

EARLY YEARS IN MACHINE TRANSLATION

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Volume 97

W. John Hutchins (ed.)

Early Years in Machine Translation
Memoirs and biographies of pioneers

EARLY YEARS IN
MACHINE TRANSLATION

MEMOIRS AND BIOGRAPHIES OF PIONEERS

Edited by

W. JOHN HUTCHINS

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CONTENTS

Preface	vii
Common abbreviations	x
Acknowledgements, Cyrillic transliteration	xi
The first decades of machine translation: overview, chronology, sources. <i>John Hutchins</i>	1
Warren Weaver and the launching of MT: brief biographical note. <i>John Hutchins</i>	17
Erwin Reifler and machine translation at the University of Washington. <i>Lew R. Micklesen</i>	21
Early research at M.I.T.: in search of adequate theory. <i>Victor H. Yngve</i>	39
Machine translation at Harvard. <i>Anthony G. Oettinger</i>	73
The Georgetown project and Leon Dostert: recollections of a young assistant. <i>Muriel Vasconcellos</i>	87
Is FAHQ(M)T possible? Memories of Paul L. Garvin and other MT colleagues <i>Christine A. Montgomery</i>	97
The early days of GAT-SLC. <i>Michael Zarechnak</i>	111
Machine translation: just a question of finding the right programming language? <i>Antony F.R. Brown</i>	129
From Serna to Systran. <i>Peter Toma</i>	135
My early years in machine translation. <i>Winfred P. Lehmann</i>	147
David G. Hays. <i>Martin Kay</i>	165
Gilbert W. King and the USAF Translator. <i>John Hutchins</i>	171
Translation and the structure of language. <i>Sydney M. Lamb</i>	177
Pioneering MT in the Soviet Union. <i>Olga S. Kulagina</i>	197
Machine translation and formal linguistics in the USSR. <i>Igor A. Mel'čuk</i>	205

My memoirs of MT in the Soviet Union. <i>Tat'jana N. Mološnaja</i>	227
MT in the former USSR and in the Newly Independent States (NIS): pre-history, Romantic era, prosaic time. <i>Raimund G. Piotrovskij</i>	233
Machine translation: early years in the USSR. <i>Jurij N. Marčuk</i>	243
The beginnings of MT. <i>Andrew D. Booth and Kathleen H.V. Booth</i>	253
R.H. Richens: translation in the NUDE. <i>Karen Sparck Jones</i>	263
Margaret Masterman. <i>Yorick Wilks</i>	279
Yehoshua Bar-Hillel: a philosopher's contribution to machine translation. <i>John Hutchins</i>	299
Silvio Ceccato and the correlational grammar. <i>Ernst von Glasersfeld</i>	313
Early MT in France. <i>Maurice Gross</i>	325
Bernard Vauquois' contribution to the theory and practice of building MT systems: a historical perspective. <i>Christian Boitet</i>	331
Pioneer work in machine translation in Czechoslovakia. <i>Zdeněk Kirschner</i>	349
Alexander Ljudskanov. <i>Elena Paskaleva</i>	361
Memoirs of a survivor. <i>Hiroshi Wada</i>	377
Index of names	387
Index of subjects	394
Location of photographs	
Participants at US conferences	xii
Weaver, Reifler, Yngve, Oettinger	16
Dostert, Garvin, Zarechnak, Toma	146
Brown, Lehmann, Lamb	196
Participants at USSR conference; Mel'čuk, Mološnaja, Reformatskij	232
Marčuk, Piotrowski, Booth	252
Richens, Masterman, Ceccato	298
Sgall, Wada, Vauquois	386

PREFACE

“The study of the deeds of our ancestors is ... more than an antiquarian pastime, it is an immunological precaution” (Umberto Eco, *The search for the perfect language*, p.316. London: Fontana, 1997)

The history of any field of endeavour is based not only on contemporary records but also on biographical memories of those who lived through the events themselves. We can learn a great deal about the history of attempts to program electronic computers for translating languages from the books, articles, reports and conference proceedings written at the time. But such documents can tell us only the results of experiments, they tell us little about the people behind the experiments. The aim when compiling this volume has been to hear from those who participated directly in the earliest years of mechanical translation, or ‘machine translation’ (MT) as it is now commonly known, and, in the case of those major figures already deceased, to obtain memories and assessments from people who knew them well. Naturally, it has not been possible to cover every one of the pioneers of machine translation, but the principal researchers of the United States, the Soviet Union, and Europe (East and West), are represented here.

Contributors were encouraged not just to “tell it how it was” and to recount the origin and development of their own research programmes, but also to include personal details and anecdotes and to give their impressions of the impact of political and social events on the development of the field. Each contributor has approached the task in his or her own way; there are inevitable differences of coverage and content, and quite often differences in the perception and recollection of the same events. In this new field, there were many competing views, and contemporary rivalries (personal and institutional) are remembered as much as the excitement of a new and important sphere of activity. Writers have also commented about later developments in machine translation, and about the continuing validity and relevance of older methods and ideas. Despite dramatic changes in computer technology, the same basic issues confront today’s researchers, and these papers should serve as reminders of the wide range of alternative approaches proposed in the past – some only recently rediscovered – and thereby, in part, help us to distinguish the true advances from variants of old themes.

The origins of this collection can be traced back to the MT Summit Conference in October 1997 (in San Diego, California), which marked 50 years

of machine translation – since the letter from Warren Weaver to Norbert Wiener in March 1947 about the possibility of using the newly invented computer to translate natural languages. At this conference, a panel of MT pioneers spoke informally about some of their experiences in the 1950s and 1960s. In early 1998 I invited as many of the pioneers that I knew to contribute to this collection; later others were traced and invited, and for those already dead I invited contributions from those who knew them. Finally, I added articles on Weaver, Bar-Hillel and King, and provided an introduction.

The arrangement is geographical and partly chronological. First come the US pioneers of the very earliest days: Warren Weaver (by the editor), Erwin Reifler (by Lew Micklesen), Victor Yngve, and Anthony Oettinger; then those associated (wholly or briefly) with the Georgetown project: Léon Dostert (by Muriel Vasconcellos), Paul Garvin (by Christine Montgomery), Michael Zarechnak, Tony Brown, and Peter Toma; and then those who entered slightly later: Winfred Lehmann, David Hays, Gilbert King (by the editor), and Sydney Lamb. The next block is represented by recollections by researchers from the former Soviet Union: Olga S. Kulagina, Igor A. Mel'čuk, Tat'jana N. Mološnaja, Raimund G. Piotrovskij, and Jurij N. Marčuk. They are followed by British pioneers: Andrew D. and Kathleen Booth on their experiences in the UK and Canada, and memoirs and assessments of Richard Richens by Karen Sparck-Jones, and of Margaret Masterman by Yorick Wilks. Finally researchers from elsewhere: Yehoshua Bar-Hillel (by the editor), Silvio Ceccato (by Ernst von Glasersfeld), Maurice Gross on early MT in France, an assessment of Bernard Vauquois' contribution by Christian Boitet, Zdeněk Kirschner on the Czech experience, Elena Paskaleva on Alexander Ljudskanov and the Bulgarian scene, and finally the memories of Hiroshi Wada of his own and others' pioneering years in Japan.

In each article, a selected bibliography has been provided of publications by the individual concerned, covering both their MT-related writings and, to a lesser extent, their contributions to other fields; in addition, where relevant, the bibliography includes publications by other members of the MT group to which the pioneer belonged. References to publications by others, whether contemporary or later, are listed separately. In some contributions I have also provided footnotes which give further information about individuals or systems mentioned, but I have not usually given cross-references to accounts of the same events in other papers of this volume. For the assistance of readers who want to follow up different perspectives on personalities and events there are indexes of personal names (with full names and dates where ascertainable) and of subjects (including institutions, organizations, and languages.)

Supplementary biographical information has been added for most authors of articles, giving details of careers, honours and contributions in other fields. Photographs of MT pioneers will be found at various places in the

volume, and there are also some group photographs of participants at major conferences in the USA and in the USSR.

In the introduction I have provided a general overview of the issues, approaches, methods, problem areas, attitudes, circumstances and background of MT activity in the 1950s and 1960s, and an outline chronology of the main events up to the mid 1970s. Both the overview and the chronology are necessarily brief and cannot cover all research groups, all the problems and methods, and all the controversies and activities of the period. A select bibliography of the main sources, both contemporary and later, has been provided for those interested in learning more about the early history of machine translation and of computational linguistics.

The pioneer period of MT ended in the early 1970s after the dispersal of many of the research groups, directly or indirectly as a consequence of the 1966 ALPAC report. Some pioneers continued in the field until recent times: Zarechnak, Piotrovskij, Marčuk, Vauquois, Lehmann, Garvin, and Toma. Others moved (on or back) into other spheres in which their interests had grown over the years, e.g. Lamb, Mel'čuk, Mološnaja, and Yngve into linguistics; Bar-Hillel and Ceccato into philosophy; and Booth, Brown, and Oettinger into other areas of computer science and technology. Many left the field in some pessimism; progress had not been as great as they might have expected – the complexities of language and translation were found to be too great for dramatic 'breakthroughs', and indeed subsequent years have witnessed only gradual improvements. Today, however, there are cost-effective and time-saving uses made of still 'less-than-perfect' translations in ways that few would have imagined forty years ago or more when the foundations of MT were being laid.

As explorers in a new land, the pioneers encountered false trails, barren deserts, and treacherous swamps, but they also charted the fertile terrain which their successors have exploited and developed to the present day. It is to be hoped that this collection will bear witness and do honour to their groundbreaking achievements.

Norwich, UK., May 2000

John Hutchins

COMMON ABBREVIATIONS

AFOSR	Air Force Office of Scientific Research	ITMiVT	Institute of Precision Mechanics and Computer Technology
AI	artificial intelligence	KGB	Komitet Gosudarstvennoj Bezopastnosti (<i>USSR security service</i>)
ALPAC	Automatic Language Processing Advisory Committee	LGU	Leningradskij Gosudarstvennyj Universitet (Leningrad State University)
AMTCL	Association for Machine Translation and Computational Linguistics	MIAN	Mathematical Institute of the (USSR) Academy of Sciences
CETA	Centre d'Etudes pour la Traduction Automatique	MIT	Massachusetts Institute of Technology
CIA	Central Intelligence Agency	MT	machine translation
CL	computational linguistics	<i>MT</i>	Mechanical Translation
CLRU	Cambridge Language Research Unit	NBS	National Bureau of Standards
CPU	central processing unit	NL	natural language
ELT	Electro-Technical Laboratory	NLP	natural language processing
FAHQ(M)T	fully automatic high quality (machine) translation	NPL	National Physical Laboratory
GU	Georgetown University	NSF	National Science Foundation
IBM	International Business Machines	OCR	optical character recognition
ICCL	International Committee on Computational Linguistics	ONR	Office for Naval Research
IFIP	International Federation for Information Processing	RADC	Rome Air Development Center
IR	information retrieval	UC	University of California
		UCLA	University of California at Los Angeles
		USAF	United States Air Force

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The editor's thanks must go above all to the contributors to this collection, some of whom have waited over a year for its completion, and to the series editor, Konrad Koerner, whose example in linguistic historiography inspired the compilation of this volume dedicated to the early history of machine translation.

The photographs in this collection are reproduced with permissions of the following: Christian Boitet (for Bernard Vauquois [p.386]), Andrew and Kathleen Booth [p.252], Tony Brown [p.196], Ernst von Glasersfeld (for Silvio Ceccato [p.298]), Zdeněk Kirschner (for Petr Sgall [p.386]), Sydney Lamb [p.196], Winfred P. Lehmann [p.196], Jurij N. Marčuk [p.252], Igor A. Mel'čuk (for the Moscow conference 1958 and his own portrait [both p.232]), Tat'jana Mološnaja (portrait with A.A.Reformatskij [p.232]), Christine Montgomery (for Paul Garvin [p.146]), Anthony Oettinger [p.16], Raimund G. Piotrovskij [p.252], Praetorius Ltd. (for Peter Toma [p.146] from *Language Monthly*, no.30, 1986), Mrs Henrietta Reifler (for Erwin Reifler [p.16]), Mrs. Ruth Richens (for Richard H. Richens [p.298]), Rockefeller Archive Center (for Warren Weaver [p.16]), Yorick Wilks (for Margaret Masterman [p.298]), Victor H.Yngve (for the Princeton conference 1961 and the AMTCL conference 1963 from *Mechanical Translation* (vol.6, 1961, and vol.8, 1964) [both p.xii], and for his own portrait [p.16]), Hiroshi Wada [p.386], and Michael Zarechnak (for Leon Dostert and for his own portrait [both p.146]).

CYRILLIC TRANSLITERATION

The following system has been used for the transliteration of Cyrillic words, names and titles (except in the article by Zarechnak where the Georgetown University system is used.)

Аа	a	Бб	b	Вв	v	Гг	g	Дд	d
Ее	e	Ёё	ë	Жж	ž	Зз	z	Ии	i
Йй	j	Кк	k	Лл	l	Мм	m	Нн	n
Оо	o	Пп	p	Рр	r	Сс	s	Тт	t
Уу	u	Фф	f	Хх	x	Цц	c	Чч	č
Шш	š	Щщ	šč	Ъъ	"	Ыы	y	Ьь	'
Ээ	é	Юю	ju	Яя	ja				

THE FIRST DECADES OF MACHINE TRANSLATION OVERVIEW, CHRONOLOGY, SOURCES

JOHN HUTCHINS

1 *Overview*

The use of computers for the translation of natural languages was probably the first application of the newly invented electronic computers to non-numerical tasks, and it was certainly the first application in what was later to be known as computational linguistics. It was also one of the first areas of research in the field of artificial intelligence. For these reasons, the contributions in this volume by and about pioneers in the field of ‘machine translation’ or ‘mechanical translation’ as it was often called in the early days (or ‘automatic translation’ as it was called usually outside the English-speaking world) are of considerable interest not only for the historical antecedents of this particular area of research and development¹, but also for the beginnings of computational linguistics, natural language processing, linguistic aspects of artificial intelligence, and much more.

Pioneers in machine translation (henceforth abbreviated as MT) came from a wide variety of backgrounds: electrical engineering, physics, linguistics, interpretation, philosophy. They came armed with a variety of motives and approaches to the problem. For some it was a practical task, and the aim was to produce a system capable of producing some kind of ‘useful’ translation, however deficient in terms of accuracy and style. For others it was an intellectual challenge, the formalization of a pre-eminently difficult human activity, with the prospect of revealing the workings of language and the human mind. For some it was part of the popular cybernetics and ‘information theory’ movements initiated in the mid 1940s by Norbert Wiener, Claude Shannon, and others. The link was particularly strong in the Soviet Union, where cybernetics was part of the liberation of science from the stultification under Stalin. For some it was a major testbed for structural and formal linguistics, founded at first on the language interests of logicians such as Rudolf Carnap and Hans Reichenbach and later on the work of Ferdinand de Saussure, Leonard Bloomfield, Otto Jespersen, Charles Fries, Zellig Harris,

¹ The main sources for the history of MT (and for this overview) are listed in the third section of this introduction.

and (to a lesser extent) Noam Chomsky. For others the data of language had to be analysed in new ways for treatment by computers, as the existing formulations and categorizations were found grossly inadequate – leading to the creation of an ‘applied linguistics’ (later called ‘computational linguistics’), where the capacity of computers for statistical analyses was a major benefit. From the beginning, the outstanding problem was ‘meaning’, or more precisely the differences between how different languages expressed the same objects, ideas and concepts. For some it was a question of expanding (and formalizing) dictionaries, with differences of syntactic structure relegated to secondary operations. For others it was a question of exploring the internal semantics of the human mind itself, with translation as an operation involving ‘atomic’ (perhaps ‘universal’) meaningful units, or of investigating the basic thought processes underlying communication.

In the earliest period, the question of what constituted an intermediary language (‘interlingua’) and how it might be created preoccupied many researchers. It was closely related in the minds of many at the time with what was seen as parallel activity in the field of information retrieval (the use of computers to identify and access documents relevant to a particular query) towards a universally applicable ‘information language’.

With such close interests in and by other scientific disciplines and with such widespread public interest in the potentialities of the ‘electronic brains’ (as they were often called), it is not surprising that presentations of MT took place at a wide range of conferences, wherever there was interest in the use of computers for exploring language and communication (e.g. conferences on cybernetics, information retrieval, linguistics, etc.), and that statements about the immediate prospects of working systems attracted publicity which was not always welcomed by those in the field.

In time, the practicalities of obtaining useful translations (whatever the initial methodology) drew attention to the limitations of dictionary-based systems and to the importance of analysing and transforming syntactic structures. From the early 1960s onwards, syntax was the principal focus of nearly all the MT groups. There was initial interest in the theories of Chomsky, but in time, parsers for syntactic structure analysis developed independently of the dominant developments in theoretical linguistics – a prime example being the ‘predictive analyser’ developed by Rhodes at the US National Bureau of Standards (NBS) and further developed under Oettinger at Harvard. MT researchers tended to prefer the dependency approach of Tesnière (e.g. Hays, Mel’čuk) or multiple-pass approaches based on traditional grammar (e.g. Garvin, Zarechnak, Lehmann). At this time were proposed multiple levels of representation (morphological, surface syntactic, ‘deep’ syntactic, semantic or logical, and interlingual) in a stratificational framework (Lamb, Mel’čuk), and these ideas influenced other MT groups (Lehmann, Vauquois). Basic system

design moved away from the earlier ‘direct translation’ approach, which had built essentially on word-for-word lexical substitution and structure modification, e.g. Reifler, King, Panov, Bel’skaja. Overall design was tending towards a three (or more) stage approach involving largely independent processes of analysis, transfer, and synthesis (or generation) – seen earliest in the MIT group under Yngve.

The problems faced by pioneers were manifold and complex. Computers were for a long time limited in storage and speed, expensive to use and not widely available. Many of the earliest groups concentrated on manual and punched card ‘simulations’ with the intention of eventual computer implementation. In the case of some Russian groups, computers remained unavailable until the 1970s, and even then they were far behind American models in capacity and speed. For good reason, the field was known usually as ‘mechanical translation’ at the time. Input was cumbersome; texts had to be laboriously coded onto punched cards (most groups devised their own coding systems) – a recurrent demand of the time was for optical character readers, which seemed attainable in the short term but which was not in fact met until the 1980s. Output was in the form of reams of large sheets of computer paper, printed line by line in capital letters, often nearly illegible. Off-line storage was either on punched cards or on paper or steel (magnetized) tapes. The problems of dictionary storage and reasonably fast and accurate access preoccupied most MT researchers, and led to the development of procedures which are now taken for granted. Programming was performed in machine code, later ‘assembly language’, before suitable high-level languages were available in the 1960s. One of the first non-numerical programming languages was COMIT developed specifically for handling language data within the MT context – it preceded the ultimately more popular LISP language by some two years.

At the beginning, there were hopes that MT would be able to deal with the whole range of written texts – spoken language translation was also a dream of some – and there were serious suggestions for automatic translation of literary works. However, most realised that scientific and technical language was the only feasible area. There, it was believed, the problems of ‘multiple meaning’ would be absent or greatly reduced – most assumed that ‘microglossaries’ (specialized subject dictionaries) would solve most problems (e.g. Reifler). The aim in nearly all cases was translation indistinguishable from (or at least comparable to) human translation. Despite the arguments of Bar-Hillel from 1951 onwards about the unattainability of FAHQT (fully automatic high quality translation), it remained the unstated aspiration of many researchers. There had been, of course, from the beginning those who looked only for practical working systems producing ‘usable’ translations (e.g. Dostert, King); and it was assumed that, for the foreseeable future at least, there would have to be substantial human involvement in the revision (post-

editing) of output and perhaps also in the control (pre-editing) of input texts. However, there were also others who saw MT as essentially a long-term 'academic' objective, as the focus for basic linguistic or computational linguistic research (e.g. at Rand and by Lamb and Mel'čuk), and where practical applications were something for the more distant future.

The funding for MT came almost exclusively from government, military, and intelligence sources – the sources in fact for most computer developments in the 1950s and early 1960s before commercial systems began to be sold to universities and corporations. It is not surprising therefore that in both East and West MT research focussed on Russian and English – with inevitably some duplication of effort. But there were exceptions: Reifler began his research on German, the focus at MIT (Yngve) and Texas (Lehmann) was always German; at Georgetown, Brown investigated French; at Birkbeck (Booth) the languages were French and German, and at Cambridge (Masterman) and Milan (Ceccato) there were experiments with Italian and Latin. At a later date, there was also US interest in Chinese and Japanese, which came some years after earlier projects in China and Japan set up in the 1950s (e.g. at ETL by Wada and at Kyushu by Takahashi). However, the widest language coverage was always found in the Soviet Union (particularly in Leningrad), and in the 1960s, various MT groups (some very small and short lasting) were set up in France, Germany, India, China, Mexico, etc. introducing a much greater spread of languages (cf. Hutchins 1986).

In comparison with today, MT was an 'academic' affair. Research was conducted mainly at universities and research institutes with traditions of scientific openness. Communication was maintained primarily by the exchange of internally produced reports and by participation in conferences, although both were inhibited by political and intelligence considerations. Direct collaboration between groups was weak, partly because of the variety and divergence of basic methods and approaches – indeed the variety was encouraged by the funding bodies who did not see duplication of effort as wasteful, as long as the research was fruitful.

Progress was much slower than most had expected; claims for imminent operational systems were increasingly seen as unjustified (even fraudulent); and in the United States, the situation was investigated by a committee (the now well-known ALPAC), which recommended the end of support for MT; but even before its report in the mid 1960s, some groups had been dispersed, disillusioned in the face of the seeming intractability of dealing with core problems of syntax and semantics. For more than a decade afterwards, MT was regarded as having been a 'failure'. Its revival came during the 1980s and 1990s, when the original 'Cold War' incentives were replaced by more pressing political and commercial demands (e.g. from the European Union and from the multi-national corporations). After many years of practical use and

further fundamental research in computational linguistics, MT is now seen in a different light: it is accepted as a practical aid producing ‘less-than-perfect’ translations, either for the large-scale production of multilingual technical documentation, with more or less substantial human involvement at many stages of the process, or for the rapid (often on-line) production of rough versions for immediate (real-time) communication (e.g. on the Internet) – all on scales and in contexts unimaginable when research began in the 1950s.

2 *Chronology*

The following paragraphs provide a brief synopsis of events in the history of MT (and of some related areas), which are recounted more fully in articles in this collection and in the sources listed below.

2.1 *Before the computer*

It is possible to trace ideas about mechanizing translation processes back to the seventeenth century in connection with ideas on ‘real characters’ and ‘universal’ or ‘philosophical languages’ (Descartes, Leibniz, Wilkins, etc.), but realistic possibilities came only in the 20th century. In the mid 1930s, a French-Armenian Georges Artsrouni and a Russian Petr Trojanskij applied for patents for ‘translating machines’. Of the two, Trojanskij’s was the more significant, proposing not only a method for an automatic bilingual dictionary, but also a scheme for coding interlingual grammatical roles (based on Esperanto) and ideas for analysing sentences and generating texts in other languages (Hutchins and Lovtskij, in press).

2.2 *First pioneers: 1947-1954*

Neither of these precursors was known to Andrew Booth (London University) and Warren Weaver (Rockefeller Foundation) when they met in 1946 and 1947 and put forward the first tentative ideas for using the newly invented computers for translating natural languages. Weaver’s attempt to interest the cyberneticist Norbert Wiener failed, but during 1948 Booth worked with Richard H. Richens on a punched card system for dictionary-based ‘translation’ with some morphological analysis. By this time, the idea of mechanical translation (as it was known almost invariably in the period up to the early 1960s) had occurred independently to a number of people (see Hutchins 1997b). In July 1949 Weaver put forward specific proposals for tackling the obvious problems of ambiguity, based on his knowledge of cryptography, statistics, information theory, logic and language universals. His memorandum stimulated others to undertake research: Abraham Kaplan (Rand), Victor Oswald (UCLA), and, in particular, Erwin Reifler, later to lead the MT group at the University of Washington.

Then, in May 1951 came the appointment of the ‘first MT researcher’, the Israeli philosopher-logician Yehoshua Bar-Hillel. During that year he visited all those interested in the subject and wrote a state-of-the-art report, in which he outlined some of the basic approaches to MT questions. In June 1952, he convened the first MT conference (at MIT), which was attended by nearly everyone already active in the field. Ideas for pre-editing and post-editing, for microglossaries, and for syntactic analysis were proposed, as well as suggestions for future activity (Hutchins 1997a). In particular, Léon Dostert from Georgetown University, who had come as a sceptic, argued that what was required was a demonstration of the feasibility of MT in order to attract research funding. On 7th January 1954, at the IBM headquarters in New York, a small-scale demonstration of a Russian-English system was given – the joint effort of Peter Sheridan of IBM and Paul Garvin at Georgetown University (Hutchins 1994).

2.3 *Age of innovation and enthusiasm: 1954-1959*

Despite its limitations the demonstration attracted a great deal of interest from both the general public and the funding agencies, not just in the US but worldwide. Already in 1954 the MIT group led by Yngve received funding from the National Science Foundation (NSF). After seeing a demonstration of the IBM-Georgetown system – while on a visit to US computer installations – the Russian Dmitrij Panov initiated MT research in the Soviet Union, at his own Institute of Precision Mechanics and Computer Technology (ITMiVT). Later the same year MT research began also under A.A.Ljapunov at the Steklov Mathematical Institute (MIAN) and under A.A.Reformatskij at the Moscow Institute of Linguistics. In March 1954, the first journal in the field (*Mechanical Translation*) was founded at MIT by William N.Locke and Victor Yngve, and the next month saw the first PhD thesis (at Harvard by Anthony Oettinger).

From this time onwards, activities in the field increased and expanded rapidly. In January 1955, a number of MT people (among them Bar-Hillel, Léon Dostert, Kenneth Harper, Erwin Reifler, and Victor Yngve) spoke at the Conference on the Practical Utilization of Recorded Knowledge, held at Western Reserve University, Cleveland, Ohio. During 1955 Andrew D. Booth received his first grant from the Nuffield Foundation for research at Birkbeck College, and he edited with Locke the first book-length MT publication, a collection of articles based in part on the 1952 MIT conference (Locke and Booth 1955). In this year, research began at a number of other centres: at Ramo-Wooldridge, by Don R. Swanson; at Michigan, by Andreas Koutsoudas; in Milan, by Silvio Ceccato; and at the Cambridge Language Research Unit (CLRU) under the leadership of Margaret Masterman. In September 1955 the

third international conference on Information Theory was held in London, and included presentations from Booth, Ceccato, and Yngve.

During 1956, grants were given by the USAF to support research by Reifler and others at the University of Washington to build a Russian-English dictionary for Gilbert King's photoscopic store developed at the International Telemeter Corporation. In the same year, the Georgetown project under Dostert received its first funds from the CIA (via the NSF), and funds went also from NSF to expand Oettinger's work at Harvard University. In October, Panov, Ljapunov and Muxin presented the first results of Soviet research to a session of the USSR Academy of Sciences, and the first Russian publications appeared (in *Voprosy Jazykoznanija*, by Kuznecov, Ljapunov and Reformatskij, and by Kulagina and Mel'čuk). In the same month, *Babel* (the international journal for translators) devoted a whole issue (vol.2 no.3) to MT, including papers by Booth and Brandwood (Birkbeck), Heberden (Milan), and Yngve (MIT). Also in October, MIT convened the first 'international' conference devoted specifically to MT (with contributions from Reifler, Koutsoudas, Dostert, Yngve, King, Harper, and a report of Soviet work from the sole Russian participant, Panov) – abstracts were printed in *Mechanical Translation* vol.3, no.2 (November 1956). It may be noted also that Artificial Intelligence, one offspring of cybernetics, was 'officially' founded at Dartmouth College in September 1956.

In 1957 there were further grants to support US groups, at Ramo-Wooldridge under Swanson from the USAF, and at Rand under David G. Hays from the USAF (on the basis of research stretching back to 1953). The CLRU was also the recipient of US funds (from NSF). In the same year another Russian group was set up at the Thorez Institute for Foreign Languages under Viktor Ju. Rozencveijg, who also initiated the Moscow-based Association for MT Problems (*Ob''edinenie po problemam mašinogo perevoda*). A Czech group was formed at the Charles University, Prague, and in Japan the Electro-Technical Laboratory (ETL) in Tokyo began research under Hiroshi Wada. The main conference of the year was the Eighth Annual Round Table on Linguistics and Language Studies at Georgetown University in April, which was devoted entirely to MT (Dostert 1957). Towards the end of the year, Peter Toma demonstrated a system at California Institute of Technology which aroused considerable interest, and the COMIT programming language (the first for symbolic non-numerical processing) was completed at MIT. The most significant external event with impact on MT was the launch of the first man-made satellite (sputnik) in October 1957, which alerted the US to the technical progress of the USSR, and gave added impulse to the quickening pace of MT research in both East and West.

In May 1958, the Russians held their first MT conference (Moscow) with 340 participants from 50 institutes and universities, and illustrating a huge

range of languages being studied for possible MT systems (Tezisy 1958). Further evidence of the scale of Russian research was revealed in September at the Fourth International Congress of Slavists in Moscow (attended by Lew Micklesen from Reifler's group). In this year, a MT group was established in Leningrad under Nikolaj Andreev, devoted in later years to work on interlinguas and on statistical analyses of language, and there were also various MT groups set up in China with close USSR contacts. In the US Peter Toma joined the Georgetown group, and Adriane Lukjanow left it to found her own company; and further research groups were formed by Harry Josselson at Wayne State University, by Sydney Lamb at University of California Berkeley, and, after a seminar in October, by Winfred Lehmann at Texas University.

During the latter part of 1958 Bar-Hillel conducted a survey of MT and in February 1959 he distributed his report. It was highly critical of the (in his view) mistaken perfectionist aims of most MT groups, and he advocated the less ambitious development of translation aids. There were three major conferences in 1959. First, the Seminar on Mathematical Linguistics in Moscow (in April), devoted largely to MT (Tezisy 1959); second, the Unesco conference on Information Processing in Paris in June (UNESCO 1960), which included a session on MT with papers from Panov and Bel'skaja (ITMiVT), Takahashi and Wada (ETL, Japan), Oettinger (Harvard), Yngve (MIT), Parker-Rhodes (CLRU), Harper and Hays (Rand), and where A.F.R. Brown gave a demonstration of his Georgetown French-English system; and third, the International Conference for Standards on a Common Language for Machine Searching and Translation (at Cleveland, Ohio) in September, which included papers on MT by Booth, Ceccato, Micklesen, Pačak, Parker-Rhodes, Reifler, Yngve, and from Russia by Andreev, Kulagina, and Ivanov (Kent 1961)

During 1959 Gilbert King transferred his research activity to IBM, and gave the first of many demonstrations of the essentially word-for-word system based on the photoscopic store; from now on it produced translations for the Foreign Technology Division (USAF). Another important demonstration was that of the Georgetown Russian-English system at the Pentagon in June.

The Unesco conference was the main inspiration for the establishment of ATALA in France (Léon 1998) – its journal *Traduction Automatique* appearing first in April 1960. New groups began also at the National Bureau of Standards (NBS) under Ida Rhodes, to develop her predictive syntactic parser, and in the UK at the National Physical Laboratory (NPL) for a Russian-English system. On the computer front came the important first developments of time sharing, and the launch by IBM of its 1400 series and 7070 and 7090 computers.

In February 1960, the most important US conference since 1956 was held at UCLA (Edmundson 1961), with papers from all active US groups. The greater emphasis on syntactic analysis marked a change of focus from the earlier concentration on lexicographic approaches. There was evidence also of

growing rifts between theoretical and practical orientations, and doubts about the possibility of good quality working systems in the near future. The major problem was now being recognized as semantic. At this conference working groups were set up for a proposed society (under David Hays), for the journal (*Mechanical Translation*) and for future meetings. The first result of the latter initiative was the conference for groups sponsored by US Federal agencies (NSF, CIA, DoD, USAF, ONR, US Army, US Navy) – i.e. embracing virtually all active US groups and some foreign ones (CLRU, Milan) – which was held in July at Princeton and organized by Harry H. Josselson of Wayne State University. Its success in encouraging improved exchanges of information led to further ‘Princeton-type’ conferences in following years. Elsewhere, the Russians held a national conference in Černovcy (Chernovtsy), Ukraine, with important contributions from all the leading researchers; and there were further signs of worldwide interest. Groups were set up in Mexico (National University, Sergio Beltran and Morris Swadesh), in Japan (Kyushu University, Tsuneo Tamati), in East Germany (Akademie der Wissenschaften, Erhard Agricola), and in France (CETA), with two groups in Paris (under Aimé Sestier) and Grenoble (under Bernard Vauquois).

In May and June 1960 a committee of the US House of Representatives held hearings on MT progress, with testaments from Dostert, Yngve, Lehmann, Oettinger, King, Josselson, and others, which largely endorsed current activity and proposed the establishment of a national MT production centre (US House of Representatives 1960). However, in the same year, Bar-Hillel’s critical report received wider circulation in *Advances in Computers* (now expanded by additional information about Russian groups); it was perhaps the first indication beyond MT circles that prospects were not bright for automatic production of good quality translations – in particular his demonstration of the ‘nonfeasibility’ of FAHQT served to persuade many (then and in later years) that MT was a ‘failure’ (Bar-Hillel 1960). In the wider sphere, notable advances in computing during 1960 were the introduction of ALGOL (a major influence on Vauquois at CETA in France), COBOL and LISP.

2.4 *Age of growing sophistication and disillusion: 1960–1964*

As the 1960s progressed there was increasing disillusion, the expected benefits of syntactic analysis did not materialize, the problems of semantics became increasingly obvious, the quality of MT output was not improving, Bar-Hillel’s pessimism was shared by a growing number of researchers, and research groups began to be either disbanded or diverted to other NLP areas.

Outwardly, however, everything continued as before. In 1961 new groups were being founded: in Nancy (France), under Bernard Pottier, and at Ohio State University under William S-Y. Wang for studying Chinese MT. In January 1961, a large conference on MT and NLP was held in Moscow, and in

April the second ‘Princeton-type’ conference was held in Georgetown. In September, the (British) National Physical Laboratory organized the largest international MT conference so far, at Teddington near London, attended by 170 researchers from 15 countries, although only one (Kulagina) was from the Soviet Union. The emphasis on general syntactic problems and methods, evident at UCLA in February 1960, was even clearer, and there was further evidence of growing doubts during the discussion periods (NPL 1962). As the title of the conference indicated, MT research was expanding beyond translation into the wider field of ‘applied language analysis’. After the NPL conference, the CLRU held a colloquium (King’s College, Cambridge) devoted to semantic issues in language, i.e. also going beyond MT as such (CLRU 1962). However, there was now more promising developments in the practical utilization of MT: in December, the Georgetown system was demonstrated at Euratom (Ispra, Italy), and the system installed.

The next year saw more conference activity. A conference on MT and mathematical linguistics in March in Budapest, Hungary (March 1962), resulted in the establishment of a group at the Computing Centre of the Hungarian Academy of Sciences led by Ferenc Kiefer. In June 1962 the third ‘Princeton-type’ conference took place, again at Princeton, during which the already long-desired MT association was formed, called Association for Machine Translation and Computational Linguistics (AMTCL) – the addition of the words ‘computational linguistics’ (later also attached to the journal *Mechanical Translation*) was already a sign that perceptions were changing. A month later, NATO organized a summer school in Venice with contributions from leading researchers: Yehoshua Bar-Hillel, A.F.R. Brown (GU), Silvio Ceccato (Milan), Lucien Dupuis (CETA Paris), Maurice Gross (MIT), David Hays (Rand), A.F. Parker-Rhodes (CLRU), Ida Rhodes (NBS) and Bernard Vauquois (CETA Grenoble). The lectures were as much, if not more, about general methods of processing natural languages by computer (computational linguistics) than about the summer school’s title, the ‘automatic translation of languages’ (NATO 1966). The last important conference of the year was the second Information Processing conference, held by IFIP in Munich, which included a session on MT chaired by Oettinger.

The year 1963 saw the first signs of the turning tide: the CIA withdrew its funding of the Georgetown group, for reasons still not clear, and in October, the NSF requested the setting up of a committee (ALPAC) to investigate the current state of MT and to consider whether research support from US funding agencies should continue. The newly formed AMTCL held its first annual meeting (August 1963, Denver), with many contributions (see *Mechanical Translation*, vol. 7, no.2) focussed more on NPL in general than on MT per se. In the Soviet orbit the most significant event was the Fifth International Congress of Slavists in Bulgaria, with many MT contributions.

The ALPAC got underway in April 1964 under the chairmanship of John Pierce from Bell Labs. Although membership included David Hays and Anthony Oettinger, most of the committee was from outside the field (Hutchins 1996). Ironically (in retrospect) it was in the same month that the first joint US-Japan seminar on MT took place in Tokyo (April 20-28) – recognition that MT had a role beyond Russian and European languages. Equally significant was that in Germany Peter Toma began the development of what was to become the Systran system. The second Georgetown system was installed at the Oak Ridge National Laboratory, and the IBM-USAF Translator was demonstrated to the general public in its Mark II version at the New York World Fair.

In 1965, the fourth ‘Princeton-type’ conference took place in Las Vegas, and the second US-Japan seminar on MT was held. More significant for the future was the first of the international computational linguistics (‘Coling’) conferences, held in New York (May 1965), and the formation of the International Committee on Computational Linguistics (ICCL) – including many MT researchers – for the organization of future conferences. It was the definitive sign that computational linguistics had become fully independent from its parent MT.

This year too, there was the first sign of MT activity in Canada: the Canadian National Research Council set up the Centre de Traitement Automatisé des Données Linguistiques (CETADOL) at Montreal under Guy Rondeau, with MT investigation as a major task; and Kathleen Booth began MT research at Saskatchewan University. Finally, as an indicator of the times, at Computer Technology Inc. in the US, Bernard Scott started research on an English-Vietnamese system (ancestor of the current Logos systems).

2.5 The ALPAC report and its impact: 1966-1975

In November 1966 the ALPAC report appeared (ALPAC 1966), with its recommendation that support for MT in the US should be discontinued. Its conclusions were already known to most researchers, and many groups had already been disbanded or had changed direction (e.g. MIT, Harvard, Berkeley). Even in Russia, where conditions were quite different, the authorities began to withdraw support, on the argument that if the Americans with their much more powerful computers could not succeed then there was little chance of the Russians achieving success.

The AMTCL conferences in July 1964 (Bloomington, Indiana), in May 1965 (New York, as the first Coling conference), in July 1966 (Los Angeles) and in April 1967 (Atlantic City) had already been devoted almost exclusively to non-MT subjects. It was no surprise that at its meeting in Urbana in July 1968 the association dropped machine translation from its name and became the Association for Computational Linguistics (ACL). Researchers were now keen to distance themselves from the ‘stigma’ of MT. At later ACL

conferences and at the second and third Coling conferences (Grenoble, August 1967, and Stockholm, September 1969) papers on MT research were almost completely absent.

However, MT development did continue, e.g. the USAF funded research at Texas until 1968 and the ONR funded the Wayne State group until 1972. As for operational systems, in late 1969 the Systran system replaced the IBM-USAF Translator Mark II at FTD for Russian-English translation, in the Soviet Union the English-Russian system for patent documentation (under development since 1963) came into operation at the USSR Central Patent Office in Moscow; and two other subject-restricted systems began to be used in the following years: TITUS, developed by the Institut Textile de France, in 1970 for translating stylized abstracts between four languages; and in 1972, the CULT system (Chinese University of Hong Kong) for translating mathematical texts from Chinese. On the research front, Yorick Wilks began in 1970 at Stanford University to investigate a semantics-based approach to MT; and in the same year, a MT group was formed at Brigham Young by Eldon G. Lytle, which ultimately led to the first microcomputer systems (ALPS and Weidner) in the early 1980s.

The first public signs of the revival of MT after ALPAC came in the mid 1970s. In 1974 Systran was used by NASA for translation work during the Apollo-Sojuz project, and in the Soviet Union, the All-Union Centre for Translation was established with as one of its functions the centralization of MT research (under Jurij Marčuk). Then in June 1975 demonstrations of Systran in Bonn, Zürich and Luxembourg resulted in the adoption of Systran by the European Commission as the basis for its MT operations. And next year, in 1976, the Météo system developed by the TAUM group in Montréal (which had emerged from CETADOL in 1970) was installed to translate weather reports – the first fully automatic ‘sublanguage’ system. From now on MT was no longer a subject of (perhaps suspect academic) theoretical research, it was something which could be of practical assistance, and the revival of MT began.

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WARREN WEAVER AND THE LAUNCHING OF MT BRIEF BIOGRAPHICAL NOTE

JOHN HUTCHINS

The memorandum written by Warren Weaver in 1949 is perhaps the single most influential publication in the earliest days of machine translation. Written before most people had any idea of what computers might be capable of, it was the direct stimulus for the beginnings of research in the United States.

1 *The background*

Warren Weaver was born on the 17th July 1894 in the small Wisconsin town of Reedsburg, into a family descended from German 18th century immigrants. Fascinated by all things mechanical and electric – a lifetime hobby was the construction of radio sets – he seemed destined for an engineering career, and he entered the University of Wisconsin in 1912 to study civil engineering. However, he became fascinated by differential calculus and probability, and after graduating from Wisconsin (B.S, 1916) he was appointed to teach mathematics at the newly established Throop College, Pasadena, California (later California Institute of Technology.) After wartime work for the Bureau of Standards in Washington, and returning briefly to Caltech, he was invited in 1920 back to the University of Wisconsin, where he became an enthusiastic teacher and eventually chairman of the department.

In late 1931 he was invited to apply for the directorship of the Natural Sciences Division of the Rockefeller Foundation, the post he was to fill with great distinction for the next 27 years. Among his first actions was the inauguration of a programme to support quantitative experimental biology and molecular biology, not just in the United States but in Europe and elsewhere, with crucial and far-reaching consequences (Weaver 1970).

During the War, at the invitation of Vannevar Bush – the pioneer of the mechanical and electronic analogue calculator – Weaver joined the Office of Scientific Research and Development, first to head the “fire control” section and then a new agency, the Applied Mathematics Panel (AMP), directing the work of several hundred mathematicians on operations research. In this position, he had close contacts with many of the American computing pioneers, such as Howard Aiken, George Stibitz and John von Neumann. Indeed, it was at Weaver’s invitation that von Neumann wrote for AMP a report that

represented an important step towards the formulation of the ‘blueprint’ for all future computers (Aspray 1990:240).

After returning to the Rockefeller Foundation in 1945, he inaugurated and carried through a globally important program of agricultural research in Central and South America, India and the Philippines. At the same time, he had a major impact on the development of US science through membership (often chairman) of bodies such as the Naval Research Advisory Committee, various boards of the War Department, the National Science Foundation and the American Association for the Advancement of Science (the latter as president in 1952). He collaborated with Richard Courant in plans for strengthening advanced mathematics research in the United States, and the establishment of the Courant Institute of New York University (its main building is named Warren Weaver Hall.) A report he wrote at the end of the war (“Comments on the general theory of air warfare”) was a significant factor in the foundation of the RAND Corporation.

Throughout his career, Weaver was very active in the dissemination of science, both to students and the general public. At Wisconsin, he collaborated with his teacher and later colleague, Charles Sumner Slichter, in the third edition of a mathematics textbook (Slichter 1925), and with Max Mason on a textbook of electromagnetism (Mason and Weaver 1929). After the war, his now best-known collaboration was with Claude Shannon on the first book on ‘information theory’ – he wrote the non-technical introduction which contributed much to the success of the book (Shannon and Weaver 1949), and he also wrote an article for *Scientific American* (Weaver 1949b). Later he had even greater success with his popular book *Lady Luck* on his own speciality, the mathematics of probability (Weaver 1963). He was the author of many articles of popular science (collected as Weaver 1967b) and editor of a collection of introductory articles by eminent scientists (Weaver 1947). Finally, in his retirement, he was sponsored by the American Academy of Arts and Sciences to prepare a book on United States philanthropic foundations (Weaver 1967a), and at the same time he wrote his own autobiography (Weaver 1970).

Weaver had a particular fascination with Lewis Carroll’s *Alice’s Adventures in Wonderland*, and over the years built up a collection of translations of this classic of children’s literature. He wrote a number of articles on the book and its author, including, as might be expected, one on Charles Dodgson (Lewis Carroll) as a mathematician, and finally a book on the problems of translating *Alice*, with a bibliography of translations based on his own collection (Weaver 1964).

2 *The memorandum*

Weaver had first mentioned the possibility of using the computer to translate in March 1947 in a letter to the cyberneticist Norbert Wiener and in a

conversation with Andrew Booth, a British x-ray crystallographer who was visiting various locations in the United States where computers were being built. On his return to England, Booth worked on ideas for a mechanical dictionary and collaborated with Richard H. Richens in a punched card experiment for producing word-for-word translations of scientific abstracts (Richens and Booth 1955). By 1949, the idea of MT was occurring to others, and so, having been urged by colleagues at the Rockefeller Foundation, Weaver elaborated his ideas in a memorandum (Weaver 1949) sent to some 20 or 30 acquaintances (Weaver 1970: 106). His aim was to suggest more fruitful methods than obviously limited word-for-word approaches, and he put forward four proposals, based mainly on the new theories of cybernetics and information theory. (For more details see Hutchins 1997).

The first proposal was that the problem of multiple meanings might be tackled by the examination of immediate context, i.e. the words to left and right of an otherwise ambiguous word (e.g. *fast* meaning “rapid” or “motionless”). Weaver expected the amount of context required to vary from one subject to another, but he did think that “relatively few nouns, verbs and adjectives” were actually ambiguous, so he believed the problem was not large.

His second proposal was founded on the logical bases of language. He drew attention to the work of McCulloch and Pitts (1943) on the analogies between the neural structure of the human brain and ‘logical machines’, which suggested that “insofar as written language is an expression of logical character,” the problem of translation is formally solvable.

The third proposal concerned the possible applicability of cryptographic methods. Weaver had been impressed at the success of wartime cryptanalysis based, as he put it, 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.*” His suggestion was linked to the statistical ‘information theory’, recently advanced by Claude Shannon, which embraced “the whole field of cryptography”.¹

His fourth proposal was based on the belief that underlying the statistical regularities of languages were logical and linguistic universals² constituting “the real but as yet undiscovered universal language”, and which suggested an easier route than translation directly from one language to another. His memorable analogy was that of people attempting to communicate by shouting from tower to tower rather than going down to their common basements. He admitted that this approach would involve a “tremendous amount of work.”

¹ Shannon had been the author of one of the most influential papers on cryptography, a wartime classified report (1 September 1945), not published until October 1949 (Shannon 1949), but seen by Weaver before its declassification, as he stated in the memorandum.

² Weaver cited remarks by Hans Reichenbach on common logical structures of grammars and by Erwn Reifler on semantic convergences between unrelated languages. (Hutchins 1997).

The memorandum met with scepticism among some recipients, and enthusiasm among others, including Erwin Reifler (University of Washington) and Abraham Kaplan (Rand), who both began investigations. It was a major stimulus to research activity, thanks in large part to Weaver's personal standing and influence. It led to the appointment at MIT of Bar-Hillel and the convening of the first MT conference (Hutchins 1997). The outcome of the conference was the first book-length treatment of MT, to which Weaver contributed the foreword (Weaver 1955). In this, his last contribution to the new field of MT, he states his optimism for the "new Tower of Anti-Babel" under construction "not to charm or delight, not to contribute to elegance or beauty; but to be of wide service in the work-a-day task of making available the essential content of documents in languages which are foreign to the reader." And so it has proved.

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4 *Other references*

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ERWIN REIFLER AND MACHINE TRANSLATION AT THE UNIVERSITY OF WASHINGTON

LEW R. MICKLESEN

Prof. Erwin Reifler richly deserves the title of a “pioneer” in the field of machine translation (MT). When he became aware of Warren Weaver’s seminal “Memorandum” of 1949, the notion of MT captured his very active imagination. He literally hit the ground running, and he fired off a series of his own memoranda exploring various basic aspects of this new area for linguistic and engineering investigation. Let me supply a reasonable amount of biographical data and attempt to demonstrate how his varied intellectual interests may have led him to MT.

Erwin Reifler was born (June 16, 1903), raised, and educated in Vienna, Austria. He completed the Gymnasium in 1922 and at the same time studied Modern Greek in the Oriental Academy there in Vienna. In 1931 he received the degree of Doctor rerum politicarum from Vienna University. Here he was also associated with the Oriental Institute, where he studied Modern and Literary Chinese. The subject of his doctoral thesis was “The State and Government in Ancient China”. In spite of his keen interest in languages he emerged from his advanced studies with a degree in political science. His fascination with languages remained, however, the center of his activities. During his formal study of Chinese at Vienna University he had even begun informal private studies of the language under the guidance of a certain Rosthorn, a former Austrian ambassador to China, who had returned to Vienna with an extensive library of Chinese books. Reifler was permitted access to these books. In his early years he also taught himself Classical Hebrew, and he read carefully and critically the Hebrew Bible. His critical reading of the Bible was to lead him in later years to his groundbreaking work in metrology that he carried on simultaneously with his interests in MT.

Reifler’s study of the Chinese language introduced him to the Chinese community in Vienna, and he made valuable contacts from which he benefited greatly not only in China but also in Austria and Germany. Even during his university years he was a librarian from 1924 to 1927 in the Chinese Library in the Imperial Palace in Vienna. In 1927-28 he worked in Berlin as an editor for the Chinese News Agency for Europe. In 1928-30 his command of Chinese and his contacts gained him membership on three Chinese Government

committees visiting Germany and Austria. In 1930-31 he functioned as interpreter for Chinese police officers undergoing training in the Police Academy in Vienna.

The year 1932 was to become something of a real watershed in his life. From July to September of that year he occupied the position of Assistant to the Austrian Advisor to the Municipal Government of Shanghai. That same year he became associated with Chiaotung University. There for the next five years he had the title of Professor of the German language. He mainly taught German to Chinese, but he also taught Chinese to various foreigners in that international city. One of his students was Henrietta Brown, whom he married in 1939. He was very effective as a teacher. Not only did he have an excellent command of Chinese grammar, but he also had developed useful mnemonic devices for the inculcation of Chinese characters. This pedagogical assault on the intricacies and vagaries of Chinese characters eventually led him to serious etymological studies of the characters.

This may be a good point at which to pause for a moment and underline the fact that Reifler was an indefatigable and enthusiastic scholar, who did not shy away from tackling huge problems. It seems quite possible that the first of such problems was the exotic Chinese language with its challenging writing system. And once he was immersed in the language itself, the vast mystery of the origin of the characters captured his attention. And, of course, such etymological investigations brought to light multifarious semasiological developments that suggested similar semasiological parallels in other languages of his acquaintance.

The Sino-Japanese War broke out in 1937. At one point early in the war in 1937 Reifler was on a ship returning to Shanghai from a trip to Japan. Because of fighting in the area the ship was unable to dock and was directed to Hong Kong, where he was forced to settle down. He easily resumed his academic activities in Hong Kong and was affiliated both with the University of Hong Kong, where he was a visiting professor of the German language, and with the Chinese Language School in the General Chamber of Commerce, where he was in charge of Chinese courses. Finally in 1940 he was able to return to Shanghai, where he taught as Professor of Philology (German and Latin) successively in the National Medical College and in the School of Pharmacy of the Sino-French University. Eventually he settled into the position of Professor of Sinology at l'Université l'Aurore, where he was in charge of the administration and teaching of Chinese courses.

The year 1947 proved to be another watershed in Reifler's career. A friend recommended him to the American Junior Chamber of Commerce in Shanghai, and he received funds to depart on a lecture tour of the United States from April to August. During this tour he attended a meeting of the American Oriental Society, where he met Prof. Wittfogel. The latter recommended

Reifler to Prof. George Taylor, who was building the remarkably stimulating scholarly complex, the Far Eastern Department, at the University of Washington. Reifler accepted an offer to join the Far Eastern Department as a visiting professor for the 1947-48 academic year, and he moved his family to Seattle early in the fall of 1947. The next year he became an associate professor in the Department. The rest of his active career was spent in the Far Eastern Department at the University of Washington until his premature death at the age of 61 on April 25, 1965. Throughout his teaching career he taught courses in the etymology of Chinese characters, Ancient Chinese, and Chinese grammar. I know that he was highly regarded by his students. The history of the development of Chinese characters and Ancient Chinese are closely related subjects and easily whetted his appetite for semasiological studies. The renowned linguist, Fang-kuei Li, used to comment that there was much speculation in Reifler's historical treatment of Chinese characters, but there is no doubt that many of Reifler's derivations were ingenious. I am sorry that he never published this material. He did mimeograph his extensive collection of derivations and made the material available to his students. When we worked together on MT in the latter 50s, he presented me with the complete set of derivations. I have treasured them to this day. He also had great success with his courses in Ancient Chinese. Here too he applied his knowledge and ingenuity to the decipherment of very difficult and laconic texts.

It was entirely natural that Reifler's research into the semantic aspect of the development of Chinese characters should turn his attention to similar developments in other languages within his purview. Some data of this sort were discussed by Reifler at the annual meeting of the American Philosophical Society in April 1948 in a paper entitled "The Chinese Language in the Light of Comparative Semantics". This paper was reported in *Science*, June 4, 1948 (vol. 107, page 586) and was seen there by Dr. Warren Weaver, Director of Natural Sciences, the Rockefeller Foundation. One of Reifler's examples of semasiological parallels was even mentioned by Weaver in his famous "Memorandum" about the possibility of MT (Weaver 1949). Weaver dispatched a copy of this memorandum to Reifler, and so began Reifler's intense preoccupation with MT in its very early years. The example quoted by Weaver is so important for Reifler's introduction to MT that I am going to elaborate upon it. Besides, it will give the reader a better idea of the Chinese phase of Reifler's scholarly activity. This example involves the etymological similarity between the English verb "to bow" and the noun "bow" (as in "bow and arrow"), on one hand, and the Chinese verb 射 (shè) meaning "to shoot" (e.g., an arrow) and another verb 謝 (xiè) meaning "to thank, to resign, or to wither", on the other hand. According to Reifler, then, the verb "to bow" developed because one bends his body like the noun "bow". The Chinese

character 射 (to shoot) is composed of two parts, 身, the modern form of the original pictograph 𠂔 (a human body) and 寸, the present form of the original 𠂔 (a hand, 手, with an extra finger 扌, suggesting pulling or drawing a bow). Our second character, 謝 is also made up of two parts, the already discussed 射 and an added radical 言 (from an original 言, suggesting something coming out 言 of a mouth 口, the whole signifying “speech”). Thus developed the meaning “to thank” in the sense of “shooting out speech”. The connection here with the English “to bow” resides in the possibility that, when one is to thank someone, he is apt “to bow”, bending his body like a bow. This character also means “to resign” or “to be dismissed” from an office or position. The connection with the semantic development in English lies in the notion that one might “bow out” of a position. As already noted, this same Chinese character may also signify “to wither”. Here it is quite reasonable to think of a withered flower as one that has “bowed out”. The most obvious conclusion here is that English “to bow” and the device “a bow” go back to one and the same word. And the Chinese “to shoot” and “to bow, to bow out” are closely related. In both languages the semantic development was quite parallel.

By this time it should be obvious that Reifler’s study of a plurality of foreign languages, his experience in translation and interpreting, his teaching of foreign languages, and his research in comparative semantics all more than piqued his curiosity when he received Weaver’s “Memorandum” in the summer of 1949. At the same time, however, these linguistic activities caused him at first to relegate the idea of mechanical translation to the realm of the impossible. But the idea of MT had struck home and lodged in his mind, and it appears that Reifler was the first to respond in writing to Weaver’s memorandum. From all I have read about the very early years of MT I would infer that Reifler’s influence in the first phases of the field here in America was second only to that of Yehoshua Bar-Hillel. He threw himself wholeheartedly and very seriously into this uncharted field of endeavor. Beginning in January of 1950 and extending through 1953 he produced a series of ten mimeographed papers formally entitled *Studies in Mechanical Translation* Nos. 1-10. Except for the two articles, *SIMT* No. 5, “Report on the First Conference on Mechanical Translation, June 17-20, 1952 at MIT” and *SIMT* No. 7, “The Mechanical Determination of the Constituents of German Substantive Compounds”, both published in the journal *Mechanical Translation* (Reifler 1954, 1955a), I have never seen the other mimeographed publications, but their content has been admirably summarized by John Hutchins (1997). These papers were also briefly outlined in *Mechanical Translation* Vol. 1, No. 1, March 1954. I assume that Reifler distributed these papers to parties interested in MT. Two of them were readied for publication but never distributed, *SIMT*

No. 9, "Elimination and Mechanical Determination of Forms of Dual Nationality" and *SIMT* No. 10, "A Short Report on the Present Stage in the Development of Mechanical Translation". By the time I appeared on the scene in late 1954 Reifler did not pass them all on to me either because he had abandoned some of the ideas in the articles or because the contents had been further developed elsewhere. I did see, however, Nos. 5, 7, and 8 on determining the constituents of German compound substantives and on operational form-classes, respectively. In those early years Reifler almost became the spokesperson for the field of MT in his various mimeographed articles exploring all possible ways of easing the task of the word-for-word translation mode of the day. For the source language he proposed human pre-editors (abandoned later by him), the selective capitalization of nouns, verbs, and adjectives, and encoding into an artificial language. For the translation process, he suggested the possibility of a pivot-language embodying any language universals to be discovered. For the target language he discussed the use of human post-editors (a term he himself coined) and the use of an adjusted grammar. In the latter concept he was obviously influenced by a colleague of his in the English Department of the University of Washington, Prof. Stuart C. Dodd, who advocated his Model English which utilized just one word order, one meaning for each word, and one form for each word (Dodd 1955). The two of them attended the first MT conference held at MIT in June 17-20, 1952. We are indebted to Reifler for his comprehensive report on the proceedings of this conference (Reifler 1954).

It is true that much of what Reifler wrote in those years, while it embraced really burning issues of the time, is today and was only a few years later, only of historical interest. I want to emphasize here, however, that I consider Reifler's *SIMT* No. 7, "The Mechanical Determination of the Constituents of German Substantive Compounds" as the crowning and lasting achievement of those years. This work was inspired by the experimentation of the English pioneers, Drs. A. D. Booth and R. H. Richens, as early as 1948 with the automatic dissection of words into their stems and endings (Richens and Booth 1955). Reifler's article was published later in 1955 (Reifler 1955a). It represents an exemplary and exhaustive treatment of a readily recognized problem in German: the presence in German texts of countless numbers of extemporized nominal compounds that, of course, can in no way be entered *in toto* in any dictionary. Reifler proceeded directly to the heart of the problem: the possible existence of so-called X-factors, where a letter or sequence of letters may belong to the preceding or following constituents of a given compound. In thorough fashion he lists and exemplifies all the theoretically possible types of nominal compounds with two constituents – 25 in all. He demonstrates that ten of these are linguistically impossible, that four of the remaining fifteen represent the same compounds leaving eleven all told. Two

of the eleven amount to one type in terms of his matching mechanism; therefore, only ten types remain. The two collapsed types are the only ones that happen to yield an ambiguous result on translation. That is, in only one case out of ten compound types will there be a nonunique translation of the two constituents. I quote his example “Senninschrift”, composed of Senn + the X-factor –in- + schrift. It must yield the ambiguous results 1) Senn + inschrift “Alpine herdsman’s inscription” and 2) Sennin + schrift “Alpine herdsman’s writing”. In addition, Reifler detailed the matching procedure for the mechanical recognition of the constituents of all nominal compounds demonstrating the advantages of right-to-left and left-to-right matching and tabulating the number of requisite matching steps. This was an excellent scholarly production and set the stage for any engineer to encode this material into the logical circuitry of a computer.

Another important *SIMT* well worth mentioning because it tackled some basic problems in MT is *SIMT* No. 8, “The MT Form-Class Filtering System”. It was not published, but the gist of its contents was included in his 1955 article, “The Mechanical Determination of Meaning” (Reifler 1955b). This article summarizes his thinking to date, but we are interested only in his discussion of form classes. He brings up the well-known problem of polygenetic meaning (multiple grammatical and nongrammatical meaning), and for the solution of this problem he establishes the category of operational form classes more or less similar to the traditional parts of speech. He uses the term “operational” form classes because of their pinpointing ability, i.e., their ability to interact with other form classes and aid in the resolution of multiple meaning. Here we are dealing with Reifler’s pinpointers and pinpointees. I am not sure whether these terms spread beyond the confines of the University of Washington, but we used them constantly. In this article Reifler was working with his native language, German; and he set up groups of form classes with large memberships vs. groups with small memberships and within each of these categories both paradigmatic and nonparadigmatic classes. He also pointed out that operational form classes may be identical to traditional parts of speech, may be more detailed, or may be more inclusive than traditional parts of speech. In those days he would assign the various operational form classes to different magnetic drums.

The spate of memoranda and articles in these early days earned Reifler research grants in 1952 and 1953. Like everyone else Reifler was eager to see how MT might work in practice. His grant in 1952 was supposed to support the simulated machine translation of a lengthy article in Dodd’s Model English into idiomatic Mandarin, Classical Chinese, Japanese, German, French, Russian, and Hebrew. This would have been an ambitious project, and I assume that it was engendered by Reifler’s feeling that general MT would require a universal grammar or an artificial exchange language. Apparently this

work did not materialize. I never saw it or heard Reifler mention it. The first MT conference that year and all the MT memoranda he churned out in 1952 kept him busy enough.

Another attempt to create a modest operational MT system was initiated by Reifler possibly in 1953. It was not mentioned until November, 1955 in *Mechanical Translation*, Vol. 1, No. 2, where it was characterized as a small German-to-English relay-type mechanical translator with a stored vocabulary of sixty representative German words and their English equivalents, each consisting of not more than seven letters. It was described as being under construction. This work was written up and illustrated as completed by a graduate student in Electrical Engineering, Geoff Douthwaite in the February 1956 issue of the *Washington Engineer* (Douthwaite 1956). The bulk of the engineering design was solved by another graduate student, Roy S. Waggoner. Besides matching the limited vocabulary of German words to their English equivalents, both stored on two separate drums, the machine could successively drop the last letters of unstored German plurals or compounds until either a match is obtained or the original German word is typed out. This work ostensibly began after Reifler had completed his article on the mechanical dissection of German compounds because he planned this pilot project to demonstrate the mechanical identification of the constituents of German compound substantives and to exemplify the direct transfer from the input to the output of words shared by both German and English. He was interested in reducing the stored vocabulary and in solving the problem of input words not found in the memory. One would have expected him to set up a pilot project for Chinese-to-English MT because in his report on the first conference on mechanical translation he recommended Chinese-to-English MT as an ideal pair of languages because of their highly regular nature in both morphology and syntax. Of course, however, he was primarily interested in testing the efficacy of his routine for the dissection of German compounds. It is important to call attention here to the fact that this pilot project brought Reifler into closer contact with the members of the Electrical Engineering Department of the University of Washington, Prof. W.Ryland Hill and graduate students, T.M. Stout and R. E. Wall, Jr., who had been conferring with Reifler even since 1952. T.M. Stout also worked on the German-to-English problem to the extent that he wrote an article entitled "Computing Machines for Language Translation" published both in *The Trend in Engineering* at the University of Washington (Stout 1954a) and in *Mechanical Translation*, (Stout 1954b). He discussed the language problems in general terms and then turned to engineering problems such as coding, input and output devices, storage, and dictionary search. I should re-emphasize that this limited project laid the foundation for years of valuable cooperation between the Far Eastern

Department and the Department of Electrical Engineering once the University of Washington MT project got under way.

Since some of Reifler's writings about German have arisen in recent paragraphs, I want to call the reader's attention to a very ingenious lexicographical scheme that he mentioned to me during our work on the Russian-English MT project but that he did not make public until the National Symposium on Machine Translation in 1961. He introduced this idea only at the very end of his contribution to Session 7: The Dictionary in a short article entitled "The Solution of MT Linguistic Problems Through Lexicography" (Reifler 1961c). Reifler was dealing here with what he termed "pseudo-idioms" like the expression "the basic idea" in English. Such expressions are extremely numerous in all languages, usually consist of several free-form constituents, and are not recorded in dictionaries because the meaning of the parts is intelligible to all readers. Most of such expressions are encapsulated in German in compound words. Indeed, the best translation of the above example in English is the German compound, "der Grundgedanke", which can be found in German dictionaries. Reifler made the reasonable suggestion that the idiomatic equivalents of such high-frequency concepts in whatever languages could be collected from German dictionaries in wholesale lots to improve the quality of machine translations and to obviate the search for such expressions in the literature. This was another one of his clever, practicable ideas for overcoming the difficulties of MT through lexicography.

We are finally approaching the era of the University of Washington Russian-English MT project, which came into being, I presume, by virtue of Reifler's many contributions to the field and by virtue of the participation of the Slavic section of the Far Eastern Department. I joined the Slavic section in the fall of 1953. In the course of that academic year I heard of Reifler's preoccupation with MT; and, I must confess, I was mightily skeptical about the success of such a venture. Finally, in the fall of 1954 I approached Reifler and spoke with him. I found him very receptive to my inquiries and very enthusiastic about the possibilities of MT. Before long I too became imbued with almost the same degree of enthusiasm especially because, as a senior scholar, he was highly supportive of my interest and encouraged me to engage in some work with Russian as a source language. The following year, 1955, I applied for and received a junior faculty grant for summer research, during which period I explored further Reifler's ideas about operational form classes from the standpoint of structural linguistics with some application to Russian. In this work I reiterated the requirement that all MT form classes should, if possible, contain only pinpointers, and I emphasized the point that MT form classes are realistic since they are based on a consideration of the total (grammatical and nongrammatical) meaning of linguistic forms (Micklesen 1956).

Our MT work at the University of Washington was sponsored by the Directorate of Intelligence and Electronic Warfare of the Rome Air Development Center, Air Research and Development Command. Their interest in sponsoring Russian-English MT research was undoubtedly spurred by four factors: the conditions created by the Cold War, the proliferation of scientific literature, the promise of almost unlimited storage capacity offered by a photoscopic disc under development at International Telemeter Corporation, and, as I have written above, the visibility of Reifler in the field aided by the facilities of the Slavic section of the Far Eastern Department. I believe that we established contact with representatives of the Rome Air Development Center in the fall of 1955. At the same time the Rome Air Development Center was supporting the work of Dr. Gilbert W. King at the International Telemeter Corporation. Dr. King had invented a photoscopic glass disc that offered high storage capacity and low access time, features that had great promise for MT (King et al. 1953). Negotiations between the University of Washington and the Air Force continued successfully until in June, 1956 we were awarded a subcontract to prepare a Russian-English dictionary for this photoscopic memory device and translation system under development at the International Telemeter Corporation in Los Angeles. The so-called Initial Project was funded by a grant of \$30,000. Prof. Reifler was the principal investigator and director of the entire project, work he performed most capably. Since MT had and has a rather narrowly circumscribed area of lexical interest – contemporary scientific prose, we decided to gather a basic vocabulary of general scientific terms and general-language terms likely to occur in scientific texts. There was no ready-made source for this kind of lexical material in Russian; so we chose 111 one- or two-page passages from 105 different texts representing 40 fields and subfields of science. In this way we acquired a wide variety of technical terms, many of which had multiple meanings both technical and nontechnical. To the technical meanings we assigned numerical codes referring to a synoptic chart for science and technology compiled by Prof. W. R. Hill. It featured nine columns for basic fields of science and nine rows for subfields of each science. These assigned codes were utilized at a later date for research into the automatic solution of multiple nongrammatical meanings. This chart proved not to be a panacea for relieving the clutter of target-language equivalents in our output. Later we discovered in actual practice that the synoptic table was somewhat limited by being only two-dimensional; two references axes were not sufficient. Besides, the field areas were defined historically rather than logically and did not provide a distinctive reference in all cases.

The 111 text passages amounted to about 30,000 running words; and their vocabulary, when augmented by items from some strategic word lists, totaled some 14,000 entries. Even though Russian is a highly inflected language, we did not resort to dissecting stems from endings but entered into

the dictionary *in toto* all paradigmatic forms discovered in the original Russian text passages. Let me explain this decision. We were commissioned primarily to provide a fairly sizable bilingual lexicon for the purpose of testing the storage capacity and access time of Dr. King's machine and secondarily to do some translating. Since at that time the possibilities for logical processing on the photoscopic disc seemed minimal, Reifler made the important decision to seize this opportunity to exploit lexicography and lexicographical procedures to solve as many grammatical and nongrammatical problems as possible. In other words, we embarked on a vast lexicographical exercise involving source-target semantics in terms of Russian and English. This lexicographical research distinguished our project from all other projects, where all kinds of morphological and syntactic research were being conducted for MT purposes. They were pursuing basic syntactic research for the purpose of eventually solving multiple grammatical meaning and perhaps some multiple nongrammatical meaning, while we were tackling directly multiple grammatical and nongrammatical meaning with a view to resolving the residual problems of meaning by later syntactic research. Our project was unique also in that we expected to use our word-for-word translations at various stages of our work as sources for research on the eventual removal of grammatical ambiguity and perhaps that of nongrammatical ambiguity. We envisioned building from time to time intermediate translation systems and at each stage feeding large quantities of source-language text material into the device and studying the output. We counted on these intermediate products as valuable research tools for the improvement of MT. Dr. King himself presumed that high-quality translations would only be achieved in successive stages, and we embraced enthusiastically the research and development pattern in our own work.

Lexicographical research, therefore, lay at the heart of our work. Since the photoscopic translation system did not have any logical accessories for the selection of appropriate target equivalents for ambiguous source-language forms, our research aimed at reducing the ambiguity of Russian words to only those equivalents in English that we deemed absolutely necessary for accurate intelligibility. I was in charge of the lexicographical work; but, of course, I conferred constantly with Prof. Reifler. I worked closely with eight members of the faculty and graduate students in the Slavic section of the Far Eastern Department. The work was very exacting and tedious but yet extremely critical. We were very fortunate to be associated with a group of very knowledgeable and talented people, who contributed everything to the success of the bilingual dictionary. The production of the English equivalents for the Russian words in the 111 sample text passages was a very complex process and required many steps. Each individual investigator translated into English the text passages assigned to him. He then transferred each Russian vocabulary

item to a card and recorded the target equivalent he had chosen and underlined it. To this target equivalent he added eclectic choices from Russian-English dictionaries at his disposal. He also recorded in coded form the distributional class to which the item belonged together with the proper inherent grammatical features and any English equivalents that might be associated with such grammatical features. He also recorded any noninherent grammatical features (situations where the form in question might interact with other forms in the same sentence as pinpointer or pinpointee). This information was intended for use when we finally engaged in logical processing. At this point we did permit ourselves a modicum of logical processing by recording separately readily translatable prefixes, suffixes, and constituents of compound words. The crux of the whole operation was the selection of appropriate target equivalents for ambiguous source-language words. We expended much effort in eliciting just one or two English equivalents that would at least suggest the usual English equivalents. To avoid as much clutter as possible in the output we set the limit of three equivalents for nontechnical meanings but no limit for technical meanings. The latter, of course, were supplied with subscript numbers taken off our Synoptic Chart for fields of science and technology. We also carded any genuine idiomatic sequences and pseudo-idiomatic sequences that occurred in our texts. Besides all this information we had to provide control symbols (to capitalize first letters, preserve spaces between words, etc.) and editorial symbols to aid the reader of the output. The carded entries (the source semantic units, control symbols, editorial symbols, and target-language equivalents) were then typed up on lists using special symbols for Russian letters with no obvious equivalent in the English alphabet and for those control and editorial symbols not found among the IBM symbols. These lists were sent to the Machine Accounting Department of the University for transfer to IBM punch cards. The IBM punch cards were then consigned to the engineers working on the project who converted all the information on the cards via the IBM 650 computer into a form suitable for transfer to the photoscopic disc.

It is quite apparent that the high-speed photoscopic memory with a permanent storage capacity at that time of 30×10^6 bits and a random access time of 0.05 seconds thrust upon us all this lexicographical work to the exclusion of any linguistic investigation of methods for the automatic parsing of sentences of the source language. This would have seemed to be detrimental to the interests of the linguists on the project, but the gratifying storage capacity of the photoscopic disc entailed many advantages in those days and swept us along into the thick of source-target semantics, the most daunting area of MT where we had to operate without benefit of any semantic theory. Very much the same situation exists even today; and, while I cannot speak for Prof. Reifler, I would be strongly tempted to follow the same line of research and development working from a similar but vastly more comprehensive bilingual