributions to MT date from the middle of the 1960s, his influence continues to the present day. Many external (and internal) commentators go back to his famous ‘demonstration’ of the non-feasibility of FAHQT, and many extend its applicability (in ways Bar-Hillel would probably have disapproved) to MT in general. The practice of MT has changed profoundly since his day; “man-aided” MT systems are increasingly used economically and cost-effectively by large organizations, “low quality autonomous” output is increasingly used for rapid access to foreign-language documents (particularly on the Internet), and the massive development of “machine aided” translation – where his foresight is rightly acknowledged (Macklovitch 1996) – is revolutionizing the translation profession. Yet his basic insights remain valid to the present time, and repay repeated reading by all involved in the field.

8 *Selected writings by Yehoshua Bar-Hillel on MT and related topics*⁶

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- Bar-Hillel, Y. 1959. *Report on the state of machine translation in the United States and Great Britain*. Technical report, 15 February 1959. Jerusalem: Hebrew University.
- Bar-Hillel, Y. 1960. “The present status of automatic translation of languages”. *Advances in Computers* 1.91-163. [Sections 1.2-1.5 repr. as “Aims and methods in machine translation”, Bar-Hillel 1964a.166-173, and Appendix III repr. as “A demonstration of the nonfeasibility of fully automatic high quality translation”, Bar-Hillel 1964a.174-179.]
- Bar-Hillel, Y. 1962a. “The future of machine translation”. *Times Literary Supplement* 20 April 1962, p.268. [Repr. Bar-Hillel 1964a.180-184.]

⁶ A full bibliography for Bar-Hillel is given in Kasher 1976, pages xix-xxviii, which also includes a photograph.

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