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TRANSLATION

Descriptive
Translation Studies
– *and beyond*

Gideon Toury

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DESCRIPTIVE TRANSLATION STUDIES AND BEYOND

GIDEON TOURY
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JOHN BENJAMINS PUBLISHING COMPANY
AMSTERDAM/PHILADELPHIA



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Library of Congress Cataloging-in-Publication Data

Toury, Gideon.

Descriptive translation studies and beyond / Gideon Toury.

p. cm. -- (Benjamins translation library, ISSN 0929-7316 ; v. 4)

Includes bibliographical references (p.) and index.

1. Translating and interpreting--Methodology. I. Title. II. Series.

P306.2.T68 1995

418'.02'01--dc20

95-9966

ISBN 90 272 2145 6 (Eur.) / 1-55619-495-1 (US) (Hb; alk. paper)

CIP

ISBN 90 272 1606 1 (Eur.) / 1-55619-687-3 (US) (Pb; alk. paper)

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John Benjamins Publishing Co. • P.O.Box 75577 • 1070 AN Amsterdam • The Netherlands
John Benjamins North America • P.O.Box 27519 • Philadelphia, PA 19118 • USA

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INTRODUCTION

A Case for Descriptive Translation Studies

In contradistinction to non-empirical sciences, empirical disciplines are devised to account, in a systematic and controlled way, for particular segments of the 'real world'. Consequently, no empirical science can make a claim for completeness and (relative) autonomy unless it has a proper *descriptive branch*. Describing, explaining and predicting phenomena pertaining to its object level is thus the main goal of such a discipline. In addition, carefully performed studies into well-defined corpuses, or sets of problems, constitute the best means of testing, refuting, and especially modifying and amending the very *theory*, in whose terms research is carried out. Being reciprocal in nature, the relations between the theoretical and descriptive branches of a discipline also make it possible to produce more refined and hence more significant studies, thus facilitating an ever better understanding of that section of reality to which that science refers. They also make possible the elaboration of *applications* of the discipline, should one be interested in elaborating them, in a way which is closer to what is inherent to the object itself.

Whether one chooses to focus one's efforts on translated texts and/or their constituents, on intertextual relationships, on models and norms of translational behaviour or on strategies resorted to in and for the solution of particular problems, what constitutes the subject matter of a proper discipline of Translation Studies is (observable or reconstructable) facts of real life rather than merely speculative entities resulting from preconceived hypotheses and theoretical models. It is therefore empirical by its very nature and should be worked out accordingly. However, despite incessant attempts in recent decades to elevate it to a truly scientific status, as the empirical science it deserves to become Translation Studies is still in the making. This is clearly reflected in that, among

other things, it is only recently that deliberate efforts have begun to establish a descriptive branch as an integral part of its overall program, i.e., as a vital link between successive phases of its own evolution as well as between the discipline itself and its extensions into our world of experience. Consequently, translation scholars still find themselves in a tight spot whenever they are required to put their hypotheses to the test, insofar as the hypotheses themselves are formed within the discipline to begin with, and not imported wholesale from other frameworks, be they even those regarded as “Voraussetzungswissenschaften für die Übersetzungswissenschaft” (Kühlwein et al. 1981: 15).

The main reason for the prevailing underdevelopment of a descriptive branch within Translation Studies has no doubt been an overriding orientation towards practical applications, which has marked – and marred – scholarly work ever since the sixties. Thus, whereas for most empirical sciences, including even Linguistics, such applications – important as they may be – are presented merely as extensions into the world, the immediate needs of particular applications of Translation Studies have often been taken as a major constraint on the formation of the theory itself, if not its very *raison d'être*. Small wonder that a scholarly framework geared almost exclusively towards applicability in practice should show preference for prescriptivism at the expense of description, explanation and prediction.

What the application-oriented variety of Translation Studies normally amounts to is an admixture of speculation, if not sheer wishful thinking, and research work pertaining to some *other* discipline which, for one reason or another, is considered more prestigious, sometimes just more fashionable, for a limited period of time. By contrast, it tends to shun research within its own terms of reference. In fact, many writers on translation still look down on studies into actual practices and their products, the more so if these studies are properly descriptive, i.e., if they refrain from value judgments in selecting subject matter or in presenting findings, and/or refuse to draw any conclusions in the form of recommendations for ‘proper’ behaviour. Somewhat paradoxically, it is precisely writers of this denomination who are also the first to lament the yawning gap between ‘theory’ and ‘practice’. Even though gaps of this kind are best bridged by taking heed of the full range of real-life behaviour (practice!), along with the factors underlying, and conditioning them (theory!), the lack of a truly descriptive-explanatory branch within Translation Studies has never really bothered these writers. Often quite the contrary. After all, this attitude spared them the need to justify their own preferences in the face of the fact that in real-life

situations, priority has often been given to quite different options. Not without reason, to be sure.

The practice of ignoring regularities of behaviour also made it easy to back one's claims with mere 'examples'. Recourse to randomly selected translation solutions has thus come to be associated with appliedly oriented writing, often unjustifiedly presented as 'theoretical', whereas writings of other denominations were given to severe criticism on account of a scarcity of examples; as if a handful of quotes torn out of their original co-texts and contexts can attest to anything at all. And, in fact, the main consideration underlying the selection, often even invention of an example was normally its *persuasiveness*, i.e., its alleged capacity to assist in driving a point home, rather than its *representativeness*. Consequently, standard behaviour was not merely overlooked either. At least by implication, it was also marked as downright unsuitable, thus requiring change.

All this is not to say that no attempts have been made to account for actual translational behaviour and its results. Quite the contrary. However, most descriptive studies have been performed within disciplines other than Translation Studies; e.g., Contrastive Linguistics, Contrastive Textology, Comparative Literature, *stylistique comparée*, or – in more recent days – Text-Linguistics, Pragmatics, or Psycholinguistics. Thus, while their subject matter could well have been deemed translational, the theoretical and methodological frameworks within which it was handled could not, if only because their interests lacked the wish to fully account for all that translation may, and does involve.

What is missing, in other words, is not isolated attempts reflecting excellent intuitions and supplying fine insights (which many of the existing studies certainly do), but a systematic branch proceeding from clear assumptions and armed with a methodology and research techniques made as explicit as possible and justified within Translation Studies itself. Only a branch of this kind can ensure that the findings of individual studies will be intersubjectively testable and comparable, and the studies themselves replicable, at least in principle, thus facilitating an ordered accumulation of knowledge. This is what the present book is about: Its main aim is precisely to tackle some of the main issues involved in establishing such a branch and embedding it at the very heart of the discipline as it grows empirical.

In many ways, the book is not just a sequel to, but actually a replacement of my programmatic *In Search of a Theory of Translation*, published some fourteen years ago and out of print for almost as long. In fact, I have long resisted all temptation to have that 1980 book published in a second edition, a temptation which has recently turned into growing pressure, from colleagues

and publishers alike. The reason for my reluctance has been a firm belief that books of this kind should only be taken as interim reports of ongoing projects, which entails rapid dating. Be that as it may, no particular acquaintance with that book is presupposed. Precautions have even been taken to keep the number of references to it to a bare minimum, so as not to burden the reader unnecessarily. Instead, theoretical issues which bear directly on the present discussion have been taken up again and presented in some detail. In one case a whole chapter was reproduced, albeit in a highly revised form. This is the programmatic essay on the role of norms in translation, a notion which started my own thinking, back in the seventies, and which is still central to my entire position. Three of the chapters are offered as excursions: Though digressing from the main line of argumentation, the extra light they cast on essential issues was deemed reason enough to include them in the book. The semi-independence they have been given should make them easy to either skip or focus on, as the reader sees fit. A change of type-face marks shorter digressions within the chapters themselves.

Work on the book has taken quite a while. Over the years, some of the ideas comprising it were presented in separate articles, albeit always in a provisional manner. Tackling a topic, often selected in accordance with the requirements and limitations of a particular conference or volume, inevitably resulted in shedding new light on old ideas and often gave rise to new points, to be dealt with more thoroughly later on. At the same time, the book raises a whole garnut of new issues, many of them for the first time. When the argument of an existing article was used, the need to come up with a unified book imposed another measure of changes, often resulting in complete rewriting. Finally, rethinking and rewriting were also prompted by some of the more serious criticisms leveled against my work, for which I am grateful to dozens of colleagues throughout the world. None of them should be held responsible for any of my arguments, but they were all instrumental in their shaping. Special thanks are due to Mrs. Miriam Shlesinger, a fellow translation scholar and a fine editor, who has applied her final touches to my manuscript. I will no doubt soon regret all those instances where I decided not to adopt her editorial suggestions!

*

Part One of the book is expository: It deals at some length with the pivotal position of descriptive studies – and of a descriptive-explanatory branch – within Translation Studies. By implication, it also supplies justification to the author's initial decision to devote a book neither to a purely theoretical presen-

tation nor to a full-fledged study of a particular corpus, or problem, but rather to the issue of approaching translation empirically as such.

Part Two comprises a series of methodological discussions, constituting a Rationale for descriptive studies in translation. As such, it serves as a necessary framework and background for Part Three, where an assortment of case studies is presented, referring to issues of various scope and level, from a whole historical move through the translation of single texts to the translational treatment of lower-level entities. Each chapter is greatly self-contained, and can therefore be read in and for itself. However, the framework in which all studies were carried out lends them a high degree of methodological unity, which ties back to the Rationale. The guiding principle here was to tackle each issue within higher-level contexts: texts and modes of behaviour – in the appropriate cultural array; textual components – in texts, and through these texts, in cultural constellations again. The overriding need to contextualize is also stressed in the critical presentation of the use of experimental methods in the study of translation, as well as in the programmatic exposé of the gradual emergence of a translator – a highly neglected research domain of Translation Studies.

Finally, in Part Four, the crucial question is addressed which will already have come up in the expository part: what is knowledge accumulated through descriptive studies performed within one and the same framework likely to yield. Formulating laws and drawing implications for applied activities undoubtedly lie *beyond* the scope of these studies as such, and it is they which have contributed the ultimate part of the book's title. I intend to pursue them towards another book; hopefully, in less than fourteen years.

*

One problem with books is that they seem so final. If an author may venture a request, I would ask my readers to regard what they are about to read as just another interim report; at best, a stepping stone for further developments of the discipline in one particular direction. Far from wishing to attain general agreement, my intention is to stir a debate. The former I don't believe in anyway; the latter seems vital, if any real progress is to be achieved.

Tel Aviv, February 1994

PART ONE

The Pivotal Position of Descriptive Studies and DTS

It has been over twenty years since the Third International Congress of Applied Linguistics (Copenhagen, 1972), when James S Holmes put forward his ideas on "The Name and Nature of Translation Studies". In an oral presentation bearing that title Holmes envisioned a full-scale scientific discipline which would apply to the whole "complex of problems clustered round the phenomenon of translating and translations" (1988: 67), which — for very sound reasons (1988: 68–70) — he purported to call Translation Studies.¹ He then went on to outline the *structure* of that discipline, a compelling admixture of observations on what it was like, at that time, with a penetrating vision as to the form it should, and would ultimately assume.

Given the fact that very little effort had as yet been invested at that point in any "meta-reflection on the nature of translation studies" (Holmes 1988: 71), it is not unlikely that this piece would have played a substantial role in channeling the very development of the discipline, had it not been for the unfortunate fact that, until quite recently, it was anything but easy to come by. Actually, for over

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1. Since then, the name Translation Studies has been gaining more and more ground in English-speaking academic circles. It has relegated to a peripheral position alternatives such as Science of Translating (Nida 1964), Science of Translation (Nida 1969; Bausch, Klegraf and Wilss 1970, 1972; Harris 1977b; Wilss 1982), or Translatology. Unfortunately, the confusion which was cleared up that way has now been superseded by a new one: several universities in Europe have renamed their translation departments 'Departments of Translation *Studies*', despite the fact that most of their time and energy is invested in the teaching and exercising of translation (including interpreting) as a *skill*, rather than in research.

fifteen years, the full English text of the paper existed only as a mimeographed pre-publication (Holmes 1972b) – a brochure which, paradoxically enough, was only available to the already initiated, those who had become interested enough in the author's approach to take the initiative and contact him for a copy. The existence, as of 1977, of a Dutch version did not, of course, contribute too much to the dissemination of Holmes' ideas on the nature of Translation Studies, most certainly not on an *international* scale. This was the main reason for my own decision to reprint the English text in its entirety (Toury 1987: 9–24). This was not much help either, however, given the peripherality of the periodical which hosted the reprint and the scarcity of the book edition of the special translation issue which I guest-edited, at least in the West.

Be that as it may, the sad fact is that Holmes' pioneering paper was hardly ever mentioned by other authors, especially in books and articles which became standard works in the field,² nor could any real marks of its influence be traced. Thus, despite the incontestable unfolding of Translation Studies as a semi-autonomous discipline over the past two decades, a complete realization of Holmes' vision is still a long way off: his programmatic presentation may indeed serve as an invaluable *Orientierungskarte*, a grid on which individual approaches to the study of translation can be situated and their (inter)relations noted, but the 'map' itself still represents more of a *desideratum* than a reality. The first signs of change may be attributed to the inclusion of "The Name and Nature of Translation Studies" in *Translated!*, the posthumous collection of Holmes' papers, so meticulously edited by Raymond van den Broeck (Holmes 1988: 67–80). Thus, several speakers in the First James S Holmes Symposium on Translation Studies, which was held a short while after the book's publication, chose to refer to this article (see the published Proceedings: van Leuven-Zwart and Naaijken 1991). Some of them, most notably Mary Snell-Hornby (pp. 13–23), José Lambert (pp. 25–37), Theo Hermans (pp. 155–169) and myself (pp. 179–192), even took it as a basis for their own presentations.

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2. E.g., Steiner 1975, House 1977, Koller 1978, Kelly 1979, Bassnett-McGuire 1980 (not even in its misnamed 'revised' edition of 1991), Newmark 1981, Höning and Kußmaul 1982, Vermeer 1983, Reiß and Vermeer 1984, Holz-Mänttari 1984, Vermeer 1986, Snell-Hornby 1988, Bell 1991, Gutt 1991, Pym 1992. Wolfram Wilss is an exception which testifies to the rule: in his basic German book (1977: 83; English version: 1982: 78) he does devote a sentence of some 3½ lines to Holmes' lecture. However, he only refers to its one-page *abstract* (Holmes 1972a) and does not really tackle Holmes' ideas.

1. Holmes' 'map' of the discipline

For me, the main merit of Holmes' program has always lain in its convincing notion of *division*; not as a mere necessary evil, that is, but as a basic principle of organization, implying as it clearly does a proper *division of labour* between various kinds of scholarly activity. The division itself as suggested by Holmes takes after adjacent disciplines, most notably Linguistics (e.g., Fowler 1974: 33–37), and is fully in keeping with his conviction (1988: 71) that Translation Studies should emerge as an empirical science:³ main split into *Pure* vs. *Applied* branches; *Pure Translation Studies* further broken down into *Theoretical* (*General* and *Partial*) vs. *Descriptive* sub-branches, with *Descriptive Translation Studies* branching again, in terms of three different foci of research — *Function-*, *Process-*, and *Product-oriented*. The tree-diagram in Figure 1 represents Holmes' overall conception of Translation Studies.

To be sure, Holmes was scientifically-minded enough to realize that his 'flat' presentation may have created a wrong impression. Thus, towards the end of the paper he proclaimed:

in what has preceded, descriptive, theoretical, and applied translation studies have been presented as *three fairly distinct branches* of the entire discipline, and the order of presentation might be taken to suggest that their import for one another is *unidirectional*. (Holmes 1988: 78; italics added)

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3. To cite one famous formulation of the objectives of an empirical science, the opening passage of Carl Hempel's by-now classic discussion of the "fundamentals of concept formation in empirical science":

Empirical science has two major objectives: to describe particular phenomena in the world of our experience and to establish general principles by means of which they can be explained and predicted. The explanatory and predictive principles of a scientific discipline are stated in its hypothetical generalizations and its theories; they characterize general patterns or regularities to which the individual phenomena conform and by virtue of which their occurrence can be systematically anticipated. (Hempel 1952: 1)

In fact, any systematic *application* of a theory also presupposes these two objectives. In Hempel's words: "all ... application requires principles which predict what particular effects would occur if we brought about certain specified changes in a given system" (1952: 20).

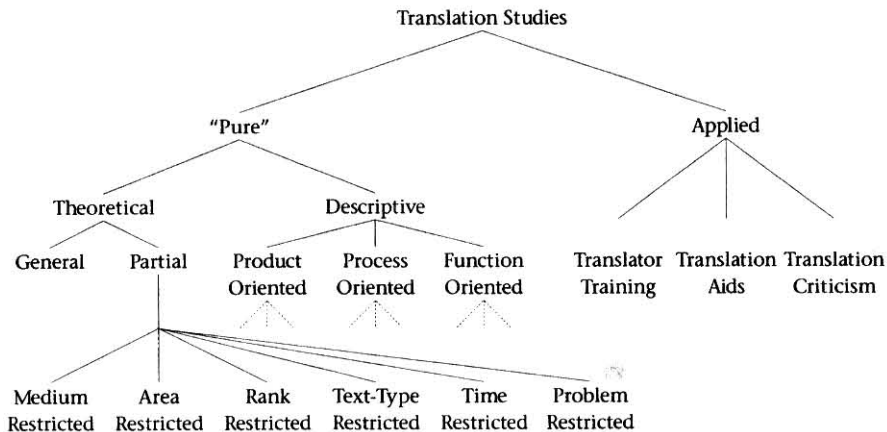


Figure 1. Holmes' basic 'map' of Translation Studies

He then added a few programmatic sentences with regard to the (inter)relations – existing, but mainly desired – between the various branches and their respective sub-branches, a topic which has rarely been taken up in any serious way since.

The rest of this opening chapter will address itself to precisely this aspect, which is of utmost importance for anybody who wishes to locate him-/herself in the middleground of Descriptive Translation Studies. In addition to enhancing the accuracy of the 'map' as such, my objective will be to make a case for the discipline's *controlled evolution*. Descriptive studies will be taken as a focal point and pivot, both as an activity and a scientific branch⁴ – in full keeping with Holmes' own way of reasoning, I should presume. My basic question will thus be as follows:

It is very clear that individual studies into translation are bound to yield isolated descriptions, so that a gradual accumulation of discrete pieces of knowledge would be an obvious result. But what contribution could descriptive studies carried out within DTS be expected to make, with respect to the discipline as such?

4. From this point on I will use the abbreviation DTS, introduced by Holmes himself (1988: 71), to refer to the scientific branch. The longer denomination, 'descriptive [translation] studies', will be retained for any research procedures addressing translational phenomena. Needless to say, my main concern throughout will be with research procedures pertaining to DTS.

2. The internal organization of DTS

The first set of relations to be superimposed on Holmes' basic 'map' applies to DTS itself as a distinct branch of Translation Studies.

Thus, it is certainly true that three approaches – function-, process- and product-oriented – are not just possible, but justified too, and that each one of them delimits a legitimate field of study of its own. To regard the three fields as autonomous, however, is a sure recipe for reducing individual studies to superficial descriptions – whether of translation's position in the culture in which it is, or will be embedded; of the process through which a translated text is derived from an original; or of the textual-linguistic make-up of a translation (or aspects of/phenomena within it), along with the relationships which tie it to its source text and/or the 'shifts' which are manifested by the one with respect to the other. Once *explanations* are also sought – and no serious study can afford to do without them – the picture is bound to change considerably, for no explanatory hypothesis which is even remotely satisfactory can be formulated unless all three aspects are brought to bear on each other.

In fact, to the extent that DTS aspires to offer a framework for individual studies of *all* kinds, at *all* levels, one cannot but proceed from the assumption that functions, processes and products are not just 'related', in some obscure way, but rather, form one complex whole whose constitutive parts are hardly separable from one another for purposes other than methodical. Consequently, whether an *individual* study is process-, product-, or function-oriented (and all three types will no doubt always be performed), when it comes to the *institutional* level, that of the discipline as a whole, the program must aspire to lay bare the *interdependencies* of all three aspects if we are ever to gain true insight into the intricacies of translational phenomena, and to do so within one unified (inter)discipline. Seen as such, individual studies of whatever denomination emerge as a twofold enterprise: each one is a local activity, pertinent to a certain corpus, problem, historical period, or the like,⁵ as well as part of an overall endeavour, an attempt to account for ways in which function, process and product can and do determine each other.

Recent attempts to apply *experimental* methods to the study of translation have certainly shed new light on the need to account for the interdependencies

5. To be sure, one cannot but wonder why Holmes neglected to duplicate his division of the *theoretical* branch into 'partial theories' in DTS. This lack was corrected by adding optional sub-branching to each descriptive branch in Figure 1.

of function, process and product, for no significant conclusion can be drawn from an experiment unless all parameters which are deemed relevant, along with their relationships, have been established. In fact, control over as many parameters as possible is a precondition for the appropriate execution of an experiment, not to mention its (relative) replicability, which renders the need to account for the variables and their interrelations all the more pressing. To be sure, the fact that this need has not been taken all too seriously to date has been, to my mind, a major impediment to this highly promising brand of descriptive-explanatory research work, which will be taken up in Chapter 12.

Thus, the [prospective] position (or function⁶) of a translation within a recipient culture (or a particular section thereof) should be regarded as a strong governing factor of the very make-up of the product, in terms of underlying models, linguistic representation, or both. After all, translations always come into being within a certain cultural environment and are designed to meet certain needs of, and/or occupy certain 'slots' in it. Consequently, translators may be said to operate first and foremost in the interest of the culture into which they are translating, however they conceive of that interest. In fact, the extent to which features of a source text are retained in its translation (or even regarded as requiring retention, in the first place), which, at first sight, seems to suggest an operation in the interest of the source culture, or even of the source text as such, is also determined on the target side, and according to its own concerns: features are retained, and reconstructed in target-language material, not because they are 'important' in any *inherent* sense, but because they are *assigned* importance, from the recipient vantage point. The establishment of a set of required (or preferred) translation relationships may also form part of the deal, but only inasmuch as the retention of one or another aspect of a source text invariant under transformation is indeed considered a necessary condition for a translation to fulfill the function allotted to it; in the *target* system, to be sure.

The prospective function of the translation, via its required textual-linguistic make-up and/or the relationships which would tie it to its original, inevitably also govern the strategies which are resorted to during the production of the text

6. The term 'function' here is used in its *semiotic* sense, as the 'value' assigned to an item belonging in a certain system by virtue of the network of relations it enters into (see, e.g., Even-Zohar 1990: 10). As such, it is not tantamount to the mere 'use' of the end product, as seems to be the case with the *Skopostheorie* (e.g., Vermeer 1986) or *Handlungstheorie* (e.g., Holz-Mänttari 1984). The correlations between the two uses of 'function', which may well exist, still await scholarly processing.

in question, and hence the translation process as such.⁷ This logic, summarized in Figure 2, has important implications for any descriptive work within Translation Studies. Thus, there is no real point in a product-oriented study without taking into account questions pertaining to the determining force of its intended function and to the strategies governed by the norms of establishing a 'proper' product. Similarly, there is little point in a process-oriented study of whatever type, unless the cultural-semiotic conditions under which it occurs are incorporated into it.

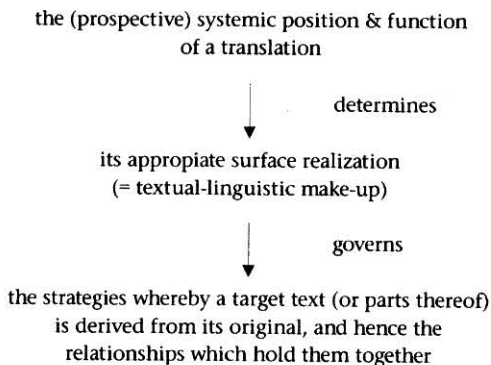


Figure 2. *The relations between function, product and process in translation*

Finally, it is not translations alone (as products, that is) whose position in the hosting culture varies. Translating itself, as a type of text-generating activity, may well vary in its position too, in terms of, e.g., centrality vs. peripherality, prevalence vs. rarity, or high vs. low prestige. This variability and its possible ramifications, e.g., in terms of texts pertaining to different types, or translation activities performed in different cultural groups, is again intimately connected with the strategies adopted by translators, and hence with the make-up of translated texts and their relationships to their sources.

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7. Even with respect to the gradual emergence of a translator under 'natural' circumstances — that is, out of any schooling system — environmental feedback greatly influences the strategies resorted to in the act of translation, thus lending them a considerable extent of uniformity across a societal group. This issue will be taken up towards the end of the book (see Excursus C).

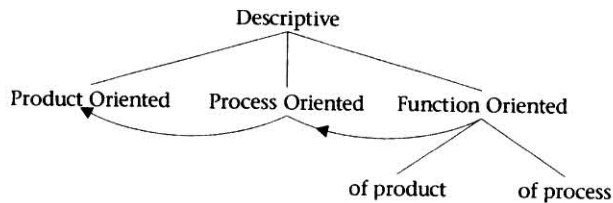


Figure 3. *The main relations within DTS*

The array of relations represented by Figures 2 and 3 may seem to work the other way around too, in the sense that the strategies a translator resorts to, and the resulting textual-linguistic make-up and translation/source relationships, may be said to affect the position of the end product in the recipient system. However, under the semiotic perspective we have adopted throughout, functions should be regarded as having at least *logical* priority over their surface realizations (also called 'carriers', or 'functors'). This idea has been a cornerstone of Dynamic Functionalism (Even-Zohar 1990; Sheffy 1992) ever since the late 1920s and early 1930s, when it was formulated by scholars such as Jurij Tynjanov, Roman Jakobson and Pëtr Bogatyrev. Once such a perspective has been opted for, the reversal of roles is no longer viable: Since translating is a *teleological* activity by its very nature, its systemic position, and that of its future products, should be taken as forming constraints of the highest order.

To be sure, this principle does not lose any of its validity when the position occupied by a translation in the target culture, or its ensuing functions, happen to differ from the ones it was initially 'designed' to have; e.g., when the translation of a literary work, intended to serve as a literary text too and translated in a way which should have suited that purpose, is nevertheless rejected by the target literary system, or relegated to a position which it was not designed to occupy. In fact, one task of descriptive studies in translation may well be to confront the position which is *actually* assumed by a translation with the one it was *intended* to have, and draw the necessary conclusions.

3. Between DTS and Translation Theory

This program, I should like to point out in no uncertain terms, pertains to Translation Studies as a *discipline* rather than to the immediate concerns of any of its practitioners as *individuals*, let alone every single study of translation. A

research program such as this certainly cuts across Holmes' distinctions: Not only can it not be located within any of DTS's sub-branches but it is in no way confined to DTS at all, given that one of the aims of Translation Studies should definitely be to bring the results of descriptive-explanatory studies executed within DTS to bear on the *theoretical* branch.

This is not to say that every study into a corpus or problem is, or even should be carried out with a view to revising the theory, in terms of which it was executed. Still, it is a fact that the findings of well-performed studies *always* bear on their underlying theories. Whether theory-relevant implications are drawn by the researchers themselves or by some other agent, most notably empirically minded theorists, they contribute to the verification or refutation of general hypotheses, and to their modification in particular. The bi-directional relations obtaining between DTS and Translation Theory are represented by Figure 4.

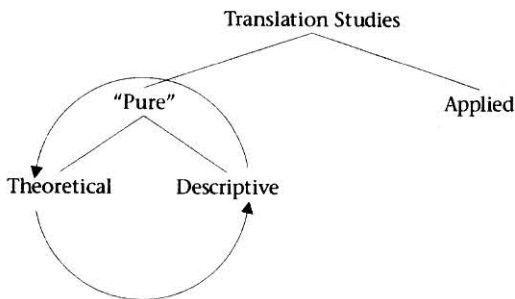


Figure 4. *The relations between DTS and Translation Theory*

To be sure, the principle of interdependency, as well as the priority of functions over their carriers, were already pronouncements of a *theoretical* nature. However, as soon as concrete relations between function, process and product are exposed by individual studies and brought into the game, the entire course taken by the discipline is bound to be affected. The reason seems simple enough: Translation Studies is called for to tackle fully and systematically three types of issues which differ in scope and level:

- (1) all that translation CAN, in principle, involve;
- (2) what it DOES involve, under various sets of circumstances, along with the REASONS for that involvement, and
- (3) what it is LIKELY to involve, under one or another array of specified conditions.

Even though the first level yields a truly theoretical framework, it is a most elementary one, in terms of a theory of *translation* – a mere system of co-ordinates which makes it possible to account for anything connected with translating and translation. The second level is, of course, tantamount to DTS's program as sketched above. Yet, the significance of studies of this kind lies not only in the possibility of supplying exhaustive descriptions and explanations of actual behaviour. No less important are their implications for the discipline as such. Thus, only when the initial potentials subsumed under (1) have been modified by diversified factual knowledge accumulated in actual studies (2) will ample grounds have been furnished for making certain *predictions*, and in a justifiable way too, as becomes the empirical status so appropriate to Translation Studies. In this vein, (3) would pertain to the theoretical branch again, only in a far more elaborate form.

To put it differently: the cumulative findings of descriptive studies should make it possible to formulate a series of coherent *laws* which would state the inherent relations between all the variables found to be relevant to translation. Lying as it does beyond descriptive studies as such, the formulation of these laws may be taken to constitute the ultimate goal of the discipline in its theoretical facet. To be sure, the envisaged laws are everything but absolute, designed as they are to state the *likelihood* that a kind of behaviour, or surface realization, would occur under one set of specifiable conditions or another. Needless to say, not even one law of the conditioned type can be formulated unless the conditioning factors have been specified. This is why the formulation of laws of this type requires the establishment of *regularities of behaviour*, along with a maximal control of the parameters of function, process and product.

Seen from a different angle, such an evolutionary process would entail a gradual transition from partial theories, which have large areas of overlap anyway, to a general theory of translation. This generalization would be achieved by introducing the principles on which the restriction of specific theories is based (e.g., medium, rank, text-type, time, problem [Holmes 1988: 74–77], and many more) into the theory itself; namely, as parameters governing the probability of the occurrence of one or another kind of behaviour, phenomenon, translation relationship, etc. Among other things, a theory thus refined will make possible the performance of yet more elaborate studies, which will, in turn, bear back on the theory, making it even more intricate.

Within such a recursive pattern, descriptive studies emerge as occupying a pivotal position: While one is always free to speculate and/or indulge in introspection, it is only through studies into actual behaviour that hypotheses

can be put to a real test. In fact, even if a study does *not* have (and it most certainly does not have to have) such testing as one of its explicit goals, this will inevitably wind up being an important concomitant; the more so if constant heed is paid to the theoretical assumptions underlying the study and to the ways its methods derive from those assumptions and are answerable to them.

4. Between Translation Studies and its applied extensions

As was to be expected, this chapter was weighted towards the internal structure of DTS, on the one hand, and towards the mutual relationships between it and Translation Theory, on the other. We should recall, however, that the first, and main split in Holmes' basic 'map' of the discipline, which we adopted as our starting point (Figure 1), was between the 'Pure' and the 'Applied' branches, so that an introductory chapter which set out to impose better order on the field as a whole could hardly ignore the relationships between the two altogether. Thus, it is precisely one of the advantages of the kind of laws envisioned here that they may be projected onto the applied extensions of the discipline with relative ease and make possible the elaboration of these extensions in a way which is much closer to reality – hence enhancing their chances of being successfully implemented.

To be sure, it has always been my conviction that it is no concern of a scientific discipline, not even in the so-called 'sciences of man', to effect changes in the world of our experience. Thus, as it should have become clear by now, I would hardly subscribe to the view (epitomized by Peter Newmark but shared by so many) that "translation theory's main concern is to determine appropriate translation methods" (Newmark 1981: 19); definitely not any more than "linguistics' main concern is to determine appropriate ways of language use". Strong as this conviction is, however, it does not preclude the possibility of drawing conclusions from theoretical reasoning, or scientific findings, to actual behaviour, be its orientation retrospective (such as translation criticism) or prospective (such as translator training or translation planning). This possibility does exist, of course. However, drawing conclusions is up to the *practitioners*, not the scholars. It is they who must bear the consequences anyway, and they might just as well be ready to take full responsibility rather than blame the 'theory' for their own blunders in the 'practice', as is all too often the case.

Thus, the practitioners I have in mind are those who indulge in the *applied activities* themselves, e.g., translation critics, teachers of translation and translation planners, and not practicing translators, unless the latter wish to train

themselves for the profession in a fully conscious way. Translation is, of course, the *object-level* of Translation Studies in all its branches, and not an ‘application’ of any of them, just as speaking, in either L_1 or L_2 , is not an application of Linguistics; language teaching or speech therapy are.⁸

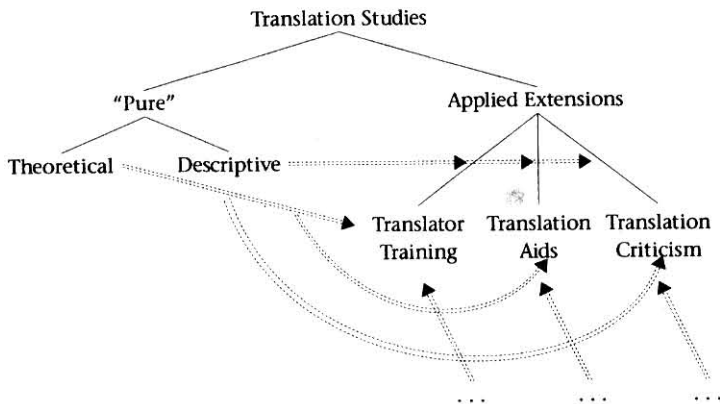


Figure 5. *The relations between Translation Studies and its applied extensions*

Needless to say, no transition from Translation Studies proper to any of its extensions into the world can be made directly. Even less so is the derivation of one from the other automatic in any way. Rather, this transition necessitates the application of some *bridging rules* – which renders the relations between Translation Studies and its applied extensions slightly different from all previous ones, as indicated by the use of a different type of arrows in Figure 5. Furthermore, the bridging rules are bound to be different for different types of application, and, at any rate, none of them can draw on Translation Studies alone. This fact is indicated by an additional set of incoming arrows, which point towards the various extensions. For instance, a set of modifying rules for translator training would, in all likelihood, come from a theory of teaching and learning, and hence include notions such as ‘exercise’ and ‘drill’, or ‘input’ vs. ‘intake’. These

8. Needless to say, what goes on in an applied extension of any discipline can become an object of study too. It will, however, constitute not only a different *object*, but also an object of a different *order*.

concepts would probably be of very little relevance to an extension such as translation planning, whose establishment would require a completely different set of bridging rules. This inherent heterogeneity is precisely the reason why the label 'applied extensions' (of Translation Studies) seems so preferable to Holmes' straightforward but simplistic, and hence potentially misleading 'Applied Translation Studies' (1988: 77): each of the branches is an extension 'into the world' of the discipline, but not of it alone.

In contrast to the two 'Pure' branches of Translation Studies, which are *theoretical* and *descriptive*, respectively, its applied extensions cannot be anything but *prescriptive*; even if they are brought closer to reality, as is the aspiration here, and even if their pluralism and tolerance are enhanced. They are not intended to account either for possibilities and likelihoods or for facts of actual behaviour, but rather to set norms in a more or less conscious way. In brief, to tell others what they should have done and/or should be doing, if they accept these norms (or, very often, the authority of their proponents) and submit to them.

One level where the inherent differences between the various branches are manifested most univocally is in the use of *verbs*. Thus, verbs of different categories are typical of each branch, and their actual use in a discourse about translation may therefore serve as a marker of its respective place in the discipline, even if (or, rather, precisely when) it is masked; most notably, when recommendations for 'proper' behaviour appear in the guise of theoretical or descriptive pronouncements. To take so-called 'translation relationships' as a case in point, this difference finds its expression in the existence of four levels of observation, along with the different criteria (or types of conditions) for the application of the relationships in question, as summed up in Table 1.

Table 1. *The differences between Translation Theory, DTS, and the applied extensions of the discipline as exemplified by the use of verbs*

Type of Relationship	Criterion (or Type of Condition)	Typical Verbs	Branch of Translation Studies
possible probable	theoretical conditional	<i>can be</i> <i>is likely to be</i>	translation theory, basic translation theory, modified
existing	empirical	<i>is</i>	DTS
required	postulated	<i>should be</i>	applied extensions of Translation Studies



PART TWO

A Rationale for Descriptive Translation Studies

Having established descriptive studies in a key position within Translation Studies and in its evolution, the next logical step would be to ask how any given study would proceed, if indeed it is to go beyond the individual instance. Our Rationale for studies aiming to expose the interdependencies of function, product and process will be presented in a number of successive stages, each building on the preceding ones.

In the first chapter, the question of what would constitute an object of study within a target-oriented approach will be submitted to detailed scrutiny. The discussion will be supplemented by a sketchy account of discovery vs. justification procedures as they apply to the objects we have in mind. This methodological survey will be followed by an excursus into so-called pseudo-translations and their possible relevancy for Translation Studies as well as a chapter on the nature and role of norms in translation and their status as reconstructable objects of study (Chapter 2).

The research method itself will be the focus of Chapter 3, in an attempt to apply a first layer of flesh to the skeletal presentation at the end of Chapter 1. To make for easier orientation, the key concepts comprising the bare bones will be highlighted in both occurrences, using SMALL CAPS. Finally, a special discussion will be devoted to the type of unit which the comparative part of the envisaged study would be applied to, along with some of its justifications (Chapter 4).

To wind up the preliminary discussion, a step-by-step presentation of an exemplary case study will be offered (Chapter 5), proceeding from a linguistic phenomenon of one basic type in its recurring use as a translational replacement in one particular tradition, and progressing toward generalizations of a higher and higher order. The intention here will not be to unfold the study itself, in all

its ramifications, but merely the movement from one stage to the next, as an illustration of the research method; hence its modest characterization as a 'study in descriptive studies'.

Chapter 1

Translations as Facts of a 'Target' Culture

An Assumption and Its Methodological Implications

The first question to be addressed within our Rationale concerns the proper object of study: How is one to determine what would be taken up and what would be left out? The current state of Translation Studies makes this question difficult to answer in any straightforward way. Not only is today's discipline a remarkably heterogeneous series of loosely connected paradigms, but there is still an overriding tendency to regard different paradigms as mere alternative ways of dealing with 'the same thing'. Which they are not, nor can we expect them to be.

Far from being a neutral procedure, establishing an object of study is necessarily a function of the *theory* in whose terms it is constituted, which is always geared to cater for certain needs. Its establishment and justification are therefore intimately connected with the *questions* one wishes to pose, the possible *methods* of dealing with the objects of study with an eye to those questions — and, indeed, the kind of *answers* which would count as admissible. The real question is not what the object 'is', so to speak, but rather what would be taken to constitute an object, in pursuit of a certain goal, such that any change of approach would entail a change of object. This is so even if they all superficially fall under the same heading: It is not the *label* that counts, but the *concept* it applies to. And concepts can only be established within conceptual *networks*; i.e., as a function of the network in its entirety.

1. Approaching translation within a target-oriented framework

Translation scholars all use the words 'translation' and 'translating'. Many of them also talk about 'transfer' and 'translational relationships', 'equivalence'

and 'adequacy', and much more. However, not only are different characterizations given to the *contents* of recurring words when used as terms, each within a different terminological system, but there are also considerable differences between various approaches as to how the object-level of Translation Studies would be delimited, to begin with, and what would therefore count as a *legitimate* object of study within it. At the same time, the fallacious rejection of somebody else's concepts on the grounds that they are untenable within one's own frame of reference is still common practice – much more so than in most other fields of study, I suspect.

It will be recalled that the mainspring of the present endeavour was the conviction that the position and function of translations (as entities) and of translating (as a kind of activity) in a prospective target culture, the form a translation would have (and hence the relationships which would tie it to its original), and the strategies resorted to during its generation do not constitute a series of unconnected facts. Having accepted this as a point of departure, we found *interdependencies* emerging as an obvious focus of interest, the main intention being to uncover the *regularities* which mark the relationships assumed to obtain between function, product and process.

In an attempt to pursue this goal, translations have been regarded as facts of the culture which hosts them, with the concomitant assumption that whatever their function and identity, these are constituted within that same culture and reflect its own constellation. To be sure, it was by virtue of such a methodological starting point that this approach to the study of translations and translating in their immediate contexts earned the nickname of 'target-oriented'.

When it was first put forward, this frame of reference was considered quite revolutionary, and its initiator a sworn *enfant terrible*. At that time, back in the seventies, Translation Studies was indeed marked by extreme source-orientedness. Most of its paradigms were application-ridden too: Whether concerned with teaching or quality assessment, their preoccupation was mainly with the source text and with the proclaimed protection of its 'legitimate rights'. Target constraints, while never totally ignored, often counted as subsidiary; especially those which would not fall within Linguistics of any kind. Many factors which govern real-life translational behaviour, and the fact that these factors have indeed resulted in a variety of very different translation traditions, were resented, or, at best, relegated to the realm of 'mere' history.¹

1. And see Delabastita's recent argument (1991) that the opposition between theoretical and historical approaches to translation is a false one through and through.

In the meantime, most translation scholars, while not abandoning the seemingly safe base of the source text, have at least come to integrate many more target-bound considerations into their reasoning. In addition, a second paradigm which was heavily target-oriented gradually emerged as an alternative. This so-called *Skopostheorie* has managed to gain considerable ground lately, albeit almost exclusively in German-speaking circles, since the writings of its proponents have mostly been confined to that language. At any rate, target-orientedness as such no longer arouses the same antagonism it used to less than twenty years ago.

Interestingly, the first formulations of the *Skopostheorie* by Vermeer (e.g., 1978) almost coincided with the beginnings of my own switch to target-orientedness (Toury 1977) – which sheds interesting light on how changes of scholarly climate occur, especially considering that for quite a while, the two of us were practically unaware of each other's work.² To be sure, even now, there is at least one major difference between the interests of the two target-oriented paradigms, which also accounts for the different assumptions each of them proceeds from: whereas mainstream *Skopos*-theorists still see the ultimate justification of their frame of reference in the more 'realistic' way it can deal with problems of an *applied* nature, the main object being to 'improve' (i.e., change!) the world of our experience, my own endeavours have always been geared primarily towards the *descriptive-explanatory* goal of supplying exhaustive accounts of whatever has been regarded as translational within a target culture, on the way to the formulation of some *theoretical* laws. Recent attempts to conduct historical studies of translation within *Skopostheorie* (most notably Vermeer 1992), on the one hand, and to apply some of the basic assumptions of the other target-oriented paradigm to translation didactics (e.g., Toury 1980b, 1984b, and especially 1992), on the other, indicate that the gap may be narrowing. This tendency is also manifest in the recent work of some second-generation *Skopos*-theorists, most notably Nord (1991), who has made an interesting attempt to integrate a version of the notion of 'translational norms', so central to my own way of reasoning and its evolution (see Chapter 2), into an account which is basically Vermeerian. Unfortunately, while doing so, Nord (re)introduced the concept of 'loyalty', and as an a priori *moral* principle at that, which may well be opening a new gap between the two approaches as the old one seems to have been closing.

My own program has not fared all that well either, the prevailing tendency having been to read it through the (often rather dark) glasses of *other* approaches rather than in its own terms. As a result, many of its claims were grossly misconceived. In view of the

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2. An interesting attempt to associate target-oriented thinking on translation, especially of my brand, and some of the basic ideas of Deconstruction, was made a few years ago by van den Broeck (1990). While I wouldn't endorse all his claims, it is certainly an intriguing article for anybody interested in the way scholarly paradigms change.

misunderstandings which have cropped up, it seems necessary to dwell a little longer on the target-orientedness of the approach in view of the type of studies envisaged here and their ultimate goals.

2. Translations as cultural facts

Strange as it may sound to the uninitiated, there is nothing too perverse in claiming that a text's position (and function), including the position and function which go with a text being regarded as a translation, are determined first and foremost by considerations originating in the culture which hosts them. In fact, this is the most normal practice of the 'persons-in-the-culture' themselves. Thus, when a text is offered as a translation, it is quite readily accepted bona fide as one, no further questions asked. Among other things, this is the reason why it is that easy for *fictitious* translations to pass as genuine ones. By contrast, when a text is presented as having been originally composed in a language, reasons will often manifest themselves – e.g., certain features of textual make-up and verbal formulation, which persons-in-the-culture have come to associate with translations and translating – to suspect, correctly or not, that the given text has in fact been translated into it. Adopting culture-internal distinctions as a starting point for the *study* of translation as it is conceived of, and executed, within the conditioning framework of a culture thus has the big advantage of not imposing on its object any distinctions which may prove alien to that culture. It thus allows one to proceed with as few assumptions as possible which could be difficult to maintain, in the face of real-world evidence.

There is no way a translation could share the same systemic space with its original; not even when the two are physically present side by side. This is not to say that, having been severed from it, a translation would never be in a position to bear on the source culture again, on occasion even on the source text itself. Texts, and hence the cultural systems which host them, have been known to have been affected by translations of theirs. It is nonetheless significant that any such practice involves a *reversal of roles*, in full accordance with our starting point: while genetically a translation, the affecting entity no longer functions as one. To be sure, it is not just any translation that would exert an influence on its original. Rather, it is a fact of a particular (target!) culture, which is, moreover, regarded as privileged *for that precise reason*. The fact that translations often serve as a basis for further acts of translation is no refutation of our target-oriented assumption either: while a translation does function as a source text in such

instances, it is still a fact of a former target culture now turned into a mediating one, and it is picked up and assigned the role of a source text not because of anything it may inherently possess, but in accordance with the concerns of a new prospective recipient system.³

On the other hand, translation activities and their products not only can, but do cause changes in the *target* culture. By definition, that is. Thus, cultures resort to translating precisely as a major way of filling in gaps, whenever and wherever such gaps manifest themselves — either as such, or (very often) from a comparative perspective, i.e., in view of a corresponding non-gap in another culture that the prospective target culture has reasons to look up to and try to exploit. Semiotically, then, translation is as good as *initiated* by the target culture. In other words, the starting point is always one of a certain deficiency in the latter, even if sometimes — e.g., in a ‘colonial’ situation — an alleged gap may be factually pointed out for it by a patron of sorts who also purports to ‘know better’ how that gap may best be filled. Even here, the more persuasive rationale is not the mere existence of something in another culture/language, but rather the observation that something is ‘missing’ in the target culture which should have been there and which, luckily, already exists elsewhere.

In the simplest of cases, both deficiency and fill-in consist in mere **textual entities**. Being an instance of performance, every individual text is of course unique; it may be more or less in tune with prevailing models, but in itself it is a novelty. As such, its introduction into a target culture always entails some change, however slight, of the latter. To be sure, the novelty claim still holds for the *n*th translation of a text into a language: it is the *resulting* entity, the one which would actually be incorporated into the target culture, which is decisive here; and this entity will always have never existed before — unless one is willing to take Borges’ speculations on Pierre Menard, author of the *Quixote*, at face value and apply them to the generation of translated texts. Alternative translations are not even likely to occupy the exact same position in the culture which hosts them even if they all came into being at the same point in time. In more complex cases, **models** may be imported into the recipient culture as well. Such a migration normally involves groups of texts which embody a recurring pattern or else are translated in a similar fashion, even though high-prestige translations of individual texts, most notably the Bible, are known to have had a similar effect.

3. An account of a whole tradition of indirect translation, proceeding on these assumptions, will be presented in Chapter 7.

The likelihood of causing changes in the receiving system beyond the mere introduction of the target text itself stems from the fact that, while translations are indeed intended to cater for the needs of a target culture, they also tend to *deviate* from its sanctioned patterns, on one level or another, not least because of the postulate of retaining invariant at least some features of the source text – which seems to be part of any culture-internal notion of translation (see Section 4 below). This tendency often renders translations quite distinct from non-translational texts, and not necessarily as a mere production mishap either; it is not unusual for a certain amount of deviance to be regarded not only as *justifiable*, or even *acceptable*, but as actually *preferable* to complete normality, on all levels at once. Moreover, even if they are not culturally favoured, deviations – even when they manifest themselves in the very make-up of the texts – do not necessarily disturb the ‘persons-in-the-culture’. Thus, more than one writer has observed that the (tentative) identification of a text as a translation

‘protects’ the reader, as it were, from misinterpreting the writer’s intentions. ... [It] implies that deviations from cultural norms are not judged as intentional, and therefore are not assigned any ‘hidden’ meaning. (Weizman and Blum-Kulka 1987: 72)

In fact, as has been suggested time and again, there are often good reasons to regard translations as constituting a special system (Dressler 1972), or ‘genre’ (James 1989: 35–36) of their own within a culture.⁴ What is totally unthinkable is that a translation may hover in between cultures, so to speak: As long as a (hypothetical) interculture has not crystallized into an autonomous (target!) systemic entity, e.g., in processes analogous to pidginization and creolization, it is necessarily part of an *existing* (target!) system.

3. In need of proper contextualization

Bringing all this to bear on our starting point, the target-oriented assumption, the latter could be reformulated to read as follows:

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4. The most extensive account of a translational sub-system out of the literary domain is Vehmas-Lehto’s 1989 study of Finnish journalistic texts translated from the Russian. According to her characterization, these texts are characterized by ‘quasi-correctness’, in terms of domestic norms of acceptability. ‘Cultural license’ for their deviation from predominant norms of Finnish journalism, which serves to add to their acceptability, is given precisely on the combined assumption that they have been *translated* – and from the *Russian*, at that.

translations are facts of target cultures; on occasion facts of a special status, sometimes even constituting identifiable (sub)systems of their own, but of the target culture in any event.

This reformulation implies that, while certainly indispensable, establishing the culture-internal status of a text as a translation does not in itself provide a sufficient basis for studying it as one. Any attempt to offer exhaustive descriptions and viable explanations would necessitate a proper **contextualization**, which is far from given. Rather, its establishment forms part of the study itself which is applied to texts assumed to be translations. In an almost tautological way it could be said that, in the final analysis, a translation is a fact of whatever target sector it is found to be a fact of, i.e., that (sub)system which proves to be best equipped to account for it: function, product and underlying process. This, then, is the ultimate test of any contextualization. Consequently, the initial positioning of a translation, which is a *sine qua non* for launching a meaningful analysis (see Lambert and van Gorp 1985), may be no more than *tentative*; it may often have to undergo revision as the study proceeds — on the basis of its interim findings, i.e., in a process of continuous negotiation.

Above all, it shouldn't be assumed that the identity of the (sub-)culture which hosts an assumed translation is known just because one knows what *language* it is formulated in. Seemingly an easy way out, the assumption of a one-to-one relationship between culture and language often proves misleading, the more so as the exact identity of the target language itself may have to be reconsidered in the course of the study.

To take an extreme example: one of the versions of the note which warns passengers on German trains against improper use of the emergency brake seems to be an intended utterance in the English language. In spite of the fact that it is only presented as *parallel* to three other versions of the same note (Hartmann 1980), there are sufficient indications for tentatively regarding it as a *translation*, and of the German version, at that. At the same time, this note does not pertain to any of the institutionalized cultures which have English as a 'national' language, and not just because each one of these cultures has a codified version of the warning in its repertoire, all different from the present one. Ignoring the ridiculous possibility that it is not attributable to any culture, there is no escape from regarding the English version as situated in the *German* culture, albeit in a very specific section of it which does take the intended language into account.

Thus, the system which may be said to host this (translated) note is the artificial sub-culture shared by the speakers of various languages who also have English, for as

long as they are in Germany (or at least on board a German train). To be sure, this is the only contextualization which ensures a satisfactory explanation of the linguistic make-up of the text and the (reconstructed) practice resorted to by its translator, which will be submitted to closer analysis in Chapter 4.

Needless to say, proper contextualization also involves a heightened differentiation between translational items pertaining to one culture; namely, in terms of their respective positions within it. As already indicated, not even two translations of a single text are likely to occupy exactly the same position, least of all as a mere reflection of the position of the original in the source culture. If differences of textual-linguistic make-up, or of relationship to the shared source text, are to receive a viable explanation, the position appropriate to each translation will have to be established — and taken into account in all seriousness.

To my mind, failure to do precisely this is a major flaw in much of the otherwise impressive work emanating from the Göttingen group researching literary translation, especially as they purport to arrive at a *cultural history* of translation into German, and hence claim to carry out their research within a strict cultural-historical framework. All too often, one simply doesn't know the extent to which a case they are describing is representative, or significant in any other way. In fact, in spite of their proclaimed intention, the textually interesting has often been given precedence over the historically significant.

The systemic position most relevant to the kind of questions we wish to pursue is of course the one a translation was designed to occupy when it first came into being. This is the only position which may be claimed to have actually governed its generation and the decisions made in its course. However, it is only towards the end of a study that an intended position can be established with reasonable certainty. This would be achieved by weighting the original position of the text against the findings concerning its make-up and formulation, and the way it represents its original, while taking into account what is already known about the translational tradition in which it came into being. Consequently, such a positioning would be established as an *explanatory hypothesis* rather than as a 'fact'.

Also significant is the possibility that translations which retain their status as facts of the target culture may nevertheless change their position in it over time. Of course, such changes can have no bearing on either the intended, or even the initial position of a translation. On the other hand, they may often

shed considerable light on preferences of later periods of time, at least some of them pertinent to translation as performed in those periods.

4. The notion of 'assumed translation' and its contents

In most paradigms, a definition of translation would have been expected by now — a list of (more or less) fixed features which, if accepted as a starting point and framework for research, would entail a *deductive* mode of reasoning. However, the obsession with restrictive definitions proves counter-productive precisely when the aspiration is to account for real-life phenomena in their immediate contexts; they tend to hinder rather than advance descriptive-explanatory work.

Thus, any a priori definition, especially if couched in essentialistic terms, allegedly specifying what is 'inherently' translational, would involve an untenable pretense of fixing once and for all the boundaries of an object which is characterized by its very *variability*: *difference* across cultures, *variation* within a culture and *change* over time. Not only would the field of study be considerably shrunk that way, in relation to what cultures have been, and are willing to accept as translational, but research limited to these boundaries may also breed circular reasoning: to the extent that the definition is indeed adhered to, whatever is studied — selected for study because it is known to fall within it, in the first place — is bound to reaffirm the definition. Unless one is willing to transcend the arbitrarily set boundaries, that is; which is what research practice seems to have always involved, even if performed within an essentialistic, and hence classificatory frame of reference.

Thus, the unpleasant truth is that even those who have sworn by the need to proceed deductively, on a well-formulated initial definition, never hesitated to pick texts (or other phenomena) for study on the grounds that they had been presented, or otherwise regarded as translational. Within a particular culture, that is. It appears that they too were adopting the pre-systematic attitude of the 'persons-in-the-culture', but as no more than a necessary evil: they were never willing to follow it, or the procedures it may have suggested, in any *consequential* way. What they did instead was to tamper with their data; e.g., by imposing on it distinctions between 'fuller' and 'less full' realizations of the framing definition, which was thus elevated to a kind of maximum, or else by introducing additional (a priori, and hence non-cultural and ahistorical) distinctions, e.g., between 'translation' and 'adaptation'. Of course, the number of these distinctions could be multiplied almost indefinitely, and, at any rate, they do not offer

very much explanatory power, when it comes to culturally contextualized phenomena.⁵ Not for a moment did the possibility cross their minds of giving this forced target-orientedness of theirs a *systematic* status, which would have entailed an *inductive* attempt to derive general principles from the facts themselves rather than speculation within a rigid frame of reference.⁶

To be sure, the principles we formulated ourselves have never been put forward as an alternative definition of the "Gegenstand der Übersetzungswissenschaft" as such, as wrongly posited by some critics, most notably Koller (1990, 1992). Rather, what they have always constituted is a *working hypothesis* providing guidelines for the establishment of corpuses for studies of one particular kind, sharing one particular set of goals. Within our frame of reference, the assumption is applied to **assumed** translations; that is, to all utterances which are presented or regarded as such within the target culture, on no matter what grounds. Under such observation, there is no pretense that the nature of translation is given, or fixed in any way. What is addressed, even in the longest run, is not even what translation is *in general*, but what it proves to be *in reality*, and hence what it may be expected to be under various specifiable conditions.

There may of course be any number of reasons for regarding a target-language utterance as a translation. On the other hand, there is also the possibility of encountering phenomena, which could have plausibly been regarded as translations but which were not — whether they were regarded as something else or whether the distinction between translations and non-translations was simply non-functional, and hence a non-fact, in cultural terms. Items of this kind can of course be studied too, but an account will have to be given precisely of the fact that they were *not* presented/regarded as translational within the culture

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5. In almost any other paradigm, the Hebrew translation of a *Schlaraffenland* text, which will be submitted to detailed discussion in Chapter 8, would probably have passed for an adaptation. Consequently, it would have been banned from further treatment within DTS, in spite of the fact that it was *presented* — and *accepted* — as a translation, and in spite of the light this treatment sheds precisely on the concept of translation pertinent to the culture in question.
 6. Menachem Dagut was probably the first to have put his finger on the basic differences between various scholarly treatments of translation in terms of whether their orientation was deductive or inductive. In fact, he made this distinction a main line of argumentation in a lengthy review of my 1980 book (Dagut 1981). He himself was all in favour of deductive work, but even he could not but involve bits and pieces of inductive reasoning in the descriptive part of his work (Dagut 1978).

which hosts them; otherwise the required goal will never be attained.

The whole point here is precisely to tackle questions such as why something was, or was not, presented/regarded as translational, and not why it should have been (much less why it should not have been) presented in that way. Whatever the justification for this hypothesis, and for the heuristics deriving from it, it is thus intimately connected with these particular interests and no others. Adopting our assumption as a working hypothesis thus involves two important benefits:

- (1) a considerable *extension* of the range of objects of study, in full agreement with those real-life situations that we set out to account for;
- (2) *functional operativity* even in cases where the basic principle might have seemed factually inapplicable.

Proceeding from culture-internal notions often involves local, pre-systematic nomenclature as well: It stands to reason that many of the distinctions recognized as functional within a culture would also find expression in language, labelling being one important indication of cultural institutionalization. This possibility notwithstanding, our principles have not been put forward with respect to the English word 'translation', as strangely posited by Gutt (1991: 7), so that there is hardly room for his doubt as to their applicability to German *Übersetzung*, Amharic *tirgum*, or any other 'ethnic' label. It is the *notion* of (assumed) translation that is at stake here; and no matter what name it goes under, this notion can be accounted for as a cluster of (at least three) postulates:

- (1) The Source-Text Postulate;
- (2) The Transfer Postulate;
- (3) The Relationship Postulate.

While all three may look familiar, their status within a target-oriented frame of reference is very different from the one they may have had in any other paradigm of Translation Studies: regarded as postulates, they are all *posited* rather than factual; at least not of necessity. Therefore, rather than constituting answers, they give rise to *questions* to be addressed by anyone wishing to study translation in context.

Let us look briefly into these lower-level assumptions in an attempt to clarify their posited status and the way they combine to form the overall culture-internal notion of assumed translation.

(1) *The source-text postulate*

Regarding a text as a translation entails the obvious assumption that there is another text, in another culture/language, which has both chronological and

logical priority over it: not only has such an assumed text assumedly preceded the one taken to be its translation in time, but it is also presumed to have served as a departure point and basis for the latter.

The crucial thing is that it is not the source text as such, nor even the possibility of actually pointing to it, that is at stake here, but the *assumption that one must have existed*. Therefore concrete texts in languages other than the target's are not part of the necessary equipment for launching research either: even if none is used, the study will still pertain to Translation Studies as long as the assumptions of their temporal preexistence and logical priority are taken into account.

To be sure, a target fact which was tentatively marked as a translation, with the Source-Text Postulate implied, may then turn out to lack a corresponding text in any other language/culture, and not only when one has simply failed to locate it. A concrete source text may never have existed, to begin with. Consequently, so-called pseudotranslations emerge as legitimate objects of study within our paradigm: until the mystification has been dispelled, the way they function within a culture is no different from the way genuine translations do (see Excursus A). On the other hand, an assumed translation may later on be found to have had more than one source text, being a case of compilative translation, or a single source text which, however, *differs* from the one it was initially assumed to have, as in the case of indirect translation.⁷ (See Chapter 3, Section 3.)

(2) *The transfer postulate*

The Source-Text Postulate also entails the assumption that the process whereby the assumed translation came into being involved the transference from the assumed source text of certain features that the two now share. This assumption is a clear result of bringing two different kinds of knowledge to bear on one another: knowledge about products, on the one hand, and about (cross-linguistic and cross-cultural) processes, on the other.

When regarded from a target-oriented point of view, transfer operations — their very existence as well as their exact nature (and that of the transferred features) — first manifest themselves as posited too. Of course, both aspects can be put to the test, but they would remain distinct in their very essence: even if

7. Considerable research on *medieval* translations has indeed been carried out in the absence of a source text (but on the assumption that one must have existed!), or in view of several candidates to serve as one, including the possibility of a 'combined' source text.

recourse to transfer operations has been confirmed, these will not necessarily be found to match the posited one. To be sure, either one can be put to the test only after the appropriate source text has been secured.

(3) *The relationship postulate*

Finally, adopting the assumption that a text is a translation also implies that there are accountable relationships which tie it to its assumed original, an obvious function of that which the two texts allegedly share and which is taken to have been transferred across the cultural-semiotic (and linguistic) border.

A target-internal Relationship Postulate can be highly intricate, down to very specific hypotheses as to the level(s) where translation relationships may be expected to occur. Some of these relationships may even be postulated as necessary and/or sufficient, within the normative structure of the culture in question. However, all this needn't be reflected in reality either: upon examination, relationships actually tying together pairs of texts, or parts thereof, may well be found to *differ* from the postulated ones. Since there is no inherent need for intertextual relationships to always be of the same kind or intensity, the nature and extent of these relationships, as well as their correspondence to the culture's attitudes, constitute just another set of questions, to be settled through concrete research work rather than mere speculation.

Another indication of the posited nature of translation relationships, under the present observation, is the fact that they can often be (tentatively) accounted for even in the absence of a source text, namely, on the basis of certain features of the assumed translation itself, and on the concomitant assumption that it was indeed translated. In fact, pseudotranslations often manage to pass for genuine ones without arousing too much suspicion, by making manipulative use of this precise fact. On the other hand, this often serves as a sound basis for establishing the appropriate (immediate) source(s) of an assumed translation, e.g., when it is suspected of being compilative, or indirect.

If we now proceed to take the three postulates together, an assumed translation would be regarded as any target-culture text for which there are reasons to tentatively posit the existence of another text, in another culture and language, from which it was presumedly derived by transfer operations and to which it is now tied by certain relationships, some of which may be regarded – within that culture – as necessary and/or sufficient.

5. Discovery vs. justification procedures

It should have become clear by now that neither source text nor transfer operations and transferred features, nor even translation relationships, would have been excluded from a target-oriented program of DTS. They were just given a different status. This is also to say that 'orientedness' is far from tantamount to 'exclusiveness', as wrongly interpreted by many: the present approach is characterized as target-oriented *because this is where its observations start*. By no means should it be taken to mean that this is where these observations would also be exhausted.⁸

Looking at it from another angle, it is only reasonable to posit that a study in translation activities which have already yielded their products would start with the *observables*; first and foremost, the translated utterances themselves, along with their constituents. From there on, the study could proceed to facts which are observational 'in the second order' (i.e., facts which need (re)construction before they can be submitted to scrutiny), most notably the relationships which tie together the output and input of individual acts, the ultimate intention being to end up reconstructing the *non-observables* at their root, particularly the processes whereby they came into being.

Assumed translations would thus constitute the most obvious first candidates for study. Moreover, they should be submitted to study *under that assumption*. Such texts, or aspects thereof, would first be submitted to study individually, namely, in terms of their acceptability on all relevant levels; not only as target-language texts in general, but also, more specifically, as translations into it. Later on, when a text in another language has been tentatively established as its source text, the next obvious step would be to map the assumed translation onto that assumed counterpart, in an attempt to determine the (uni-directional, irreversible) relationships which obtain between the paired texts and hold them together as the target- and source-texts they are now presumed to be. Such mapping would also be the safest means of conclusively settling the appropriateness

8. In fact, I cannot see why such an approach would concern itself with transfer any less than, say, the position the Göttingen group claims to have adopted (e.g., Frank 1990: Section II), to a great extent as an (over)reaction to my own program. It is my firm conviction that, even though transfer can be addressed in other frameworks too, it remains at best only partly explainable unless all *target* constraints are taken into consideration, which can only be done within a target-oriented paradigm.

of the source-language text in question as a source text, a process whose possible trickiness should not be underestimated.

Due to many inherent limitations, some of them no doubt cognitive, it will normally be target-text segments (rather than the text as one entity) which would be mapped onto segments of the source text. In the process of mapping, the status of the former as translational replacements would be established, along with what they may be said to have replaced, thus shedding light on problems as they may have presented themselves in the particular act which yielded the text under observation, and on their solutions. Shifts (e.g., from a certain notion of 'maximal', or 'optimal' rendering) can also be studied, if deemed justified, interesting and/or feasible.

Having been established for a series of paired segments, and grouped together on the basis of the results of the comparisons themselves, translation relationships would then be referred to the concept of translation underlying the text as a whole. This will be done through the mediation of a notion of translation equivalence; namely, the one which would have emerged as constituting the norm for the pair of texts in question. It is these last two concepts which would form the ultimate goal of studies into individual pairs of texts: Nothing on the way to establishing the norm of translation equivalence, and of the underlying concept of translation, can be fully accounted for without reference to these concepts. On the other hand, they themselves cannot be established in any controlled way prior to the exhaustion of the entire set of discovery procedures. Even though intuitions as to their nature, often very good ones, may well be present much earlier, these intuitions would have to undergo justification, if they are to be accepted as proper explanations; and systematic justifications necessitate systematic studies.

Once the prevailing concept of translation, established for one target text, is put within a broader context, it may also become possible to speculate on the considerations which may have been involved in making the decisions whose results were encountered at the beginning of the analysis, along with the factors which may have constrained the act. This speculating may involve a confrontation of the competing models and norms of the target and source texts and systems, which were responsible for the establishment of the individual replacing and replaced segments, along with the relationships shown to obtain between them.

Justification procedures thus emerge as a mirror image of the corresponding discovery procedures, as shown in Figure 6. At the same time, it would of course be wrong to assume that, in the course of a study, justifications start being offered only when the discovery procedures have already been exhausted.

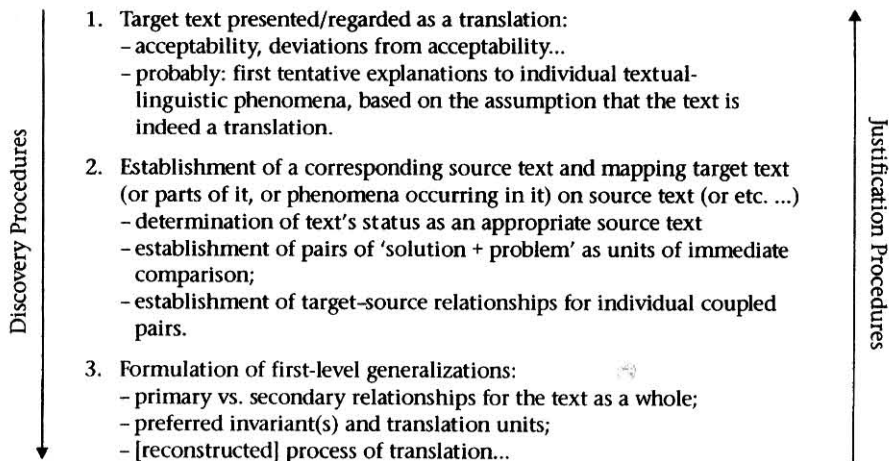
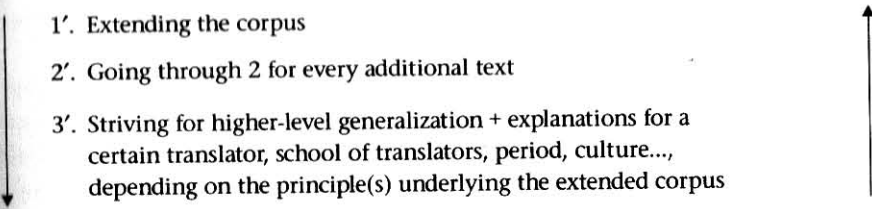


Figure 6. *Discovery vs. justification procedures for one pair of texts*

Rather, in every phase, from the very start, explanatory hypotheses will be formulated, which will then reflect backwards and affect subsequent discovery procedures. The normal progression of a study is thus helical, then, rather than linear: there will always remain something to go back to and discover, with the concomitant need for more (or more elaborate) explanations.

Needless to say, one assumed translation, or even one pair of texts, would not constitute a proper corpus for study, if the intention is indeed to expose the culturally determined interdependencies of function, process and product, not even for that one translation. Any aspiration to supply valid explanations would therefore involve an extension of the corpus according to some principle: translator, school of translators, period, text-type, text-linguistic phenomenon, or any other principle which could be given a justification (see Figure 7). Explanations which have been formulated in previous studies, pertaining to other texts, groups of texts or phenomena, can also be brought to bear on that corpus, which would involve further extension in an indirect way. In this sense, single texts, or pairs of texts, can be taken as objects of study after all.



1'. Extending the corpus

2'. Going through 2 for every additional text

3'. Striving for higher-level generalization + explanations for a certain translator, school of translators, period, culture..., depending on the principle(s) underlying the extended corpus

Figure 7. *Discovery vs. justification procedures for an extended corpus*

Excursus A

Pseudotranslations and Their Significance

Being persons-in-the-culture themselves, producers of texts are often well aware of the position translations and translating have in their culture, which may go hand in hand with some identifiable textual-linguistic features (see Chapter 1, Section 2). On occasion, they may even decide to make active use of this awareness of theirs and present, even compose their texts as if they were actually translated. It is texts which have been presented as translations with no corresponding source texts in other languages ever having existed – hence no factual ‘transfer operations’ and translation relationships – that go under the name of pseudotranslations, or fictitious translations.

The interesting paradox is that this classification can only be applied after the veil has been lifted. Consequently, texts can be approached – and studied – as *pseudotranslations* only when the position they were intended to have, and once had in the culture which hosts them has already changed; whether the fact that they used to function as translations still has any reality left or whether it has been completely erased from ‘collective memory’. Only then can questions be asked as to why a disguised mode of presentation was selected, to begin with, and why it was this [presumed] language, or textual tradition, that was picked up as a ‘source’, as well as what it was that made the public fall for it for a longer or a shorter period of time. At the same time, if any historically valid answers are to be attempted, the texts will have to be properly contextualized to be studied, i.e., they will have to be reinstated in the positions they had occupied before they were found out to be fictitious. Needless to say, there may exist myriad pseudotranslations, with respect to which the mystification has not been dispelled, and maybe never will be. These texts can only be tackled as *translations* whose sources are unknown; but then, so many *genuine* translations are in that same

position, and there is no real way of distinguishing between the two groups.

To be sure, pseudotranslating has not always been so marginal as it may now seem to be. True enough, in modern times, with the world quickly transforming into Marshall McLuhan's 'global village', and especially with the tightening of copyright laws, fictitious translations would usually no longer be situated in the most canonized sectors of a culture; certainly not in central positions within it, where the mystification would simply not hold for long. At the same time, pseudotranslations are far from a mere curiosity, which is how they have been treated all too often in the literature.¹ In fact, they often prove highly revealing for cultural studies, especially in their historical facet, including culture-oriented Translation Studies.

1. Some uses of pseudotranslating

From the point of view of cultural evolution, the most significant aspect of the production and distribution of texts as if they were translations is the fact that this constitutes a convenient way of introducing novelties into a culture. In fact, it has often been one of the only ways open to a writer to do so without arousing too much antagonism, especially in cultures reluctant to deviate from sanctioned models and norms. Having normally been regarded as a *secondary* mode of text-generation, in terms of cultural organization (Even-Zohar 1978a), there can be no wonder that deviations occurring in texts which are culturally acknowledged as translations often meet with much greater tolerance – which explains why so many innovators throughout the ages have disguised their own texts as translations, and not necessarily in *belles-lettres* alone. An extreme case which immediately comes to mind is that of the *Book of Mormon* (1830): Here, the innovations introduced into American [Christian] culture of the time by means of a text presented (and composed) as a translation gave birth to an altogether new Church, which caused a redeployment of much more than the

1. One exception which confirms the rule is Santoyo's 1984 article: far from treating pseudotranslating in an anecdotal fashion, the author approached it first and foremost as a *textual* fact (a 'narrative technique', in his terminology) rather than in its role in the evolution of a culture (i.e., as a *cultural* fact), even less so – as a *translational* phenomenon. The article includes a select list of European pseudotranslations throughout the ages (pp. 50–51), which should prove useful to the reader.

religious sector of the culture in question.² One cannot but wonder what history would have looked like, had Joseph Smith Jr. claimed he had been given golden plates originally written in English, or had everybody taken the claim he actually made as a mere hoax!

Sometimes the innovation is not so much in terms of culture at large, but rather relative to the previous activities of a particular author who is now seeking to change course and who wouldn't like his/her new endeavours to be associated with what his/her name already stands for. As examples one could cite Horace Walpole's *The Castle of Otranto* (1765), "translated by William Marshal, Gent.", not accidentally "from the ... Italian", or Karen Blixen's 1944 *Gegældelsens Veje* "by Pierre Andrézel", "translated into Danish by Clara Svendsen". At the same time, the decision to present a text as a translation, let alone compose it with that aim in mind, always suggests an implied act of subordination, namely, to a culture and language which are considered prestigious, important, or dominant in any other way. An attempt is thus made to impart to the text some of the superiority attributed to that culture, thereby manipulating the text's reception by the audience. It is a way of hiding behind a pseudonym, then, with the added value of possibly benefiting from the status assigned to translations at large, or, more likely, to a certain translational tradition, in the domestic culture.

Another explanation which has sometimes been offered for disguising texts as translations (e.g., Montesquieu's *Lettres persanes* [1721]) is an author's fear of censorial measures against him-/herself or his/her work. Obviously, neither such a motivation nor the naive claim that one is simply trying to make some, or some more money that way, in any way contradicts the previous, more comprehensive explanations. After all, it is precisely deviations from what is culturally sanctioned that are most likely to meet with opposition; and the way censorship is applied to translations has often been much more lenient. One reason for this difference is precisely the fact that the presumed non-domestic origin of translations makes them look less menacing; another is that there seems to be no way of actually going after the 'absent' author, who should presumably take most of the blame.

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2. Another aspect of the novelty of the *Book of Mormon* is possibly literary. Thus, it has been claimed that "the book is one of the earliest examples of frontier fiction, the first long Yankee narrative that owes nothing to English literary fashions. Except for the borrowings from the King James Bible, its sources are absolutely American" (Brodie 1963: 67).

Needless to say, the decision to put forward a text as if it were a translation is always an *individual* one. At the same time, there seem to be circumstances which give rise to a *multitude* of pseudotranslations, often from the same 'source' language, thus creating a whole tradition whose cultural significance is much greater than that of the sum of its individual members. Such a proliferation always attests to the internal organization of the culture involved and very little else. In particular, it bears out the position and role of [genuine] translations, or of a certain sub-system thereof, within it, which the pseudotranslators simply put to use. (Of course, any belief in a greater potential for generating income also reflects an awareness of a cultural constellation and an attempt to make the best of it!)

For instance, it has been shown that Russian literature of the beginning of the 19th century was crying out for texts which would have the identity of, and hence the prestige associated with, English novels of a particular kind. In response to this internal demand, a great number of books were produced in Russia itself which were presented as translations, many of them novels "by Ann Radcliffe", who was then regarded by the recipient culture as a most characteristic representative of the English novel in its required form (Masanov 1963). In a similar vein, Yahalom (1978: 42-52; 74-75) has argued convincingly that one of the most effective means of bringing about changes in French writing of almost the same period was to lean heavily on translations from English, genuine as well as fictitious, all serving the very same need. As a third example of an overriding tendency towards pseudotranslating I would cite Weissbrod's demonstration of the decisive role that fictitious translations, mainly "from the English", have played in establishing particular sectors of non-canonized Hebrew literature of the 1960s, most notably westerns, novels of espionage, romances and pornographic novels, where undisguised texts of domestic origin would have been considered downright inappropriate (1989: 94-99; 355-356).³

Cultural mechanisms can of course be put to deliberate use too, in acts of culture planning. On occasion, such a practice may actually be imposed from above, by agents endowed with the power to do precisely that, most notably political institutions in a totalitarian society. Thus, in his memoirs (1979:

3. Hints of similar practices are often scattered in scholarly works of the historical denomination which do not, however, regard the use of pseudotranslation as deserving independent, and in-depth discussion. See, for example, Thomas (1920: *passim*) for Spanish and Portuguese Romances of Chivalry, or Stewart (1969: *passim*) for the French Memoir-Novel.

161–162), the composer Dmitri Shostakovitch gives an example of how pseudo-translating was used, misused and abused in Stalin's Soviet Union.⁴

According to his account, there was an old Kazakh folk singer named Džambul Džabayev, who was famous throughout the Empire as a patriotic poet. Yet, nobody has ever encountered that man's poems in anything but Russian, a language he himself did not know. "And it turns out it was all made up. I mean, naturally, Dzhambul Dzhabayev existed as a person, and the Russian texts of his poems existed too; the translations, that is. Only the originals never existed".

The 'translations' of Džambul's non-existent poems were in fact written by "an entire brigade of Russian poetasters", who, in turn, didn't know any Kazakh. Some of them were rather well known figures in Soviet culture, which is why they were assigned the job in the first place: they knew only too well what was expected of them and of their poems. In spite of the fact that Džambul "didn't know anything about 'his' poems", he went on signing contract after contract, being "sure that it was his signature that he was paid for". He was also assigned the task of traveling throughout the Empire and promoting the poems by his personal appearance in public. The team "wrote fast and prolifically, and when one of the 'translators' dried up, he was replaced by a new, fresh one". "The factory was closed down only on Dzhambul's death", when he could no longer be taken advantage of in person.

Evidently, the authorities resorted to this practice in a highly calculated attempt to meet two needs at once, each drawing on a completely different source:

- (1) the poems had to praise 'the great leader' and his deeds in a way deemed appropriate. People of the Russian *intelligentsia* were in the best position to do that;
- (2) on the other hand, the new norms which were then being adopted in the Soviet Union demanded that "the new slaves ... demonstrate their cultural accomplishments to the residents of the capital" (Shostakovitch 1979: 164). Consequently, an author for the concoction had to be found in the *national republics*, and not in the Russian centre.

Significantly, comparable methods were used in music and in several other arts too, which renders the use of pseudotranslations in Stalin's Soviet Union part of a major of culture-planning operation, and a very successful one too, from the point of view of those who thought it out: mere disguise systematically turned into flat forgery.

4. I would like to thank Werner Koller for having brought this interesting case to my attention. In spite of his own skepticism as to the legitimacy of treating pseudo-translations within Translation Studies (e.g., 1992: 207), he was evidently able to realize the cultural significance of this phenomenon.

2. Pseudotranslations and Translation Studies

From what we have said so far it would seem clear that pseudotranslating is closely linked to genuine translation in terms of *cultural position*. Thus, texts come into being disguised as translations not just because there exists a notion of translation in a culture, but first and foremost because this notion and its realizations are assigned certain *functions* within it, which are, moreover, recognized and acknowledged by its members.

This in itself would have been reason enough to tackle fictitious translations together with genuine ones in any *function-oriented* type of observation. However, there is much more to this status, which qualifies pseudotranslating to feature in *product-oriented* studies too, and to a certain extent even in *process-oriented* ones, even though, admittedly, no genuine source text, hence no real translational relationships, (will) have been established.⁵ Thus, it is not the case that anything and everything presented as a translation would also pass for one, let alone without arousing any real suspicion. An author wishing to put on a serious act as a (pseudo)translator would do well to invest some efforts. S/he would not only have to find (or carve out) the appropriate niche for his/her prospective text (in terms of the systemic organization of the culture which would host it), such as combining a (pseudo-)source language with a text-type which would be in keeping with it. S/he would also have to invest some efforts in the formation of the text itself in a way which would be sufficiently persuasive.

What pseudotranslators often do, towards the attainment of this last goal, is incorporate in their texts features which have come to be associated, in the (target) culture in question, with translation – more often than not, with the translation of texts of a specific type and/or from a particular source language and textual tradition. By enhancing their resemblance to genuine translations they simply make it easier for their texts to pass as such. It is due to this practice

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5. To be sure, to the extent that the source of a pseudotranslation can be pointed to, it would normally consist in a group of texts, even the model underlying that group, rather than any individual text. For instance, it is clear that the author of the *Book of Mormon* took advantage of [a certain part of] the tradition of Bible translation into English. In a similar vein, one possible way of settling the long dispute over the authenticity of Macpherson's Ossianic poetry as a translation is precisely to maintain that it is various elements of a *tradition* of oral poetry in Gaelic rather than a finite number of actual texts which underlie this body of texts. (The possibility that there may have been one source text for any one of his target texts has long been ruled out.)

that it is sometimes possible to reconstruct from a fictitious translation at least bits and pieces of a fictitious source text in a particular language, as is the case with so many genuine translations whose sources have not (yet) been found. In fact, this is what Macpherson is believed to have done, when asked to produce the originals of his Ossianic poems: he went backwards and 're'translated some of his own English texts into a kind of Gaelic.

As is the case with parodies, which are akin to them in more than one respect, pseudotranslations often represent their pseudo-sources in a rather exaggerated manner. After all, the possibility, if not the need of actually activating a non-existent original in the background of the text is often an integral part of its proper realization as an intended translation.⁶ No wonder, then, that pseudotranslations are in a position to give a fairly good idea about the notions shared by the members of a community, not only as to the *status* of translated texts, but also as to their most conspicuous *characteristics*. "The point is that it is only when humans recognise the existence of an entity and become aware of its characteristics that they can begin to imitate it" (James 1989: 35), and overdoing in imitation is a clear, if extreme sign of such a recognition.

Thus, while I do not wish to claim that they constitute the most central objects of Translation Studies, pseudotranslating (as a practice) and pseudotranslations (as individual manifestations thereof) nevertheless emerge as proper objects of the discipline. In fact, they are no less objects of Translation Studies than normative pronouncements on translation, which will be highlighted in Chapter 2; and for the same reason too: they testify to what a society has become *conscious of* in its conception of translation, the establishment of which is, after all, an ultimate goal of the kind of studies advocated here.

6. For instance, occasional quotation from the Old Testament is one of the literary devices of the New Testament, although used quite sparsely. By contrast, the pseudotranslator of the *Book of Mormon* definitely overdoes it: about 25,000 words in his text consist of passages from the Old Testament, and about 2,000 more words were taken from the New Testament. It is almost as if, whenever "his literary reservoir ... ran dry ... he simply arranged for his Nephite prophets to quote from the Bible" (Brodie 1963: 58). At the same time, he "made minor changes in these Biblical extracts, for it seems to have occurred to him that readers would wonder how an ancient American prophet could use the exact text of the King James Bible. But he was careful to modify chiefly the italicized interpolations inserted for euphony and clarity by the scholars of King James; the unitalicized holy text he usually left intact" (ibid.). In the same vein, "the phrase 'and it came to pass'", which is typical to the book's style, appears at least two thousand times (Brodie 1963: 63).

3. The enlightening case of *Papa Hamlet*

To conclude our cursory discussion of pseudotranslations and their uses, let us dwell on some of the possibilities of their study by means of a more detailed presentation of one test case. The text in question pertains to German literature of the end of the 19th century – another period when pseudotranslating was resorted to quite massively, in all seriousness as well as with parodistic intentions. It is therefore an enlightening case in more than one respect.

In January 1889, a small book was published in Leipzig, whose title-page is reproduced as Figure 8. The 182 pages of this book contained three pieces of prose fiction: “Papa Hamlet” (pp. 11–90), “Der erste Schultag” [First Day at School] (pp. 91–149) and “Ein Tod” [A Death] (pp. 151–182). It opened with the translator’s preface, announced on the title-page (pp. 3–10) – a rather common habit at that time, especially with respect to translations which made a claim of importance. The preface itself was typical too. In the main, it consisted of an extensive biography of the author, Bjarne Peter Holmsen, a young Norwegian (b. 1860) who was claimed to be almost unknown even in his own country. One of its central passages discussed the difficulties encountered by the translator and the method he adopted, one of maximum acceptability on the linguistic level. It also expressed some (implicit) fear that alien forms may nevertheless have crept into his German:

Die Uebersetzung war, wie sich aus dem Vorstehenden wohl bereits von selbst ergibt, eine ausnehmend schwierige. Die speciell norwegischen Wendungen, von denen das Original begreiflicherweise nur so wimmelt, mussten in der deutschen Wiedergabe sorgfältig vermieden werden. Doch glaube ich, dass dies mir in den meisten Fällen gelungen ist. Ich habe keine Arbeit gescheut, sie durch heimische zu ersetzen, wo ich nur konnte. (Holz and Schlaf 1889: 8)

During the first few months after its publication, *Papa Hamlet* enjoyed relatively wide journalistic coverage. It was reviewed in dozens of newspapers and periodicals, regional and local as well as national, where it was always, apparently with no exception, approached and treated bona fide as a translation. At the same time, none of the reviewers, typical representatives of the German cultural milieu of the turn of the century (with the exception of at least one Norwegian critic, who claimed scanty acquaintance with the German literary scene but could have been expected to have at least some knowledge of the source literature), had any idea about Mr. Holmsen and his literary career.

BJARNE P. HOLMSEN.

PAPA HAMLET.

UEBERSETZT

UND MIT EINER EINLEITUNG VERSEHEN

VON

DR. BRUNO FRANZIUS.



LEIPZIG.

VERLAG VON CARL REISSNER.

1889.

Figure 8. *Papa Hamlet* – Title page

All of the information they supplied on these matters was drawn directly from the preface by the translator, whose doctoral degree must have enhanced the trust they placed in it – as did the fact that the profile and biography of the author seemed to correspond so very closely to those which could be expected

from a contemporary Scandinavian writer. Comical as it may sound, at least one reviewer went so far as to draw conclusions from the author's *portrait*, which was printed on the book's jacket.

Thus, everybody proceeded from the assumption that the author of *Papa Hamlet* was indeed a younger compatriot of Henrik Ibsen and Bjørnstjerne Bjørnson, and therefore likely to deserve attributes such as 'realistic', 'impressionistic' and 'pessimistic'; and the reviews indeed overflow with these. As to the translator, even though he was even less known to them than the author was, having published nothing before and not having betrayed any identifying details in his preface, some of the reviewers also referred to his translation work and its quality, in phrases such as: "the translator alone can be held responsible for the style", on the one hand, and "a very dexterous translation", "a good (sometimes even "very good") translation", or "the translation [is] ... a wonderful achievement" (but at least once to the opposite effect: "it seems that the translator has done Mr. H.[olmsen] serious injustice!"), on the other. All this in spite of the fact that none of them produced — or, for that matter, tried to produce — a copy of the original; that is, on the clear assumption that a book *presented* as a translation actually *is* one, and should therefore also be treated as a translation through and through.

Unless, of course, there is strong evidence to the contrary.

And, indeed, a few months later, counter-evidence began to pile up, until it became publicly known that *Papa Hamlet* was not a translation at all. Rather, the three stories were original German texts, the first literary results of the joint efforts of Arno Holz (1863–1929) and Johannes Schlaf (1862–1941). The two had also thought up the names of the author (most probably as a pun on Holz's own name: /bjARNE HOLmsen/) and translator, fabricated the translator's preface, including the whole of the author's biography, and even lent the latter a face (which was, in fact, the countenance of a cousin of Holz's, one Gustav Uhse [see Holz 1948: 291]).⁷

Thus, towards the end of 1889 it was the uncovered device of *pseudo*-translation which became a literary fact (in the sense assigned to this notion by

7. Due to these and many other details, German literary scholarship has normally attributed the whole book first and foremost to Arno Holz. It is only lately that some second thoughts have arisen, on the basis of comparative stylistic analysis as well as first consideration of some of the manuscripts (e.g., Stolzenberg 1977). The opinion prevailing at the moment seems to be more balanced, and hence more complex.

Jurij Tynjanov [1967b (¹1924))] within the German culture.⁸ Later on this device almost completely lost its status and remained partly a biographical (or genetic), partly a textual fact (since the 'translator's' preface, still signed 'Dr Bruno Franzius', was retained as part of the book even after it had already been assigned to its genuine authors, constituting a kind of frame-story, together with an added report of the dispelling of the mystification and an assortment of quotations from those reviews which had fallen for it). However, an essential factor for both Historical Poetics and Translation Studies is that, for several months in 1889, *Papa Hamlet* did function as a translation of a contemporary Norwegian book by a nice-looking young man who could boast a definite and convincing personal and literary biography — a translation which had been done by one Bruno Franzius, of whom nothing was known except for his doctoral degree and his assumed knowledge of the Norwegian language and literary scene. All this proved to be *factually wrong*, but it had nevertheless been *functionally effective*.⁹

What were Holz and Schlaf trying to achieve by producing — and selling — their own work disguised as a translation? In retrospect, it is easy to see that their main objective in writing *Papa Hamlet* was to experiment in freeing themselves from the narrow confines of French naturalism dominating the contemporary German literary scene — and in getting away with it. And they chose to do so by adopting certain norms and models of contemporary *Scandinavian* literature, which were indeed considered 'naturalistic' in a different way, because Scandinavian literature was rapidly gaining in popularity and esteem in Germany. As such, it was in a good position to contribute norms and models to German literature, and ultimately even reshape certain sections of its centre. At the time when Holz and Schlaf were writing *Papa Hamlet*, however, German

8. "Die Existenz eines Faktums als eines literarischen hängt von seiner differentialen Qualität ab, das heißt: davon, daß es auf eine sei es literarische, sei es außerliterarische Reihe bezogen wird. Anders ausgedrückt: von seiner Funktion.

Was in der einen Epoche ein literarisches Faktum ist, stellt in einer anderen ein umgangssprachliches Phänomen dar — oder umgekehrt: *je nach dem literarischen Gesamtsystem, in dem das Faktum fungiert*. Ein Freundesbrief Deržavin's ist ein Milieufaktum, ein Freundesbrief der Zeit Karamzins oder Puškins — ein literarisches Faktum" (Tynjanov 1967c [¹1927]: 43; italics added).

9. Interestingly, when I started investigating the fate of *Papa Hamlet*, the copy owned by the National Library in Jerusalem was still catalogued under 'Holmsen'. It was only after I contacted them, back in 1981, that Holz and Schlaf gained another entry.

literature itself was still highly resistant to the new trends, and Scandinavian-like innovations were still acceptable only as long as they were intimately tied up with actual *texts* of Scandinavian origin. Disguising a German literary work which took after Scandinavian models as a translation was thus a most convenient way out of a genuine dilemma.¹⁰

Nor was *Papa Hamlet* the only case of 'Scandinavian' pseudotranslation in Germany of those years, although in most of the other fictitious translations, the *parodistic* element was much more marked.¹¹ No less significant is the fact that most of these texts came from the very same literary circles in Germany, thus constituting a sub-culture, or a mini-tradition in the truest sense of the word. One literary-historical factor which may have furthered Holz and Schlaf's decision to resort to fictitious translating was the enormous success enjoyed only seven years earlier by Carl Bleibtreu's pseudotranslation (from the French): *Dies irae: Erinnerungen eines französischen Offiziers an die Tage von Sedan* (Stuttgart: Carl Krabbe, 1882). Interestingly, the whole book was [back-]translated into French; and the French translation, now regarded as an original, was then retranslated into German. (See Bleibtreu's foreword to the 1884 edition.) A similar route for *Papa Hamlet*¹² seems to have been blocked, maybe due to the fact that the mystery had been solved early on.

What Holz and Schlaf actually did, in terms of introducing novelties while tying them to a hypothetical alien tradition, was to embed a host of Scandinavian-like features in their stories. Needless to say, these were not features taken *directly* from Norwegian, or from any other Scandinavian literary works, to which the two had no access anyway. Rather, they were linguistic, textual and literary features pertinent to German *translations* of impressionistic and naturalistic texts of Scandinavian origin. Looked at from the German vantage point, these were therefore features which had come to be associated, whether correctly

10. In this connection, see also Z. Shavit's hypothesis concerning the entrance of a new model into a system, which reads: "a new model can enter a system only under disguise" (1989: 594).

11. For instance: Henrik Ipse. *Der Frosch: Familiendrama in einem Act*. Deutsch von Otto Erich [Hartleben]. Leipzig: Carl Reißner, 1889, or: [August Strindberg]. "Der Vater: Vierter Act". "Die Übertragung ins Deutsche hat Otto Erich [Hartleben] besorgt". *Freie Bühne* 1:37 (15.10.1890), 972-973.

12. According to the authors' own testimony: "... unser Buch, das übrigens — der Kuriosität wegen sei es erwähnt! — [wird] zur Zeit von Herrn Harald Hansen in Christiania [= Oslo] ins Norwegische übersetzt" (Holz and Schlaf 1890).

or incorrectly, with the latter. It is reasonable to assume that it is the adoption of these features, which indeed approximated the make-up of their texts to that of genuine translations, that helped persuade the readers and critics that *Papa Hamlet* was appropriately situated in the niche allocated to it. At any rate, it is a fact that very little suspicion arose.

The pseudotranslators were quite successful in attaining their goal, and *Papa Hamlet* indeed introduced 'Scandinavian-like' novelties into the German system. In fact, it came to be regarded as one of the most important forerunners of so-called *konsequenter Naturalismus*, the German brand of naturalism, which owes quite a bit to Scandinavian prototypes. It is however typical of the way pseudotranslations tend to be regarded by students of translation, even within Comparative Literature, that the book failed to receive any attention from those (like Bruns 1977) who purported to study the role of translation from Scandinavian literatures in the (re)shaping of German literature at the end of the 19th century.

Chapter 2

The Nature and Role of Norms in Translation

However highly one may think of Linguistics, Text-Linguistics, Contrastive Textology or Pragmatics and of their explanatory power with respect to translational phenomena, being a translator cannot be reduced to the mere generation of utterances which would be considered 'translations' within any of these disciplines. Translation activities should rather be regarded as having cultural significance. Consequently, 'translatorship' amounts first and foremost to being able to *play a social role*, i.e., to fulfil a function allotted by a community – to the activity, its practitioners and/or their products – in a way which is deemed appropriate in its own terms of reference. The acquisition of a set of norms for determining the suitability of that kind of behaviour, and for manoeuvring between all the factors which may constrain it, is therefore a prerequisite for becoming a translator within a cultural environment.

The process by which a bilingual speaker may be said to gain recognition in his/her capacity as a translator has hardly been studied so far. It will be speculated upon at some length towards the end of the book (Excursus C). In the present chapter the nature of the acquired norms themselves will be addressed, along with their role in directing translation activity in socio-culturally relevant settings. This presentation will be followed by a brief discussion of translational norms as a second-order object of Translation Studies, to be reconstructed and studied within the kind of framework which we are now in the process of sketching. As strictly translational norms can only be applied at the *receiving* end, establishing them is not merely *justified* by a target-oriented approach but should be seen as its very *epitome*.

1. Rules, norms, idiosyncrasies

In its socio-cultural dimension, translation can be described as subject to constraints of several types and varying degree. These extend far beyond the source text, the systemic differences between the languages and textual traditions involved in the act, or even the possibilities and limitations of the cognitive apparatus of the translator as a necessary mediator. In fact, cognition itself is influenced, probably even modified by socio-cultural factors. At any rate, translators performing under different conditions (e.g., translating texts of different kinds, and/or for different audiences) often adopt different strategies, and ultimately come up with markedly different products. Something has obviously changed here, and I very much doubt it that it is the cognitive apparatus as such.

In terms of their **potency**, socio-cultural constraints have been described along a scale anchored between two extremes: general, relatively absolute *rules* on the one hand, and pure *idiosyncrasies* on the other. Between these two poles lies a vast middle-ground occupied by intersubjective factors commonly designated *norms*. The norms themselves form a graded continuum along the scale: some are stronger, and hence more rule-like, others are weaker, and hence almost idiosyncratic. The borderlines between the various types of constraints are thus diffuse. Each of the concepts, including the grading itself, is relative too. Thus, what is just a favoured mode of behaviour within a heterogeneous group may well acquire much more binding force within a certain (more homogeneous) section thereof, in terms of either human agents (e.g., translators among texters in general) or types of activity (e.g., interpreting, or legal translation, within translation at large).

Along the **temporal axis**, each type of constraint may, and often does move into its neighbouring domain(s) through processes of rise and decline. Thus, mere whims may catch on and become more and more normative, and norms can gain so much validity that, for all practical purposes, they become as binding as rules; or the other way around, of course. Shifts of validity and force often have to do with changes of *status* within a society. In fact, they can always be described in connection with the notion of norm, especially since, as the process goes on, they are likely to cross its realm, i.e., actually become norms. The other two types of constraints may even be redefined in terms of norms: rules as '[more] objective', idiosyncrasies as '[more] subjective [or: less intersubjective]' norms.

Sociologists and social psychologists have long regarded norms as the

translation of general values or ideas shared by a community – as to what is right and wrong, adequate and inadequate – into performance instructions appropriate for and applicable to particular situations, specifying what is prescribed and forbidden as well as what is tolerated and permitted in a certain behavioural dimension (the famous ‘square of normativity’, which has lately been elaborated on with regard to translation in De Geest 1992: 38–40). Norms are acquired by the individual during his/her socialization and always imply *sanctions* – actual or potential, negative as well as positive. Within the community, norms also serve as criteria according to which actual instances of behaviour are *evaluated*. Obviously, there is a point in assuming the existence of norms only in situations which allow for different kinds of behaviour, on the additional condition that selection among them be nonrandom.¹ Inasmuch as a norm is really active and effective, one can therefore distinguish **regularity of behaviour** in recurrent situations of the same type, which would render regularities a main source for any *study* of norms as well.

The centrality of the norms is not only metaphorical, then, in terms of their relative position along a postulated continuum of constraints; rather, it is essential: Norms are the key concept and focal point in any attempt to account for the social relevance of activities, because their existence, and the wide range of situations they apply to (with the conformity this implies), are the main factors ensuring the establishment and retention of social order. This holds for cultures too, or for any of the systems constituting them, which are, after all, social institutions ipso facto. Of course, behaviour which does *not* conform to prevailing norms is always possible too. Moreover, “non-compliance with a norm in particular instances does not invalidate the norm” (Hermans 1991: 162). At the same time, there would normally be a price to pay for opting for any deviant kind of behaviour.

One thing to bear in mind, when setting out to study norm-governed behaviour, is that there is no necessary identity between the norms themselves and any formulation of them in language. Verbal formulations of course reflect *awareness* of the existence of norms as well as of their respective significance. However, they also imply other interests, particularly a desire to *control* behaviour – i.e., to dictate norms rather than merely account for them. Normative formulations tend to be slanted, then, and should always be taken with a grain of salt.

1. “The existence of norms is a sine qua non in instances of labeling and regulating; without a norm, all deviations are meaningless and become cases of free variation” (Wexler 1974: 4, n. 1).

2. Translation as a norm-governed activity

Translation is a kind of activity which inevitably involves at least two languages and two cultural traditions, i.e., at least two sets of norm-systems on each level. Thus, the 'value' behind it may be described as consisting of two major elements:

- (1) being a text in a certain language, and hence occupying a position, or filling in a slot, in the appropriate culture, or in a certain section thereof;
- (2) constituting a representation in that language/culture of another, pre-existing text in some other language, belonging to some other culture and occupying a definite position within it.

These two types of requirement derive from two sources which – even though the distance between them may vary greatly – are nevertheless always different and therefore often incompatible. Were it not for the regulative capacity of norms, the tensions between the two sources of constraints would have to be resolved on an entirely *individual* basis, and with no clear yardstick to go by. Extreme free variation may well have been the result, which it certainly is not. Rather, translation behaviour within a culture tends to manifest certain *regularities*, one consequence being that even if they are unable to account for deviations in any explicit way, the persons-in-the-culture can often tell when a translator has failed to adhere to sanctioned practices.

It has proven useful and enlightening to regard the basic choice which can be made between requirements of the two different sources as constituting an **initial norm**. Thus, a translator may subject him-/herself either to the original text, with the norms it has realized, or to the norms active in the target culture, or in that section of it which would host the end product. If the first stance is adopted, the translation will tend to subscribe to the norms of the source text, and through them also to the norms of the source language and culture. This tendency, which has often been characterized as the pursuit of adequate translation,² may well entail certain incompatibilities with target norms and practices, especially those lying beyond the mere linguistic ones. If, on the other hand, the second stance is adopted, norm systems of the target culture are triggered and set into motion. Shifts from the source text would be an almost inevitable price. Thus, whereas adherence to source norms determines a

2. "An adequate translation is a translation which realizes in the target language the textual relationships of a source text with no breach of its own [basic] linguistic system" (Even-Zohar 1975: 43; my translation).

translation's **adequacy** as compared to the source text, subscription to norms originating in the target culture determines its **acceptability**.

Obviously, even the most adequacy-oriented translation involves shifts from the source text. In fact, the occurrence of shifts has long been acknowledged as a true universal of translation. However, since the need itself to deviate from source-text patterns can always be realized in more than one way, the actual *realization* of so-called obligatory shifts, to the extent that it is non-random, and hence not idiosyncratic, is already truly norm-governed. So is everything that has to do with non-obligatory shifts, which are of course more than just possible in real-life translation: they occur everywhere and tend to constitute the majority of shifting in any single act of human translation, rendering the latter a contributing factor to, as well as the epitome of regularity.

The term 'initial norm' should not be overinterpreted, however. Its initiality derives from its superordinance over particular norms which pertain to lower, and therefore more specific levels. The kind of priority postulated here is basically *logical*, and need not coincide with any 'real', i.e., *chronological* order of application. The notion is thus designed to serve first and foremost as an *explanatory tool*: Even if no clear macro-level tendency can be shown, any micro-level decision can still be accounted for in terms of adequacy vs. acceptability. On the other hand, in cases where an overall choice has been made, it is not necessary that every single lower-level decision be made in full accord with it. We are still talking regularities, then, but not necessarily of any absolute type. It is unrealistic to expect absolute regularities anyway, in any behavioural domain.

Actual translation decisions (the results of which the researcher would confront) will necessarily involve some ad hoc combination of, or compromise between the two extremes implied by the initial norm. Still, for theoretical and methodological reasons, it seems wiser to retain the opposition and treat the two poles as distinct in principle: If they are not regarded as having distinct *theoretical* statuses, how would compromises differing in type or in extent be distinguished and accounted for?

Finally, the claim that it is basically a norm-governed type of behaviour applies to translation of all kinds, not only literary, philosophical or biblical translation, which is where most norm-oriented studies have been conducted so far. As has recently been claimed and demonstrated in an all too sketchy exchange of views in *Target* (M. Shlesinger 1989b and Harris 1990), similar things can even be said of *conference interpreting*. Needless to say, this does not mean that the exact same conditions apply to all kinds of translation. In fact, their application in different cultural sectors is precisely one of the aspects that

should be submitted to study. In principle, the claim is also valid for every society and historical period, thus offering a framework for historically oriented studies which would also allow for comparison.

3. Translational norms: An overview

Norms can be expected to operate not only in translation of all kinds, but also at every stage in the translating event, and hence to be reflected on every level of its product. It has proven convenient to first distinguish two larger groups of norms applicable to translation: preliminary vs. operational.

Preliminary norms have to do with two main sets of considerations which are often interconnected: those regarding the existence and actual nature of a definite translation policy, and those related to the directness of translation.

Translation policy refers to those factors that govern the choice of text-types, or even of individual texts, to be imported through translation into a particular culture/language at a particular point in time. Such a policy will be said to exist inasmuch as the choice is found to be nonrandom. Different policies may of course apply to different subgroups, in terms of either text-types (e.g., literary vs. non-literary) or human agents and groups thereof (e.g., different publishing houses), and the interface between the two often offers very fertile grounds for policy hunting.

Considerations concerning **directness of translation** involve the threshold of tolerance for translating from languages other than the ultimate source language: is indirect translation permitted at all? In translating from what source languages/text-types/periods (etc.) is it permitted/prohibited/tolerated/preferred? What are the permitted/prohibited/tolerated/preferred mediating languages? Is there a tendency/obligation to mark a translated work as having been mediated, or is this fact ignored/camouflaged/denied? If it is mentioned, is the identity of the mediating language supplied as well? And so on.

Operational norms, in turn, may be conceived of as directing the decisions made during the act of translation itself. They affect the matrix of the text – i.e., the modes of distributing linguistic material in it – as well as the textual make-up and verbal formulation as such. They thus govern – directly or indirectly – the relationships as well that would obtain between the target and source texts; i.e., what is more likely to remain invariant under transformation and what will change.

So-called **matricial norms** may govern the very *existence* of target-language

material intended as a substitute for the corresponding source-language material (and hence the degree of *fullness* of translation), its location in the text (or the form of actual *distribution*), as well as the textual *segmentation*.³ The extent to which omissions, additions, changes of location and manipulations of segmentation are referred to in the translated texts (or around them) may also be determined by norms, even though the one can very well occur without the other.

Obviously, the borderlines between the various matricial phenomena are not clear-cut. For instance, large-scale omissions often entail changes of segmentation as well, especially if the omitted portions have no clear boundaries, or textual-linguistic standing, i.e., if they are not integral sentences, paragraphs or chapters. By the same token, a change of location may often be accounted for as an omission (in one place) compensated by an addition (elsewhere). The decision as to what may have 'really' taken place is thus description-bound: What one is after is (more or less cogent) *explanatory hypotheses*, not necessarily 'true-to-life' accounts, which one can never be sure of anyway.

Textual-linguistic norms, in turn, govern the selection of material to formulate the target text in, or replace the original textual and linguistic material with. Textual-linguistic norms may either be *general*, and hence apply to translation qua translation, or *particular*, in which case they would pertain to a particular text-type and/or mode of translation only. Some of them may be identical to the norms governing non-translational text-production, but such an identity should never be taken for granted. This is the methodological reason why no study of translation can, or should proceed from the assumption that the latter is representative of the target language, or of any overall textual tradition thereof. (And see our discussion of 'translation-specific lexical items' in Chapter 11.)

It is clear that preliminary norms have both logical and chronological precedence over the operational ones. This is not to say that between the two major groups there are no relationships whatsoever, including mutual influences, or even two-way conditioning. However, these relations are by no means

3. The claim that principles of segmentation follow *universal* patterns is just a figment of the imagination of some discourse and text theoreticians intent on uncovering as many universal principles as possible. In actual fact, there have been various traditions (or 'models') of segmentation, and the differences between them always have implications for translation, whether they are taken to bear on the formulation of the target text or ignored. Even the segmentation of sacred texts such as the Old Testament itself has often been tampered with by its translators, normally in order to bring it closer to *target* cultural habits, and by so doing enhance the translation's acceptability.

fixed and given, and their establishment forms an inseparable part of any study of translation as a norm-governed activity. Nevertheless, we can safely assume at least that the relations which do exist have to do with the initial norm. They might even be found to *intersect* it – another important reason to retain the opposition between ‘adequacy’ and ‘acceptability’ as a basic coordinate system for the formulation of explanatory hypotheses.⁴

Operational norms as such may be described as serving as a model, in accordance with which translations come into being, whether involving the norms realized by the source text (i.e., adequate translation) plus certain modifications, or purely target norms, or a particular compromise between the two. Every model supplying performance instructions may be said to act as a *restricting* factor: it opens up certain options while closing others. Consequently, when the first position is fully adopted, the translation can hardly be said to have been made into the target language as a whole. Rather, it is made into a model-language, which is at best some part of the former and at worst an artificial, and as such nonexistent variety.⁵ In this last case, the translation is not really *introduced* into the target culture either, but is *imposed* on it, so to speak. Sure, it may eventually carve a niche for itself in the latter, but there is no initial attempt to accommodate it to any existing ‘slot’. On the other hand, when the second position is adopted, what a translator is introducing into the target culture

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4. Thus, for instance, in sectors where the pursuit of adequate translation is marginal, it is highly probable that indirect translation would also become common, on occasion even preferred over direct translation. By contrast, a norm which prohibits mediated translation is likely to be connected with a growing proximity to the initial norm of adequacy. Under such circumstances, if indirect translation is still performed, the fact will at least be concealed, if not outright denied.
 5. And see, in this connection, Izre'el's "Rationale for Translating Ancient Texts into a Modern Language" (1994). In an attempt to come up with a method for translating an Akkadian myth which would be presented to modern Israeli audiences in an oral performance, he purports to combine a "feeling-of-antiquity" with a "feeling-of-modernity" in a text which would be altogether simple and easily comprehensible by using a host of lexical items of *biblical* Hebrew in *Israeli* Hebrew grammatical and syntactic structures. Whereas "the lexicon ... would serve to give an ancient flavor to the text, the grammar would serve to enable modern perception". It might be added that this is a perfect mirror image of the way Hebrew translators started simulating spoken Hebrew in their texts: spoken lexical items were inserted in grammatical and syntactic structures which were marked for belonging to the written varieties (Ben-Shahar 1983), which also meant 'new' into 'old'.

(which is indeed what s/he can be described as doing now) is a *version* of the original work, cut to the measure of a preexisting model. (And see our discussion of the opposition between the 'translation of literary texts' and 'literary translation' in Excursus B as well as the detailed presentation of the Hebrew translation of a German *Schlaraffenland* text in Chapter 8.)

The apparent contradiction between any traditional concept of equivalence and the limited model into which a translation has just been claimed to be moulded can only be resolved by postulating that it is **norms that determine the (type and extent of) equivalence manifested by actual translations**. The study of norms thus constitutes a vital step towards establishing just how the functional-relational postulate of equivalence (see Chapter 1, Section 5 and Chapter 3, Section 6) has been realized – whether in one translated text, in the work of a single translator or 'school' of translators, in a given historical period, or in any other justifiable selection.⁶ What this approach entails is a clear wish to retain the notion of equivalence, which various contemporary approaches (e.g., Hönig and Kußmaul 1982; Holz-mänttari 1984; Snell-Hornby 1988) have tried to do without, while introducing one essential change into it: from an ahistorical, largely prescriptive concept to a historical one. Rather than being a single relationship, denoting a recurring type of invariant, it comes to refer to any relation which is found to have characterized translation under a specified set of circumstances.

At the end of a full-fledged study it will probably be found that translational norms, hence the realization of the equivalence postulate, are all, to a large extent, dependent on the position held by translation – the activity as well as its products – in the target culture. An interesting field for study is therefore comparative: the nature of translational norms as compared to those governing non-translational kinds of text-production. In fact, this kind of study is absolutely vital, if translating and translations are to be appropriately contextualized.

4. The multiplicity of translational norms

The difficulties involved in any attempt to account for translational norms should not be underestimated. These, however, lie first and foremost in two

6. See also my discussion of "Equivalence and Non-Equivalence as a Function of Norms" (Toury 1980a: 63-70).

features inherent in the very notion of norm, and are therefore not unique to Translation Studies at all: the socio-cultural specificity of norms and their basic instability.

Thus, whatever its exact content, there is absolutely no need for a norm to apply – to the same extent, or at all – to all sectors within a society. Even less necessary, or indeed likely, is it for a norm to apply across cultures. In fact, ‘sameness’ here is a mere coincidence – or else the result of continuous contacts between subsystems within a culture, or between entire cultural systems, and hence a manifestation of interference. (For some general rules of systemic interference see Even-Zohar 1990: 53–72.) Even then, it is often more a matter of apparent than of a genuine identity. After all, significance is only attributed to a norm by the *system* in which it is embedded, and the systems remain different even if instances of external behaviour appear the same.

In addition to their inherent specificity, norms are also unstable, changing entities; not because of any intrinsic flaw but by their very nature as norms. At times, norms change rather quickly; at other times, they are more enduring, and the process may take longer. Either way, substantial changes, in translational norms too, quite often occur within one’s life-time.

Of course, it is not as if all translators are *passive* in face of these changes. Rather, many of them, through their very activity, help in shaping the process, as do translation criticism, translation ideology (including the one emanating from contemporary academe, often in the guise of theory), and, of course, various norm-setting activities of institutes where, in many societies, translators are now being trained. Wittingly or unwittingly, they all try to interfere with the ‘natural’ course of events and to divert it according to their own preferences. Yet, the success of their endeavours is never fully foreseeable. In fact, the relative role of different agents in the overall dynamics of translational norms is still largely a matter of conjecture even for times past, and much more research is needed to clarify it.

Complying with social pressures to constantly adjust one’s behaviour to norms that keep changing is of course far from simple, and most people – including translators, initiators of translation activities and the consumers of their products – do so only up to a point. Therefore, it is not all that rare to find side by side in a society three types of competing norms, each having its own followers and a position of its own in the culture at large: the ones that dominate the center of the system, and hence direct translational behaviour of the so-called *mainstream*, alongside the remnants of *previous* sets of norms and the

rudiments of *new* ones, hovering in the periphery. This is why it is possible to speak – and not derogatorily – of being ‘trendy’, ‘old-fashioned’ or ‘progressive’ in translation (or in any single section thereof) as it is in any other behavioural domain.

One’s status as a translator may of course be temporary, especially if one fails to adjust to the changing requirements, or does so to an extent which is deemed insufficient. Thus, as changes of norms occur, formerly ‘progressive’ translators may soon find themselves just ‘trendy’, or on occasion as even downright ‘*passé*’. At the same time, regarding this process as involving a mere alternation of generations can be misleading, especially if generations are directly equated with age groups. While there often are correlations between one’s position along the ‘dated’-‘mainstream’-‘avant-garde’ axis and one’s age, these cannot, and should not be taken as inevitable, much less as a starting point and framework for the study of norms in action. Most notably, young people who are in the early phases of their initiation as translators often behave in an extremely epigonic way: they tend to perform according to dated, but still existing norms, the more so if they receive reinforcement from agents holding to dated norms, be they language teachers, editors, or even teachers of translation.

Multiplicity and variation should not be taken to imply that there is no such thing as norms active in translation. They only mean that real-life situations tend to be complex; and this complexity had better be noted rather than ignored, if one is to draw any justifiable conclusions. As already argued (mainly in Chapter 1, Section 3), the only viable way out seems to be to contextualize every phenomenon, every item, every text, every act, on the way to allotting the different norms themselves their appropriate position and valence. This is why it is simply unthinkable, from the point of view of the study of translation as a norm-governed activity, for all items to be treated on a par, as if they were of the same systemic position, the same significance, the same level of representativeness of the target culture and its constraints. Unfortunately, such an indiscriminate approach has been all too common, and has often led to a complete blurring of the normative picture, sometimes even to the absurd claim that no norms could be detected at all. The only way to keep that picture in focus is to go beyond the establishment of mere ‘check-lists’ of factors which may occur in a corpus and have the lists *ordered*, for instance with respect to the status of those factors as characterizing ‘mainstream’, ‘dated’ and ‘avant-garde’ activities, respectively.

This immediately suggests a further axis of contextualization, whose necessity has so far only been implied; namely, the *historical* one. After all, a norm can only be marked as ‘dated’ if it was active in a *previous* period, and if, at

that time, it had a different, 'non-dated' position. By the same token, norm-governed behaviour can prove to have been 'avant-garde' only in view of *subsequent* attitudes towards it: an idiosyncrasy which never evolved into something more general can only be described as a norm by extension, so to speak (see Section 1 above). Finally, there is nothing inherently 'mainstream' about mainstream behaviour, except when it happens to function as such, which means that it too is time-bound. What I am claiming here, in fact, is that historical contextualization is a must not only for a *diachronic* study, which nobody would contest, but also for *synchronic* studies, which still seems a lot less obvious, unless one has accepted the principles of so-called 'Dynamic Functionalism' (for which, see the Introduction to Even-Zohar 1990⁷ and Sheffy 1992: *passim*).

Finally, in translation too, *non-normative behaviour* is always a possibility. The price for selecting this option may be as low as a (culturally determined) need to submit the end product to revision. However, it may also be far more severe, to the point of taking away one's earned recognition as a translator; which is precisely why non-normative behaviour tends to be the exception, in actual practice. On the other hand, in retrospect, deviant instances of behaviour may be found to have effected *changes* in the very system. This is why they constitute an important field of study, as long as they are regarded as what they have really been and are not put indiscriminately into one basket with all the rest. Implied are intriguing questions such as who is 'allowed' by a culture to introduce changes and under what circumstances such changes may be expected to occur and/or be accepted.

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7. "There is a clear difference between an attempt to account for some major principles which govern a system outside the realm of time, and one which intends to account for how a system operates both 'in principle' and 'in time.' Once the historical aspect is admitted into the functional approach, several implications must be drawn. First, it must be admitted that both synchrony and diachrony are historical, but the exclusive identification of the latter with history is untenable. As a result, synchrony cannot and should not be equated with statics, since at any given moment, more than one diachronic set is operating on the synchronic axis. Therefore, on the one hand a system consists of both synchrony and diachrony; on the other, each of these separately is obviously also a system. Secondly, if the idea of structuredness and systemicity need no longer be identified with homogeneity, a semiotic system can be conceived of as a heterogeneous, open structure. It is, therefore, very rarely a uni-system but is, necessarily, a polysystem" (Even-Zohar 1990: 11).

5. Studying translational norms

So far we have discussed norms mainly in terms of their activity during a translation event and their effectiveness in the act of translation itself. To be sure, this is precisely where and when translational norms are active. However, what is actually available for observation is not so much the norms themselves, but rather norm-governed instances of behaviour. To be even more precise, more often than not, it is the products of such behaviour. Thus, even when translating is claimed to be studied directly, as is the case with the use of 'Thinking-Aloud Protocols' (see Chapter 12, Section 3), it is only *products* which are available, although products of a different kind and order. Norms are not directly observable, then, which is all the more reason why something should also be said about them in the context of an attempt to *account* for translational behaviour.

There are two major sources for a reconstruction of translational norms, textual and extratextual.⁸

- (1) **textual**: the translated texts themselves, for all kinds of norms, as well as analytical inventories of translations (i.e., 'virtual' texts), for various preliminary norms;
- (2) **extratextual**: semi-theoretical or critical formulations, such as prescriptive 'theories' of translation, statements made by translators, editors, publishers, and other persons involved in or connected with the activity, critical appraisals of individual translations, or the activity of a translator or 'school' of translators, and so forth.

There is a fundamental difference between these two types of source: Texts are *primary* products of norm-regulated behaviour, and can therefore be taken as immediate representations thereof. Normative pronouncements, by contrast, are merely *by-products* of the existence and activity of norms. Like any attempt to formulate a norm, they are partial and biased, and should therefore be treated with every possible circumspection; all the more so since — emanating as they do from interested parties — they are likely to lean toward propaganda and persuasion. There may therefore be gaps, even contradictions, between explicit arguments and demands, on the one hand, and actual behaviour and its results, on

8. Cf., e.g., Vodička (1964: 74), on the possible sources for the study of literary norms, and Wexler (1974: 7-9), on the sources for the study of prescriptive intervention ('purism') in language.

the other, due either to subjectivity or naivete, or even lack of sufficient knowledge on the part of those who produced the formulations. On occasion, a deliberate desire to mislead and deceive may also be involved. Even with respect to the translators themselves, intentions do not necessarily concur with any declaration of intent (which is often put down *post factum* anyway, when the act has already been completed); and the way those intentions are realized may well constitute a further, third category still.

Yet all these reservations – proper and serious though they may be – should not lead one to abandon semi-theoretical and critical formulations as legitimate sources for the study of norms. In spite of all its faults, this type of source still has its merits, both in itself and as a possible key to the analysis of actual behaviour. At the same time, if the pitfalls inherent in them are to be avoided, normative pronouncements should never be accepted at face value. They should rather be taken as *pre-systematic* and given an explication in such a way as to place them in a narrow and precise framework, lending the resulting *explicata* the coveted systematic status. While doing so, an attempt should be made to clarify the status of each formulation, however slanted and biased it may be, and uncover the sense in which it was not just accidental; in other words, how, in the final analysis, it does reflect the cultural constellation within which, and for whose purposes it was produced. Apart from sheer speculation, such an explication should involve the comparison of various normative pronouncements to each other, as well as their repeated confrontation with the patterns revealed by [the results of] actual behaviour and the norms reconstructed from them – all this with full consideration for their contextualization. (See a representative case in Weissbrod 1989.)

It is natural, and very convenient, to commence one's research into translational behaviour by focussing on *isolated* norms pertaining to well-defined behavioural dimensions, be they – and the coupled pairs of replacing and replaced segments representing them – established from the source text's perspective (e.g., translational replacements of source metaphors) or from the target text's vantage point (e.g., binomials of near-synonyms as translational replacements). However, translation is intrinsically *multi-dimensional*: the manifold phenomena it presents are tightly interwoven and do not allow for easy isolation, not even for methodical purposes. Therefore, research should never get stuck in the blind alley of the 'paradigmatic' phase which would at best yield lists of 'normemes', or discrete norms. Rather, it should always proceed to a 'syntagmatic' phase, involving the *integration* of normemes pertaining to various problem areas. Accordingly, the student's task can be characterized as an attempt

to establish what *relations* there are between norms pertaining to various domains by correlating his/her individual findings and weighing them against each other. Obviously, the thicker the network of relations thus established, the more justified one would be in speaking in terms of a normative *structure* (cf. Jackson 1960: 149-160) or *model*.

This having been said, it should again be noted that a translator's behaviour cannot be expected to be fully systematic. Not only can his/her decision-making be differently motivated in different problem areas, but it can also be unevenly distributed throughout an assignment within a single problem area. Consistency in translational behaviour is thus a *graded* notion which is neither nil (i.e., total erraticness) nor 1 (i.e., absolute regularity); its extent should emerge at the end of a study as one of its conclusions, rather than being presupposed.

The American sociologist Jay Jackson suggested a 'Return Potential Curve', showing the distribution of approval/disapproval among the members of a social group over a range of behaviour of a certain type as a model for the representation of norms. This model (reproduced as Figure 9) makes it possible to make a gradual distinction between norms in terms of *intensity* (indicated by the height of the curve, its distance from the horizontal axis), the *total range of tolerated behaviour* (that part of the behavioural dimension approved by the group), and the *ratio* of one of these properties of the norm to the others.

One convenient division that can be re-interpreted with the aid of this model is tripartite:⁹

- (a) **Basic (primary) norms**, more or less mandatory for *all* instances of a certain behaviour (and hence their minimal common denominator). Occupy the apex of the curve. Maximum intensity, minimum latitude of behaviour.
- (b) **Secondary norms, or tendencies**, determining favourable behaviour. May be predominant in certain *parts* of the group. Therefore common enough, but not mandatory, from the point of view of the group as a whole. Occupy that part of the curve nearest its apex and therefore less intensive than the basic norms but covering a greater range of behaviour.
- (c) **Tolerated (permitted) behaviour**. Occupies the rest of the 'positive' part of the curve (i.e., that part which lies above the horizontal axis), and therefore of minimal intensity.

9. Cf., e.g., Hrushovski's similar division (in Ben-Porat and Hrushovski 1974: 9-10) and its application to the description of the norms of Hebrew rhyme (in Hrushovski 1971b).

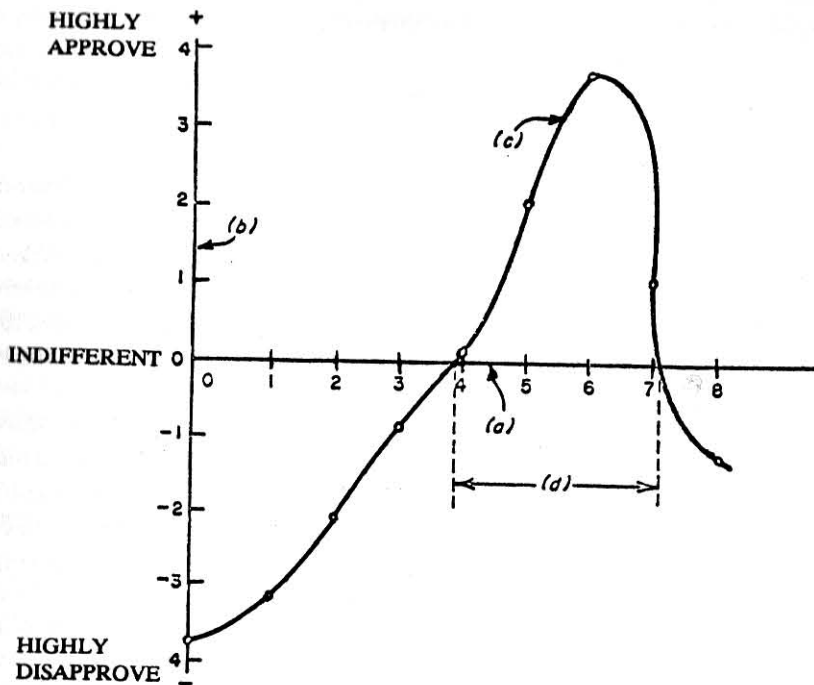


Figure 9. Schematic diagram showing the Return Potential Model for representing norms: (a) a behaviour dimension; (b) an evaluation dimension; (c) a return potential curve, showing the distribution of approval-disapproval among the members of a group over the whole range of behaviour; (d) the range of tolerable or approved behaviour. (Reproduced from Jackson 1960.)

A special group, detachable from (c), seems to be of considerable interest and importance, at least in some behavioural domains:

(c') **Symptomatic devices.** Though these devices may be infrequently used, their occurrence is typical for narrowing segments of the group under study. On the other hand, their absolute *non*-occurrence can be typical of other segments.

We may, then, safely assume a *distributional* basis for the study of norms: the more frequent a target-text phenomenon, a shift from a (hypothetical) adequate reconstruction of a source text, or a translational relation, the more likely it is to reflect (in this order) a more permitted (tolerated) activity, a

stronger tendency, a more basic (obligatory) norm. A second aspect of norms, their *discriminatory capacity*, is thus reciprocal to the first, so that the less frequent a behaviour, the smaller the group it may serve to define. At the same time, the group it does define is not just any group; it is always a sub-group of the one constituted by higher-rank norms. To be sure, even idiosyncrasies (which, in their extreme, constitute groups-of-one) often manifest themselves as personal ways of realizing [more] general attitudes rather than deviations in a completely unexpected direction.¹⁰ Be that as it may, the retrospective establishment of norms is always relative to the section under study, and no automatic upward projection is possible. Any attempt to move in that direction and draw generalizations would require further study, which should be targeted towards that particular end.

Finally, the curve model also enables us to redefine one additional concept: the actual *degree of conformity* manifested by different members of a group to a norm that has already been extracted from a corpus, and hence found relevant to it. This aspect can be defined in terms of the distance from the point of maximum return (in other words, from the curve's apex).

Notwithstanding the points made in the last few paragraphs, the argument for the distributional aspect of norms should not be pushed too far. As is so well known, we are in no position to point to strict statistical methods for dealing with translational norms, or even to supply sampling rules for actual research (which, because of human limitations, will always be applied to samples only). At this stage we must be content with our intuitions, which, being based on knowledge and previous experience, are 'learned' ones, and use them as keys for selecting corpuses and for hitting upon ideas. This is not to say that we should abandon all hope for methodological improvements. On the contrary: much energy should still be directed toward the crystallization of systematic research methods, including statistical ones, especially if we wish to transcend the study of norms, which are always limited to one societal group at a time, and move on to the formulation of general laws of translational behaviour, which would inevitably be probabilistic in nature (see Part Four). To be sure, achievements of actual studies can themselves supply us with clues as to necessary and possible methodological improvements. Besides, if we hold up research until the most systematic methods have been found, we might never get any research done.

10. And see the example of the seemingly idiosyncratic use of Hebrew *ki-xen* as a translational replacement of English 'well' in a period when the norm dictates the use of *u-vexen* (Chapter 4, Section 3).

Chapter 3

Constituting a Method for Descriptive Studies

Let us now take one step forward and examine from closer proximity the main discovery procedures and the basic notions mentioned in the course of their skeletal presentation.

1. Assumed translations and their acceptability

When proceeding within a target culture, what first lends itself to observation is a body of *texts*, which can, and should be approached on the assumption that they are translated.

Obviously, there may be many reasons for regarding a text as a translation. The text may be explicitly *presented* as one, but it may just as well exhibit certain features which, in the culture in question, have come to be associated with translations (or, more narrowly still, texts translated from a particular culture/language). Knowledge of the existence of a text in *another* language and culture, which a target-language text is taken to have replaced, may also serve as a trigger for adopting the assumption that that text is a translation. This last possibility is of paramount heuristic importance for cultures, or historical periods, where translations exist as concealed facts¹ – whether it is only the presentation of a text as being of a derived nature which is not customary or whether the very distinction between translations and non-translations is not

1. I am grateful to José Lambert for this invaluable observation.

culturally functional and is hence blurred.²

Unlike the first two options, to start off with an entity which has absolutely no standing in the target culture would seem at odds with the recommended order of discovery procedures, if not the very target-orientedness of our approach. In fact it is not: only an 'alien' text which would have been found to actually have at least one counterpart in whatever *target* culture one is interested in would become an object of study; thus, whereas target-language texts can be studied as (assumed) translations even in the absence of corresponding source texts, a study reduced to a *non*-target text would never constitute a study in translation. It is precisely the earmarking of *another* text as its assumed translation which is necessary for admitting it into such a study. Once a text has thus been earmarked, research can proceed in the foreseen order. In fact, recourse to the (assumed) source text can easily – and justifiably – be suspended until it is called for, the more so as the possibility is always there that the target text in question will be found *not* to have been derived from it, after all, or not from it alone.

All in all, whatever the reasons for the tentative marking of a text as a translation, it is advisable to start by studying assumed translations, along with their constituents, in terms of their ACCEPTABILITY in the system(s) of which they purportedly form part. To be sure, the main justification for tackling acceptability first is *methodical*. In principle, this issue can be dealt with at any point. It is only that once 'alien' texts have been brought into the picture, it tends to get blurred – especially in cases where the acceptability of a translation *qua* translation does not fully concur with acceptability in general; that is, when the norms governing the formulation of translated texts differ from those which govern original compositions. From that point on, it may be difficult to re-adopt the initial 'naive' stance and approach the translation as a text in its own right, and not just as a representation of another text.

The important thing here is that, even when assumed translations are studied as target-language texts, translation description may already be said to take place, both in cases where the principles embodied in them are found to *concur* with the ones manifested by 'native' texts and in those where *differences* are observed in the make-up of texts pertaining to the two groups. Most signifi-

2. In cases of the latter type (e.g., medieval cultures) it may prove useful to temporarily hold all texts as suspect of having come into being through translation, and then go about reducing the corpus by elimination, on the basis of the study's own findings. In my ongoing study of Hebrew writing at the beginning of the Enlightenment period, this heuristics has proved its usefulness beyond any doubt.

cant is the latter case, where the differences are found to reveal *regularities* which may be tentatively attributed to the texts' being members of functionally distinct (sub)systems. Later on, some of the differences observed at this initial stage may well be attributable to formal relationships to an underlying source text in a certain language/cultural tradition. However, as such relationships can often be tentatively concluded from the assumed translation alone, they may be found to be pertinent to *pseudotranslations* too. Hypothetically identified relationships may also give rise to the assumption that a target text drew on a text in a language other than the assumed one, or on more than one source text, in more than one language.

2. Types of comparison at the initial stage

The first stage of the study also offers a possibility of several types of comparative work, which may add a dimension to the account of the texts' acceptability.³ Even though no source text would be *physically* present in any of these initial types of comparison, its *posited* existence will nevertheless hover in the background, giving rise to various tentative hypotheses of an explanatory nature.

The easiest comparative study to perform involves various parallel translations in one language, which came into being at one point in time. This kind of comparison is also the easiest to justify, because it involves the smallest number of variables. If each translation is properly contextualized, such a study is therefore bound to shed light on the correlations between surface realization and position (or 'valence') in the target culture.

A good example of such a study would be Tiina Puurtinen's 1989 dissertation, precisely because of its modesty: it only tackled a 'small', hence relatively easily manageable comparative issue. As part of this study, two Finnish translations of L. Frank Baum's *The Wizard of Oz*, both published in 1977, were compared to each other with a view to their relative *readability*, which was taken to constitute one aspect of their overall acceptability as Finnish children's books. Of course, a translation's readability may be highly influenced by the strategies applied to the original and hence may be intimately connected with the relationships between the two texts. However, in itself it characterizes the translation *as a target-language text*. Moreover, in the case under study, differences of

3. For various types of comparative research within Translation Studies see also Reiß 1981 (who, however, does not include all the possibilities mentioned in this section).

linguistic choice which have affected the relative readability of the two translations were found to have had very little to do with features of the source text, but only with choices made by the translators within the target system itself. (See Puurtinen 1989a, 1989b.)

Due to greater availability of a number of parallel translations into one language, which came into being in different periods of time, their comparison has been even more common. Such comparisons are possible, of course, but they represent a much more complex task than one would think. Thus, if the study is to have any real significance, at least the notion of (one) target language would have to be modified, in view of the fact that languages undergo constant changes. The need for such a modification would become all the more urgent as the intervals between the translations grew longer, or as the pace of the changes grew more rapid.

Then again, one may wish to compare different phases of the emergence of a single translation, trying to trace at least the way individual translators waver between different concepts of acceptability as they move along. Chapter 9 will be devoted to the specific possibilities of, and problems involved in studies of this kind, whereas Chapter 10 will bring an account of the successive revisions of one textual portion, a Hebrew translation of Hamlet's monologue "To Be or Not To Be", which will also show how instructive it may be to suspend the use of the (assumed) source text.

In this particular case, an aspiration to approximate the target text to the original does not seem to have affected the translator's subsequent decisions. Rather, his concern was more and more with the position of his own text in a particular niche of the *recipient* culture. So marked was his tendency towards acceptability that even the question of what he used as an immediate source text will emerge as marginal. Intriguing as it may be, its conclusive identification would change practically nothing in our understanding of the translated version, or even of the process by which it came into being. There are, of course, acts of translation where measures *are* taken to bring the emerging translation closer to [a certain notion of the 'adequate' translation of] the original. In these cases, introducing the source text at a later stage, which is the procedure recommended here, would be much more revealing.

Finally, several (assumedly parallel) translations into different languages can also be studied comparatively, e.g., as a means of assessing the impact of various factors on the modelling of a translation, with an eye to drawing the distinction between what is universal and what is culture-, or language-specific

in this type of behaviour. Differences of language and cultural tradition are of course much more difficult to handle than a mere difference of time within a single tradition. Therefore, if anything is comparable here, it is the findings of separate analyses rather than the texts themselves, or any segments thereof.

Even though definitely of an exploratory nature, a good example of an investigation of this kind is Nitsa Ben-Ari's 1988 MA Thesis, where phenomena such as repetitions, omissions, additions and attenuation were studied in the English, French and Italian translations of a sample of German literary texts and interesting conclusions were drawn on the way to establishing some general Laws of Translational Behaviour.

3. Coming up with the appropriate source text

The study of acceptability involves the relationships between (assumed) translations and other members of their hosting systems, as well as these systems as organized wholes. This is why no need for a source text has arisen so far. The culturally determined notion of translation, however, also entails the relations which tie together the target- and source-texts. Once one's attention is turned to finding out how the Relationship Postulate was realized in a particular case (see Chapter 1), it is clear that one would first have to establish the SOURCE-TEXT'S IDENTITY, and in an appropriate way too.

Of course, there are many cases where the problematic nature of this task is neutralized by the straightforward nature of the data. However, when devising a research method, provisions should be made for any kind of possible complication; and there are indeed several cases where a **multitude of candidates for a source text** may exist. In cases of this kind, any attempt to justify a researcher's selection of a source text would depend, at least in part, on what the target text itself exhibits, which would render the establishment of the source text's identity part of the comparative analysis itself. In each one of these cases, the reasons *why* the text actually picked was deemed preferable as a source text constitute an interesting issue in itself. Uncovering these reasons may even have important implications for the overall account of the relationships between function, process and product, e.g., in the form of what I have called 'preliminary norms' (Chapter 2).

First, there are situations where, even in one source culture and language, assumed source texts exist in more than one version. Who could tell for sure, prior to at least some deliberate work, which one of them might have served as

the immediate source of a particular translation, if it was indeed a single text, and not a combination of several? To be sure, this is a real issue not only for remote periods in the past. Two possible literary examples in modern times would be D.H. Lawrence's *Lady Chatterley's Lover* (the two Hebrew translations of the novel were based on two different English versions) and John Fowles' *The Magus*.

Then again, there are cases where candidates for a source text appear in more than one language. Of course, not only may any one of these texts have been used as a basis for a certain translation, but a *compilative* source text is always a possibility too. Medieval translations are probably the most often cited example here, and extensive efforts are indeed invested in establishing the internal relations between different versions, within one language as well as across languages.

A specific group which is of particular interest for later periods involves so-called *self-translations*. Thus, what version of a Beckettian text was used by one or another of his/her translators — the French, the English or an ad hoc, idiosyncratic combination of both? Surely nobody could tell, unless the target text was regarded from that standpoint, or, better still, unless some comparative analysis has already taken place. Needless to say, it won't do to pick up for further comparison just any version, much less so the one which the researcher him-/herself regards as most 'befitting' of the status of a source text, whether because it was the first to come into being or because it is allegedly the best.⁴

Another group of assumed source texts in different languages involves the possible use of *mediating translations*, done this time by persons other than the original author. Being a text in its own right, a translation can easily function as a proper source text in spite of its derived nature, and it is this text that should be compared to the target one which is found to have proceeded from it. Picking up the first link of the chain just because of its privileged status as a kind of an

4. Claims have recently been made that, in the case of self-translators, 'bilingual works' composed of the various parallel unilingual texts they produced would constitute ultimate texts; e.g.: "Because self-translation ... makes a text retrospectively incomplete, both versions become avatars of a *hypothetical total text* in which the versions in both languages would rejoin one another and be reconciled" (Beaujour 1989: 112; emphasis added). One author even devoted a whole chapter to experimenting with the establishment of bilingual versions of some of Beckett's texts, using several principles (Fitch 1988: Chapter 11). Interesting as this may be, it comes to bear on the subject at hand only when it happens to do so; i.e., when a *translator* has actually done what Fitch was just experimenting with.

'ultimate original' is bound to lead the investigator astray, the more so if the mediating text was adjusted to norms of acceptability in its own (target) culture. True enough, many producers of second-hand translations would have failed to make this fact known, especially in cultural contexts where translation is regarded as just one of several ways of generating texts, or else where prevailing norms disfavour indirect translation. However, the assumed translation would often betray the fact and literally oblige the conscientious researcher to track down the required mediating version. The question of appropriate source text would thus have been settled in the course of the study again, as one of its very first results.

Let us illustrate the points we have made so far, this time on the basis of *subtitles* as they are used in films or TV programs:

In cultures which resort to this practice, subtitles tend to be regarded as translated utterances. Very often they also *function* as such, so that they are sometimes even read differently from texts which are taken not to have been translated. The reason is not only that, in this particular case, one always receives two parallel texts (normally a spoken and a written one), and that the two are in two different languages, to begin with; it also has much to do with cultural *conventions* as to the status of printed lines which appear in a certain position on the screen (a position which is cultural-bound as well).

In pre-systematic situations it is the spoken version that is normally regarded as the source text, and target-culture viewers who can do so often find themselves comparing the subtitles to it. However, this would not do as a premise for an *investigation* of subtitles as translations. After all, in reality, there can be many candidates for a source text; e.g., in addition to the spoken version, which often turns out not to have served as an immediate source at all, a script in the language used for the spoken version, a previous text that the script itself drew on, a translation of that text into either the language used for the subtitles or any other language, a translated script, and, of course, a combination of some (or all) of these alternatives. There is really nothing one could take for granted, and if the study is to lead anywhere at all, the identity of the source text(s) will have to be established for each case anew.⁵ To be sure, the possible multiplicity of candidates for a source text has no bearing on the status of the subtitles them-

5. Quite surprisingly, in a long and elaborate "checklist of questions with which the researcher has to approach the empirical facts" in the case of "film and T.V. translation" (Delabastita 1989), the need to establish the source text *properly* as a precondition for the extraction of translational relationships is not so much as mentioned. The author seems to have accepted willy nilly the pre-systematic assumption that, in subtitling, there exist only "the source verbal text", which is basically spoken, and "the subtitled text".

selves as assumed translations, which is established in the *target* cultural context alone – sometimes with a pre-systematic notion of the possible source text, and often with no regard for the very question. By contrast, the establishment of the source text will underlie all that will be done from that point on by way of investigating the subtitles.

This situation could manifest itself in a more complex form still; namely, when more than one set of subtitles exists; in different versions of the film, and especially in one and the same copy (e.g., Flemish and French in Belgium, Hebrew and Arabic, or – lately – Hebrew and Russian in Israel). Is there any way of knowing, prior to a deliberate study, whether the subtitles in all languages drew on the same source(s), or whether any source-target relations exist between the different subtitles themselves, such as having one set translated directly from another? Proper answers to these questions are bound to bear strongly on the study and its findings. Thus, due to power relations within Belgian culture, French subtitles ‘imported’ from France or translated at home, either from a ‘real’ source text or from a preexisting set of Flemish subtitles (or the script underlying them), may well have a very different status, which, moreover, will often be realized by the spectators and will therefore direct their perception of the text. Defining that status, and the reasons underlying it, is therefore a precondition for any viable *explanation* of similarities and differences shown by these subtitles.⁶

4. (Translation) solutions and (source) problems

It would have been nice, even though difficult to handle, had only textual wholes been bound by translation relationships. As it is, these relations normally obtain first and foremost between TEXTUAL SEGMENTS, very often even small-scale, rather low-level linguistic items. Mapping each assumed translation onto its assumed source would thus result in assigning the status of outright translation SOLUTIONS to various constituents of the target text, which would so far have been considered, rather vaguely, as ‘translational phenomena’. Due to this procedure, which yields a series of (ad hoc) coupled pairs of replacing + replaced segments, a target-text solution would never just *imply* the existence of a corresponding PROBLEM in the source text. Rather, **the two should be conceived of as determining each other in a mutual way.**

The kind of problems which are relevant for a retrospective study are

6. A whole MA Thesis (Desmet 1989) was recently devoted to uncovering the ‘real’ source texts underlying the (Flemish and French) subtitles of the German film “Ganz unten” as well as some of the implications of their use. I would like to thank José Lambert and Clem Robyns for drawing my attention to this dissertation.

therefore *reconstructed* rather than given: like the appropriateness of the source text itself, they have to be established in the course of a comparative analysis rather than on the basis of the source text alone, let alone its initial translatability into the target language in question. Consequently, what is identified as a problem vis-à-vis one pair of texts will not necessarily emerge as a problem at all, much less so a problem of the same kind and magnitude, within another comparative study, even if that other study only involves a different translation of the same text.

What the last assertion implies is the claim that, under a retrospective observation, only those facts of the source text are of significance which can be shown to have *actually* posed a problem; and this status of theirs can only be established through a concurrent identification of the respective solution. To be sure, even if all potential difficulties established in a thorough analysis of a (source) text itself are realized, facts which seem to present no initial difficulty may nevertheless turn out to have constituted a problem under a reconstructive observation, as exemplified, e.g., by the places where translators feel an urge to revise their emerging texts 'as well as by the nature of the revisions themselves. Problem items of this kind would go completely unnoticed, unless they are established 'in reverse'.⁷

A major issue in connection with the notion of the coupled pair concerns its boundaries: how are they to be determined, and what kind of justification would be given to their determination? The difficulty in answering these questions lies in two basic facts: (a) any source-text entity, at any level and of any scope, may in principle turn out to have represented a translationally relevant segment; and (b) there is no need for a replacing entity to be identical,

7. And see Wolfgang Lörcher's characterization of 'translation problem' as occurring "when a subject realizes that, at a given point in time, s/he is unable to transfer or to transfer adequately a source-language text segment into the target-language" (1991: 80). This characterization was offered within a *psycholinguistic* frame of reference, and with the translating *subject* as its focal point. "In other words," says Lörcher, "only those text segments which *the subjects* cannot translate or which *the subjects* have tried to translate but whose results they then consider to be inadequate, represent translation problems" (ibid.). The fact that, in spite of the difference of theoretical framework and immediate concerns, this conception is so close to the one adopted for our type of investigation goes a long way towards justifying it. See also Lörcher's more detailed discussion of "The Concept of 'Translation Problem'" in Section 5.2.1 of his book (1991: 92-96), which owes a great deal to Krings' treatment of the subject (1986: 112-171).

in rank or in scope, to the replaced one. One, but not both of them can even be zero, as in the case of omission vs. addition (see Section 5 below).

The solution suggested here is offered only as a *methodical* way out, even though it may well have *theoretical* implications too. Thus, the analyst will go about establishing a segment of the target text, for which it would be possible to claim that – beyond its boundaries – there are no leftovers of the solution to a translation problem which is represented by one of the source text's segments, whether similar or different in rank and scope. It is this procedure that I had in mind when I characterized the determination of the members of a coupled pair as being mutual. We will return to the nature of the coupled pair of replacing + replaced segments and its justification in Chapter 4. At this point, let us just look at an example, designed to demonstrate the ad hoc nature of the pair and its great versatility.

In his famous "Juvenile History in Seven Tricks", *Max und Moritz*, first published in 1865, the German writer Wilhelm Busch wrote:

[Durch den Schornstein mit Vergnügen
Sehen sie] die Hühner liegen
Die schon ohne Kopf und Gurgeln
Lieblich in der Pfanne schmurgeln. (Busch 1949: 7)

In themselves, these verses are a mere fact of the German text, not even, one must admit, a very central one. Thus, the status of their unparenthesized portion as a 'problem', let alone the nature of that problem, would only be established in relation to a translation such as the following Hebrew one (here in a literal English rendering):

[Through the chimney they see]
on the stove pots full
of cooking chicken
which are thoroughly roasting;
in fat soup the legs,
the wings, the upper legs
float tenderly, and from sheer delight
they almost melt there like wax. (Luboshitsky 1898: 9)

These verses, in turn, would simultaneously emerge as the 'solution' of that problem; i.e., as the replacing segment of the same coupled pair. There is hardly room for breaking down the two segments any further, in relation to each other.

As it turns out, it is mainly the confrontation of two contending modes of cooking chicken, pertinent to two different cultures, that can be held responsible for establishing this particular pair. Of course, the mere existence of such an incompatibility does not

automatically ensure the triumph of the *target* model, as was the case here. The norms expressed by the source text may well be preferred, at the expense of the target text's acceptability, or else some (hitherto non-existing) compromise between the two contending sets of norms can be adopted, where a price would be paid in terms of both acceptability and fullness of reconstruction. The point is that while analyzing each of these cases in retrospect, the coupled pair, and hence each one of its members, would be established *in its own terms*, and hence they may well be unique to the case under study.

Thus, the problem of the appropriate mode of preparing chicken for eating is not a feature of the source text as such. Moreover, to the extent that it is a property of the two underlying cultures in relation to each other, it has become relevant only because it has actually been realized in an act of translation. Its nature as an ad hoc relational feature manifests itself very clearly as soon as a different Hebrew translation of *Max und Moritz* is mapped onto the original; for instance:

[They smell the meal,
they peep through the chimney,]
without heads, without throats
the cock and each one of the hens
are already in the pan. (Busch 1939: 12)

It is very clear that here, the unparenthesized portion (which, to be sure, is not even identical to the one we had before!) should not be regarded as one segment at all. Rather, it will have to be broken down further, along with the corresponding German text portion, until some smaller-scale coupled pairs have been established under the 'no leftover' maxim. It is these (partial) pairs that would then undergo comparative analysis, which, in all likelihood, would yield very different conclusions.

The nature of comparative analysis deserves some more comments, by way of a brief reminder:

- (1) Every comparison is *partial* only: it is not really performed on the objects as such, only certain aspects thereof.
- (2) A comparison is also *indirect* in its very essence; it can proceed only by means of some intermediary concepts, which should be relatable to the compared aspect(s) of *both* texts.
- (3) These intermediary concepts should also be relatable to the *theory* in whose terms the comparison would be performed.

Thus, it is the underlying theory, the aspects to be compared and the intermediary concepts that would ultimately determine the establishment of a coupled pair as a unit of study, which may well involve continuous revisions.

Once established, the members of a pair can be compared with each other in greater detail. After a large number of isolated pairs have been studied, *regular patterns* should be looked for which may have governed all these pairs, or sub-groups thereof.

5. Prospective vs. retrospective stances exemplified by metaphor

When trying to devise a research method which would be answerable to Translation Studies, we have deliberately refrained from importing models and methods from other disciplines and applying them to translational phenomena with little or no modification. Before going on doing just that, let us dwell a little on the consequences of any failure to realize the differences between a prospective and a retrospective viewpoint with respect to translation. Metaphor will be taken as a case in point precisely because it has so often been presented as a kind of ultimate test of any theory of translation.

The nature of metaphor as a problem of Translation Studies has normally been established in the *source* pole, proceeding from source-text items identified as metaphors. In the simplest cases (e.g., Dagut 1976; 1978: 91–120; Newmark 1981: 84–96) this was done on purely *linguistic* grounds, in more complex ones (e.g., van den Broeck 1981) — on both textual and linguistic grounds. Very often, the source-language metaphors were then given tentative target-language replacements regarded as ‘good’ (or ‘bad’), in terms of some *preconceived* balance between the features of the original metaphor, mainly meaning, constituents and [type and extent of] metaphoricity. If the behaviour of a body of metaphors under translation was studied at all, the material thus collected was normally approached as if it constituted a mere reservoir of isolated ‘examples’ rather than an organized whole, testifying to more or less regulated behaviour under certain circumstances.

Needless to say, generalizations, and especially guidelines for future behaviour, hardly ever reflected the regularities which could have been established for the corpus in question. Rather, they were filtered through an a priori concept of what would count as a ‘better’ (or ‘worse’) solution, which, even though based on just another normative attitude, nevertheless allowed the researcher to claim better knowledge than that of the translators whose behaviour s/he was purportedly studying. This also accounts for the fact that generalizations tended to be put forward as principles of *general* rather than limited validity, applicable at most to the pair of languages which had supplied the actual data. Only rarely has the focus been on the solutions *as they really are*, with no a priori criteria and value judgments (most notably in Kjær 1988), but on no occasion were

the problem-items themselves approached as they manifest themselves from the vantage point of their replacements.

Thus, it is symptomatic that the pairs of replacing + replaced segments established by most scholars who have done any work on metaphor and translation tend to fall into one of only three categories:⁸

- (1) metaphor into 'same' metaphor
- (2) metaphor into 'different' metaphor
- (3) metaphor into non-metaphor.

Even among the alternatives which proceed from the source pole, one possibility is thus often neglected:

- (4) metaphor into 0
(i.e., complete omission, leaving no trace in the target text).

This disregard no doubt reflects an a priori, prescriptive attitude: it is not that (4) is impossible (in principle), or non-existent (in actual reality), but only that writers intent on the rights of the source text refuse to grant 'zero' replacements *legitimacy* as translation solutions, or else they would do so only in the case of 'unimportant' metaphors, or those used in 'unimportant' texts — in source-oriented terms, of course.⁹

Now, even when proceeding from the *source text*, there can be absolutely no guarantee that the mere existence of a metaphor would ensure its translational treatment as *one unit*, be its replacement metaphorical ((1)-(2)), non-metaphorical (3), or even zero (4). This is just another issue which the use of the coupling method may shed light on. Thus, the element 'metaphor' in all formulas may have to be replaced by '(x + metaphor)' or '(metaphor + x)', with a considerable increase in the number of types of coupled pairs (and reconstructable processes). To those who are interested in metaphors and nothing but metaphors, the additional x's may seem superfluous. What they are

8. (Reconstructed) processes have been preferred to the actual pairing of target- and source-text segments only because they reflect more closely what the writers in question normally do. The terms used are Dagut's. Van den Broeck employs a different terminology, which, however, refers to more or less the same categories: he calls (1) translation 'sensu stricto', (2) substitution, and (3) paraphrase. Other writers on the subject resort to different terminologies. Some of them even have other accounts of metaphoricity as their theoretical framework. All this, however, makes very little difference for our kind of argument.

9. Here is what Werner Koller has to say about this precise issue: "... die Auslassung, die G. Toury als Lösung bei der Übersetzung von Metaphern gelten lassen will, ist zwar empirisch wohl für die meisten Übersetzungsprobleme belegbar, als *Übersetzungslösung* kann sie in den wenigsten Fällen (und schon gar nicht in systematischer Hinsicht) gelten" (1992: 208).

vital for is any genuine attempt to reconstruct translation decisions: one simply cannot take it for granted that, just because there *is* a metaphor in a text, it would always be approached as an integral unit. In terms of the method suggested here, this represents the application of the 'no leftover' principle (either way) to any coupled pair submitted to comparative analysis.

When proceeding from the *target text*, the four basic pairs listed above immediately find themselves supplemented by two inverted alternatives where the notion of 'metaphor' appears in the *target* rather than the source pole; as a *solution* rather than a *problem*:

(5) non-metaphor *into* metaphor

(6) 0 *into* metaphor

(i.e., addition, pure and simple, with no linguistic motivation in the source text).

Here again, the adoption of a target-oriented approach leads to an *extension* rather than reduction of scope, in keeping with actual reality.

In principle, all six types of coupled pairs may be relevant to any study of metaphor-and-translation, to the extent that it aspires to attain exhaustive descriptions and viable explanations, let alone some predictions as to how translators would tend to behave under recurrent circumstances — and not just in terms of the type of source-text metaphor and/or its use, the only parameters which have normally been assigned any significance.

Among other things, the broader framework may facilitate the account of **compensation**, if such a mechanism is found to offer a viable explanation of a (reconstructed) translation process. To be sure, care should be taken not to *automatically* couple the obliteration of a source-text metaphor (4) in one place with the introduction into the target text of another metaphor (6) elsewhere in such a way as to regard the two as constituting evidence of compensation. The two practices may well have been independent, reflecting two different, totally unconnected sets of considerations. The point is, however, that compensation would be practically impossible to detect if the first four translational options were the only ones allowed in a descriptive system.¹⁰

10. In response to my suggestion in the 1985 version of the Rationale, Uwe Kjær (1988) indeed decided to supplement the main part of his comprehensive study, the one devoted to the account of source-text metaphors and their replacements in the corresponding target text, with an inverted analysis, supposedly designed to find-out to what extent target-language metaphors of the type he was studying occur as translational replacements of source-language non-metaphors. However, for reasons beyond my comprehension, he also reversed the roles of the target and source languages. Interesting as such a comparison may be, it can hardly be taken to shed light on any observation made with respect to the main corpus. (For more details see Toury 1989.)

The addition of (5) and (6) may give rise to other kinds of explanatory hypotheses too. For example, it seems reasonable to assume that the use of metaphors in a target text is reduced, even so much as blocked on occasion, by certain *target norms*, and not by anything in the nature of the (source) metaphors, the texts they are incorporated in, or the discrepancy between the target and source languages, which source-oriented approaches rather naively regard as the only possible (or worthy of treatment?) causes for shifts — along with individual translators' shortcomings, which many scholars are all too eager to emphasize from the patronizing stance they love to assume. Such a hypothesis would of course be enhanced by the absence of *added* metaphors and weakened in direct proportion to their occurrence. Moreover, it may well be possible to establish some correlations between it and translation behaviour in *other* domains, towards the establishment of a dominant norm, even an overall concept of translation underlying the corpus. For while it cannot, and should not be assumed that phenomena other than metaphor would be approached in exactly the same way, it would not do to proceed on the assumption that each type of phenomenon elicits a completely different treatment either.

6. Uncovering the underlying concept of translation

One major object of coupling textual segments and comparing the members of the pairs to each other has long been presented as the identification of SHIFTS with respect to a maximal, or optimal notion of the reconstruction of a source text.

Notwithstanding the enormous problems involved in its establishment, practical as well as conceptual, there is no doubt that the notion itself is valid: shifts do occur in translations, and therefore they have their place in Translation Studies. However, it is my conviction that too much emphasis has come to be placed on this notion, not least by the present author. This conviction draws primarily on the totally **negative kind of reasoning** required by any search for shifts, which — in a weakened, and hence more realistic version — would encompass all that a translation *could have had* in common with its source but *does not*. High value is thereby assigned to failure, which implies — together with its twin-sister, success — that an attempt was in fact made to achieve something which is now taken as a yardstick: a certain *preconceived* principle which may or may not have been relevant to any particular case. This includes my own old-time hobbyhorse, the hypothetical construct of the 'adequate translation' (Toury 1980a: 112–121). I am more and more convinced now that any real understanding of what translation is (rather than what it 'succeeds' or 'fails' to be) would be achieved by uncovering those principles which are *internally* relevant to a corpus, which would

amount to going about studying translation in basically positive terms.

Be that as it may, even if the establishment of shifts is attempted, as is the case, e.g., in van Leuven-Zwart's intricate method (1989, 1990), it should never be regarded as an end in itself. Like the establishment of TRANSLATION RELATIONSHIPS, with which it is intimately linked, the identification of shifts is part of the *discovery* procedures only, i.e., a step towards the formulation of *explanatory hypotheses*. The latter, in turn, necessitate the establishment of the overall CONCEPT OF TRANSLATION underlying whatever corpus one sets out to investigate: one text within a broader context, one problem-area across texts, or a body of texts selected according to one principle or another.

The apparatus for describing all types of relationship which may obtain between target and source items, segments, even whole texts, is one of the tools DTS should be supplied with by the *theoretical* branch of the discipline. Fortunately, translation theory can offer great help here, due to a long tradition of preoccupation with problems of 'equivalence' vs. 'formal correspondence'. What it must still do, however, is rid itself of vestiges of the *prescriptive* bias, which is only pertinent to the *applied* extensions of the discipline.

In establishing translation relationships, which involves not only similarities/differences, but also uni-directionality, the focal concept is that of *invariance*. However, the crucial point is that, whereas the very *existence* of an invariant core is posited, that which the members of concrete coupled pairs *actually* share is never given, and should therefore be submitted to deliberate study. In principle, this invariant may be found in either substance or function, on the purely linguistic (i.e., habitual) as well as (ad hoc) textual level. Consequently, the relationships which Translation Studies as a discipline should cater for are both *formal* and *functional*, on either level. An important methodological implication is that terms of the same type and rank be applied to both members of a coupled pair; otherwise, no real comparison would be possible.

Of course, every segment — 'replacing' and 'replaced' alike — is at once a set of elements of a lower order as well as a constituent of another entity of a higher order (and see Jakobson's classical discussion of "Parts and Wholes in Language" [1971a]). Translation relationships for a single pair may therefore be functional on one level and formal on another. At the same time, it wouldn't do to settle for a mere *enumeration* of the types of relationship pertinent to that pair, not even with the addition of an explicit reference to the level(s) to which each type applies. Rather, a *hierarchical ordering* should always be attempted, which would be taken to represent the *overall* relationship exhibited by the pair. Similar sets, applying to a significant number of individual pairs and weighted against each

other, should finally lead to the establishment of the hierarchy of relationships pertinent to the entire text under study.

It is here that the notion of TRANSLATION EQUIVALENCE makes its debut, even though as a different notion from the one known from other paradigms of Translation Studies. Thus, equivalence as it is used here is not one target-source relationship at all, establishable on the basis of a particular type of invariant. Rather, it is a *functional-relational* concept; namely, that set of relationships which will have been found to distinguish appropriate from inappropriate modes of translation performance for the culture in question.

Since what is at stake here is the possibility of accounting for every kind of behaviour which may be culturally regarded as translational, no target-source relationship can be excluded from candidacy as [part of] translation equivalence: in principle, at least, all relationships may, on occasion, assume the above-mentioned distinguishing role. The entire set of possible relationships would therefore be taken to constitute *potential* equivalence, i.e., to belong to the theoretical branch of the discipline, whereas the proper place of any *actual* (or *realized*) equivalence would be in DTS. Methodologically, this means that a descriptive study would always proceed from the assumption that equivalence does exist between an assumed translation and its assumed source. What remains to be uncovered is only the way this postulate was actually realized, e.g., in terms of the balance between what was kept invariant and what was transformed.¹¹

Equivalence, again, is of little importance in itself. There is a point in establishing it only insofar as it can serve as a stepping stone to uncovering the overall CONCEPT OF TRANSLATION underlying the corpus it has been found to pertain to, along with derived notions such as DECISION-MAKING and the factors which may have CONSTRAINED it; not an *idealized* process, as presented, for instance, by Jiří Levý (1967), but real-life decision-making under real-life constraints, as reconstructed, e.g., by Even-Zohar (1975) with respect to the Hebrew translation of Baudelaire's "Spleen". Finally, the notion of equivalence may also facilitate the explanation — in reverse order — of the entire network of translational relationships, the individual coupled pairs (as representing actual translation units under the dominant norm of translation equivalence) and the textual-linguistic representation of the translational solutions, which has made them into (surface) translational phenomena, in the first place.

11. Needless to say, what belongs in the applied extensions of Translation Studies is a subset of the entire set of potential equivalence which functions as a *requirement*, on one basis or another. See Table 1 in Part One above as well as Part Four below.

Chapter 4

The Coupled Pair of Replacing + Replaced Segments

As we have argued, much as one would like to regard the text as an ultimate unit, the mapping of a translation onto its assumed source is impracticable unless both texts are broken down, often drastically. Nor is this necessity devoid of theoretical justification. After all, no act of translation is conceivable without *serial* operations. By contrast, the claim that these operations are also accompanied, let alone governed, by some mental 'map' of the two integral texts (e.g., Holmes 1978) is no more than a *possibility*.

What such a possibility implies is, of course, previous acquaintance with the original as a textual whole. Apparently a minimum requirement, no descriptive study can, or indeed should proceed from it. As is so well known, many real-life translation situations indeed involve no complete acquaintance with the original prior to the commencement of its translation, and not only in the obvious case of simultaneous interpreting, where the source text itself comes into real being as the interpreter is already busy establishing its translation. The restrictive requirement that only processes which do entail such an acquaintance (and their products) be submitted to study is untenable. For one thing, there is no way of giving a theoretical justification to it, if real-life behaviour is at stake rather than any ideal(ized) version of it. For another, one would never know in advance what kind of process has yielded a given text, so that only 'appropriate' ones would be picked for study. Whether an act of translation entails thorough knowledge of the original is another *question*, then, and an intriguing one too, rather than a starting point of any standing.

Finally, even if it does occur, the realization of this possibility tends to be only *partial*, e.g., a gradual establishment of a set of fractional maps as a concomitant of the inescapable linear progression. Thus, while the textual option

should be provided for by translation *theory*, its all-pervasiveness cannot be adopted as a starting point for any *study* of translation behaviour and its results – all the more reason to suspect the usefulness of constructs such as the ‘adequate translation’ as part of a positively oriented account of a translation (see Chapter 3, Section 6).

1. The need for a unit of comparative analysis

What would normally be mapped onto each other, then, is segments of an assumed translation onto segments of its assumed source, rather than the two texts as wholes. But what would be the logic underlying the establishment of these segments? And what kind(s) of segments would such a logic give rise to, as units of comparative analysis? The crucial requirement seems to be that whatever units one chooses to work with should be *relevant to the operation which would then be performed on them*: in our case, an attempt to gradually reconstruct both translation decisions and the constraints under which they were made.

Translation scholars throughout the ages have resorted to two different notions of relevancy, which may concur in certain concrete cases but which are nevertheless incompatible in essence. The first, and by far most common notion of relevancy is *source-governed*, and its application to translation is *prospective*. In its most elaborate form the claim is made that it is the position of a feature in the internal network of relationships constituted by the original text that determines its relevancy to any would-be translation, irrespective of anything else. However, as we have already argued (Chapter 3, Section 5), it cannot (and should not) be assumed that a unit enjoying *prospective* relevancy will have been subscribed to in any single act of *past* translation. There is simply no taking it for granted that whenever a feature occurs in a text, be it ever so high in its internal hierarchy, it will also be picked by translators, and retained, ready to be detected, in the translated text. Consequently, the establishment of units which would be submitted to comparative analysis focussed on reconstructing rather than implementing translational decisions, cannot proceed on the sole basis of the position and role, let alone mere presence of features in source-language texts.

What makes our task more difficult still is the fact that, for a *retrospective* kind of study, no unit can be postulated for either text – at least as soon as more is ventured than the mere establishment of shifts with respect to the ‘adequate’ reconstruction of a source text in a target language. Units which are sure to be

relevant to the kind of comparative study we have in mind can only be established ad hoc; i.e., as the translation is being mapped onto its source-language counterpart. Moreover, if their comparison is to be justifiable, units cannot be established for the two texts in isolation. Rather, segments of both should be defined *simultaneously*, determining each other, so to speak.

In this sense, the units of comparative analysis would always emerge as **coupled pairs of target- and source text-segments**, 'replacing' and 'replaced' items, respectively. Nor is there any need for the members of a pair to be of the same rank or scope, which would have sneaked in an undesired prerequisite again. The pairing is subject to a *heuristic* principle instead, namely that beyond the boundaries of a target textual segment no leftovers of the 'solution' to a certain 'problem', posed by a corresponding segment of the source text, will be present. Established as they are in the course of the comparison itself, these coupled pairs would be submitted to further analysis as the study proceeds, and it is the relationships found to obtain between their members which would underlie any generalization concerning the pertinent kind of translation equivalence.

2. An exemplary analysis of one pair of texts

Consider the following two English texts, two versions of a warning against the improper use of an emergency brake:

- | | | | |
|-----|--|-----|---|
| (1) | Alarm Signal
To stop train
pull handle
Penalty £50
for improper use | (2) | Emergency brake
Pull brake only in
case of emergency
Any misuse will
be punished |
|-----|--|-----|---|

(1) is found on British trains, (2) on former West German ones. Incidentally (but very significantly, from a cultural point of view), whereas British trains carry only English notices, in the German context the warning is presented in parallel in four languages: German, English, French and Italian. Presumably, each version is also intended for a different audience. Example (3) below shows what the German version looks like.

(1) and (3) are two independent texts in two different languages. Due to their habitual use, they may be regarded as *codified entities*: while never ceasing to be texts, they also constitute elements of certain repertoires pertaining to the

- (3) Notbremse
Griff nur bei
Gefahr ziehen
Jeder Mißbrauch
wird bestraft

two cultures in question. And since the two are used in comparable situations, they can be taken as functionally parallel, hence initially interchangeable with respect to the situation at hand.

(1) and (2), in turn, represent parallel texts of a different kind: (2) plays the same role for readers of English in the German context as (1) plays for all readers in the British one. In both situations, not all readers will have English as their mother tongue; only in the UK those who do would form a majority, whereas in Germany they would normally constitute a negligible minority.

Incidentally, there is a third kind of parallelism here, namely, the one between the neighbours (2) and (3). It is this kind of parallelism which is by far the most typical candidate for a study in translation proper.¹

Obviously, when the decision to fill the English slot on German trains was first made, decades ago, the *Deutsche Bundesbahn* (or was it still pre-war *Deutsche Reichsbahn*?) could well have turned to a functionally parallel version in the English language, whether the British one or any other.² It could have adopted this version verbatim, or with minor modifications, most notably omitting the exact penalty for improper use, which is not specified in the German situation. Had this solution been adopted, the coupled pair which would have presented itself to retrospective analysis would have consisted in *full textual entities* in German and English: the two titles, on the one hand, and the remainder of the

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1. The most pertinent discussion of parallel texts and their various types remains Hartmann's (especially 1980 and 1981). At the same time, his application of the notion to translation, and especially the use he finds for it in Translation Studies, differ from mine considerably.
 2. The following are some additional English versions:
Emergency Brake / Use forbidden without valid reason (Montreal; translation of a French version);
Conductor's Valve / [for] emergency only (Canada);
Alarm. / Break glass in emergency / A penalty for misuse – R10,00 (South Africa; 'literal' translation of an Afrikaans version).

I wish to thank Dr. Judith Woodsworth (Montreal) and Mrs. Alet Kruger (Pretoria) for their invaluable assistance in locating these versions.

texts, on the other. No need would have arisen to break down the texts any further. In fact, breaking them down could hardly have been given a justification, in terms of a (reconstructed) translation process. The only justified conclusion would have been that the source text itself had *not* been broken down during the act, so that replacement indeed occurred on the repertoremic level: a codified entity for a codified entity of the same rank. It is not that lower-rank coupled pairs could not have been established in this case too; e.g., 'pull + ziehen', 'handle + Griff', 'penalty + wird bestraft' and 'improper use + Mißbrauch'. It is only that those pairs would have been *irrelevant* to the texts' respective roles as entities in a reconstructed translation process: They would have reflected the mere fact that similar (but not identical!) verbal formulations were selected by members of different societies to indicate similar norms of behaviour under similar circumstances.

Be that as it may, the fact is that the German company did *not* resort to any ready-made English note. Rather, the German version (3) was picked and was submitted to an act of bona fide translation involving decomposition, a series of transfer operations and recomposition, ultimately resulting in (2). While doing so, (3) itself acquired the status of a proper source text: not only did it precede (2) in time, but it also served as a textual-linguistic basis for deriving it.

Appearing as they do side by side, (2) and (3) may seem to represent two texts, in two different languages, which are simply *physically* parallel. However, there can be little doubt that they are meant to be taken as *functionally* parallel too: messages in two acts of communication, addressed to different audiences but intended to make them do (or refrain from doing) the very same thing under a certain specified set of circumstances. Where does this kind of equivalence reside, then, and what strategies were employed to attain it? What concept of translation may be said to have governed these strategies, and why was this concept adopted rather than the former, 'wholesale' one? This time no answer can be ventured unless the English text (2) is mapped onto the German one (3) as a translation onto its original. As this is done, a need to break down the texts immediately arises — both of them, and in a mutually determining way. The obvious result of such a procedure would be a series of lower-rank coupled pairs.

But what segments would be marked relevant, in this particular case, and hence constitute the members of the various pairs? And what kinds of criteria could be assumed to underlie them?

It is clear that the initial decision to have each version take up exactly five lines, one for the title and four for the text itself, acted as a constraint of sorts on

the formulation of the derived English notice.³ However, apart from the two titles, there emerges no reason to establish 'line + line' as the appropriate coupled pair: 'Pull brake only in + *Griff nur bei*'? This clearly will not do; the target-language word 'pull' would be an obvious leftover. 'Case of emergency + *Gefahr ziehen*'? Hardly; this time the source-language word *ziehen* would appear as a leftover, while 'case of' may pose some difficulties of its own. The same holds for 'any misuse will + *jeder Mißbrauch*', and even for 'be punished + *wird bestraft*'. The pairs which first present themselves as coupled under the 'no leftover' condition are no doubt 'two lines + two lines' and/or 'sentence + sentence'. (In this particular case the two principles coincide.)

However, it is clear that the second coupled sentence, 'any misuse will be punished + *jeder Mißbrauch wird bestraft*', as well as the pair of titles, cannot be fully accounted for in terms of a reconstructed translation process unless further broken down. After all, even though the 'no leftover' condition did direct their establishment, this condition can be re-applied to parts of them.

Thus, the conclusion that the analysis will yield is that during the act of translation whereby the English version of the warning (2) came into being, and for the purpose of producing that version, the German text (3) was decomposed into lower-rank constituents in descending order: sentences, phrases, words, sometimes even single morphemes. Each ultimate segment was translated *separately*, in its own turn, so that the end product emerged not only *gradually*, but in a basically *sequential* manner too.

For instance, while it is certainly a proper English word, 'misuse' can hardly be said to have been selected *en bloc*, on the grounds of its *semantic* interchangeability with the corresponding source-language word, *Mißbrauch* alone, much less on the grounds of their *pragmatic* interchangeability in the situation at hand (where 'improper use' seems to be the norm).⁴ It may be hypothesized that this word was arrived at through a process which lends itself to the following explanatory reconstruction:

(a) *Mißbrauch* was decomposed into its lower-rank constituents, the bound morpheme (prefix) *Miß-* and the free morpheme *Brauch* (noun);

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3. And compare the English versions cited in note 2, where a smaller number of lines (2-3) was used, which resulted in a different kind of segmentation.
 4. The only case I was able to find, where 'misuse' is used on British trains and undergrounds, is when this noun is premodified; e.g., "There is a £50 penalty for *deliberate misuse*" (italics added). The multiply premodified 'deliberate improper use' would probably have been deemed *stylistically* inferior.

(b) each morpheme was then replaced by one of its most common English equivalents, *mis-* and *use*, respectively;

(c) the two replacing morphemes – again, a prefix and a noun – were then combined to yield *misuse*; and

(d) the resulting entity was checked against the target-language lexicon – the one stored in the translator’s ‘bilingual brain’ or in an actual dictionary – for its appropriateness as an English noun.⁵

A similar process can be hypothesized for ‘Emergency brake’ (as a replacement of *Notbremse* broken down into *Not* + *Bremse*) as well as ‘[noun] + will be + punished’ (as a replacement of the German sequence ‘[noun] + *wird* + *bestraft*’, with an *inanimate* word, ‘misuse’, occupying the ‘noun’ slot). What all three English substitutes have in common is that, whereas in terms of the target *language* they are perfectly acceptable, none of them reveals a high rate of availability for the *communicative situation*, or even for the *text variety* under consideration. In terms of translational decisions, the inevitable conclusion is that solutions were sought on a level which is *lower* than the one on which they would have been selected in the case of pragmatically equivalent, or ‘adequate’ translation; in other words, the source text’s status as composed of lower-rank elements was preferred to its role as a codified entity, which would be a fair characterization of any kind of so-called ‘literal’ translation. The fact that one explanatory hypothesis can be offered to so many segments of a text which is that short and that simple may itself be taken as strong support of the validity (i.e., explanatory power) of that hypothesis. Thus, the norm of equivalence pertinent to the pair of notices, and hence the overall concept of translation which may be said to underlie it, are both found to have been determined on purely *linguistic* grounds.

To be sure, in translational acts of the type we have just reconstructed, the last stage (d) will sometimes be found to have been skipped – a practice which may even result in the emergence of truly translation-specific lexical items (see Chapter 11). Here, however, a positive reply to the question of *linguistic* appropriateness can be taken to have furthered the insertion of the resulting segments in the English text which was gradually coming into being. Moreover, the

5. The use of ‘misuse’ on South African trains (see note 2) may well have been caused by a similar kind of translation performed on the parallel Afrikaans version: “Alarm. / Breek die glas in nood. / ‘n Boete vir *misbruik* – R10,00” (italics added). Obviously, there is no reason to assume that the word in its present use was imported to Germany from South Africa, of all places.

possibility cannot be excluded that the sequence of partial decisions of this recurrent type was followed by a question as to the well-formedness of the entire end product as an English *text*. Even though such a hypothesis cannot be put to the test in a retrospective study, in principle there is no difficulty in giving a tentative positive answer to it either. This may serve as further explanation of the willingness with which (2) was adopted as a fill-in for the 'English' slot on German trains – a decision which, moreover, has not been reversed for decades. By contrast, no question seems to have been asked as to the normality of this text in the *situation* at hand.

The obvious question which manifests itself now, with respect to the reconstructed process of translation, is why it all happened the way it did. After all, nothing would seem more straightforward than contacting British Rail, or any other company in an English-speaking country, and asking for the version in actual use. This question gains in significance given that the production of the English text on German train followed a completely different pattern from the establishment of the other two parallel versions, the French and the Italian, which were indeed taken wholesale from the two respective repertoires.

One possible answer can be sought in the different status of the three 'target' languages. The most significant single fact here is that – in contradistinction to French and Italian, English was probably not conceived of as the language of any *particular* speech community. In all probability, it was defined in negative terms; i.e., as the language of a kind of *residual* group, membership in which is determined by not having any of the other three languages, at least not as a dominant language.⁶ To be sure, some of the members will still have English as their mother tongue, even though not necessarily of the same brand. However, a large, and growing number would not have English as first language, and would be resorting to it as a kind of *lingua franca*. This is the obvious grounds for my claim (in Chapter 1, Section 3) that the culture which may be said to host the English warning as the translation it is is the artificial sub-culture shared by the speakers of various languages who also have English for as long as they are in Germany (or at least on board a German train).

The validity of this kind of explanation for the adoption of the kind of strategy we have been able to reconstruct for deriving the English version (2) from the German one (3) is reinforced by noting, e.g., the English parallel text

6. One type of evidence which enhances the explanatory power of this hypothesis is the fact that, on German trains, notices intended for the personnel alone have always been given in the three other languages only.

in use on *Italian* trains (4), which differs from all authentic English ones as well as from the 'German' English version:

(4)

Alarm
Pull the handle
in case of danger
Penalties for
improper use

What this text manifests is an interesting mixture of leaning on an authentic English tradition ('Penalti[es]', 'improper use') and on the Italian warning (5), in a way similar to that exhibited by the 'German' English text ('the handle + *la maniglia*', 'in case of danger + *in caso di pericolo*):

(5)

Allarme
Tirare la maniglia solo
in caso di pericolo
Ogni abuso
verrà punito

One thing is sure, though: by no means is it a 'native' utterance, with respect to the situation at hand. It is no codified entity, either, in any culture using the English language; again, this is in full analogy to the 'German' English text (2).

3. Justifying the use of the coupled pair

Let me now elaborate a little on the *justification* which can be offered for using coupled pairs of mutually determining 'replacing' and 'replaced' segments during a comparative analysis of translation vs. source text, which I started by presenting as a heuristic device and then showed to be operative.

Even if this were just a convenient working tool, I would have stuck to it, at least until something better could be produced which was defensible too — within the framework I have chosen to work in, that is. Moreover, as claimed above, this unit is not just workable; it is easy to justify, in methodological as well as theoretical terms. These kinds of justification notwithstanding, though, the notion of the coupled pair would certainly have gained considerably more weight, had it been possible to claim that pairs established during a retrospective comparative study may be taken at least as *indicative* of the units actually resorted to in the translating act itself.

Incidentally, the suggestion that the pairing of source- and target-language

segments may have some *psychological* validity has already been made, and from a very different vantage point, at that, with completely different objectives in mind. I am referring first and foremost to Brian Harris, who recently made a first attempt to transfuse into the bloodstream of Translation Studies the concept of *bi-text* (Harris 1988a: 8).

A source text and its translation, says Harris, are usually viewed as leading "semi-independent lives": even though they share the same author and the same content, their readerships are different. However, "there is one person par excellence for whom, at least briefly, ST and TT are not separated but on the contrary are simultaneously present and intimately interconnected in his or her mind. That person is *the translator*" at the moment when s/he is translating. The two texts "sewn firmly together ... constitute a *bi-text*".

"Another way of putting it", he goes on, "is to say that a bi-text is not two texts but a single text in two dimensions, each of which is a language", or, better still (as he himself has it later on): "a *construct* of two or more related texts". Even this image, however — "the image of the [one] text in two dimensions" — is not quite appropriate, because it "refers to the text as a whole. Yet translators do not translate whole texts in one fell swoop. They proceed a little at a time, and as they do each spurt, each segment forms a *fragment of bi-text* in their minds" (*italics added*). That is to say, "not only [is] the whole text a bi-text, but each segment combines ST and TT". These segments, in turn, are correlated with so-called 'translation units'.

More important still, the possibility of intimately interconnecting source- and target-language texts, or textual fragments, is not lost forever when the translation process is over. "Those of us who *are* bilingual and who want to study the translation can also 'restore' both versions, or *at least parts of them* [*italics added*], in our minds simultaneously, and consider them together". Consequently, for practicing translators, "retrieval of a translation unit of ST from a bi-text will always bring with it the corresponding unit of TT", the one that it stands for in the 'economy of the text',⁷ whereas for retrospective translation scholars it is the other way around: retrieval of a translation unit of TT will always be associated with a corresponding unit in SL.

To be sure, Harris' main concern is with the *implementation* of his notion, first and foremost for the computer. To me, this is a major explanation of his insistence on

7. Harris himself has 'meaning' here. However, inasmuch as the basis for the coupling is actual behaviour, and not any a priori assumption, it is not at all necessary that membership of a 'micro bi-text' be determined by meaning, or meaning alone; not even in cases of what Harris (1988a: 9) calls 'strictly translations' (in contradistinction to 'parallel texts' of other types).

assigning bi-text a kind of *textual status*.⁸ After all, only text-able entities can form proper computer 'files'. Harris does argue however that, as a theoretical concept, bi-text is basically *human*, and its "primary nature ... is psycholinguistic". Moreover, his basic definition of bi-text is *cognitive*: "ST and TT as they co-exist in the ... mind"; "the translator's mind at the moment of translating" or the researcher's mind as s/he compares the two texts (or parts of them).

Granted, this is just another untested hypothesis. It certainly makes sense, on an intuitive basis, but it definitely needs some verification before it can be accepted. There is, however, at least one kind of evidence to support this hypothesis; namely, the phenomenon of *stock-equivalents*.

As is well known, translators often come up with the very same target-language item to replace a recurring source-language segment, whether the two are indeed interchangeable in the situation at hand or not (which is tantamount to saying that no real concern is paid to their pragmatic, or textual appropriateness). Nor does this practice necessarily presuppose initial cross-lingual *semantic* equivalence; at least not semantic equivalence *alone* (and see note 7 again). In fact, quite a number of these automatic 'responses' are *idiosyncratic*, which they wouldn't have been, had there been a reason behind their adoption, other than the growing habit of an individual. Even though there is always the possibility that idiosyncratically created pairs will be picked up later on by additional translators, so that the replacing segment may even become a binding norm, this possibility has no bearing on the initial phase we are dealing with now.

Let us look at an example:

Going through hundreds of pages of prose fiction translated into Hebrew in the last sixty years, the following historical move can be reconstructed with respect to the Hebrew items habitually associated with the multi-functional English pragmatic void connector 'well'.

In the thirties and forties, the most common replacement of 'well' was 0: normally, a Hebrew translation had neither a lexical nor any other representation of that item, such as a change of word order, or a punctuation mark. In the remaining cases, a multitude

8. "Bi-text is, to be sure, a kind of text", he says, and his alternative definition of the notion reads: "bilingual ... text stored in such a way that each retrievable segment consists of a segment in one language linked to a segment in the other language which has the same meaning" (Harris 1988a: 8-9. For Harris' use of 'meaning', in this connection, see note 7).

of different replacements were used, with no clear preferential pattern. One of these, and not a very frequent one, was the Hebrew word *u-vexen*, a traditional interjection having a (very) small area of pragmatic overlap with 'well'.

In the fifties and sixties, the share of *u-vexen* among the translational replacements of 'well' grew incessantly, irrespective of the latter's function in the source text, and by the seventies, the association of *u-vexen* and 'well' had become habitual. In translations from other languages this item was much rarer, nor did it represent a recurring source-language element. *U-vexen* was even rarer in original Hebrew writing, negligible in formal speaking and hardly ever used in informal circumstances. As a result, especially when massively resorted to within a single text, this item has become an indicator of translation from English: an almost univocal marker of the occurrence of 'well' in a corresponding position in the source text. The coupling of '*u-vexen* + well', which started as an ad hoc association of individual translators, has thus undergone *generalization* as a translation replacement.

At the peak of this process, when almost everybody used *u-vexen* as a matter of course, there was one prolific translator of prose fiction who refused to adhere to the habitualized replacement. He came up with a coupled pair of his own instead, '*ki-xen* + well', which he activated with even greater regularity than did the average user of *u-vexen*. His practice, which can be regarded as an idiosyncrasy within the limits of the norm, is all the more peculiar in view of the fact that, unlike *u-vexen*, *ki-xen* hardly had any status in a Hebrew tradition of any kind.

In the eighties, the use of *u-vexen* as a stock replacement of 'well' decreased again. This can be attributed to an overall change of norms in translating from English, even though the role played by certain scholars and critics, the present writer included, in accelerating the downfall of this particular item should not be underestimated. It is now in wide use mainly in film and TV subtitling, whose dominating norms are lagging behind those affecting other types of written translation. Thus, it is not uncommon to see the word *u-vexen* appearing in isolation in the middle of the screen and staying there for a seeming eternity, which is about the most non-Hebraic use of the word one could imagine. What it reflects is, of course, one of the main uses of English 'well' in spoken language; namely, to express hesitation and/or leave the channel open.

Phenomena of this kind are easy enough to trace on the word level. However, they may be found on higher textual-linguistic levels too, and not only with respect to codified entities either. To quote Harris again: "in fact [translation units] mostly consist of whole phrases and even clauses and sentences. Bi-text therefore binds together not the individual words of ST and TT but those somewhat longer segments" (1988a: 8). As far as human translators go, limitations of memory may well have a substantial role to play here.

4. Testing the coupling hypothesis in real time

It may seem, then, that the connection established between source- and target-language segments during an act of translation does not necessarily dissolve when the act is over, to be resumed (or not resumed) in future acts. Rather, it often leaves more or less permanent imprints in the translator's mind, so that micro bi-texts, or pairs of replacing and replaced items, are pushed to his/her long-term memory and stored there *as coupled units*. From that point on, certain target-language items tend to be called up automatically to replace those source-language segments which have come to be coupled with them in his/her mind. Consequently, translation may well become more and more *proceduralized* for that person. In fact, a (gradual) building-up of a repertoire of coupled pairs, on various levels and of varying scope, may well be one thing that acquiring skill in translation involves – one possible component of Levý's famous 'minimax strategy'.⁹ More time and effort can now be spent on those aspects of the act which are less proceduralized, which certainly adds much in terms of efficacy.

Be that as it may, one strong point of the coupling (or 'bi-text') hypothesis is that it is *testable*. It deserves to be tested too, in both ontogenesis and phylogenesis. At the same time, there can be no wonder that it has hardly been put to any deliberate test, so far. This is not only because the hypothesis itself has not been around long enough, but also because, in general, very little psycholinguistic research was applied to translation until the mid-eighties. However, from this little work, some inferences can already be drawn which bear on our hypothesis – especially from the tentative results of the application to translation of the experimental method for the collection of introspective data on cognitive processes known as Thinking-Aloud Protocols.

In spite of their inherent weaknesses, some of which will be discussed in Chapter 12, Thinking-Aloud Protocols do throw some light on the coupling hypothesis. One thing they have clearly demonstrated is that, even with the most unskilled of translators, parts of the process are indeed proceduralized. In those parts, target-language segments of various scope and rank are retrieved *in toto*, as *automatized* 'responses' to the 'stimulus' provided by certain segments of the source text, with no covert planning activities following the segmentation itself.

9. "That one of the possible solutions which promises a maximum of effect with a minimum of effort" (Levý 1967: 1179).

Consider the following protocol extract, cited from Dechert and Sandrock (1986: 117):

Reading L2		for example		are all written...
Reading L1	<i>London</i>			
Translating			<i>zum Beispiel</i>	
Writing				<i>z.B.</i>
Pauses	(0.63)			
Speaking time	(0.72)	(0.9)	(0.98)	(1.91)
Writing time				(2.56)

Previous findings in Psycholinguistics (e.g., Ericsson and Simon 1980: 225) have demonstrated that, as a process, only retrieving from short-term memory can be reported. The inevitable conclusion is that automatic retrieval indeed involves *long-term* memory, which is where the coupled pairs seem to be stored.

Unfortunately, within a psycholinguistic framework, comparisons have barely begun between translation performance of novices and that of experienced translators (e.g., Séguinot 1989b, Lörcher 1991). Therefore we still don't know for sure whether the *number* of stored pairs, and/or the *rank* and *scope* of their members, indeed increase with experience in translating. We do know, however, that practice is a normal way of acquiring proceduralized knowledge. With respect to linguistic skills we further know that "the more competent a hearer or speaker in his first (as well as second) language is, the more proceduralized" these skills are (Dechert and Sandrock 1986: 112), and to claim that translating behaves in a way which is *totally* different from any other linguistic skill would seem dubious.

It is clear that, for language learners at least, the number of coupled pairs is rather limited, due to their deficient knowledge of L₂ as well as their lack of experience in translation from and into it. On the other hand, the rank and scope of the members of some of those pairs may be rather high. Be that as it may, for those having "rudimentary ability to mediate" (Lörcher 1991: 41-47), the greatest part of the translation process would seem to be *non-proceduralized*, and hence to necessitate relatively slow search activities. This, however, does not imply that no segmentation and no pairing are involved. What we learn from the protocols is rather that most source- and target-text segments are established and paired in an ad hoc way, at the end of a search and on the basis of it. This again enhances the justifiability of using the coupling principle in *retrospective* analyses of the type discussed here.

Much more must undoubtedly be done to close in on the translation

process. Until **this** is achieved, hypotheses will necessarily continue to be formulated as **conjectures** – on the basis of studies of translations vs. source texts, among **other** things; and these studies should be well suited for the purpose too.

An Exemplary 'Study in Descriptive Studies' Conjoint Phrases as Translational Solutions

Before moving on to presenting an assortment of actual studies into translation behaviour and its external manifestations, a second layer will now be added to our Rationale through a step-by-step unfolding of an exemplary study in descriptive studies. The presentation will revolve around one type of linguistic phenomenon, conjoint phrases of near-synonyms, as they occur in translated texts within one cultural tradition. The discussion is offered as a *methodological exercise*, though, rather than as an outright case study. Consequently, the focus will be on the **stages** themselves of a full-fledged study as outlined in Chapter 1 (see Figures 6 and 7) and on the transition from one to the next, including the types of **questions** pertinent to each stage, rather than the **answers**.

Texts presented (or regarded) as translations will thus first be situated within the target system and accounted for with phrases of this type in mind. At this stage, mainly questions of *significance* and *acceptability* will be addressed. Next, individual translations will be mapped onto their assumed sources, never losing sight of this phenomenon. A conjoint phrase will thus constitute the target-language member of every coupled pair and will be tackled as a replacement of any variety of source-text items. *Shifts* and *translation relationships* will be the key-words here. To complement and balance the picture, pairs having conjoint phrases as a *source-text* member will not be ignored either. Some first- and second-level *generalizations* will then be attempted, with prospects for further study in ever-widening contexts, not only beyond the individual translation (or corpus of translations) in one and the same culture, but also beyond the boundaries of that culture. Finally, possible *implications* of a descriptive-explanatory study such as this one for conscious decision-making in subsequent translation work will also be touched upon.

1. The phrases' significance assured

Conjoint phrases of synonyms or near-synonyms consist in two (occasionally more than two) (near-)synonymous items of the same part of speech, combined to form a single functional unit.¹ For instance, English *able and talented, law and order, harmless and inoffensive*, or German *für immer und ewig, nie und nimmer*. When the constitutive elements are nouns, the conjoint phrase may well refer to a single object ($x + y = x$) (Quirk et al. 1985: 955). In the most extreme cases, such a unit would seem to be completely tautological: *behold and see, might and main, (mit) Schimpf und Schande, er redete und sprach* (Leisi 1947: 2).

On close inspection, practically every written Hebrew text, original and translated alike, will be found to include quite a number of conjoint phrases of this type. Such pervasiveness is enough to warrant study into this phenomenon, the more so as its use in *translated* texts manifests some patterns of its own and makes it stand out against the common background.

Obviously, the establishment and use of conjoint phrases of (near-)synonyms is not unique to Hebrew. In fact, *any* language can have them, inasmuch as (near-)synonymy and conjunction are semantic and grammatical universals, respectively. Any language probably does have them too. What would distinguish different languages and language varieties, as well as various periods in the history of one and the same language, is mainly the *extent* to which this potential has been realized, including the ratio of *fixed* vs. *new* expressions, the exact *ways* it is realized (e.g., favoured parts of speech, preferred order of constituents, exact relationship between them, etc.) and the *purposes* it serves; all norm- rather than rule-governed aspects, to be sure.²

Old written Hebrew texts, from the Bible onwards, have shown a clear preference for conjoint phrases of near-synonyms. Many of the phrases, especially those

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1. See especially Malkiel (1968: 323–324). In the literature, this phenomenon goes under various names, e.g., 'tautological word pairs', 'repetitive word pairs' and 'binomial expressions'. The names as such will not concern us here, although they do entail slight differences of object. Another issue which will not concern us is that of synonymy and its gradation, even though it is of paramount relevancy for the conduct of particular studies: one cannot even arrive at the systematic delineation of a corpus without taking it into account.
 2. For Old and Early Middle English see Christiani 1938, Koskeniemi 1968; for Caxton's version of *Eneydos* Leisi 1947; for modern English Gustafsson 1975; for German prose style of the 14th and 15th century Wenzlau 1906.

used in texts which underwent canonization, later fossilized into fixed expressions – whether their meaning remained literal or whether it became idiomatic. Due to their high prestige, these texts were then taken as reservoirs of items and forms to be extracted and inserted wholesale into newly written texts, or to serve as models for new coinages. It is this prestige which explains the perpetuation of recourse to conjoint phrases, mainly of the fixed type.

Towards the end of the 18th century, in its Enlightenment period, Hebrew embarked on a struggle to adjust its limited linguistic means to new modes of writing, imported from the surrounding European cultures. At that time, the technique of conjoining near-synonymous items awoke to new life. Never quite obsolete, it now enjoyed enormous productivity again. As a result, the first generations of 'secular' Hebrew writing were marked by a plenitude of conjoint phrases, old and new, in texts of all types, including even the shortest of lyrical poems. Nor were there necessarily any local (textual or communicative) reasons for many of the phrases, especially as, quite often, both constituents were almost identical in meaning – a meaning which tended to be vague anyway (Even-Zohar 1976). Rather, they seem to have been picked up, or created, first and foremost for the sake of playing it safe in writing 'authentic' Hebrew, which is precisely what was required by the ideological stance which the writers had adopted: realize new text-types, or new modes of writing, in old linguistic forms.

It has taken Hebrew writing over 150 years to gain confidence in the new abilities acquired by the language. During this period, it has gradually reduced its reliance on conjoint phrases too, to the point where they are now used more discriminately, and hence in clearer functions, mainly for well-defined stylistic or rhetorical purposes. Also, it is *lexicalized* expressions that are now preferred, and even these – in dwindling numbers and lower density. Only seldom is a newly created phrase incorporated in a text, so that the productivity of the technique has almost been nullified. In fact, when this device is used, the result is often felt to be rather ridiculous, which was never the case in earlier times; otherwise this technique would hardly have survived as long as it did.

There was, however, a rather long interim period – several decades – when an abundance of fixed binomials, and especially the creation of new conjoint phrases, were no longer the norm in the center of Hebrew writing but they were still resorted to, quite substantially, in the periphery – mainly in children's literature, on the one hand, and in translations of all kinds, on the other. What is reflected here are two well-known facts: (a) secondary types of activity tend to adhere to norms which have become obsolete; and (b) both translation and children's literature indeed tend to assume a secondary position within a culture

(see especially Even-Zohar 1990: 45–51 [1978]; Z. Shavit 1981).³ Further verification of this hypothesis will be supplied further on.

2. The use of binomials in translated texts

Even if one never proceeded to compare the conjoint phrases encountered in a translation to their counterparts in the corresponding source text, there is evidently much to be learnt here that is relevant to research in translation; in our case, that translation into Hebrew has come to accord special status to this device. What seems most typical, and its typicality has even grown over the years, is a combination of two features:

- (1) the sheer *number* of conjoint phrases in an average translation tends to be higher than their average number in a text originally written in Hebrew. In fact, they are sometimes so dense as to border on parody. Not deliberately, one should assume.
- (2) the share of ad hoc, *free combinations* among those phrases is also considerably higher in translations. By contrast, the *semantic difference* between their constitutive elements is often much smaller, sometimes virtually nil.

These facts together offer evidence enough of the gradual formation of two different norms of acceptability (in relation to this particular device) for two different modes of text-production, translation vs. original writing. In fact, very often, especially in the second quarter of the 20th century, density of binomials of near-synonyms and/or the occurrence of newly coined conjoint phrases was quite readily associated with translations. Attentive readers were thus in a position to tentatively mark texts as probable translations merely on the grounds of these phenomena, the more so as their occurrence and density often coincided with various other features usually taken to be markers of translation.

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3. The differences in use of conjoint phrases of (near-)synonyms between books for adults and for children were demonstrated in 1984 by a student of mine, Ms. Gitit Holzman, with respect to one author, Benjamin Tamuz, who indulged in both types of writing. Ms. Holzman was also able to show clearly the overall decrease in the use of this device by Tamuz between the years 1950 and 1984. The use of conjoint phrases of (near-)synonyms is also very much alive in the rhetorics of *politicians* (for which cf. Y. Shlesinger 1985), especially those whose linguistic behaviour tends towards pomposity — an epigonic use of old-time norms of *written* language, to be sure.

To take a small example: in three consecutive pages (60–62) of a text which utilized this device in full accord with the prevailing norm, but almost *ad absurdum*, a 1933 translation of Ludwig Lewisohn's *The Last Days of Shylock*, 22 conjoint phrases were found, two of them with three members each. Almost all were free combinations. Translated by a prominent writer and translator, Reuben Grossman (Avinoam), whose adoption of a great variety of translational solutions reveals recurrent patterns, this book could hardly have been read as anything but a translation — or else as a text which originated decades earlier.⁴

When conjoint phrases of near-synonyms encountered in a Hebrew translation are mapped onto their counterparts in its source, in no matter what language, they are usually found to have replaced single lexical items. In extreme cases there is even *zero* lexical substance in the corresponding place in the source text, so that the phrases emerge as pure additions (see Levenston 1985: 86–87; Toury 1977: 162–171, 265–266). This is enough to warrant the claim that it was first and foremost an aspiration for enhanced *Hebraity* which was at issue here.⁵

Thus, in Grossman's translation of Lewisohn's *Shylock* (see pp. 57–59 of the source text), 11, i.e., exactly 50% of the Hebrew conjoint phrases, were found to have replaced single English items (pretext, to smite [us], made [them], disciplined, thought, [that] hoary trick, rang, to found for, bitter, enemy, entered [into him]), while 7 represent source-language couples. The remaining 4 were added to the text, often in a purely conjectural manner. The cumulative effect of this device in the text under study thus seems indisputable even before shifts and translational relationships have been explored.

Obviously, there are also cases where source-text conjoint phrases, both fixed and newly coined, have *not* been replaced by Hebrew ones. However, most of these cases can easily be written off to accidental lack of (near-)synonyms in

4. Two to three decades later, translations testifying to such an attitude started being characterized by critics as 'texts which had become dated before they were published'. The example quoted in the text was taken from Toury (1977: 167–168), where a full list of the Hebrew phrases as well as the source-text items they replace can also be found. The three pages referred to were selected at random, but seem to be quite representative of the whole book — and of its translator.

5. And see, in this connection, the detailed account of a 1922 Hebrew version of a *Schlaraffenland* text (Chapter 8, especially Section 6).

the Hebrew lexicon, or else to the fact that the original expression is an idiom, whose overall meaning was preferred to its conjoined structure and to the individual meanings of its constitutive elements.

3. Shifts, relationships, first-level generalizations

Frequent recourse to conjoint phrases in a translation is bound to have far-reaching implications for the way it would represent its original. In addition to the identification of local shifts, mapping the Hebrew phrases onto their counterparts in the source text also makes it possible to establish the relationships obtaining between the members of each pair and look for some recurring patterns there, on the way to establishing the pertinent norm of equivalence and the underlying concept of translation:

As a result of its length in relation to each one of its constituents as well as its (greater or lesser) semantic repetitiveness and combinatorial structure ($x_1 + x_2$), the main shift caused by the use of a conjoint phrase to replace a single lexeme (which is by far the most common case in our corpus) amounts to *re-distributed information*. This redistribution, which stems from the changing ratio of semantic load vs. linguistic carriers, tends to manifest itself as either redundancy or informational intensification, depending on the surrounding context. It gains in prominence and weight in direct proportion to the density with which the device is used in a text, to the point of effecting an overall change of the latter's semantic structure.⁶

Another kind of common shift, this time stemming from the stylistic

6. There is in fact at least one contemporary case, the Hebrew translation of Heinrich Böll's *Ansichten eines Clowns*, where the translator relied first and foremost on the added length of conjoint phrases and used a host of them, mostly new coinages, many with three, sometimes even four(!) constitutive elements, as part of an overall attempt to make the translation longer. A sample analysis (in Toury 1980c) showed that the average lengthening thus achieved (with respect to a 'normal' translation into Hebrew) was almost 30%. The reason for this attempt may well have been the fact that the basis for translation fee in Israel is the physical length of the *translated* version. Not only does every letter count, then; it is literally counted! On the other hand, it may also have been the translator's proven epigonism. Be that as it may, the Hebrew version certainly looks rather old and obsolete, even though the book is modern and the translation was only done in 1971.

markedness of many of the binomials and of the technique itself, is *stylistic elevation*, which used to characterize translation into Hebrew anyway, not necessarily as a norm in itself, but often as a result of the activity of other norms (Toury 1980a: 128); a by-product which the culture was not inclined to resist and towards which it had no aversion. What makes this by-product so significant is the high rate of acceptability it came to enjoy for such a long time.

The following translation of Goethe's second "Wanderers Nachtlied", one of over 40 existing Hebrew translations of the poem, is a particularly striking case in point. Being a very concise text, it illustrates in an extreme way, yet this time without sliding into parody, the use of our conjoint phrases at the end of the 19th century – in itself, i.e., as a piece of Hebrew poetry, as well as vis-à-vis the original (the phrases have been italicized):

mi-kol *kípot* u-*šfáyim* }

m(e)al kol giv(')a ráma }

takšévna ha-oznáyim -

ax *hášket* u-*dmáma*;

bá('a)le *kánaf* va-*éver*

ba-yá('a)r yišnu áta,

xaké na m(e)át ha-géver,

od tanú'ax gam áta! (Mandelkern 1889: 102)

[From all peaks and bare heights / from every high hill / the ears hear – / but quietness and stillness; / winged and pinioned creatures / are asleep now in the forest, / just wait a little, o man, / you too will yet come to rest.]⁷

Due to the greater number of syllables in an average Hebrew word, any attempt to translate a German (or English) metrical text into this language while trying to retain its prosodic traits entails an inescapable need to give up parts of the semantic-lexical substance.⁸ This need is all the more pressing when the source text consists of a small number of words which are relatively short too, as does Goethe's poem:

7. Since Mandelkern's translation leans heavily on biblical structures and lexical items, it was only reasonable to base its literal rendering on an older English translation of the Bible. I have chosen the Revised Standard Version for that purpose.

8. It may be interesting to note that newer translations of Goethe's poem not only shun conjoint phrases altogether in favour of an attempt to retain a greater portion of the original semantic substance, but also try their best to use short Hebrew words *for their sheer shortness* – a clear sign of growing emphasis on source-induced considerations, in this particular respect. Lacking words of this kind which would also fit the prosodic scheme, some of these translations have chosen to coin new ones. This decision is in

Über allen Gipfeln
Ist Ruh.
In allen Wipfeln
Spürest du
Kaum einen Hauch;
Die Vögelein schweigen im Walde.
Warte nur, balde
Ruhest du auch.

The original poem thus has 38 syllables constituting 24 words, i.e., an average of 1.58 syllables per word. Mandelkern's Hebrew translation, in turn, comprises almost the same number of graphical words, 26, but these encompass 53 metrical syllables: an average of 2.038 metrical syllables per word, or almost 1.3 times as much as an average source-text word. (Some of the grammatical syllables tend to be compressed in the metrical reading, as indicated by the parenthesized elements in the transliteration.)

It is clear that our translator struck a balance of sorts between the constraints exerted by the prosody of the original and its wording: he used a greater number of syllables than Goethe, but omitted some of the semantic contents of the poem at the same time. What he seemed to have no misgivings about was wasting a substantial part of the limited textual space at his disposal on conjoint phrases where — prosodically and linguistically alike — he could easily have made do with just one of their constituents: 14 (20, if the first two lines are taken as an additional conjoint phrase, which they should be) of the 53 metrical syllables he used, or 26.4% (37.7%), are devoted to them, so that all in all, 8 (or even 14) syllables would seem superfluous. It is almost as if Mandelkern added quite a few syllables for no other reason than to accommodate his conjoint phrases.

Thus, the translator could easily have done much greater justice to both prosody (i.e., utilize a smaller number of syllables and words) and semantic content (i.e., give up a smaller part of it), were it not for his wish to produce a **passable 19th-century Hebrew poem**. Under that aspect he simply could not have done without a number of conjoint phrases. In his eyes, as in the eyes of his contemporaries, these items probably evoked *biblical parallelism*, which made them representative of the most prestigious kind of Hebrew. (It should perhaps be added that the translator was the same Mandelkern who,

keeping with another target-literature norm of that period, but would have been completely out of line at the end of the 19th century. As a result of these manipulations, the number of metrical syllables in the translations gradually comes to approximate that of the source text, and the average length of a target-language word drops to about 1.85. At the same time, it makes for an altogether abnormal Hebrew text, even a lyrical poem.

in 1896, also produced the most popular Hebrew concordance of the Bible.) However, he overdid it, so to speak: the density of conjoint phrases in his version of the Goethe poem is considerably higher than a Hebrew poem of the late 19th century would have required. This should no longer come as a surprise, though, given the secondary position translation normally assumes, combined this time with Mandelkern's own proven epigonism as a poet.

In a small percentage of the examples included in our Hebrew corpus, which we keep extending incessantly, conjoint phrases of near-synonyms may be locally accounted for as yielding *enhanced referential equivalence* as a kind of 'hendiadys' ("the use of words with different but overlapping semantic spectra to denote the area of overlap"; Beeston 1970: 112), at the clear cost of non-equivalence at the level of meaning carriers as well as lower acceptability as constituents of an (original) Hebrew text. The hendiadys explanation was indeed suggested for similar phenomena occurring in, e.g., translations from European languages into *Arabic* (Somekh 1975: 6–7; 1981b: 210) and from Sanskrit into Tokharian (Aalto 1964: 69), and it may well apply in those cases. With modern Hebrew translation, however, the hendiadys is rarely a convincing explanation, and it is hardly thinkable in the present case. All in all, the enormous diversity of the relationships found to obtain between Hebrew conjoint phrases and their counterparts in the source texts, and especially their completely irregular distribution, inevitably bring us back to our previous hypothesis concerning the translator's aspiration for enhanced Hebraity.

What we would thus claim is that it was not the wish to retain a semantic invariant of any kind that directed a search for translational replacements, in which conjoint phrases of near-synonyms were used — or even created — time and time again. Quite often it was no source-induced constraint at all, but a norm which originated in the *target* pole and was designed to serve its own needs. Nor is this kind of explanation at odds with hypotheses already put forward concerning the abundance of word pairs in translation into other languages. In this connection, suffice it to mention the "Schönheit" explanation given by Leisi (1947: 111–113) for the use of what he calls the 'tautological word-pairs' device in Caxton's version of *Eneydos*:

Caxton neigt zum tautologischen Wortpaar, weil er es *an sich* als schön empfindet ... Wir haben lediglich davon auszugehen, dass zu Caxtons Zeit das tautologische Wortpaar allgemein als schön gilt. (p. 112; emphasis added)

4. Second-level generalizations and further research prospects

Which leads us right into the possibility of combining the results of various studies into a recurring translational solution, not just within one culture, but

also beyond and across linguistic and cultural boundaries. If arrived at within compatible frameworks, and if the differences in circumstances are also taken into account, knowledge thus accumulated could very well yield general hypotheses which may even bear on translation theory itself.

In our case, one possible claim could be that abundant use of conjoint phrases of near-synonyms, binomials or free combinations, especially in lieu of source-text single lexical items or as outright additions, may represent a **universal of translation into systems which are young, or otherwise 'weak'**; perhaps as part of an attempt to develop indigenous linguistic abilities in the face of this sensed weakness. A few studies have already hinted at the feasibility of such a hypothesis. However, as far as I know, it has been given no explicit formulation so far, except in an outline of the present 'study in descriptive studies' included in a previous version of my own Rationale (Toury 1985a: 38). Let me mention three of these studies:

1. Wenzlau's 1906 survey of the use of binomials and trinomials in German translations of the 14th and 15th centuries;
2. Leisi's aforementioned dissertation on tautological word-pairs in Caxton's *Eneydos* (1947); and
3. a tentative discussion of literary translation into Arabic in the 19th century (Somekh 1981a).⁹

Unfortunately, these and other studies were not carried out for the same purpose; moreover, they made use of diverse methodologies. Their findings are therefore all but easy to compare, making it unclear whether our sweeping hypothesis would stand. On the other hand, this hypothesis certainly warrants further, larger-scale studies.

5. Applying research findings in actual translation

As claimed in Part One, once they have been explicitly formulated, the findings of a descriptive study can also be put to deliberate use. One objective of using such findings may well be to model one's future strategies on actual translation behaviour, past or present, only this time in fuller consciousness and under much more control.

9. See also Sørensen 1960: 128-132.

Thus, in my own translation of Mark Twain's *Connecticut Yankee in King Arthur's Court*, I made rather frequent use of conjoint phrases of near-synonyms, in an attempt to create a parodistic air of 'stylistic archaism'. To be sure, this translation was made in 1983, that is, when the use of the device had become quite obsolete in translation into Hebrew too. True enough, Twain used a host of English conjoint phrases himself as part of his own parodistically archaising style, which certainly reinforced my decision to adopt this particular stratagem. However, the replacing phrases were normally new coinages of mine, and in no way did they necessarily match up with conjoint phrases used by Twain. Finally, my use of the device was denser than his, in view of the differences between the two languages even in older times. Thus, my use of conjoint phrases was not just a function of the way they were used in the source text. Rather, it was meant first and foremost as a step back into the history of Hebrew *translation*, which I had become aware of through my own studies and those of others. To judge from critical reviews of the translation, the intended impact on the reader was indeed achieved.

Translation-in-Context

An Assortment of Case Studies

Time has come to present an assortment of case studies which were performed within our target-oriented framework, on the basis of the assumptions elaborated in Part Two and their methodological implications. In order to achieve some unity and continuity and enhance the persuasive force of the descriptive part as a whole, studies were selected which pertain to a single cultural sector, namely, Hebrew literature in its evolution during the last 200 years. To be sure, the accelerated pace in which changes occurred in this particular system renders it most suitable precisely for tracing the dynamics of the concept of translation. Despite the high rate of uniformity of Part Three, in terms of both methodology and subject matter, each chapter is, to a great extent, self-contained and can therefore be read in and for itself.

Basically, the chapters progress from the general to the specific, beginning with whole historical moves (Chapters 6 and 7) and gradually narrowing their focus, first to the translation of single texts (Chapters 8–10) and finally to one recurring pattern of decision-making (Chapter 11). The guiding principle here is that of continuous contextualization in higher-order, and conditioning environments. The principle as such is taken up again in Excursus B, in an attempt to establish a functional characterization of 'literary translation', thus distinguishing it from any other mode of translating a literary text. The overriding need to contextualize is also stressed in the critical presentation of the application of experimental methods to the study of translation (Chapter 12), as well as in the programmatic exposé of the gradual emergence of a translator (Excursus C) — a domain which still awaits some dedicated research work.

Between a 'Golden Poem' and a Shakespearean Sonnet

1. Prior to 1916: A meaningful void

Shakespeare was not so much as mentioned in Hebrew sources until the Enlightenment period, usually dated for Hebrew literature as beginning in the mid-18th century. Needless to say, no work of his had as yet been translated into that language, just as hardly anything else had been translated from English literature, let alone directly from the English language (see Chapter 7). By the time of the Enlightenment itself (as amply demonstrated by Dan Almagor in a comprehensive bibliographical survey [1975]),¹ Hebrew writers and critics came to regard the Bard as one of the major figures in world literature, along with writers like Homer, Goethe and Tolstoy. This change, however, was rather superficial; for a major part of the Hebrew literary milieu of the period, Shakespeare was hardly more than a name to be dropped as a sign of cultural *bon ton*. In actual fact, his position in Hebrew literature remained marginal and his impact on its development virtually nil. Moreover, inasmuch as Shakespeare's name was mentioned in Hebrew writings of the period, what was referred to almost exclusively was his reputation as a playwright. His first translations into Hebrew too,² and for almost a whole century thereafter, were either monologues or other short passages from *plays*, almost exclusively *tragedies* (Almagor 1975: 769–784), and most of them were presented – and read – as *poems*.

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1. I wish to thank Dr. Almagor for his assistance in various bibliographical matters.
 2. The first translations from Shakespeare's writings into Hebrew are listed in note 8 to Chapter 7.

Thus, despite the fact that, in most European cultures, Shakespeare's Sonnet Cycle has already come to be regarded as an important part of his *œuvre*, and even though, in the Hebrew tradition itself, the sonnet had always played a more important role and enjoyed a much more central position than any dramatic genre, Shakespeare's career as a sonneteer was hardly ever mentioned. While translations of Shakespeare's sonnets have long figured in literatures with which Hebrew literature had close contacts – most notably translations into German, Russian and Polish – it has taken even longer for them to begin being translated into Hebrew: the first known translation of a Shakespearean sonnet dates from 1916, by which time there had already been Hebrew translations not only of dozens of passages from Shakespeare's plays but also of five of his tragedies in their entirety: *Othello* (1874), *Romeo and Juliet* (1879), *Macbeth* (1883), *King Lear* (1899) and *Hamlet* (1900–1901). Even after the arrival of the first Shakespearean sonnets, the pace of their inclusion in the repertoire of texts available for translation into Hebrew was rather slow and hesitant.

To anyone who wishes to proceed from the assumption that translations form an integral part of the *recipient* culture, the delayed arrival of a translation would seem a 'meaningful void', an absence deserving explanation. In the case under review here, we need not beg the question though: the explanation is integral to the material itself. Thus, the most likely, and most historically oriented³ explanation for the fact that translations of Shakespeare's sonnets were so late, slow and hesitant in arriving, even when compared to the translation of Shakespeare's works in general, is precisely that unlike drama (as well as other literary genres which also supplied texts for translation), the sonnet had a fairly extensive tradition within the Hebrew system itself.

The Hebrew sonnet first appeared in Italy around 1300, with Immanuel of Rome (c. 1260–c. 1330) – i.e., a whole generation before Petrarch. Thus, Hebrew literature actually pre-dated by far all literatures which adopted this Italian genre. However, Immanuel's was an isolated episode, and a true sonnet tradition in Hebrew literature had to wait another century or so to emerge. The Hebrew sonnet, modelled on the Italian example, then became, especially in the 17th–18th centuries, the most widespread genre in Hebrew poetry in Italy (Landau 1970). Moreover, not only secular

3. Explanations such as 'the sonnets were unknown', or 'less known than the plays', or the like, are not necessarily wrong with respect to any specific *individual*. However, their explanatory power in *historical* terms is rather small.

poems were written in Hebrew in the sonnet form, as in the Italian tradition itself, but sacred ones as well, including liturgical hymns.

The Hebrew sonnet was even accorded a name of its own, which is one possible sign of its appropriation within a cultural tradition: *šir zahav*. Literally, this expression means 'a poem of gold'. However, what its establishment actually represents is a numerological manipulation known as *gematria*, whereby the sum of the values assigned to the [consonant] letters of the word *zahav*, actually written *zahab* ($z + h + b$), is fourteen — the fixed number of lines in a sonnet. At the same time, the selection of the word *zahav* ('gold') from among all the possible Hebrew words which could have represented the value 14 testifies not only to the *institutionalization* of the genre in Hebrew literature, but also to the *prestigious status* it acquired there.

The sonnet, then, in its modified Italian version, had a relatively uninterrupted history in Hebrew literature. It flourished in Italy itself mainly in the 17th–20th centuries, and later figured in the poetry of the Hebrew Enlightenment of Central and Eastern Europe as well, and in the poetry that developed out of it in Europe, and eventually in Palestine, on the one hand, and in the United States on the other. Thus, there was no 'void' with respect to this genre in Hebrew literature, so that no need was felt to compensate for one. To put it somewhat differently, no need arose to put any (literary and linguistic) instruments to the test in preparation for assimilating the sonnet into the target system, as was the case in other domains of literary translation into Hebrew at various periods.

This explanation seems all the more valid considering the many other cases of avoidance, when it came to the translation of sonnets into Hebrew. For instance, of the fairly large number of sonnets written by Goethe, why was not a single one translated, even though Goethe was among the poets most massively rendered into Hebrew (as exemplified by Lachover's bibliography [1952–1953])? The same holds true for many other authors whose poetic crop also included sonnets. And as far as sonneteers *par excellence* are concerned, among them Petrarch himself, Hebrew translations of their works did not appear until much later.

2. 1916–1923: Modified 'Golden Poems'

It was not until 1916, then, that the first two of Shakespeare's sonnets (nos. 18 and 60) appeared in Hebrew translation. The period was one of mass immigra-

tion to the United States by Jews from Eastern Europe, which, at that time, still functioned as the Hebrew literary center. An obvious result was that many Jews were beginning to come into direct, more and more intense contact with the English culture, including Shakespeare's literary works. The first translator, Israel Jacob Schwartz (1885–1971), is fully representative of the new trend: Born in Kovno (Lithuania), Schwartz attended a *yeshiva* (religious school). In 1906, at the age of 21, he immigrated to the US, where he became a teacher. His literary work was mostly confined to Yiddish, but he also tried his hand at Hebrew, both original writing and translation.

Schwartz's first translations of Shakespearean sonnets appeared in an American Hebrew periodical (*Ha-toren*), which, in terms of the Hebrew culture at large, was rather marginal. This explains how these translations managed to escape attention. Even Almagor, who has done extensive digging into the chronology of Shakespeare in Hebrew, failed to notice them until after his comprehensive bibliographical survey was published in 1975. Seven years later, in 1923, the same Schwartz published another ten sonnets. This time they featured in one of the central organs of Hebrew literature, the journal *Ha-tkufa*, and hence attracted much greater attention. At the same time, they hardly left any mark in the Hebrew culture nor indeed were they ever regarded as a part of its literary heritage.

These ten sonnets appear, in sequence, as a unitary group, under the inclusive – and misleading – heading “The Sonnets of Shakespeare”. The selection they represent does not even constitute a consecutive series. It is just an *assortment* of sonnets from Shakespeare's Cycle, selected at random, which have not even been printed in their original order (66, 33, 73, 61, 71, 34, 32, 29, 27, 17). In fact, nowhere is any reference made to those numbers, which might at least have *hinted* at their order in the Shakespearean Cycle. All this indicates that there is no ground for assuming any Sonnet Cycle consciousness on the translator's part, or any awareness of the individual sonnets' relative positions within the cycle, whatever one may feel about the origin and/or significance of their sequence and sequencing (which does constitute a problem in Shakespearean studies).

What is most striking about Schwartz's translations is that although all twelve of them belong to that part of the Cycle known as the “Sonnets to a Young Man” (nos. 1–126), all of them, except sonnet no. 60, which contains no markers either way, have been translated as *addressing a beloved woman*. To be sure, the ‘feminine’ or ‘masculine’ character of an utterance which is basically conative would always be much more conspicuous in Hebrew than in English,

since the distinction between the two is *obligatory* in the second person singular – in the pronouns, in noun declensions, in prepositions and other particle declensions and in conjugations: There is simply no way to compose a conative utterance which is not marked for gender. At the same time, the actual number of feminine markers often far exceeds whatever would have been classified as ‘obligatory’. Thus, in a single translated sonnet as many as 16(!) such markers may occur (no. 18). As a result, eleven of the twelve sonnets translated by Schwartz became love poems which are unequivocally directed at a woman, in marked contrast to their original intent. This was hardly to recur after 1923, except in isolated cases, even though the “Sonnets to a Young Man” were often the more popular among the translators, and not just because they form the majority of the original Cycle either.

Schwartz’s behaviour in this respect is not difficult to account for, in light of the prevailing norms of the period: During the first third of the 20th century, much of Hebrew poetry, original as well as translated, was still being written by observant Jews, or, at least, by Jews who had had intensive religious training. They were also intended for an audience of the same background. For them, love between two men – whatever such love may have meant for Shakespeare and his contemporaries – was simply out of bounds. Feminizing the masculine sonnets was an observant Jew’s way of establishing a compromise between his admiration of Shakespeare and his sonnets, reflected in a strong desire – innovative in itself – to introduce them to the Hebrew reader, and the demands of the rigid cultural model laid down by the receptor culture. As is well known, a similar trend could be observed in other literatures as well.⁴ However, in Hebrew, this type of compromise is not only much more striking than in any one of these traditions, due to the specific features of the language, but it is being struck long after being abandoned by translators into other languages, due to the belated start of modern, secular Hebrew literature and the specific course of its development.

In this particular case, Schwartz’s compromise can be characterized as involving voluntary *ensorship*. However, the need to compromise was not confined to this thematic domain, where a global decision should be assumed which was then systematically realized in dozens of individual instances in the texts themselves. After all, compromise is a key-word in any attempt to account

4. And see what Delabastita (1985: 119–121) has to say about the ‘platonization’ and the ‘bowdlerization’ of the “Sonnets to a Young Man” in French, German, even a particular English edition of Shakespeare.

for translations and translation practices; namely, a compromise between competing models, drawing on the source-language text, on the one hand, and on internal target-culture norms, on the other. In Schwartz's case, this compromise, as was still customary with literary translations into Hebrew at that period, tends to lean much more heavily on *target* models and norms than on the reconstruction of features which are [more] relevant from the point of view of the source text. In other words, *acceptability* is given precedence in most domains. The price that is paid in terms of translational adequacy is considerable, even though not total. Thus, Schwartz's end product is never a mere reproduction of the Shakespearean Sonnet, but is not a pure 'Golden Poem' either.

Chief among the features of the sonnets where Schwartz's translational approach reveals its peculiar compromise is the *formal* one. The Shakespearean sonnet is known to differ widely from the Italian one – a model which was at the base of the Hebrew sonnet down through the ages. Schwartz too was raised on the Italian model in its Hebrew modification, until such time as he first encountered Shakespeare's sonnets. As the first person to translate these into Hebrew, Schwartz had a clear opportunity to introduce a novelty into Hebrew literature in the form of an unknown, or 'deviant' genre model. On the other hand, he could also opt to subject his translations to whatever model prevailed in Hebrew literature itself. In view of the secondary status of English literature as a source for translations into Hebrew at that time, one might have expected the translator to adopt a rather conservative approach vis-à-vis the formal traits of the Shakespearean sonnet, which indeed he did. At the same time, he did not confine himself to the purely Hebrew option either, but chose a kind of a middle course. This decision – and it involved real, non-automatic decision-making – may be taken as an indication of the as yet undefined, if not insecure status which Shakespeare's sonnets must have had for Schwartz, not necessarily as an individual, but certainly as a representative of the receptor culture and literature; i.e., in his capacity as a *translator*.

As far as the *rhyme scheme* is concerned, for example, the Shakespearean sonnet follows the ABAB CDCD EFEF GG pattern; i.e., it includes three quatrains, each of them with a different alternate rhyme. As against this, the Hebrew (Italian-like) sonnet prefers the embracing rhyme pattern (ABBA etc.) and uses two identical rhyme-members (rhymemes) in its two quatrains. Now, in Schwartz's translations, of the 168 lines (12x14), only 8 (4.76%), amounting to two quatrains out of 56 (3.57%), follow the Shakespearean rhyme scheme. All the other lines abide by the scheme established in the Hebrew tradition, ignoring, however, the norm of having two identical rhymemes in the first two quatrains.

By contrast, in terms of the rhymes (even though not from the typographical-visual standpoint), the translated sonnets *are* divided into three quatrains + a closing couplet, with seven different rhymemes, as in the sonnets which served as their sources, rather than into two quatrains + two tercets based on only five rhymemes, as in the Hebrew-Italian tradition. The independence of the rhyme of the closing couplet of each sonnet is also retained — unlike anything in the Hebrew sonnet until then, however flexible the rhyme patterns of its sestets had been.

With respect to *meter*, Schwartz adopts not merely the *iamb*, but the iambic *pentameter*. Thus far, however, the Shakespearean model still tallies with the tradition established by the Hebrew Golden Poem, a tradition which prevailed ever since Hebrew poetry first appropriated the tonic-syllabic metrical system (Hrushovski 1971a). In fact, while practically all the translators of Shakespeare's sonnets into Hebrew followed the iambic pattern, several expanded the original pentameter into a *hexameter*, mainly in order to partly compensate for the inability of the target language to fit all of the lexico-semantic material of the English original into the original Procrustean bed, since Hebrew words tend to comprise a much higher number of syllables.

While Schwartz did establish the pentametric norm as a point of departure, the truth of the matter is that he too included hexametric lines in every single one of his sonnet translations. In the most extreme cases, he went so far as to include as many as eight (nos. 17, 32, 71) or, in one instance, even ten (no. 27) hexametric lines in a single sonnet. There is even one case of seven iambic feet in a line (iambic *heptameter*). It may seem, then, that the status of pentameter as the basic metrical norm is no more than a *statistical* outcome. In fact, however, a six-foot verse for Schwartz is neither a basic nor even a secondary norm. It is simply a 'tolerated' phenomenon (see Chapter 2), a way of coping with a specific difficulty. This claim is further attested to by the fact that he opts for brevity of formulation. Thus, he avoids wasting too much space on long and complex expressions, including conjoint phrases of (near-)synonyms, to replace single and/or simple lexical items — a practice which was so common in the tradition of poetry translation upon which he largely drew (see Chapter 5). In this respect too, then, Schwartz appears to have arrived at a definite compromise between traditional Hebrew norms and the ones reflected by the original sonnets.

One purely Hebrew aspect of the compromise in the prosody of the translated sonnets lies in the fact that all of Schwartz's lines are penultimately stressed, thus ending in a so-called 'feminine' rhyme. It was the tradition of 'Sephardic' Hebrew poetry in Italy that laid down the demand to refrain from

mixing ultimately and penultimate stressed lines, a tradition which was later to work its way into the Hebrew poetry of Central and Eastern Europe as well, which reflected a very different, 'Ashkenazi' pronunciation. Conveniently enough, the imported prosodic norm was matched by the properties and possibilities of the language itself, since the stress in Ashkenazi Hebrew is usually on the penultimate syllable to begin with – greatly facilitating the use of feminine rhymes. Nonetheless, here too Schwartz permits himself a minor deviation: he does not confine his rhyming words to those characterized by penultimate stress in both Ashkenazi Hebrew and that Sephardic pronunciation which prevailed in Italy (Pagis 1973: 660), thus failing to obey another norm in the area of rhyme.

From the standpoint of the **language** of his translations, Schwartz is very close to the poetry of the Enlightenment, whose norms were still active in Hebrew literature, especially the translated part, although these too were gradually relegated to a peripheral position and superseded by the poetic norms of the Revival Period. His adherence to those Enlightenment norms may be accounted for, at least in part, by his place of residence, the United States, where he was cut off from the mainstream of Hebrew literature and its major course of development. Schwartz's adherence to the older norms may have been reinforced by the status assigned to the *original* texts as 'classics', even though that stand was much more characteristic of the translational attitude towards Shakespeare's *tragedies*,⁵ and, at any rate, epigonism was typical of most of the Hebrew writers and translators who have lived in the US throughout the 20th century.

Looked at from the opposite direction, translational solutions were all but dictated by a direct acceptance of the verbal formulation of the source texts as a major constraint. Clearly, they were not a result of confronting that formulation with the *full range* of possibilities afforded by the Hebrew language of the period. In fact, Schwartz approached those texts equipped with a relatively *limited* repertoire of target-language options, put at his disposal by the prevalent linguistic and rhetorical models (Toury 1980a: 131–135). It is mainly this repertoire, taken as a more or less closed list of translation 'solutions', that

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5. "An adherent of this ideal would translate Shakespeare into a poetic language and set of poetic norms that in his view function as 'classical', both for himself and for his prospective readers. This usually means translating into the poetic language and norms of a previous generation of Hebrew poetry, whether immediately preceding the translator's or further removed" (Golomb 1981: 205).

dictated his translational behaviour on the language plane, which may hardly be said to relate in any direct way to the nature of the originals. The most conspicuous manifestation of this attitude is Schwartz's strong tendency to rely on fragments of verses from the Hebrew canonical sources, mainly the Bible, and to string these together. In fact, it often happens that the choice of one such fragment affects the manner in which the translation continues; the emerging text is, as it were, forced to follow through on it, rather than adhering to the wording of the original text.

One thing that follows is that one would be hard put to define the translational unit — i.e., the linguistic-textual unit in the original text within which the translator tended to work — since the role of the target language, and even of the emerging target text in defining each unit is at least as marked. Be that as it may, if one proceeds by examining the series of coupled pairs of replacing + replaced segments which emerge through a systematic contrastive analysis of the translations vs. their sources, one discovers at least this much, that Schwartz's translational unit is fairly large. In fact, if the 'no-leftover' condition is adhered to, the unit pertinent to his translational behaviour is often found to transcend the boundaries of the poetic line.

Trying to sum up Schwartz's approach to the sonnets, it turns out that he chose to place his translations at a point where the innovation they represent through the reflection of source-text features will be tempered by habitual and established practices sufficient to ensure that the text as a whole will not deviate unduly from the prevalent norms. The outcome of his endeavours may therefore be characterized as a kind of Shakespearean-like Golden Poem, which differs from other varieties of the Hebrew Golden Poem also used at that time in original writing as well as translation.

3. 1929: An alternative point of departure

This then was the main point of departure for future translations of Shakespeare's sonnets into Hebrew. A few years later, however (1929), one of the ten sonnets translated by Schwartz for *Ha-*tkufa** (no. 66) appeared in another Hebrew translation. The principles ingrained in this second translation, which, chronologically speaking, still belongs in the same period, indicate the possibility of a different point of departure for translating the sonnets into Hebrew. The periodical in which this translation was published, *Ktuvim*, the organ of Hebrew modernism, was at that time in the throes of a massive attack on the poetic

norms of the previous literary generation, the so-called Revival Period, from which it was to emerge victorious (see Z. Shavit 1982).⁶ The translation itself is unsigned, but it has been ascribed to Avraham Shlonsky (1900–1973), the main spokesman of Hebrew modernism and the creator of a new, and highly influential ‘school’ of literary translators. In all likelihood, just as other Shakespearean translations of Shlonsky’s were not made directly from the English original, neither was this one.

By and large, this translation is in keeping with the poetics of Hebrew modernism⁷ whose norms – both in original writing and in translation – were then still on the periphery of Hebrew literature, struggling towards its center. Among its most conspicuous characteristics the following deserve to be mentioned:

- (1) In utter conformity to the modernist rebellion against outmoded literary institutions, the poem is not presented as a sonnet at all, nor does it contain any typographical indication to this effect – neither a division into quatrains and tercets nor any prominence to the closing couplet. Only by counting the lines can one discover that the poem is in fact a sonnet. Needless to add, there is no mention anywhere of the number of the original in a Cycle.
- (2) On the other hand, the translated poem is given a full-fledged title, severing it entirely from the Shakespearean Cycle. In itself, this title is in full keeping with modernist poetics: it is embodied in the single ‘big’ word “World”. This same practice was to be duplicated in future translations of Shakespearean sonnets as well, done by members of the modernist school or its aftergrowths, whether published in isolation or in small groups.⁸

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6. The fact itself that a Shakespearean sonnet appeared in a militant periodical of modernism, and in a strategic position at that, at the center of its front page, may seem unusual. However, the whole issue in question was an exception to the normal ways of the periodical, in that it was based almost exclusively on translations of short excerpts of fiction, essays and poetry from various languages (including passages of Jack London, Bernard Shaw, Upton Sinclair, John Ruskin, August Strindberg, Walt Whitman and John Bunyan).
 7. It may well be that [some of] the properties of this translation were actually drawn from the mediating translation which was used. This however makes little difference, when it is not the text as such which is at stake but rather the different translational option suggested by it.
 8. In this connection, it is worthwhile mentioning especially those translated sonnets which were included in the *Hebrew Anthology of English Verse*, edited by Reuben Avinoam (Grossman) (1942–1943): “Thou” (no. 1); “’gainst Time’s Scythe” (no. 12); “Who will

- (3) The rhyme scheme of the translation alternates masculine and feminine rhymes, as was the custom in modernist Hebrew poetry, in the wake of Russian poetry. This contrasted not only with the Shakespearean sonnet but also with the poetic tradition of the Hebrew Golden Poem, which demanded utter separation between poems whose rhyme scheme was based on ultimate-stress meter and those whose rhyme scheme was based on penultimate-stress meter (Pagis 1973: 659). By contrast, the rhyme pattern, and hence the implied division of the poem into stanzas, are Shakespearean (ABAB CDCD EFEF GG), and the translated sonnet is unequivocally directed at a *man* (it has two definite masculine markers).
- (4) The language of the poem is no longer typical of the Enlightenment, but neither is it modernist in nature. Actually, in this respect, the translation is more or less representative of the norms occupying the *center* of the system at the time, those norms against which the modernists wished to rebel. In fact, as translators, Shlonsky and his contemporaries were rather slow and cautious in adopting the new norms, which again goes to testify to the *conservatory* power of translation in most situations.

The model underlying this isolated translation, then, represents another possible point of departure for translating Shakespeare's sonnets into Hebrew. Indeed, elements were later taken from this moderately modernized model and added to the vestiges of the Hebrew Golden Poem, along with several Shakespearean features couched in the Hebrew language, to establish together the basic principles of that translation of the sonnets that was to gain canonization in Hebrew literature only one or two decades later.

4. Moving away from the Golden Poem

As already indicated, Hebrew literature had not yet come to regard Shakespeare's Sonnet Cycle as an entity in itself. The explanation for this attitude seems to be

believe" (no. 17); "Eternal Summer" (no. 18); "With Eyes Closed" (no. 43); "Would That..." (no. 71); "The Twilight of Love" (no. 73); "Love" (no. 116); "When Thou Play'st" (no. 128); "Thine Eyes" (no. 132); "Love's Best Habit is in Seeming Trust" (no. 138); "As a Poor Infant" (no. 143). The translations are all Shalom's (see Section 4 below), but the titles were given by the editor, who was an acknowledged epigone in other domains as well. (Another example of his was brought in Chapter 5.)

a function of more than mere quantity especially bearing in mind that groups of texts published together retained neither their consecutiveness nor even their order. It appears that the complete lack of a 'cycle' notion in the *Hebrew* sonnet tradition is at least as much of a factor: the only thing which was known to it was the fifteen-sonnet cycle, where the last sonnet is made up of the first lines of the other fourteen, and even this notion was rather marginal. In the late thirties, the poet Sh. Shalom (Shalom Shapira; 1904–1990) was to launch his major project, which was to yield, within a few years, the first version of the entire collection of Shakespeare's sonnets in Hebrew translation.

Shalom began by publishing 16 of the "Dark Lady" Sonnets (127–134 and 137–144), in their original order and with express mention of their numbers, in the important *Gazit* periodical. These were followed, two years later, by the entire "Dark Lady" Cycle in a special booklet, and another two years later, Shalom put out the first 17 of the "Sonnets to a Young Man" in the *Moznayim* monthly. In 1943 all 154 sonnets were finally published together for the first time. Despite the reservations expressed by several critics when the book first appeared, this was to become the canonical translation of Shakespeare's sonnets for at least 40 years to come. Among other things, when referred to in secondary Hebrew sources, Shakespearean sonnets were almost always quoted in this particular version, even when there were already several versions to choose from (see Section 5). In the same vein, Shalom's translation became the main contributor to anthologies of poems in existing translations to this very day. True enough, in 1977, a second full translation of the sonnets was published, but the translational approach it reflects represented a marked regression, especially with respect to the linguistic constituents of the model which shaped his decisions, and his version did not really supersede Shalom's.

The canonical status of Shalom's translation cannot be accounted for solely by the fact that it remained the only full translation of the sonnets for over thirty years. After all, it could easily have fallen into oblivion much earlier, or even right away. In order to explain this status, one would have to consider the enormous conformity of this translation to the norms dominating Hebrew literature at the time, original and translated alike.

Thus, while Shalom's version did represent a further move away from the old-time tradition of Hebrew Golden Poem, this was not merely a matter of striving for a fuller reconstruction of the properties of the Shakespearean sonnet. It also involved the adoption of significant parts of **Hebrew modernist poetics** — precisely those parts that had meanwhile become established and had entered the epicenter of the system, thus in effect losing much of their initial markedness as sectorial.

An especially apt example of mainstream modernist poetics is Shalom's recourse to a considerable number (dozens, if not hundreds) of neologisms, which is hardly a feature of Shakespeare's own sonnets. True, the need for various types of new words – especially those with penultimate stress, which are the exception in the 'Sephardic' pronunciation which now prevailed – arose from prosodic constraints, on the one hand, and from the overall need to minimize the number of syllables in the Hebrew replacements, on the other. Still, the very *readiness* to make use of neologisms, let alone in such profusion, would have been inconceivable in the previous period, even though both the prosodic constraints and the problem of Hebrew word length were nothing new. Therefore it should be taken as a by-product of the modernist poetics which had already provided for their legitimation. (Incidentally, the number of penultimately stressed words, and with it the probable number of new coinages, would have been even greater, had the translator adopted the norm of using masculine and feminine rhymes alternately, which had meanwhile become a salient characteristic of Hebrew poetry in general (Hrushovski 1971b: 744). Shalom, however, did not submit to the internal Hebrew constraint in this respect.)

The **expansion of the linguistic reservoir** available to the translator, in this and in other ways, was also instrumental in allowing him to reconstruct a greater part of the features of the original texts. Thus, despite its dilatory starting point, the direction taken by the translation of Shakespeare's sonnets from Schwartz to Shalom was in full keeping with the overall evolution of literary translation into Hebrew in the 20th century: from acceptability-bound considerations within the confines of the target literature towards a growing concern for translation adequacy.

A **reduction of the translational unit** was another concomitant outcome: this unit often shrunk to the size of a single phrase, if not a single word. Contributing to this trend was the fact that English was just then beginning to serve as a primary source of texts for translation. As the quantity of Hebrew translations from English began to increase, a gradual 'realignment' of the target language and literature vis-à-vis their English counterparts occurred as well, as a result of continual contact between them. This realignment was gradually to increase the initial translatability of English texts – including many features of the Shakespearean sonnet – into Hebrew. It also facilitated working in rather small units, often without incurring the cost of excessive deviation from norms of (at least linguistic) acceptability. This tendency was to gain momentum later on, although it never became the only one.

5. 1943 onwards: A mixed situation

In the following decades, dozens of occasional translations of sonnets, either in isolation or in small groups, appeared in Hebrew newspapers, periodicals and anthologies. At first sight, this may seem a regression to the period when the existence of an original Sonnet Cycle was totally ignored. In fact, however, the intermittent appearance of sonnets when there is no full cycle to fall back on — as was the case until 1943 — is in no way similar to the situation when such a cycle does exist and when every new attempt at translating one of Shakespeare's sonnets can be, and often is regarded in light of an existing canon. This is all the more true considering that most of the translations from then on bore the number of the corresponding source text in the Cycle as their only title.

These scattered translations reveal a very mixed situation. In essence, they reflect parts of different, and competing sets of linguistic, literary and translational norms — old and new, central and peripheral. To be more precise, they point to a state of *flux* between various norm systems, and a situation which is very far from any clear decision, even in the case of poet-translators whose original works (or other translations) represent the latest trends in Hebrew poetry, such as Meir Wieseltier or Menachem Ben (Braun).

As already mentioned, another complete translation of the Shakespeare sonnets appeared in 1977, after parts of the Cycle had been published over several years in various periodicals. This translation was made by the editor Ephraim Broide (1912–). Here again we observe a clear decision — one which, however, reflects a preference for **old-time norms** which no longer played any substantial role in translation into Hebrew except among an ever-diminishing group of *translators* (of whom Broide himself is one) and a fairly well-defined group of *texts* considered 'classical' (of which Shakespeare's sonnets have obviously become part). In many respects, the norms Broide subscribes to are even more old-fashioned than the ones Shalom subjected himself to three decades before, which may well be (part of) the explanation why Broide's sonnets never really superseded Shalom's. The regression is compatible with the situation revealed by the historical move of the translation of Shakespeare *the playwright* into Hebrew. It is not surprising that the 'classical' norm has always prevailed in the translation of his *tragedies* (see note 5), and not necessarily of his *comedies*.

The critical reviews of Broide's translation shed further light on the norms under which he worked, and especially on the *status* of these norms in Hebrew culture in the late seventies. Thus, his critics fall into two diametrically opposed groups: fervent proponents vs. no less fervent detractors. As could be expected,

this division runs parallel to the affiliation of the critics themselves (as contemporaries, as critics, in some cases even as writers and translators) with the obsolete norms, on the one hand, and with the new ones, on the other. Interestingly enough, the critics affiliated with these two well-defined classes cite the very same properties of the translations to prove and illustrate their contentions, which are, as we recall, at opposite poles.

6. A glimpse into the future

In 1993, a third Hebrew version of the entire Shakespearean Cycle was published, made by the comparatist and translator Shimon Sandbank. Representing as it does another huge step away from the Golden Poem and towards a Hebrew Shakespearean-like Sonnet, this translation may well come to mark a new beginning. Implied is a rather unified set of norms, which — unlike Broide's version — may eventually replace the ones still marked by the canonization of Shalom's translation.⁹ Thus, Sandbank chose to lean heavily on the linguistic experience accumulated by modern Hebrew poetry, especially in the wake of the poetics of Natan Sach and Yehuda Amichai. Among other things, he waived the need to establish exact rhymes and gave up other aspects of the structural requirements of the sonnet. In exchange, more or less well-formed Hebrew poems were established, through which major aspects of the originals can however still be read.

An interesting aspect of the alternative offered here seems to be the way it gradually crystallized. After all, Sandbank was among those who published the above-mentioned isolated translations. Several of his translations appeared in the seventies and eighties in more than one version — offering a unique opportunity of tracing the evolution of a translator's attitude towards the Shakespearean sonnet towards the end of the 20th century.

9. A few months later, a revised edition of Broide's translation was also published. His revisions, however, were all made within the old paradigm itself, which gives it a slim chance indeed of constituting a true alternative to either Shalom's version or to Sandbank's, which will probably start its struggle for priority now.

Chapter 7

A Lesson from Indirect Translation

Like everything in and about translation, the (in)directness with which the act is performed can be norm-governed too. In fact, in its *cultural* facet, recourse to indirect translation¹ is often highly significant. Unlike individual instances of translators turning to existing translations as their immediate sources, which may indeed represent no more than simple inability, or even a sheer whim, the recurrence of this practice, especially if regular patterns can be detected, should thus be taken as evidence of the forces which have shaped the culture in question, along with its concept of translation.

1. Mediated translations as an object for study

From the point of view of DTS this would mean that second-hand translation is not some kind of a disease to be shunned, as has long been the dominant attitude. Such an approach only reflects a fallacious projection of a currently prevalent norm, ascribing uppermost value to the ultimate original, onto the plane of theoretical premises. By contrast, mediated translation should be taken as a *syndromic* basis for descriptive-explanatory studies – a configuration of interrelated symptoms which should be laid bare. These symptoms may be unraveled by trying to give satisfactory answers to a set of questions like the

1. For clarity's sake, I will follow Kittel's example (1991: 26, n. 6) and distinguish – in the realm of texts – between *intermediate* (first-hand) and *mediated* (second-hand) translations. The activity itself will be called *mediated*, *intermediate*, *second-hand* or *indirect*, interchangeably.

ones listed in Chapter 2: In translating from what source languages/text-types/periods (etc.) is it permitted/prohibited/tolerated/preferred? What are the permitted/prohibited/tolerated/preferred mediating languages? Is there a tendency/obligation to mark a translated work as having been mediated, or is this fact ignored/camouflaged/denied? If it is mentioned, is the identity of the mediating language supplied as well? And so on.

As a culturally relevant phenomenon, second-hand translation forms much more than a mere *legitimate* object for research (as shown, in a sporadic way, in von Stackelberg 1984). Rather, it presents a convenient means of moving from observable phenomena to underlying factors, precisely because its external manifestations are often easy to discern and its contours relatively simple to draw. In fact, the claim should be even stronger. I would go so far as to argue that no *historically* oriented study of a culture where indirect translation was practiced with any regularity can afford to ignore this phenomenon and fail to examine what it stands for. This is in fact how mediated translations as texts, and the practices which give rise to them, should be approached, along with whatever changes may have occurred in them: not as an issue in itself, but as a **juncture where systemic relationships and historically determined norms intersect and correlate**. It is an approach designed to establish precisely those norms in their interrelationships.

In this chapter, second-hand translation will be tackled as practiced by Hebrew literature of the last 200 years, with special emphasis on a symptomatic reversal of roles of German and English as mediating and mediated, respectively. The peculiar history of modern Hebrew literature, which had a late start, compared with those literatures within which it was embedded, offers a unique opportunity for close observation of historical processes which normally tend to be rather diffuse.² The accelerated pace of its efforts to catch up with the Western world, along with its longstanding status as an epigone vis-à-vis various (and varying) cultures, render the contours of its development considerably sharper, and its phases easier to discern.

2. And compare, in this connection, a series of historically oriented studies produced lately at the Göttingen *Sonderforschungsbereich* "Die literarische Übersetzung" with respect to the triplet English-French-German; especially Graeber 1991, 1993; Roche 1991; Kittel 1991. See also the extensive bibliography of English literature translated into German via its French translations (Graeber and Roche 1988).

2. The 'German' period in Hebrew literature

Even to the uninitiated forerunners of the *Haskala*, the Hebrew Enlightenment movement, it was clear that there was virtually no chance of catching up with the modern world without investing major efforts in translating as a convenient means of trying their hand and tools on things already marked 'literary' (hence worthy of treatment) by virtue of their association with another, preferably prestigious literature. However, right from the start a distressing tension emerged between this recognized need for imported goods and the factual inability of Hebrew literature to express with its own meagre means everything that had been – let alone could have been – formulated elsewhere.

It was on the *ideological* plane that efforts were made to alleviate this tension. The projected solution consisted of an ingenious reversal of a medieval practice which was still very much in use in pre-modern Hebrew literature: the customary apologetic stance involving an exaggeration of inabilities was supplanted by a conscious effort to enhance the prestige of Hebrew as a vehicle for translated texts, even if this involved false arguments. This strategy was adopted as far back as 1755–56, by Moses Mendelssohn himself, in *Kohelet Musar* (literally, 'Preacher of Morals'), the very first periodical of the Hebrew Enlightenment: of the two issues which managed to see the light of day, a whole *one eighth* was devoted to a gradual unfolding of the bold argument that, whereas "words of wisdom" were hardly translatable into Hebrew, that language could hardly be rivalled when it came to *literary* translation. By harping relentlessly on the ability of Hebrew to do precisely that which held so many difficulties in store, the proponents of this stance succeeded in creating a very favourable ideological climate. This, in turn, made it possible to pursue the program of the Enlightenment movement and achieve its goals, despite the initial dearth of resources and the sizable losses along the way.

2.1. *The concept of translation*

This ideological solution however was supplemented by another, very congruous move of far-reaching consequences: **positing acceptability as a major constraint on literary translation, to the almost complete forfeiture of translation adequacy**; a kind of Hebraic *belle infidèle*, if you wish. This move, which no doubt contributed enormously to mitigating the problematics of translation into Hebrew, might have drawn on the example set by the neighbouring literatures, even though their own concept of translation had already undergone radical change by then.

As a rule, the norms which governed acceptability in Hebrew were a vestige of former historical phases. Indeed, being so very slow in picking up changes, these norms were most appropriate for another facet of their task (in addition to the importation of ready-made texts and models); namely, to **protect Hebrew literature from inundation by foreign waves**, in face of the huge volume of imported goods.

The most comprehensive of these protective norms, and the one which also had the most far-reaching implications for the position of translation in Hebrew culture — coupled with a tolerance of indirect translation, concerned the *linguistic model* within which the translator was obliged to manoeuvre. This model was very narrow indeed. It made available for use only a fairly small portion of the initially accessible resources, namely, the language recorded in the Old Testament. In fact, the Bible was conceived of not merely as a source of *matrices* to be filled with whatever material, but also as a reservoir of actual *forms*, to be used as more or less fixed expressions. Consequently, rather long and complex items were conceived of as most appropriate translational ‘solutions’ *in principle*; i.e., prior to the establishment of any definite ‘problem’. A set of linguistic-textual items and patterns was thus more or less sealed according to purely target-culture interests and — owing to the high prestige ascribed to it — was presented as unconditionally applicable to whatever problem arose. On top of all this, the preferable mode of usage was to take rather long expressions, torn from their original contexts, and to form still longer word chains by way of concatenation. This norm obviously served to further narrow down the translators’ options — which accounts for the high *uniformity* of texts produced throughout the period.³

Literary translation into Hebrew during the Enlightenment, and for a long time to come, was thus a unique blend of *primary* activities on the generic, thematic and compositional planes, where innovations were indeed sought and allowed to penetrate into the very heart of the system, and *secondary* ones on most other planes, which were therefore highly resistant to change.⁴ As a result, **the borderline between original writing and translation tended to be rather obscure**; not, of course, in terms of the processes whereby the texts were generated, which were known perfectly well, but primarily in *functional* terms,

3. And see what Lotman (1976) has to say about the “recoding of a text with a large alphabet into a text with a small one”.

4. For the notions of ‘primary’ vs. ‘secondary’ types within a cultural system see Even-Zohar 1990: 20–22.

on account of the respective positions of the ensuing texts in the target culture. Indeed, it was not even necessary to mark a text as being translated. Moreover, even when a text was presented as non-original, the common practice was to attribute it first and foremost, sometimes even exclusively, to its translator, who was thus conceived of as being virtually on a par with the author of an original. Often, the fact of the translation, or the name of the source-text author, was mentioned only in passing: in short formulas, in very fine print, in parentheses or a footnote, or in the table of contents alone. Not infrequently, even if the name of the original *author* was given, the *text* which had actually been translated was often not specified. Finally, translations which were in fact *fragmentary* were often presented as being complete, sometimes even being rounded off to better achieve that end.

2.2. *The symptomatic status of indirect translation*

As implied by the non-markedness of translations, translating was regarded mainly as a convenient technique for introducing entities into a culture which so desperately needed them. Under such circumstances, **tolerance of indirect translation** seems almost self-explanatory. After all, for the mere application of translation procedures on the way to a Hebrew end product, one text is as good as any other. On the other hand, a text which was conceived of as appropriate from the point of view of the *target* literature (e.g., as a potential fill-in for a certain gap) could well have been encountered in translated form, to begin with. In fact, a translation tended to be selected for translation into Hebrew precisely as any other text would; namely, on the basis of its position in the *mediating* system, with no regard for the position of its own original in the source literature. Consequently, what was nominally second-hand translation, was functionally – that is, in terms of the structure of the target culture and the prospective position of that text within it – tantamount to first-hand translation.

Obviously, a literature which pays such little heed to adequacy in its own concept of translation is hardly apt to bother with the possible non-adequacy of the *mediating* texts as translations. And, indeed, Hebrew literature of the Enlightenment period, and for a long time thereafter, never stopped to ponder about this point. Its overall tolerance of mediated translation was reflected in an abundance of second-hand translations, which practically dominated certain domains, in terms of quantity as well as quality. It is no less clear that its tolerance of – and actual recourse to – this type of activity were bound to diminish as the concept of translation changed, and in direct proportion to a growing emphasis on the reconstruction of the source-text features.

It is evident, then, that the mode and extent of using indirect translation, along with the changes that may have occurred in them, can be viewed as symptomatic of the constellation of Hebrew literature; and since second-hand translation always involves more than just two systems, it can clearly be taken as at least a clue to the position of one literature in relation to other languages/literatures, acting as mediating and mediated, respectively.

Unfortunately, there is an inherent difficulty here, in terms of *corpus construction*: the more significant the role of intermediate translation, the more severe the problems involved in establishing the body of texts that should be taken into account.

To begin with, one of the implications of the obscured borderline between 'originals' and 'non-originals' has been the fact that many translations, mediated as well as direct, still defy identification; especially those found in the periodicals of the earliest period, which played such a decisive role in shaping Hebrew literature. To compound the problem, some of the names given there are mistaken.

Secondly, even if a text is identified as a translation — or even is presented as one — establishing the fact that it is *mediated*, let alone the positive identification of the mediating language and text, is no less problematic. This is due mainly to the considerable uniformity of the translations in terms of their linguistic make-up (see Section 2.1). Moreover, due to the overall dominance of a norm which yielded the establishment of correspondence between target- and source-language segments at the phrase, clause, or even sentence level, very few traces of *negative transfer* can be detected in the Hebrew texts, which might have served not only as evidence of the fact of mediation as such, but also as a clue to the actual mediating language and text.

In many cases, the only practicable way out would be to contextualize the individual figures involved in the act — translators, writers, editors, and the like: where they lived, what kind of education they had, which languages can be ruled out for such persons under such circumstances, how likely they were to encounter certain texts rather than others in their immediate vicinity, and so on. On the basis of information of this kind, texts which in all likelihood could *not* have been translated from the ultimate original would be tentatively marked 'mediated translations'. The next step, establishing their immediate sources, will often be only *probabilistic*. It will be based on instances where both translations not only deviate from their allegedly common source, but also reveal certain inter-correspondences, which would be difficult to interpret as anything but dependencies. What is likely to further complicate matters is the possible existence of *compilative* translations where several intermediate translations were used, into one language or several, alternately or together, or even a combination of the ultimate original and translation(s) thereof.

2.3. *The role of German culture as a supplier*

Since it was in Germany that Hebrew Enlightenment made its debut, it is no wonder that German literature was the one which was called upon to act as a main supplier of both texts and models. However, rather than turning to its model-literature in its contemporary state, Hebrew literature usually referred to previous phases of its development, and picked out items which had become canonized to a certain extent. Indeed, many of the texts selected for translation had once occupied a position near the centre of the German system, but most of them had since been relegated to a more peripheral position.⁵

Superficially, such an attitude could be described as merely 'playing it safe'; i.e., as a way of averting any doubts regarding the appropriateness of the models and norms realized in the texts, and hence likely to be introduced into the recipient culture. This might have become a crucial issue, had the replica-literature (to extend one of Weinreich's [1953] terms) resorted to a current state of the model-system, marked as it is by competition and constant flux. Be that as it may, this attitude was, in fact, very symptomatic of the epigonic position that Hebrew literature assumed during the Enlightenment period precisely with respect to its German counterpart. The slightly peripheral position of the imported materials was thus not only seen as non-detrimental, but actually marked as positive.

Whatever material was picked for transference therefore had to pass the test of **proven recognition in and by German culture**. By contrast, no bond evolved between the appropriateness of literary materials as candidates for importation into the Hebrew system and their ultimate origin. And, indeed, through close unilateral contacts, an abundance of materials of *non*-German origin were also introduced into Hebrew culture, albeit in their German version, which – due to the norms that had prevailed in the mediating system when the translations were made – was often quite different from the original one. There was of course no question of having to submit those texts and models to the test of belonging in German literature, since their being part of it was at the very root of their selection; and as for those which had actually undergone canonization in German culture, they obviously stood the ultimate test of proven recognition as well. To be sure, even in those rare cases where a text was translated from the non-German original, it was often the position of its *German* translation(s) in German culture which was crucial at least for its selection for translation.

5. A typical case in point would be Christian Fürchtegott Gellert's role in the creation of the Hebrew Enlightenment fable, probably due to massive inclusion of his texts in later readers and anthologies (see Toury 1993).

2.4. Translating English literature via German

One of the main literatures whose very presentation to the readers of Hebrew in the 18th–19th centuries was a function of their position vis-à-vis German culture was English literature. Indeed, had elements of English literature not played a significant role in shaping the *German* system of the period in question, and previous ones, and had German not played such a decisive role in establishing modern *Hebrew* literature, English literature may well have had to wait several more generations until it could finally start penetrating the Hebrew system. In that case, the entire web of their relationships might have been completely different.

Interestingly enough, the very first literary translation of the Enlightenment period is already as symptomatic as can be of the role of German culture as a mediator between English and Hebrew. Thus, Mendelssohn's argumentation in *Kohelet Musar* as to the appropriateness of Hebrew as vehicle of literary translation was supplemented by a translated excerpt, offered as an illustration of the bold but unfounded statement that, when it came to this field, Hebrew could hardly be rivalled.

At first sight, the choice of text seems rather odd, because the excerpt was not taken from a German work, as could have been expected. However, the choice was not as unlikely as it would appear. Even though the text selected for translation was English in origin, its selection had very little to do with its English-ness (referred to implicitly only, by stating that the text originated in "the island"). It had even less to do with the position of English vis-à-vis Hebrew literature, a position which, for that time, can only be described as non-existent (see *infra*). By contrast, it had everything in the world to do with *German* literature, even with a particular German *text*, precisely as could – and should – have been expected, given the circumstances. For the Hebrew translation that featured in *Kohelet Musar* comprised the first 66 lines of Edward Young's *The Complaint, or Night Thoughts on Life, Death and Immortality*.

As is well known, the German scene of the mid-18th century was marked by a strong 'Youngian' vogue, and – for a while – translations of Young's works were no less central to German literature than were the originals to English literature (see e.g. Price 1953: 113–116). This holds particularly for *Night Thoughts*, most notably in Johann Arnold Ebert's prose translation of 1752, *Nachtgedanken über Leben, Tod, und Unsterblichkeit*. It turns out, then, that Mendelssohn – in his role as a forerunner of the Hebrew *Haskala* – acted as a typical representative of the *German* literary milieu of the period, which was already moving away from its own *Aufklärungszeitalter*.

Moreover, Mendelssohn is known to have shown a keen interest in the problems of translating works by writers like Young into German. Thus, at about the same time, he published in the important periodical of literary criticism, *Bibliothek der schönen Wissenschaften und der freyen Künste* (1757), a set of general principles for the rendering of such

texts into German. He took great pains to subscribe to these principles while translating the passage into Hebrew too (see Gilon 1979: 90-91), even though they were hardly meant for that language. In addition, there is ample internal evidence — on the *matricial* as well as *textual-linguistic* levels — that Ebert's German version itself was at least closely consulted during the translation of the opening lines into Hebrew.

And this is what really makes such an out-of-the-way translation so significant. After all, Mendelssohn could well have prepared the Hebrew version directly from the English original, as he did when it came to translating English texts into *German*. His decision to pick a canonized translation into German as an ancillary, if not exclusive source for his Hebrew translation no doubt attests to his position as part of that emerging culture, which induced very different behavioural patterns than did his affinity with the German literary milieu. Nor would this remain an isolated incident in the history of literary translation into Hebrew.

All this is not to say that there were no first-hand translations of English works as early as the middle of the 18th century. After all, literature in Hebrew was also produced in Britain itself.

The need for such a literature arose first and foremost from the fact that, for the Jews who then gathered on the island after their readmission onto it and set out to form a community — a unique blend of Sephardi immigrants from Holland and Italy together with Ashkenazi immigrants from Germany and Central Europe — Hebrew was the only linguistic common denominator. However, Hebrew literature emanating from Britain was never anything but peripheral, and its texts were mostly confined to limited circles in Britain itself.

British Hebrew literature also included a small number of translations (see e.g. Schirmann 1967), most of which were soon to be made directly from the English originals, and at any rate on the basis of their positions in their home, rather than in any other culture. The norms according to which these translations were performed were even closer to medieval practices than were those which came to dominate the continental scene, and — inasmuch as they were published at all — their integration into Hebrew literature as a whole, let alone their influence on its evolution, were virtually nil. In fact, the only British-Hebrew translations that ever got anywhere near the center were those initiated by it, to start with; most notably Eduard Salkinsohn's translations of *Othello* (1874) and *Romeo and Juliet* (1879). Indeed, though nominally British, those texts were part and parcel of the *continental* mainstream: they were solicited by Peretz Smolenskin, one of the major literary figures of the period, who had been living in Vienna since 1868, prepared by a Russian Jew who converted to

Christianity and spent part of his life in England, and sold, read and reviewed mainly in Central and Eastern Europe (see Cohen 1942). In any event, even this did not happen until the centre of Hebrew literature had already moved eastward (see Section 3), by which time British-Hebrew literature as such was close to extinction.

Obviously, then, mediated translations of English texts played a much greater role in the evolution of Hebrew literature, even though only very few of them contributed to the shaping of its center, or that of any of its sectors.⁶ As a rule, the position of English vis-à-vis Hebrew literature – even when mediated – grew more and more marginal. In this respect, as well as with respect to the function of German as a mediating system, an important case in point was no doubt the failure to introduce Shakespeare's writings into Hebrew literature in any significant way, in spite of the primary position that many of his translations enjoyed in the mediating systems – German, and later on Russian.⁷

As shown by Almagor in his 1975 bibliographical survey (see Chapter 6), by the beginning of the 19th century, the Hebrew cultural milieu had come to regard Shakespeare as a major figure of world literature. And yet, this appreciation seems to have been rather superficial; a kind of lip-service to its wish to be like all 'modern' cultures. Thus, for many decades his position vis-à-vis Hebrew literature remained very marginal and his influence on its development virtually nil.

It would be recalled that Shakespeare's first translations into Hebrew, and those which followed over a period of almost sixty years, were limited to *fragments* – monologues, or other short passages, from his tragedies only – and published with great irregularity. Due to their fragmentariness, these texts were treated (by the translators) and accepted (by the readers) as instances of *poetry* rather than drama, which hardly existed in Hebrew anyway. Some of them were not even given independent status but were interpolated in articles or stories by Hebrew writers. Most 19th-century translations of Shakespeare into Hebrew were made by minor, if not completely obscure figures, absolutely none of whom won any fame or prestige through these translations. Also,

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6. An interesting case in point is *Robinson der Jüngere*, Campe's version of *Robinson Crusoe*, which has had a long-standing effect on Hebrew literature, especially for children and young people. See Ofek (1979: 87-97), and especially Z. Shavit (1986: 146-157).
 7. The literature on Shakespeare's position in various European literatures of the 18th and 19th centuries is vast, and there is no way one could do justice to it. The most recent publication is Delabastita and D'hulst 1993. Being a collection of articles on Shakespeare's translations in the Romantic Age, the book also includes invaluable references to many previous studies.

they were mostly published in rather marginal periodicals. Small wonder, then, that the great majority of the few translations which were indeed published went completely unnoticed.

All in all, from 1816 (when the first known translation of a Shakespearean passage was published) until 1874 (when the first play was translated in its entirety), we have as few as 200 identified lines in published sources, 101(!) of them drawn from three different translations of Hamlet's monologue "To Be or Not To Be";⁸ and every single one of these translations is suspect of having been mediated, mostly by German. In most cases, this suspicion is not hard to confirm either. Thus, while German literature undoubtedly played a vital role in introducing Shakespeare to Hebrew literature (rather than to the Hebrew reader, who grew increasingly adept at reading his works in translations into other languages), it was evidently less than successful — clear evidence of the peripherality of Shakespeare vis-à-vis Hebrew literature until well into the 20th century; not merely in itself, but also as the epitome of English literature.

3. Moving into the revival period

Ever since the beginning of the Enlightenment period, the centre of Hebrew culture (and literature) had been moving steadily eastward. Even within the

8. The list runs as follows:

- 1816: fifteen lines from a monologue from the peripheral play *Second Part of King Henry IV*, used as an illustration of the apostrophe in Shlomo Löwisohn's *Melitsat Yeshurun*, the first Hebrew poetics of the Bible;
- 1842: a first translation of Hamlet's monologue by Fabius Mieses. Remained unpublished until 1891, when it was included in Mieses's book of poetry, possibly in a revised version, along with a four-line epigram based on Hamlet's song to Ophelia, which may have also been made much earlier.
- 1856: the first published translation of the Hamlet's monologue. Made by Naphtali Poper Krassensohn and published in *Kochbe Jizchak* ('Isac's Stars');
- 1862: a translation by Simon Bacher of "Grablied eines Landmanns", Herder's version of a song from *Cymbeline*. Published in *Kochbe Jizchak*.
- 1868: four lines of one of Macbeth's monologues, cited in an article by Jehoshua Steinberg and published in the first Hebrew weekly periodical in Lithuania, *Ha-karmel*;
- 1871: a monologue from *Macbeth* under the title of "The Doorman", translated by S. Medliger [?] and published in the ephemeral periodical *Ha-et* ('The Time');
- 1872: four excerpts of *Hamlet*, embedded in a story by Peretz Smolenskin, including the "To Be or Not To Be" monologue.

German *Kulturkreis* itself it did not take long for this centre to shift from Berlin to the Austro-Hungarian Empire – first to Vienna, then further to the east, and finally into the Slavic cultural domain. This gradual shift, which brought Hebrew literature into contact with varying European literatures, had two complementary effects: against the background of these everchanging literatures, new gaps were constantly encountered, and, at the same time, various options for filling them also presented themselves, to be appropriated or rejected according to its own interests.

3.1. *The 'Russification' of Hebrew literature*

The gaps now encountered in Hebrew literature were no longer confined to generic, thematic and compositional models. They manifested themselves at the *linguistic* level as well, which, in view of the new tasks, could no longer be regarded as adequate, not even by way of 'wishful thinking'. For instance, not being spoken, on the one hand, and lacking in appropriate literary traditions, on the other, Hebrew completely lacked void pragmatic connectors, the use of which had become almost mandatory in certain types of modern fiction which Hebrew literature accepted as a model (see Even-Zohar 1981; 1982). In addition, it soon became clear that a good portion of the available options, including many of those which had crystallized during the Enlightenment period, could not serve the new purposes and had to be rejected. And since, as of the 1820s, Russian gradually became the closest available system, it is no wonder that this was the literature which would present Hebrew with its new challenges while also providing most of the options for meeting them. Needless to say, Russian literature also became the main supplier of texts for translation.

In fact, the behaviour of Hebrew literature in relation to Russian literature in that period, which has come to be known in Hebrew historiography as the 'Revival Period', involved much more than a simple recognition of its initial availability. In fact, one could say that it behaved as if the Russian system was *a part of it* (see Chapter 8, Section 5). For this was a case not of mere 'influence', but of evergrowing *dependency*. Indeed, Russian – both directly and via Eastern Yiddish, which was then rapidly becoming a literary language in itself and which was also, to a great and growing extent, modelled on the Russian example – played a major role in the very structuring of Hebrew literature, which was now demonstrating systemic weakness in so many respects – much more so, and with much greater, and longer-lasting effects than had German in the previous (and much shorter) period of the Enlightenment.

Consequently, the new paradigm which took shape in Hebrew literature,

especially as of the 1860s, when the dependency relationships had already been established, gradually replaced the previous one and was to dominate Hebrew culture for many generations; and even though, on the face of it, Hebrew linguistic purism was still strongly preached, the underlying model, applied to both original writing and translation, regardless of source language, was in fact highly *Russified*.⁹ The borderline between originals and non-originals was still obscure, then, but its obscurity involved a reversal of cause and effect: whereas in the 'German' period it stemmed from the translations' pretending to be original writings, it was now the original texts that were, to a great extent, molded according to a translational model.

It is easily understandable how, under such circumstances, indirect translation was still tolerated for a considerable part of the Revival Period. In fact, for a long time, the only real change was that Russian was picked up as another mediating agent, soon to encroach on the territory occupied by German and ultimately – towards the end of the 19th century – to supersede it altogether.

3.2. *The position of German and English*

The fact that German, in spite of its increasing marginality, was still being used as a mediator testifies to its having retained much of its privileged status, probably due to the similar position it then enjoyed vis-à-vis *Russian* literature. This status is further attested to in a negative manner: in contrast to other literatures, mediated translations of German texts seem to have been an extreme rarity. Rather than reflecting a mere acquaintance with German as a language, the overall regularity of the non-occurrence of second-hand translations of German literary works in a period of massive recourse to this mode of text-generating activity should be taken as an indication of non-tolerance of mediation with regard to this particular literature.

All this is not to say that the position of German vis-à-vis Hebrew culture was not in any way secondary, or indeed, subordinated to its position in relation to Russian. Thus, in the significant changes which occurred in the selection of German authors and texts for translation, the role of Russian literature seems to have been decisive. I would hypothesize, for example, that the enormous popularity of *Heine* among Hebrew translators, which did not start until the end of the 19th century (see Lachover 1956–1957), should be attributed neither to

9. And see Even-Zohar's recent attempts to characterize various aspects of the Russification of Hebrew literary models, most notably his studies on Hebrew void pragmatic connectors (e.g. 1981; 1982) and literary dialogue (1985).

his German origin nor his Jewishness. (In fact, Heine's conversion to Christianity may well have *hindered* his translation into Hebrew.) It should rather be ascribed to the boom in his acceptance into *Russian* literature, in terms of both numbers and place in the system (see Ritz 1981 and Bar-Yosef 1992). The need that now arose in Hebrew literature for new kinds of poetry, especially nature and love poems (Z. Shavit 1976), probably acted as a contributing factor, but this need too was prompted by Russian literature, mainly of former periods. Finally, the translation of German *texts* was sometimes mediated by Russian *models*. (And see the account of Bialik's translation of a *Schlaraffenland* text in Chapter 8.) In these respects, then, but not in the purely textual-linguistic domain, Russian did act as a mediating agent for the introduction of German items into the Hebrew system too during a substantial part of the Revival Period.

By contrast, the position of English literature in relation to Hebrew, on the one hand, and Russian and German, on the other, remained peripheral throughout. Thus, the number of English texts – let alone English-based models – introduced into Hebrew literature until well into the 20th century was negligible, and most of the texts which did find their way into it (including a great majority of Shakespeare's works, where a gradual increase in numbers finally occurred as of the 1870s) went on being mediated, by either German or Russian. In fact, it was only towards the end of the Revival Period, with the transition of the center of Hebrew literature to Eretz Yisrael (Palestine) and the physical abolition of Hebrew activity in the USSR, that the number of direct translations from English grew in any considerable way, until, by the end of the 1920s, English literature had become a major contributor to Hebrew letters, both in pure numbers (Toury 1977: 113–117) and in terms of its entry into previously unoccupied 'slots' in the system, most notably certain sectors of light fiction (Toury 1977: 117–118).

The main contributing factors for that growth were the following:

- the British mandate over Palestine, which made English an official language of the country and rendered it the first foreign language studied in its schools;
- the growing centrality of English vis-à-vis most western cultures;
- the establishment of a secondary cultural center in the United States, due to massive immigration of East European Jews – a centre which furnished the primary one first with information and texts (both original and translated), then with a certain demographic reinforcement of people immersed in Anglo-American culture;

- and, finally, the changes which the concept of translation itself had been undergoing towards an evergrowing emphasis on adequacy, which inevitably involved a concomitant reduction of tolerance for indirect translation as a whole.

3.3. *The Russified model and translation from other languages*

Yet, translations from languages other than Russian often looked as if they had actually been mediated by that language, without however being relatable to any particular *text* in it. This was the ultimate result of the dominance of the Russified model, especially in its second phase of crystallization, effected in Palestine, by writers with hardly any direct access to Russian literature. Most of them could not even read the language and were acquainted only with those Russian models that had been introduced into Hebrew literature and had become domesticated in it. In fact, a special kind of 'Hebrew for Russian-like texts' emerged, and came to be regarded as almost obligatory, for original writing as well as translations from all languages (see Even-Zohar 1978b: 71–73).

The ensuing uniformity again gives rise to certain problems of identification, especially towards the end of the period. Thus, when one encounters translations from the first third of the 20th century, it is often difficult to determine whether they were textually mediated at all (and if so – whether the mediating text was in Russian, Yiddish or German), or whether they only show traces of second-order interference (for which see Toury 1980a: 77–78).

An interesting case in point is the 1932 translation of Joseph Conrad's *Freya of the Seven Isles* by Yitshak Lamdan, where the entire set of void pragmatic connectors (e.g., the replacements of 'oh') is Russian-like. In addition, a very intricate system of pronominal representation of 'familiarity' vs. 'respect' was introduced into the translation, which is both non-Hebrew and without linguistic basis in the English original either, though it would have been totally appropriate for a Russian or a German (or even a Yiddish) translation of the book.

This observation notwithstanding, Lamdan could hardly have used a Russian mediating text for the simple reason that none existed, at that time (see Ehrsam 1969: 342). While a German translation of *Freya* was available as early as 1929, I have my doubts as to whether the Hebrew translator relied on it in any significant way. Interestingly enough, the use of the pronoun system in the German version is much less elaborate than Lamdan's...

4. The anglicization of Hebrew literature

Since the 1930s–1940s a struggle for domination was to ensue in Hebrew literature between the Russified model, which had risen to native-like status, and newly introduced Anglo-American norms. It was finally settled in favour of the latter. To the extent that indirect translation was still practiced, English now became a main, if not sole mediating agent too. In certain domains and in certain ways, the introduction of *German* texts into Hebrew literature also became mediated by it – a unique reversal of roles, highly indicative of the change that the Hebrew system had undergone in relation to these two literatures. Let us take a brief look at how this happened.

Between the 1930s and the 1960s translation of German literature came to a virtual standstill, as an unofficial censorial reaction to the horrors of the Nazi regime. In order for a German text to overcome the ban, it had to be an old-time classic, or else its author had to be a Jew, or an avowed anti-Nazi, preferably one who had spent the crucial years in exile (Toury 1977: 118–120). The ban was gradually lifted in the late 1960s, but direct contacts between Hebrew and German literature have hardly been resumed, particularly when it comes to the selection of texts for translation. In fact, very few German authors and texts were picked for translation into Hebrew before having gained fame and success in the English-speaking world, mainly in the US.

For instance, the decision, in the early 1970s, to revise the translations of two of Hermann Hesse's novels, *Narziß und Goldmund* and *Peter Camenzind*, prepared (from the German originals) in 1932 and 1941, respectively, and to prepare translations of several of his other books, was brought about almost exclusively by his adoption by American 'flower children' of the late sixties (see e.g. Schwarz 1970).¹⁰ In fact, insofar as it related to the literary aspect of the hippie movement, Hebrew culture confined itself to the outer trappings, making no recourse to the roots and accompanying characteristics.

Contemporary German writers such as Günter Grass and Heinrich Böll were still introduced to the Hebrew reader following their success in the US, and on the

10. Paperback copies sold in the US in 1969: *Siddhartha* – more than 270,000; *Narcissus and Goldmund* – 125,000; *Journey to the East* – 102,000; *Beneath the Wheel* – 50,000. *Siddhartha* had one million copies in print by 1969. (*The Publisher's Weekly*, 9.2.1970, as quoted in Davis 1984: 337–341. In fact, Davis [1984: 392] goes so far as to include *Siddhartha* among his (admittedly personal) "Fifty Paperbacks that Changed America".)

basis of this success. This trend only began changing in the eighties, a rather hesitant but highly significant change.

Several translations of German texts were also made via their English translations, even though mainly in the periphery; namely, in children's literature and certain sections of popular writing.¹¹

A few examples of mediated translations of children's books (in parentheses — the dates of Hebrew translation): two of Lisa Tetzner's books (1956 and 1958, respectively); four books by Friedrich Feld (Rosenfeld) (1967–1968); *14 höllenschwarze Kisten* by Gina Ruck-Pauquet (1974). A number of Walt Disney Studios' adaptations of German texts (e.g., Felix Salten's *Bambi* [1968] and Johann Wyss's *Der schweizerische Robinson* [1976]) comprise a distinct group.

As for popular writing, one name that should not be omitted is that of Johannes Mario Simmel. Indeed, Simmel is one of very few writers of popular literature in German who have managed to penetrate the English-speaking world in any considerable way. At least seven of his books for adults were translated into Hebrew too, all of them — to the best of my knowledge — mediated by their English versions. In case the title of the English translation does not coincide with the original one, the Hebrew book often carries a translation of the English title. Moreover, Simmel's very *name* is always transliterated according to its English pronunciation, and this is how he came to be known to the Hebrew public: /sim^{el}/ rather than /zim^{el}/. Only recently have Israeli libraries started listing his books under the seventh letter of the Hebrew alphabet instead of under the fifteenth letter, but this has not yet led to any real change.

Even in these two sectors, however, the share of indirect translations is not only far from striking, but is constantly on the decline, despite a considerable increase in the translation of German literature into Hebrew over the last twenty-odd years, and despite the constant shortage of competent translators from that language — another verification of the claim that indirect translation is not an automatic function of the lack of mastery of the source language, and vice versa. Indeed, had the concept of translation, and with it the overall tolerance of second-hand translation, undergone no change, the situation in the 1970s may well have been a perfect mirror-image of the one which prevailed 200

11. Several revisions of older translations, including the two Hesse revisions mentioned earlier, were also made with no recourse to the German texts, probably with a certain dependence on the English translations. A plausible explanation seems to be that, in a revision, the use of a mediating text is much easier to conceal — a concealment which the current norm definitely favours.

years before. Paradoxically, due to that change of concept, which also resulted in recourse to short and/or low-rank textual-linguistic entities as translation units, those second-hand translations which did occur in spite of the growing intolerance of them have become much easier to pin down, and the mediating languages and texts simpler to identify.¹²

12. With regard to the translation of German texts, a further complication arises: due to the lack of a suitable German-Hebrew dictionary, in terms of both volume and quality, many translators resort to a combination of German-English and English-Hebrew dictionaries. This practice gives birth to a kind of mediated 'islands' in translations which, as a whole, were made directly from the originals.

Chapter 8

Literary Organization and Translation Strategies A Text is Sifted Through a Mediating Model

This chapter presents, in considerable detail, the case of a short German tale for children translated into Hebrew in the early 1920s, transferred into and ultimately accepted by its own literature for children – an extreme yet representative example of the behaviour of one culture in one of its evolutionary phases vis-à-vis a single text from another language/literature.

On the face of it, confining such a study to just one pair of texts may seem a rather modest aspiration. The venture, however, is actually far more complex – and of far more general implications. In what follows, an attempt will be made to demonstrate, by introducing a constantly growing element of contextualization:

- (a) that decisions made by an individual translator while translating a single text are far from erratic. Rather, even though by no means all-embracing, they tend to be highly *patterned*;
- (b) that the observed regularities of behaviour can be attributed to some *governing principles*;
- (c) that the strongest of these principles originate in the *target* system itself, the one where – semiotically speaking – the act of translation is initiated and whose needs it is designed to satisfy; and, finally,
- (d) that those principles, and hence the behaviour induced by them, reflect an underlying *network of relationships* which, in our case, constitute a particular realization of the concept of literature and define translation as part of it; in other words, that – even though seemingly quite possible indeed – any other translational strategy would have been way out of line, and hence much more surprising.

The text we'll be handling is "Gan-Eden ha-taxton" [literally: "Lower Paradise"], a Hebrew translation of a version of a German fairy tale entitled "Das Schlaraffenland" (see Appendix).

The source text forms part of a small book called *Kleine Märchen, Nach H. Chr. Andersen, R. [sic] Bechstein u. den Brüdern Grimm*; ten fairy tales adapted for small children by the painter and illustrator Tom (originally: Martha Gertrud) Freud, the daughter of one of Sigmund Freud's sisters (see Murken 1981), and published in Ludwigsburg in 1921.¹ The whole collection was translated into Hebrew a few months later and was published at the end of 1922 as *Eser sixot li-ladim* [Ten Tales for Children]. It was mainly the fact that it was translated by no less a personage than Chaim Nahman Bialik (1873–1934), the so-called 'national poet' of modern Hebrew literature,² that lent the booklet whatever position it enjoyed in the recipient culture, rather than anything in or about the original, and "Gan-Eden ha-taxton" was the only one of the ten translations which gained a permanent position in Hebrew literature.

Freud's text is itself an adaptation of Ludwig Bechstein's "Das Märchen vom Schlaraffenland", one of the most popular texts in a long tradition of oral and written compositions on a legendary land which lies in a logically impossible place such as "drei Meil hinter Weihnachten" (see Richter 1984 and the many sources he quotes). Although several cultures have had similar traditions (e.g., under the name of *Coquaigne/Cokaygne*), some of them probably prior to its emergence in the German *Sprachraum* (e.g., Bolte and Polivka 1918: 244–258), it was in German culture that the *Schlaraffenland* tradition seems to have struck root. Texts which belong to this tradition tend to be *pseudo-narrative*, if not completely *non-narrative* (Jason 1971: 43–44), amounting as they do to open-ended series of exaggerations and lies based on an inversion of reality.

In her adaptation, Freud resorted mainly to omissions. In fact, being less than a quarter of Bechstein's in length, her tale constitutes an outright concatenation of sentences and phrases torn out of her immediate source, made possible by the basically *paratactic* structure of a *Schlaraffenland* text. These large-scale omissions are supplemented by one minor change of order and very few, mostly inconsequential linguistic modifications.

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1. A few years later, the book was banned by the Nazis, and there seem to be very few copies of it left. I wish to thank Mrs. Awiwa Harari (born: Angela Seidmann) of Ramat ha-Sharon (Israel), Tom Freud's only daughter, for having made a copy of this rare book available to me.
 2. The most detailed account of Bialik's involvement with Tom Freud and her husband, Jakob Seidmann, and of Ophir, the publishing house established by the three of them in 1922 for the publication of children's books in Hebrew, is offered by Ofek (1984: 77–86).

It is the need to come up with a much shorter text, cut to the measure of a certain book format, which supplies the main rationale for the massive omissions. This need itself easily ties in with the prevalent image of the intended audience of Freud's tales: very small children and their parents.³ The same holds true for many of the individual omissions — items which were hardly admissible in a German text for such an audience at that point in time.⁴ What is most noteworthy, however, is precisely that the result of these operations is not merely a fully self-contained text, but one whose 'well-formedness' is governed by the same principles as those of its immediate source. After all, being pseudo-narrative in nature, it is the *existence* of a succession of lies and exaggerations of a certain type that characterizes these texts, rather than any of the individual links, let alone their exact position in the chain.

1. Added rhymes and verbal formulation

To be sure, "Das Schlaraffenland" is already a glaring exception in Tom Freud's book. Unlike the remaining texts, it is simply no *Märchen*. Deviance of the same order is represented by the Hebrew translation. However, within *Eser sixot li-ludim*, "Gan-Eden ha-taxton" is an exception in yet another respect: it is the only piece couched in rhymed prose,⁵ which neither Freud's version nor its own immediate source were.

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3. For the effects of the existence of two different audiences on the formulation of texts for children see Z. Shavit (1986).
 4. For instance, Bechstein's version, but not Freud's, includes the possibility that, in *Schlaraffenland*, husbands whose wives have grown old and ugly can replace them by young and beautiful wives — "und bekommen noch ein Draufgeld". Coupled with a well-defined conception of the *function* of literary texts for the intended age-group, which had a strong *didactic* element to it, the image of the audience can also be taken to explain the one and only significant *linguistic* change, which is hardly in line with the *Schlaraffenland* tradition: the replacement of *essen* in Bechstein's list of occupations favoured by the loafers of the legendary land (*schlafen, essen, trinken, tanzen und spielen*) by *lesen*.
 5. The following is the rhyme-scheme of the Hebrew text: aa bb cc dd ee ff | aa bb cc ddd ee ff gggg hh ii jj kk | aa bb cc dd ee | aa bb | aa bb cc dd eee(e)ee | aaa 0. A numerical representation of the realization of the various rhememes would look as follows: 2 2 2 2 2 2 | 2 2 2 3 2 2 4 2 2 2 2 | 2 2 2 2 2 | 2 2 2 2 5(6) | 3 0. Thus, even though a pair of lines was clearly dominant as a rhyming unit, its use was by no means obligatory. The rhyming segments reveal no other metric regularities, which is why the mode has been presented as 'rhymed prose'.

True enough, the added rhymes have not been made conspicuous in any way, so that, to the cursory eye, "Gan-Eden ha-taxton" looks like any other tale in the book. There can be no doubt, however, that rhyming does represent a central organizational principle of the Hebrew text, and hence a major constraint on its verbal formulation. There are in fact several instances where linguistic selections, and shifts from adequate reconstruction of the source text, cannot be explained on any other basis.

One prominent example should suffice:

In Freud's tale, the rain that falls in the topsy-turvy world consists of sweet drops of pure honey, "da kann einer lecken und schlecken, daß es eine Lust ist". What the Hebrew text has instead is significantly different: *kol ha-rotse pošet lašon ve-lokek, ve-še-eno rotse — rokek* [whoever wants to sticks out his tongue and licks, and whoever doesn't want (it) — spits it out]. Clearly the source text, in keeping with Bechstein and the whole *Schlaraffenland* tradition, could not conceive of the possibility that even one soul in the miraculous land would not enjoy the honey raindrops. The same does not hold true for the Hebrew translator: he proves willing to pay with a certain deviation from the internal logic of the original tall tale (which he does respect, in principle) for the establishment of a rhyme-partner for the word *lokek*, itself one of the most obvious replacements of German *lecken*. Of the (very few) potential rhyme-partners in Hebrew the word *rokek* [spits out] — a near-antonym — was selected, which is phonetically very close to *lokek*.

This decision, in turn, obliged Bialik to (re)formulate a whole two-verse segment so as to fit both the selected word — and the overall value-system which was taken over from the original. It is because of the latter that it was inconceivable, even for him, that all those licking the honey drops would react by spitting them out. To acquire its justification, the act of spitting had to be attributed to a *subgroup*, which would deviate from the crowd on some reasonable grounds. The near-antonymy of the (positively marked) licking⁶ and the (negatively marked) spitting was thus supplemented by the establishment of a syntactico-semantic opposition between *ha-rotse* [whoever wants] and *še-eno rotse* [whoever doesn't want], thus establishing a delicate balance between the two contending sources of constraint, the source text vs. the internal requirements of the gradually emerging translation. To be sure, the added element of free will is all but contradictory to the conventions of the *Schlaraffenland* tradition.

6. I wouldn't be at all surprised if the positive marking of licking itself, originating in the *source* text, was further enhanced by an existing tradition in the *target* culture, namely, the ultimate test that Gideon's men had to pass in order to qualify as fit to deliver Israel (Judges 7: 2-8). Needless to say, Bialik had this literary tradition in common at least with the *parents* who were intended to read *Eser sixot* aloud to their little children.

The series of questions arising at this point is obvious: why did Bialik choose to rhyme the translation of a non-rhymed text, in the first place; how come this strategy was applied to just one of the ten texts; and why did this one text happen to be “Gan-Eden ha-taxton”? Any attempt to answer these questions will be suspended until a couple of further phenomena have been presented, since there seems to be an overall, unifying explanation for them all.

2. Adding a [fictional] epic situation and tightening the overall structure

Thus, the decision to rhyme the text was not Bialik’s only gross deviation from the German original. Rather, he made several other decisions of global implications. One of the most salient occurs at the very end of the text, where “Gan-Eden ha-taxton” has the following three-segment unit:

*Et ašer šama(‘)ti oto asapera, ve-atem šalmu li bi-mzumanim mehera, Gera
šlema va-x(a)tsi ha-Gera*

[What I have heard I will tell, and as to you, hasten to pay me cash, one
*Gera*⁷ and half a *Gera*]

This ending is an appendix to the characterization of the legendary land, which forms the core of every *Schlaraffenland* text, i.e., an outright **epilogue**. What this textual segment boils down to is the establishment of a *fictional narrator*; a major deviation indeed, this time not only from the immediate source text, but also from its underlying model which yields pseudo-narratives. Moreover, the (oral) narrator thus established does not make do with the explicit, and framing announcement that he now got to the end of his story. He also demands – and very emphatically so – proper compensation for a job well done. This draws the added epilogue even further away from the source text and from the whole *Schlaraffenland* tradition.

The position of the narrator in the Hebrew version is further enhanced by a considerable amplification of the *opening* of the text. True, the German original does start with a few brief words addressed by an external speaker to some fictional audience. However, this laconic address – “Hört zu, ich will euch von ... sagen” – is immediately followed, and overshadowed, by the succession of lies

7. *Gera* – a small coin in Hebrew written tradition. When this text was composed, it had no particular value.

and exaggerations which duly makes up the bulk of the text, and the narrator is never referred to again. Bialik has a full-fledged, and rather long prologue instead:

Gan-Eden ha-taxton mi yode'a? — Gan-Eden ha-taxton ani yode'a. od ta(')mo omed be-fi u-sfatay, re'itiv ve-lo ezkor matay, ha-be-layla im be-yom, ha-be-hakits im ba-x(a)lom. iver ra'ahu ve-elav lo karav, kite'a ba bi-s'arav, gidem patax dlatav, ve-ilem siper li zot bi-sfatav. mi xereš yasur halom, yešev ve-yišma ve-yakšiv dom.

[Who knows Lower Paradise? — Lower Paradise I know. Its taste still lingers in my mouth and on my lips, I have seen it but I can't remember when, was it in night- or in day-time, was I awake or adreaming. A blind man saw it but never got near, a man with (an) amputated leg(s) entered its gates, a man with (an) amputated arm(s) opened its doors and a mute told me all about it with his (own) lips. You, deaf ones, come along, sit down, hark and listen silently.]

What the Hebrew text has acquired, as a result of the introduction of this prologue and the aforementioned epilogue, is a proper **epic situation** enveloping the description of 'Lower Paradise'. The latter now comprises hardly three quarters of the text, a crucial change indeed of its overall nature. The changed status of the narrator finds clear expression on the linguistic level too: Whereas in Freud's tale, the first person singular is used once only, the Hebrew version resorts to it *nine times*, six in the prologue (*ani, be-fi, sfatay, re'itiv, ezkor, li*) and three in the epilogue (*šama'ti, asapera, li*).

Finally, rather than marking the text's end, the epilogue is followed by the words *amen ve-amen*, 'so be it'. However, paradoxical as it may seem, this phrase reinforces rather than weakens the epilogue's status as the closing part of a story-external epic situation, for it represents the *audience's response* to the request made by the narrator at the end of his story.⁸ This ultimate ending is printed as a separate passage. It is also the only exception to the rhyme requirement; marks of special prominence, if there were any! It is almost as if, in lieu of the original pseudo-narrative, the Hebrew reader is offered a story about how such a pseudo-narrative is being told.

8. The origin of this closing formula is, of course, the Hebrew Psalms (41:14; 72:19; 89:53). Thus, its selection may also have been governed by a target-oriented constraint and intended to enhance the translation's acceptability, an issue that will be dealt with towards the end of this chapter.

Apart from this change, which – in ‘system-internal’ terms – would amount to a genre shift, the added epic situation, like the rhymes, also contributes to the elaboration and tightening of the text as a compositional unit. The Hebrew version simply has to comply with a greater number of ‘rules’. The tightening of the text’s structure is reinforced by yet another change introduced by the translator, namely, from the **paratactic** structure so characteristic of a bona fide *Schlaraffenland* tall tale to a **hypotactic** one. Thus, whereas the description of the miraculous land, which makes up the bulk of the German tale, moves from one item to another in a manner which is basically linear, the global organization of the Hebrew version consists of three layers encapsulating each other, as represented by Figure 10.

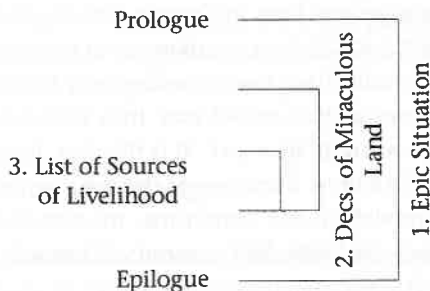


Figure 10. *The hypotactic structure of "Gan-Eden ha-taxton"*

Towards the establishment of this structure, the superimposition of a fictional epic situation as an external, all-embracing frame (1) was supplemented by rendering the list of sources of livelihood awaiting the loafers of 'Lower Paradise' as a structural component in its own right (3). Instead of being a mere link in a chain, equivalent in rank to all other details of the topsy-turvy world, the Hebrew list forms an inscription (*ktovet*) on a gate (*ša'ar*) in a wall (*xoma*), which is but one item of the intermediary 'box' (2). The subordinated status of the list in the structure of the target text is further emphasized by making explicit the way the inscription in 'box' (2) is (re)cited by the narrator, himself part of another, further removed structural layer, the external 'box' (1): *mila be-mila ve-ot be-ot* [word for word and character for character].

3. What was so wrong with the original model?

All the shifts we have encountered so far add up to an overall movement *away* from the model underlying the original. The intriguing question is, of course, why Bialik should have gone to all that trouble in the first place. Why could he not have left the original structure intact (which would have been a concomitant of translating Freud's version verbatim)? The approach he adopted is certainly at odds with Levy's 'minimax strategy' (1967: 1179), which Bialik himself seems to have adopted with respect to the rest of Freud's *Märchen*.

As we have already mentioned, "Das Schlaraffenland" was unique in the original book as well, being pseudo-narrative in nature. Tom Freud had no scruples about incorporating it in her collection, however, no doubt thanks to the deep roots of *Schlaraffenland* texts in German culture, including its literature for children. By contrast, such texts were unknown to modern Hebrew literature. In all likelihood, they would have been unacceptable to it too. A Hebrew text composed along the lines of this model may thus have been deemed at least 'non-literary', if not an outright 'non-text'. It is this fact, itself a reflection of the internal organization of Hebrew literature of the time, which probably led to a flat rejection of that model. At the same time, the text itself had to undergo translation, if only because Bialik had committed himself to the recoding of Freud's book as a whole.

However, there was a lot more to this necessity, given Bialik's preoccupation with the related theme of 'lost paradise'. It is this preoccupation — another, rather idiosyncratic aspect of the target-orientedness of translational considerations and decision making — which supplies the strongest explanation for the translator's willingness to spend so much time and effort on the reshuffling of "Das Schlaraffenland". It also accounts for several of Bialik's *local* decisions in the formulation of the tale, one of the most significant being to turn the miraculous land into a univocal children's world. This preoccupation also supplies a feasible explanation for the *title* of the Hebrew version. After all, and in spite of the subtitle of Ackermann's 1944 dissertation, fictitious *Schlaraffenland* was not identified in West European traditions with 'Earthly Paradise', which normally carried *religious* overtones (Jason 1971: 157–159). The association between the two was made against the background of the use of the Hebrew expression "Gan-Eden ha-taxton" in *Yiddish*, where it had been stripped of its original mystical connotations and made available for this kind of secular use.

The rejection of the *Schlaraffenland* model was probably further enhanced by its close association with **folklore** – an association which did not require much background knowledge – in view of the concept of ‘folklore’ entertained by Bialik and his contemporaries in the Hebrew culture; a belated (and epigonic) *romantic* concept, to be sure. Thus, to use Bialik’s own metaphor (quoted by Rawidowicz 1983: 102), folklore – even in its secondary, written form – was just “rags from a refuse dump”, “devoid of any value unless thrown into the cauldron of supreme reasoning and turned into new paper”. A precondition for the introduction of a text of folkloristic origin into literature was therefore its adjustment to the norms governing the formulation of *literary* texts, tantamount (in their view) to the imposition of ‘artistic organization’ on it. Now, whereas all the other *Märchen* in Freud’s collection already had, even according to Bialik’s conception, sufficient artistic organization (i.e., they pertained to acknowledged **literary models**), the series of exaggerations and lies constituting the *Schlaraffenland* text was most likely conceived as lacking it.

4. A mediating model and its origin

What should have become clear by now is that the structural changes introduced during the translation of “Das Schlaraffenland” into Hebrew were far from accidental. We also perceive a ‘negative’ motivation for the introduction of many of these changes, namely, the need to discard features whose retention was liable to seriously impair the target text’s acceptability as a literary text. However, a ‘positive’ question can also be raised here, namely, with respect to a possible regularity of the added and replacing elements themselves: Can any principle be shown to have governed their selection? To account for this question, let us give the epic situation and its two constitutive parts another look.

As a compositional element, the added epilogue readily ties up with folk literature: It is there that narrators not only receive, but actually demand remuneration for their performance (Jason 1971: 87), and it is there that formulas for fulfilling this need recur. When oral literature started being collected, transcribed and adapted to the requirements of written literature, these needs were transferred in it as well, often together with their habitualized linguistic realizations. This applies to our prologue too. In this case, however, the formulas which Bialik used make it possible to trace his selection to a particular repertoire, that of *Russian folklore* and its secondary, written form (probably the only one available to him anyway). In this respect, it would be most illuminating to

compare the last segments of the Hebrew prologue (quoted in Section 2) with a Russian prologue like the following one:

*Stepoj podgljadyvaet,
Gluxoj podslušivaet,
Benznogij vdogon pobežal,
Nemoj karaul zakričal.* (Sobolevskij 1902: 210–211)
[A blind man peeps, / a deaf man listens in, / a man with amputated leg(s)
chases, / a mute shouts 'help'.]

Obviously, there is nothing to rule out the possibility that the formulas comprising the epilogue were taken from the same repertoire, even though the link between them and their functions seem to be much less institutionalized, hence much less salient.⁹

True enough, the first impetus for adopting the Russian formulas as a model for the Hebrew wording may have been supplied by *Bechstein's* version of "Das Märchen vom Schlaraffenland", which has the following sentence (omitted in the adaptation):

Wer sich also auftun, und dorthin eine Reise machen will, aber den Weg nicht weiß, der frage einen Blinden; aber auch ein Stummer ist gut dazu, denn er sagt ihm gewiß keinen falschen Weg.

After all, Bialik could easily have been made aware of its existence, e.g., by Tom Freud herself. However, the verbal formulation of this sentence does not really conform to the Hebrew one, on top of which it does not cover *all* the express-

9. The following represents an 'authentic' Russian epilogue of a similar kind:

*Ne pivo pit' – ne vino kurit',
Povenčali – i žit' pomčali,
Stali žit' poživat' – i dobra naživat'.
Ja zaxodil v gostli, – ugostili xorošo:
Po gubam teklo, – a v rot ne popalo.*

[It's not to drink beer! It's not to brew wine! / They were wedded and whirled away to love. / Daily they lived and richer grew. / I dropped in to visit, right welcome they made me – / Wine runs on my lips, nary a drop in my mouth!]

"In other words," (as Roman Jakobson put it) "the still thirsty teller awaits his refreshment. Sometimes the allusions are more transparent: "This is the end of my tale, and now I would not mind having a glass of vodka" (1966: 94–95).

lons used in our prologue (and shared by its Russian counterpart). Moreover, in Bechstein's version blindness and muteness form an integral part of the characterization of the *miraculous land* itself rather than of the (non-existent) prologue. Finally, there are many additional regularities in the Hebrew text which have their counterparts in Russian traditions; and one global hypothesis which applies to a host of heterogeneous phenomena is always preferable to a list of disparate explanations accounting for one detail each.

To be sure, the hypothesized Russian source can be made more explicit still, a possibility which, of course, further increases the plausibility of the hypothesis and enhances its explanatory power. For there is one text-type which seems particularly relevant to our case: the Russian **anecdote**, whose texts amount to a combination of a prologue and an epilogue, entirely couched in rhyming 'free verse', with hardly any 'story' in between. This is how Roman Jakobson describes this variety in his classical article "On Russian Fairy Tales":

The tales of anecdotal tinge manifest a disposition to verse form, which in the fairy tales occurs only in the preludes and epilogues. This form, a spoken free verse, based on a colloquial pitch and garnished with comical, conspicuous rhymes, is related to the free meters of buffoonery and wedding orations. Expert tellers possess such an abundant hoard of rhymes and syntactical clichés that they are often able to improvise such spoken verses on any given subject, much as experienced mourners are able to improvise long dirges in recitative verse. (Jakobson 1966: 96)

The very same features were retained – if not accentuated – in the secondary, written tradition of the anecdote, which partly settled down in Russian literature for *children*.

It now becomes possible to make the claim that almost everything in the composition of "Gan-Eden ha-taxton" which deviates from the German original and the model underlying it was not just picked up in one general area, that of Russian folklore, but actually constitutes an **alternative model** picked up from a recognizable source. In fact, "Gan-Eden ha-taxton" seems to satisfy the requirements of the Russian anecdote to capacity, in terms of both the jesting quality it possesses and the plot it lacks. For, after all, what is embedded in between the added prologue and epilogue is a series of comic fantasy sentences rather than any proper 'story'. It is precisely these two features which have already characterized the *German* source and made it an exception in Freud's collection. No

wonder that, along with the need to get rid of its underlying principles, this text also triggered the adoption the principles of the Russian anecdote.

5. External source vs. internal legitimation

A clear element of mediation was thus found to have been involved in the process, even though on the textual-linguistic level it was definitely 'firsthand': the German source text was sifted, so to speak, through the model of the Russian anecdote.

Obviously, my claim is not, and cannot be that the Russian (mediating) model was any *better* than the (original) German one. It was simply much more *available* to Hebrew literature, which lacked a suitable model of its own but no longer relied on German literature for filling its systemic gaps, as it would have done a few generations earlier. In fact, the Russian model was 'legitimately usable'¹⁰ precisely *because* of its markedness as Russian. For the (uni-directional) contacts between the two systems were by then such that Russian literature served as a major reservoir for Hebrew literature, in terms of both individual texts and overall models. "Russian participated in the very making of Hebrew culture" in such a way that, in an important sense, "one could say that Hebrew literature behaved as if the Russian system were *a part of it*" (Even-Zohar 1990: 102; italics added).

Though rather extreme in its realization, sifting a non-Russian text through a Russian intermediary model can definitely count as *symptomatic*, then;¹¹ rule-governed behaviour rather than the mere whim of an individual: While every single decision was definitely Bialik's, they were all governed by one organizational principle of general validity, representing the network of relationships underlying the target culture of the period.

However, from the internal point of view of Hebrew literature of the 1920s,

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10. "The existence of a specific repertoire per se is not enough to ensure that a producer (or consumer) will make use of it. It must also be *available*, that is, being legitimately usable, not only *accessible*" (Even-Zohar 1990: 40).
 11. And see my account of how, as late as the early forties, Hemingway's *For Whom the Bell Tolls*, Upton Sinclair's *100%: The Story of a Patriot* and Bret Harte's *M'lis* were translated into Hebrew as pieces of soz-realist fiction, namely, according to Soviet requirements adopted (and adapted) in certain sections of Hebrew literature of the period (Touy 1977: 138-140, 141-142 and 262, respectively).

a completely 'Russified' text, legitimate as its introduction may have seemed, would still have had a considerable element of alienness to it. Little wonder, then, that it was into a *peripheral* section of his literary work, and of Hebrew literature as a whole, that Bialik introduced this novelty — a clear demonstration of a central principle of systemic evolution which was already formulated by the Russian Formalists. Nor was it an isolated case, even in Bialik's own literary career.¹²

Our account is not yet complete, however, since there was yet another element in a position to enhance the acceptability of a jesting text couched in rhymed prose; namely, a dead but not entirely forgotten tradition of Hebrew literature itself, that of the medieval *maqāma*. Bialik himself was well acquainted with major segments of medieval Hebrew literature. In fact, he had even contemplated publishing one of the major texts of the *maqāmāt* tradition, Alharizi's *Taxkemoni*, and between 1923 and 1927 he himself wrote a *maqāma* of sorts, "Aluf Batslut ve-aluf Shum" [Lord of Onions and Lord of Garlic] in a conscious attempt to revive this text-type.

In 1922, the *maqāma* was by no means part of living Hebrew literature, and in any case "Gan-Eden ha-taxton" would not really have qualified as a true *maqāma*, not being enough of a narrative, on the one hand, and being much too short, on the other. However, the fact that there had once been something akin to it within Hebrew literature itself may well have given Bialik another boost towards adopting the principles of the Russian anecdote, because it could serve as a seeming legitimation from within for something that was basically imported from without. To be sure, the need for a secondary justification of this kind as a 'safety net' to ensure a softer landing was often felt in the history of modern Hebrew language and literature, and played a significant role in their evolution.¹³

6. Enhancing the translation's acceptability

A case of legitimate importation, then, but importation nonetheless; and, as is always the case, full appropriation requires that the transplanted organ be

12. See, e.g., the cases discussed by Shamir 1986; Ben-Porat 1986; U. Shavit 1989.

13. See, e.g., examples of inner secondary legitimation for newly coined words such as *salfit*, *sabit* or *du'it*, which came into being as a result of modified phonetic transposition (from *falsetto*, *bit* and *duet*, respectively) in Toury 1990.

matched up with the recipient. This requirement was met by the translator's overdoing it on the *linguistic* level, where Hebrew literature found it much easier, as well as much more binding, to (pretend to) confine itself to its own resources.

Thus, in the verbal formulation of "Gan-Eden ha-taxton", Bialik subjected himself to the norms of Hebrew usage in a way which was in keeping with both the principles of the Russian anecdote and the normal practices of Hebrew literature, but he went much further, as if to counterbalance the alienness inherent to the 'Russified' model and to enhance the acceptability of the end product as a Hebrew literary text.

It will be remembered that one of the main features of Russian anecdotes is the reliance on "an abundant hoard of ... syntactical clichés" (Jakobson 1966: 96), and "Gan-Eden ha-taxton" did not fail to comply with this requirement either. In this respect, however, a basic problem arose: unlike a competent Russian story-teller, popular, semi-popular, or mock-popular, Bialik had no repertoire of ready-made Hebrew phraseologies at his disposal which were habitually associated with the telling of anecdotes, and hence readily available for this type of text production and consumption. This problem he solved by resorting to two different and distinct sources: the repertoire of **Russian formulas**, on the one hand, and the rich repertoire of **Hebrew fixed expressions**, on the other, further enriched by a number of loan-translations from Yiddish which had been accepted into written Hebrew, including, of course, *Gan-Eden ha-taxton* [lower paradise] itself.¹⁴

Literal translation of Russian expressions which had become formulaic vis-à-vis oral and written popular literature yielded Hebrew phrases which, for any reader who shared Bialik's background – the one underlying the very adoption of the intermediary model – were a clear hint to the anecdote tradition. As a result, they could pass for fixed expressions, even though, in strictly internal terms, they were *free* collocations: perfectly acceptable entities, even well-formed in grammatical terms, but in no way institutionalized as phraseological units. By contrast, the second option offered expressions which were indeed Hebrew clichés. Yet, they did not associate with any *particular* literary tradition, let alone one which would have been relevant to the text's intended role as an anecdote.

And here is where the most striking regularity occurs: the two diametrically

14. E.g., *yonim tsluyot* [roast pigeons] and *skedim ve-tsimukim* [almonds and raisins] as denominations of two special Delicatessen, or *rodef zvuvim* [fly-chaser] as a nickname for a loafer.

opposed alternatives prove to be distributed throughout “Gan-Eden ha-taxton” in a highly ordered fashion. Thus, almost all the expressions used in the epic situation, which is the most Russian element of “Gan-Eden ha-taxton” in all other respects too, represent the first option, whereas almost all the expressions occurring in the two internal ‘boxes’ represent the second one. The latter also include purely Jewish elements, such as the two holidays *Purim* and *Šavu’ot*, or the space between the two rhyming countries *Mitsrayim* [Egypt] and *Aram Naharayim* [Mesopotamia], approximately representing the Biblical Land of Israel. While a tinge was thus added to the marked Russianness of the epic situation, this clear demarcation also contributes to the mitigation of the novelty which may have resulted from the introduction of the imported model. After all, the description of the miraculous land – including the enumeration of its sources of livelihood, which abound in fixed collocations of Hebrew origin, mostly canonized sources – still forms about three quarters of the text and constitutes its core.

This massive recourse to fixed expressions, in turn, was another major cause of translational shifts. For instance, many *additions* to Freud’s text can only be accounted for as a concomitant of the introduction of phraseological units, which was itself done first and foremost in order to increase the Hebraicness of the translation (and/or yield a required rhyme-partner), and not in order to reconstruct, or represent any feature of the source text.¹⁵ Not only are these expressions longer and more complex than single words, but their semantic range tends to be rather wide, often resulting in vague denotations; much vaguer, at any rate, than the ones represented by the original. In this respect too, our case certainly fits into the mainstream of translational behaviour into Hebrew at that time (see especially Even-Zohar 1976). Nonetheless, with regard to a text which is inherently based on an inversion of reality, the ensuing change is indeed considerable.

Another characteristic of the verbal formulation of “Gan-Eden ha-taxton” is an abundance of **parallelisms** on all possible levels; from long segments like

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15. A few examples (with their first sources in the canonized Hebrew tradition indicated):
 Eierfladen → *tsapixiyot bi-dvaš* [(things which are) as sweet as honey] (Exodus 16:31)
 Lebzelten → [*mamtakim*] *ma’ase ofe* [baker’s work (sweets)] (Genesis 40:17)
 Milchbäche → *palge dvaš ve-xalav* [streams of honey and milk] (Song of Songs 4:11. The use of the rarer word order owes to rhyme requirements)
 daß es eine Lust is → *metukim la-xex ve-ta’ava la-enayim* [sweet to the palate and pleasant to the eye] (Song of Songs 2:3 + Genesis 3:6)

the ones built around the pair of (near-)antonyms *lokek* and *rokek* to phrases and to pairs of (near-)synonymous lexical items. With respect to parallelisms on the higher levels, the requirements of the Russian intermediary model seem to have concurred with Hebrew conventions. As for paired (near-)synonyms, on the other hand, their extensive use in the period in question was still a strong internal norm whose application to the Hebrew text was almost mandatory. Therefore, it may be taken as yet another marker of the translator's striving for enhanced Hebraicness (see Chapter 5). Clearly, the need to establish parallelisms further increased the number of textual-linguistic additions and other translational shifts.

Due to the normal position of translations (as texts) and translating (as an activity) within a culture, which tends to be secondary, it is not surprising that the application of this linguistic norm was more extensive in translations than in the original writings of a period, even when one and the same person was responsible for both. It is also no wonder that this norm continued to be active, even highly active, in this section of the target system when it had already been pushed to the periphery in original writing. The same holds true, and for very similar reasons, for children's literature vs. literature for adults (see, e.g., Z. Shavit 1981; 1986). Obviously, in "Gan-Eden ha-taxton" we have a combination of both, which could have been explanation enough for the frequent use of paired (near-)synonyms.

This generalization notwithstanding, nowhere in Bialik's translations or writings for children (except maybe in his translation of a version of *Don Quixote*, which he intended to be 'biblical' in style) can such a density of Hebrew phraseologies be encountered. This can be taken as a further verification of the hypothesis that they represent first and foremost a strategy for mitigating the potential alienness of the text and enhancing its acceptability.

"Gan-Eden ha-taxton" was indeed accepted as a Hebrew literary text — much more so in fact than any of the other tales in *Eser sixot li-ladim*, which very soon sank into oblivion. It was reprinted several times, with the authorship ascribed to Bialik himself. No reference was now made to any other author, either Bechstein or even Tom Freud, until the text was finally included in Bialik's representative collection of writings for children (1933: 170–174). There it was featured as a *poem*, no less;¹⁶ i.e., it was rearranged in

16. When Bialik included "Gan-Eden ha-taxton" among his poems for children, he further enhanced the subjected status of the inscription mentioned above by enclosing it in inverted commas. This testifies to his own awareness of the structure he had established, which differs essentially from that of the original text.

short verses and in stanzas on the basis of the original rhyme scheme and division into paragraphs, respectively (see note 5). This collection very soon became canonized in Hebrew children's literature, and the fact that "Gan-Eden ha-taxton" had originally been intended, presented and accepted as a translation was completely erased from 'collective memory' and became a historical rather than literary fact, until uncovered in the early eighties (Ofek 1984: 77–86).

In fact, a tentative attempt was then made to restore the original status of the text by reprinting it in its original, rhymed-prose form, in a selection of Bialik's translations for children edited by Uriel Ofek (Bialik 1983: 204–205). Moreover, a year later, the entire *Eser sixot li-ladim* was reprinted by the Israel Museum in Jerusalem, on the occasion of an exhibition of Tom Freud's illustrations (Seidmann-Freud 1984), and a select audience was again given "Gan-Eden ha-taxton" as a *translation*. Obviously enough, there can be no real retraction in history. Unlike the situation in 1922, the re-introduction of "Gan-Eden ha-taxton" in its original form was done into a system which already had within it an almost identical text, strongly associated with Bialik's name and endowed with some fifty years of proven canonicity. On top of all that, during this time, translation has become even more secondary in Hebrew literature. Chances are that "Gan-Eden ha-taxton" as a translation will remain peripheral, a kind of a neutral piece of knowledge held by the (very few) initiated, rather than becoming again an active member of a living literature.

7. Was there any alternative?

The intriguing last question is, of course, whether Bialik had any other choice. Could he have opted for a different strategy?

Translation being a decision process by its very nature, the obvious answer would be 'yes'. What is significant, however, is precisely that Bialik did *not* take advantage of that prerogative – and that, in retrospect, his preferences turn out to be that predictable, given the constellation of the literature within which he worked and for whose needs he wished to cater.

It is clear that a translator always has more than one option at his or her disposal. However, it is not the case that all these options are equally available, given the constraints imposed by the target culture (see note 10). Rather, they tend to be *hierarchically ordered*. Of course, a translator may also decide to work *against* the order offered him/her by the target literary-cultural constellation. However, any deviation from 'normative' modes of behaviour is liable to be negatively sanctioned, if only by detracting from the product's acceptability, as a translation, or even as a target-language text. Most translators are quite reluctant to pay such a price and therefore the tendency is normally to adhere to prevalent

norms. What is most significant in the present case is the fact that even a poet-translator of Bialik's calibre, who was at that time at the peak of his fame and influence, hence in a good position to introduce *changes* of norms, opted for *adhering* to them, and rather devoutly at that.

8. Appendix

Tom Freud. "Das Schlaraffenland"

Hört zu, ich will euch von einem guten Lande sagen, dahin mancher auswandern würde, wüßte er nur, wo es läge und einen Weg und eine Schiffsgelegenheit dahin.

Dieses Land heißt Schlaraffenland, dort sind die Häuser gedeckt mit Eierfladen, Türen und Wände sind von Lebzelten. Auf Birken und Weiden wachsen Semmeln frischgebacken und unter den Bäumen fließen Milchbäche. Die Käse wachsen wie die Steine groß und klein, und die Vögel fliegen gebraten in der Luft herum und jedem, der da will, in den Mund hinein.

Im Winter, wenn es regnet, regnet es lauter Honig im süßen Tropfen, da kann einer lecken und schlecken, daß es eine Lust ist, und wenn es schneit, so schneit es klaren Zucker und wenn es hagelt, so hagelt es Würfelzucker, untermischt mit Feigen, Rosinen und Mandeln. Im Buschwerk und auf Bäumen wachsen die schönsten Kleider: Röcke, Mäntel, Hosen und Wämser in allen Farben, schwarz, grün, gelb, blau und rot.

Für alle faulen Leute ist das Land vortrefflich. Jede Stunde schlafen bringt dort einen Gulden mit und jedesmal Gähnen einen Doppeltaler. (Keiner darf etwas umsonst tun und wer die größte Lüge macht, der hat allemal eine Krone dafür.)

Wer nichts kann als schlafen, lesen, trinken, tanzen und spielen, wird zum Grafen ernannt und der Allerfaulste wird König über das ganze Land.

Um das Schlaraffenland herum ist eine berghohe Mauer von Reisbrei, Wer nun hinein will oder heraus, muß sich da erst hindurchfressen. (Freud [1921])

Tom Freud / Chaim Nahman Bialik. "Gan-Eden ha-taxton"

Gan-Eden ha-taxton mi yode'a? — Gan-Eden ha-taxton ani yode'a. od ta(')mo omed be-fi u-sfatay, re'itiv ve-lo ezkor matay, ha-be-layla im be-yom, ha-be-hakits im ba-x(a)lom. iver ra'ahu ve-elav lo karav, kite'a ba bi-š'arav, gidem patax dlatav, ve-ilem siper li zot bi-sfatav. mi xereš yasur halom, yešev ve-yišma ve-yakšiv dom.

batim be-Gan-Eden ze'irim-ze'irim, tsaxim mi-šayiš u-mi-šeleg behirim, gagotehem lo teven v(e)-lo kaš, ki im tsapixiyot bi-dvaš; kirov ha-batim ve-daltotehem, xalonehem u-trishehem, min ha-misderon ve-ad ha-ulam, min ha-ma'ake ve-ad ha-sulam — mamtakim ma'ase ofe kulam. etsav motsi'im bigde tsiv'onim, minim mi-minim šonim, šeš u-txelet ve-argaman, ma'ase yede oman; ve-ugot be-karkom u-gluska'ot, rakot xamot ve-na'ot, gdelot

šam be-xol ha-ša'ot, ve-xol še-ken le-Purim u-l-xag ha-Šavu'ot; u-falge dvaš ve-xalav yarutsu be-xol ma'galav; va-xaritse gvina, xaritsim xaritsim, olim min ha'arets u-metsitsim, gdolim u-ktanim u-ktane-ktanim, kvu'im ba-mirtsefet ka-avanim, ve-yonim tsluyot, hen ve-gozalehen, me'ofefot u-va'ot el ha-pe ke-me'alehen.

gešamav – nitfe dvaš ve-yayin, metukim la-xex ve-ta'ava la-enayim, kol ha-rotse pošet lašon ve-lokek, ve-še-eno rotse – rokek. ha-šeleg šam – avkat sukar zaka, daka min ha-daka, ve-ha-barad – gargere nofet metukim, me'oravim bi-škedim u-ve-tsimukim, kol oxlehem yizkeru ta(')mam ad yetsamax lahem sfamam.

u-misaviv le-Gan-Eden xoma gvoha va-aruka, daysat orez kula, daysa metuka, va-ašer yomar lavo baysa – ve-xatsa ad tsavar ba-daysa.

u-ve-ša'ar ha-xoma ktovet kazot, mila be-mila ve-ot be-ot: mi atsel ve-holex batel babanim, mi rodef zvuvim ve-kotel kanim, halom halom Gana-Eden bo(')u, en la-atselim makom tov kamo(h)u. po yešvu be-šalom ha-batlanim kulam, yedehem xavukot u-sxaram yešulam: pehika be-Dinar u-lkika bi-šnayim, u-sxar sixa na'a – šiv(')im šiv(')atayim, ve-xol šfal-yadayim ve-zoxel al štayim yetse lo šem ben tanur ve-xirayim mi-Mitsrayim ve-ad Aram Naharayim.

et ašer šama(')ti oto asapera, ve-atem šalmu li bi-mzumanim mehera, Gera šlema va-x(a)tsi ha-Gera.

amen ve-amen.

(Freud 1922)

Excursus B

'Translation of Literary Texts' vs. 'Literary Translation'

As we have observed, translation is basically designed to fulfill (what is assumed to be) the needs of the culture which would eventually host it. It does so by introducing into that culture a version of something which has already been in existence in another culture, making use of a different language, which – for one reason or another – is deemed worthy of introduction into it. The introduced entity itself, the way it is incorporated into the recipient culture, is never completely new, never alien to that culture on all possible accords. After all, much as translation entails the retention of aspects of the *source text*, it also involves certain adjustments to the requirements of the *target system*. At the same time, a translation is always something which hasn't been there before: even in the case of *retranslation*, the resulting entity – that which actually enters the recipient culture – will definitely *not* have been there before.

In the simplest of cases, the introduced entity is just a text. In more complex cases, however, textual models (i.e., sets of rules for the generation of texts pertaining to a culturally recognizable type), even modes of language use, may be imported with groups of texts which either embody recurring patterns or are translated in a similar fashion. Nor is the novelty a mere function of what the source item manifests, either with respect to the tradition it is part of or even in comparison to the recipient culture, however striking the incongruity between the two. Rather, the novelty of an entity derives from the target culture itself, and relates to what that culture is willing (or allowed) to accept vs. what it feels obliged to submit to modification, or even totally reject.

For that reason, a question such as whether the translational transfer of an English limerick into a particular culture can yield, e.g., a four-liner, a non-anapestic entity or a text devoid of a punch line, or even all three together, is far

from a mere teaser. There is nothing in principle to *exclude* such possibilities in real-life translational behaviour. Far more important, there are circumstances which reduce the acceptability of a limerick which is transferred in an unmodified manner. The result may well seem an *irregular* textual entity. Consequently, the recipient culture may favour the adoption of precisely the four-line and/or non-anapestic option instead of the model implied by the source text. This is true especially of cultures which possess no corresponding model of their own, and most cultures, even of the West, were in precisely that situation vis-à-vis the English limerick until quite recently.¹

For the same reasons, an attempt to establish the circumstances under which the translation of a Shakespearean sonnet is [more] likely to yield an Italian-like sonnet (see Chapter 6), or even a non-sonnet, is highly pertinent. The implications of such a study for translation theory are unquestionable too. The *Schlaraffenland* example, dealt with rather extensively in the previous chapter, proved highly illuminating in precisely this respect: an unacceptable model was rejected – to be replaced by another which was deemed appropriate for both the source text and the target literature. Nor would calling this strategy ‘adaptation’, *Umdichtung* or anything of the kind solve anything, even if it were in keeping with historical distinctions pertinent to the culture in question (which it is not). Using such a label would only blur the whole issue, which is precisely one of ‘why’ and ‘wherefore’; most notably, why one particular text out of ten comprising an integral book, whose standing as such was respected, was regarded as requiring a special mode of translation, and why it was this particular mode that was selected.

It is numerous observations of this kind, and attempts to account for them

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1. To be sure, most reference books I have been able to check, in a multiplicity of languages, still refer to the limerick as an exclusively ‘English genre’, even though limericks, sometimes quite a number of them, have been composed in dozens of languages/cultures and/or translated into them, mostly during the 20th century. And cf. a typical remark in a book devoted to nonsense poetry: “Versuche, die [Limerick] Gattung mit Übersetzungen ... auch im deutschen Sprachgebiet einzubürgern, sind bis jetzt gescheitert” (Liede 1963: 266).

Whether the recipient culture would call the resulting entities ‘limericks’ or not is a completely different matter, which has very little to do with the issue at hand. One culture may adopt most features of the original model and call the result by another name (e.g., the Hebrew *xamsîr* [a five-line-poem] as of the fifties); another culture may retain the name even if it adopts the model to a much lesser extent.

within Translation Studies rather than dismissing them as simply 'non-pertinent' (in terms of an a priori, idealized concept of translation, that is), that led me to address the notion of **literary translation** itself.

1. The two senses of 'literary translation'

I soon (e.g., Toury 1984a) came to the conclusion that the term 'literary translation', as it had come to be used, was afflicted by *systematic ambiguity*, referring as it does to two different things:

- (a) **the translation of texts which are regarded as literary in the source culture:** in an extreme formulation, which has become rather obsolete, *any translation* of such texts; in a modified version — one where the focus is on the retention (or, better still, reconstruction) of the source text's internal web of relationships (e.g., Snell-Hornby 1987: 93), the one which makes that text a unique instance of performance, rather than its realization in language;
- (b) **the translation of a text** (in principle, at least, *any* text, of any type whatever²) **in such a way that the product be acceptable as literary to the recipient culture.**

The manifestations of the two senses of 'literary translation' may of course concur: There may indeed be circumstances under which a close reconstruction of a source text's web of relationships in an adequate fashion would override any problems of acceptability which may initially exist in the target literature; for instance, when the two cultures involved in the act have had very similar literary traditions anyway, often as a result of constant contacts between them, or else when the recipient system is in a weak position with respect to the source

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2. In the Middle Ages, the translation of a philosophical essay, on occasion even a text in astronomy, could well have been regarded as literary. It should be noted, in this connection, that the demarcation between 'literary' and 'non-literary' within a culture was different, in those days, and that it differed across cultures as well. Thus, there were two different Hebrew translations of the *Kuzari*, utilizing different strategies and norms, and only one of them came to be regarded as 'literary' — due to the way it was translated, to be sure, and not to the original text's position; otherwise, all versions would have been expected to occupy the same position and have the same functional identity within the recipient culture.

system, and hence willing to use the latter as a source of enrichment, in terms which transcend the individual text; or, finally, when the translator has gained a position in his/her culture which allows him/her to deviate from sanctioned behavioural patterns and get away with it, sometimes to the point of introducing changes into that culture. The point about the two senses of 'literary translation' is that there is no *inherent* need for them to concur: even though reality may bring them together, they still remain different *in essence*.

Thus, neither the 'literariness' of the source text, nor even the careful embodiment of its web of relationships within a target-language textual entity, is enough to secure a position for the end product in the recipient literature, much less so a position parallel to that which the original enjoyed in its own cultural environment. As is well known, the complete opposite is a real possibility too: a translation may well be eligible for *rejection* by a target literature — among other reasons, on the grounds that it reflects the source too closely, be it an individual text, its underlying model or the entire generating tradition.

To be sure, the distinction between the mere 'translation of texts of a particular kind' and 'a kind of translation designed to fulfill a particular function' is no peculiarity of literature alone. One could easily draw an analogous distinction between, say, the translation of legal documents (the end products of which would, for instance, be incorporated in a target-language history book and serve as a source of information *about* the translated document) and strictly legal translation (where the products are expressly designed to serve *legal* purposes, whether similar to or different from the purposes served by the original texts). By the same token, many national anthems can be found in a translated version. Very few of these translations also *function* as national anthems, and those that do do not necessarily reflect the most salient features of their parallel texts in (an)other language(s). (And see the revealing discussion of the French and English versions of the Canadian anthem in Harris 1983.) Similar things can be said of the translation of a text such as the Hebrew Bible, which became canonized as a *religious* text: only one of the possible options would be for the target text to replace the original in this particular function, and even then, there would still be a difference between functioning as a *Jewish* and a *non-Jewish* religious text. In this sense, not just any translation of the Bible would amount to biblical translation; not even necessarily a particular *mode* of translation.

Literature is just an example of a general phenomenon, then. It is a very convenient one for a critical discussion, though, precisely because, on the one hand, it is normally not regarded as so obvious a case as, say, the translation of legal texts, whereas, on the other hand, it evades many of the ideology-laden

reservations which any discussion of the translation of national anthems or the Bible is bound to entail.

2. 'Linguistic', 'textual' and 'literary' modes of translation

The essential difference between the two senses of 'literary translation' stems from the fact that literature does not boil down to a body of texts, much less so a repertoire of features which allegedly have something inherently 'literary' about them. Rather, literature is first and foremost a cultural institution.

Thus, in every culture (including different phases in the evolution of one culture), certain features, models, techniques (including modes of translation!), and — by extension — texts utilizing them, are *regarded as*, rather than *are* literary, in any 'essentialistic' sense. What lends a phenomenon or a text their position as what Tynjanov (1967b [¹1924]) called 'literary facts' is a *systemic constellation* — a network of ad hoc relationships into which they enter. Their literariness is thus established in terms of a given cultural system; never in itself, so to speak. And, indeed, features, models and techniques, as well as texts utilizing them, may both *become* literary and *lose* their literariness without undergoing any change of textual organization, linguistic formulation or whatever. It is the *systemic position*, and the *cultural-semiotic function* which it gives rise to, that make the difference, not any of the surface realizations.

Obviously enough, only rarely will two different systems fully concur; and since the (functional) identity of a phenomenon is governed first and foremost by the *internal* organization of the system which hosts it,³ the literariness of an act of translation can be said to be determined by the bearing of the *target* literature requirements upon it. Of course, these requirements can have a bearing on an act of translation to various extents too, which makes literary translation a *graded* notion rather than a matter of either/or; in fact, not only is it no less scalar than the possibility of reconstructing a source text's features to capacity, but very often in inverse proportion to that possibility.

'Literary' as a qualifier of 'translation' can thus be added to 'linguistic' and 'textual', to form a series which is hierarchically ordered in terms of the specific-

3. "[The 'literary system' is] the network of relations that is hypothesized to obtain between a number of activities called 'literary', and consequently these activities themselves observed via that network" (Even-Zohar 1990: 28).

ity of the conditions they impose on the act, while presenting *homology*, at the same time. Thus, with respect to a source text which is literary itself,

- A **linguistically-motivated translation** is any act of translation yielding a product which is well-formed in terms of the target syntax, grammar and lexicon, even if it does not fully conform to any target model of text formation. (In this case, at least partial interference of the model underlying the source text is to be expected.)
- A **textually-dominated translation**, in turn, yields products which are well-formed in terms of general conventions of text formation pertinent to the target culture, even if they do not conform to any recognized literary model within it. (Interference of the model underlying the source text is still to be expected, namely, in terms of its literary-specific features.)
- Finally, **literary translation** involves the imposition of ‘conformity conditions’ beyond the linguistic and/or general-textual ones, namely, to models and norms which are deemed literary at the target end. It thus yields more or less well-formed texts from the point of view of the *literary* requirements of the recipient culture, at various possible costs in terms of the reconstruction of features of the source text.⁴

Subjugation to target literary models and norms may thus involve the *suppression* of some of the source-text’s features, on occasion even those which marked it as ‘literary’, or as a proper representative of a specific literary model, in the first place (such as the above-mentioned five-line structure, anapestic meter or punch-line of an English limerick, or the features which are unique to a Shakespearean sonnet, or the paratactic structure of a German text pertaining to the *Schlaraffenland* tradition). It may also entail the *reshuffling* of certain features, not to mention the *addition* of new ones in an attempt to enhance the acceptability of the translation as a target literary text, or even as a target literary text of a particular type. As the *Schlaraffenland* example has revealed, the added features may occupy central positions within the translation (when looked upon as a text in its own right), even serving as **markers of its own literariness**, despite their having no basis in the original.

4. And see now Roberts’ discussion of the differences between functions of language, functions of (source) text and functions of translation (1992).

3. 'Literary translation' and target-orientedness

Unfortunately, this functional conception of literary translation has bred some gross misunderstandings among various colleagues, who somehow read in(to) it things that were never meant to be there. Thus, the line of reasoning which I have been following is a far cry from a mere variation of good old 'literary reception', as claimed by many, most notably Mary Snell-Hornby in her best-selling *Integrated Approach* (1988: 24–25).

To be sure, literary reception is a worthy field of study which hardly needs my defence. The only thing is that this kind of study has very different goals from the ones I have been pursuing. After all, something, including a translation, has to be there before it can be properly 'received' into a literature (or, for that matter, 'rejected' by it), which implies that the act itself which gave rise to it has already come to its end. By contrast, what I have been arguing concerns what goes on **during translation — and for its own purposes**. It is not *acceptance* (or reception) which is the key-notion here, but *acceptability*. Thus, what may be said to operate in literary translation (in sense (b)) is not any *fact* about the reception of its product (which is not yet there, to begin with). Only *assumptions* can be operative here, namely, as to the *chances* a text has of being accepted whose structure and/or verbal formulation would follow a certain pattern.

To the extent that they are members of the target culture, or tentatively assume that role,⁵ translators can be more or less aware of the factors which govern the prospects of texts and textual-linguistic phenomena to be accepted into, or rejected by that culture, or a particular sector thereof. If they then choose to subject themselves wittingly or unwittingly to factors which enhance acceptability, and resort to strategies which promote it, the entire act of translation would be executed under the initial norm of acceptability. Where in the target culture the result of these efforts will then be located is a totally different matter which may indeed form part of a research program in reception, literary or otherwise. While never totally ignored, this aspect is definitely *marginal* to the present line of reasoning, except for the effects it may have on later translations, that is, its implications for translational norms of the *future*.

Thus, not only can translations which have been carried out according to

5. Anthony Pym (1993–1994) has recently claimed that translators should be regarded as members of an 'interculture'. Whether one accepts this ideological stance or not, in reality there would at best be a *series* of different 'intercultures', each one pertaining to a particular target culture.

strict requirements of literary acceptability *not* be accepted into the target literature, but translations which have *not* been executed under this mandate may nevertheless carve a niche for themselves in it, even in cases where what they reflect is the source text and its underlying models – much the same as a culture may find itself appropriating lexical neologisms, syntactic deviations, or novelties of whatever sort, in terms of general conventions of text formation. This should never have been the case, had ‘acceptance’ been a mere function of initial ‘acceptability’. The circumstances under which interference on one level or another, resulting from a failure to – or a decision not to – adapt the translation to target system requirements, tends to be accepted by a culture constitute another interesting research area. Needless to say, the acceptability of interference on one level does not necessarily entail its acceptability on any other – a fact which may also have implications for policy planning in translation.

The difference in focus between translating texts which *are* literary, on the one hand, and performing translation with the intention of *establishing* proper literary texts, on the other, may easily be correlated with a distinction made on the level of scholarly approaches to translation, between the source- and target-oriented positions. Thus, any attempt to devise a (prospective) framework for dealing with literary texts and with the ways they *can*, let alone *should* (allegedly) be translated would call for a **source-oriented** position, even though any reconstruction of a source text, or of its ‘web of relationships’, in target-language material would have to be modified by target conditions – cultural, literary, textual, or merely linguistic. The same holds for any (retrospective) study confining itself to the enumeration and grouping of instances where a translator has *deviated* from the source text, with no concern for the underlying *reasons*; especially as far as so-called ‘non-obligatory’ shifts are concerned (to the extent that a shift can justifiably be called ‘obligatory’, to begin with, and, at any rate, with respect to most of the shifts actually encountered).

By contrast, anyone wishing to focus on the role of target factors in the establishment of a translation, either retrospectively or even prospectively, will find him-/herself opting for a **target-oriented** approach, even though, in the course of its application, s/he will come back to the source text, often even establishing the target text’s shifts from it. It is a matter of *orientation*, then; a difference of perspective and focus, not two diametrically opposed positions which would never converge. Starting at any end and going all the way one would normally end up at the other pole. The question is only what approach would be best suited for what one is after, especially as individual research projects would often not go ‘all the way’.

It should have become clear by now that the kind of target-oriented approach I have been advocating since the mid-seventies does *not* “have much in common with conventional studies in Comparative Literature” (Snell-Hornby 1988: 24–25). Nor is this solely because — as briefly argued in Section 1 — there is no need to limit its validity to the study of *literary* translation, in no matter what sense. (And see the detailed account of the [non-literary] English warning in Chapter 4, Section 2, where the very same theoretical framework and methods were adopted.)

Thus, even in cases where literary translation (in sense (b)) is the object, it would be simplistic to claim that the difference between “conventional studies in Comparative Literature” and my brand of target-oriented studies of translation lies in the mere fact “that the [latter] deal with translations rather than original works”, as Snell-Hornby further put it (*ibid.*). After all, even in the initial phases of a full-fledged study, when it is indeed *texts* which are taken up and analyzed, the locus of study is never the text as an entity in itself, whether a mere target-language utterance or even a replacement and/or representation in the target language of another text, pertaining to another language, literature, culture. The locus is rather what the texts can reveal as concerns the *processes* which gave rise to them: the options at the translators’ disposal, the choices made by them and the constraints under which those choices were affected, on the way to extracting such shared factors as are reflected by larger bodies of texts which have been brought together on the basis of one organizing principle or another; especially if and when those shared factors can be tied up with the organizing principle.

Incidentally, one of the most obvious principles for grouping texts together for study purposes, namely, the fact that they are [regarded as] translations of one and the same text, often proves of limited consequence, when regarded from a target vantage point. The main thing a body of texts thus constituted normally yields is a series of more or less *independent* observations as to the (honoured and not-so-honoured) ‘legitimate rights’ of the source text. Not uninteresting, not even unimportant, but hardly enough, if one is seriously interested in accounting for these phenomena as legitimate *translational* phenomena rather than mere blunders.

In order for such results to gain in significance, a lot of *contextualizing* would have to be done, which is really what target-orientedness is all about. After all, what is at issue here is translation as a *conditioned* type of behaviour, which breeds an inevitable need to establish the interdependencies between the position and role of translated texts and translational behaviour in culture, the

norms that determine their appropriateness and govern their establishment, and the modes of executing translation under various circumstances. Only why wait until the results have proven unsatisfactory; why not start contextualizing the texts right away?

All this, and much more, would make any such study, let alone the general framework in which it is performed, highly *unconventional* indeed, in terms of "conventional comparative literature". To be sure, traditional (and not-so-traditional) comparatists were among the first to disclaim the target-oriented approach to translation as a legitimate method of Comparative Literature, back in the seventies. Comparatists who found themselves attracted to it were sneered at, sometimes even deprived of the very honourable title of 'comparatists'... By contrast, it does plant this approach at the very heart of a Translation Studies.

4. Cultural distance and the gap between the two senses of 'literary translation'

One point that the *Schlaraffenland* example has hinted at deserves to be made explicit now and tied up with the rest of the discussion:

Whatever its realization under one or another set of circumstances (which is a matter of *descriptive* research, not theory and methodology construction), the potential gap between what translation of literary texts and literary translation proper involve gains in significance in direct proportion to the distance between the source and target traditions; again, in full accordance with what one would expect on the (homological yet distinct) general-textual and linguistic levels.

This probably accounts for the fact that it is mainly European scholars who have had conceptual problems with accepting a target-oriented framework for *research* in Translation Studies, even if they were willing to subscribe to a version thereof for translation *practice* and *teaching*; for example, Vermeer's highly influential *Skopostheorie* (e.g. 1983),⁶ or Holz-Mänttari's more idiosyncratic *Handlungstheorie* (1984). In fact, at times, I have been so much as accused of having devised a theory and a methodology which were only applicable to the study of 'abnormal' cultures like my own, or the Turkish one (e.g., Paker 1986), or the Arabic one (e.g., Somekh 1981a), or the old Irish one (e.g., Tymoczko

6. And see again Nord's limited application of the notion of 'translational norms' within the framework of *Skopostheorie* (Nord 1991).

1991), or even French culture (e.g., Lambert 1988, 1992, D'hulst 1982, 1987, Lambert, D'hulst and van Bragt 1985)...⁷

The truth of the matter is that most of the work of most of these skeptical scholars occurred in, and was applied to, traditions which had been in contact with each other for a long time, and the interference involved in that contact had led, as cultural contacts so often do, to considerable approximation of the initially different systems, bordering on outright convergence (Denison 1981). In situations of this kind, literary translation indeed tends to concur, to a relatively large extent, with the translation of literary texts, and attempts to reconstruct in target-language means the web of relationships exhibited by a source text often result in rather small deviations from literary acceptability, if any. Once the elements of contact accompanied by constant interference are neutralized, for the sake of argument, let alone in cases where they are simply not there, the implications of the distinction between the two senses of 'literary translation', and the two perspectives of addressing the issue, become very clear; so clear, in fact, that even the most restrained scholars cannot but smuggle in bits and pieces of a target-oriented approach when trying to explain what went on in an actual case.⁸ Let us therefore take a quick look at one famous case where the starting point indeed exhibited a huge gap coupled with almost nil previous contact.

When one goes over the English texts which were presented as translations of Japanese *haiku*, especially during the first decades of their occurrence, one encounters in them a long series of recurring phenomena. Thus, many of these texts reveal one or more of the following:

— END-RHYMES — phonetic, or sometimes visual — with the possible addition of INTERNAL RHYMES, such as in:⁹

- (3) The autumn gloaming deepens into **night**;
Back 'gainst the slowly-fading orange **light**,
On withered **bough** a lonely **crow** is sitting.

-
7. See also recent descriptive-explanatory studies on non-literary issues such as Goris' article (1993) on dubbing in the French tradition.
8. And see, for instance, Snell-Hornby's discussion of the German translation on Salman Rushdie's *Midnight's Children* (1987: 96-99) and Nord's discussion of exemplary passages out of Lewis Carroll's *Alice in Wonderland* (1991: 99-102).
9. The numbers in brackets refer to the Appendix, where 27 texts purporting to be translations of one Japanese poem are given.

- more or less heavy ALLITERATION, as in “BaRe BaRRen BRanch” (16), or most conspicuously in:

(14) On a BaRe BRanch
A ROOk ROOsts:
Autumn dusk.

- ENJAMBMENTS, or run-on lines, i.e., discrepancies (sometimes rather pronounced (2, 16)) between syntactic segmentation and line-division (1, 7),
- added TITLES such as the descriptive “A Crow on a Bare Branch” (5, 7), or even the interpretive “The End of Autumn” (13),
- an EVEN NUMBER OF LINES, two (2, 4, 5, 10) or four (17, and when the title is also taken into consideration: 5, 7, 13).
- Sometimes the lines are SYMMETRICAL too, i.e., they all contain the same number of syllables.

The most common end-rhyme in the English corpus of translations of the “crow” *haiku*, which I’ve been using as my immediate corpus (see Appendix), is – almost naturally, one would feel – “**now-bough**”, where the recurring word “now” itself represents EXPLICITATION, if not downright ADDITION, for the sake of rhyme. Other amplifications are common too, most notably added ADJECTIVES, or other explicit MODIFIERS, especially to the words denoting the bough (or tree) and the crow (e.g., “a black crow”, “a lonely crow”, “a solitary crow” or “a crow alone”).

All these features, and many more, have hardly any basis in the source text:

Kare-eda ni
Karasu-no tomari-keri
Aki-no-kure

[On a withered bough / A crow perched; / Autumn evening.]

Much less so do they have a basis in the literary model underlying it, by virtue of which the source text is attributed to the so-called ‘genre’ of *haiku*; in features which set this text-type apart from all other poetic types in *Japanese* literature, that is. Some of the features which characterize the English translations, e.g., the rhymes, the titles and the added modifiers, represent so much as a complete REVERSAL of the ones permitted by the originals. By contrast, **they are all drawn from the target repertoire of poetic markers**. Thus, their selection was no doubt intended to bring the end products close(r) to what may safely be regarded as ‘English poems’. (Not ‘English haiku’, of course, because no such thing existed at that time.)

Significantly enough, phenomena of this type are supplemented – and complemented – by the OBLITERATION of features of the source text and of its underlying model,

even those which are among their most salient. Most notably, only rarely is an English translation found where exactly 17 syllables are employed, which is probably THE *differentia specifica* of a Japanese *haiku*. Obviously, even if English and Japanese notions of a 'syllable' did concur (which they do not), nobody in the target culture would have been expected simply to count them, much less to look for an 'irregular' number such as 17, let alone in a 5-7-5 distribution. Which goes to show that the suppression of this particular feature draws from the very same kind of target orientation as the added features do: catering for the acceptability of the target text in terms of target literary requirements. In other words, performing 'literary translation' rather than merely translating the source text, either 'linguistically' or even 'textually'.

Similar trends have been shown by De Geest (1992: 40-43) to have dominated translational introduction of Japanese *haiku* into the Low Countries – the Netherlands and Flanders. (See also Hellemans 1981; De Geest 1988.) Thus, in the 40s, *haiku* poems were selected for translation (preliminary norm!) because they were conceived as concurring with a tendency towards *exoticism*, which then dominated the target culture. However, the way they were actually translated (operational norms!) was intended to draw each individual text as close as possible to the notion of a *Dutch/Flemish* poem:

Decorte renders the original *haiku* as regular quatrains, a translational strategy which results in a more 'acceptable' type of stanza; sometimes two separate *haiku* are even combined into one single poem. Moreover the specific atmosphere of the original poems has been subtly changed by adding a personalized touch. (De Geest 1992: 42)

Thirty years later, in the second phase of its introduction, Japanese *haiku* poetry was perceived as an outstanding example of such *suggestive realism* as was then popular in Dutch/Flemish culture. Now text selection was dominated by this conception, and the strategies adopted were concurrent with the need not to have the translations too far removed from that idea; again, a domestic need rather than a mere wish to reconstruct the source texts' 'web of relationships'. It did of course result in much more adequate translations, but by way of realizing interests which originated in the *recipient* culture itself, as a function of its own systemic constellation. Thus, in both periods, *haiku* was mainly submitted to 'literary' translation, except that in the time that has elapsed, the conditions of 'literariness' within the target culture in question have undergone an enormous change, as have their realizations in translational norms.

In the case of English and Dutch/Flemish, the operation proved a success: enhanced acceptability indeed resulted in growing acceptance. Consequently, at later stages, translators no longer felt as pressing a need to 'westernize' their texts as they had in the first phases. Moreover, both literatures ended up having

endogenic models of *haiku*, based to a large extent, but not exclusively on the norms which had crystallized in the translation of scores of Japanese texts. (For English, see, e.g., Swede 1981.) Of course, all this had nothing to do with the constraints under which the first-generation translators carried out their task. On the other hand, it seems to have had considerable bearing on the constraints which later translators adopted as their normative framework, as exemplified by the more recent examples of the “crow” *haiku*.

5. Appendix: 27 English translation of the “Crow” haiku

1. On a withered branch
A Crow is sitting
This autumn eve. (Aston, 1899)
2. The end of autumn, and some rooks
Are perched upon a withered branch.
(Chamberlain, 1902)
3. The autumn gloaming deepens into night;
Back 'gainst the slowly-fading orange light,
On withered bough a lonely crow is sitting.
(Walsh, 1916)
4. Lo! A crow sits on a bare bough,
'Tis a dreary autumn evening.
(Miyamori, 1930)
5. A CROW ON A BARE BRANCH
A crow is perched on a bare branch;
It is an autumn eve. (Miyamori, 1932)
6. On a withered branch
a crow has settled —
autumn nightfall. (Henderson, 1933)
7. A CROW ON A BARE BRANCH
Autumn evening now
A crow alone is perching
On a leafless bough. (Yasuda, 1947)
8. On a withered branch
A crow is perched
In the autumn evening. (Blyth, 1949)
9. Autumn evening:
A crow perched
On a withered bough. (Blyth, 1952)
10. Autumn evening —
A crow on a bare branch. (Rexroth, 1955)
11. On a withered branch
A crow has alighted —
Nightfall in autumn. (Keene, 1955)
12. On a withered bough
A crow alone is perching;
Autumn evening now. (Yasuda, 1957)
13. THE END OF AUTUMN
Autumn evening: on a withered bough
A solitary crow is sitting now.
14. On a bare branch
A rook roosts:
Autumn dusk. (Bowans, 1964)
15. A crow
Perched on a withered tree
In the autumn evening. (Blyth, 1964)

16. Bare barren branch on
which a crow has alighted autumn
Nightfall darkening. (? , 1964)
17. A black crow
Has settled himself
On a leafless tree
Fall on an autumn day. (Yuasa, 1966)
18. On a withered bow
a crow has settled...
autumn nightfall. (Amann, 1969)
19. On a bare branch
A crow is perched —
Autumn evening. (Ueda, 1970)
20. Barren branch;
balancing crow;
autumn dusk. (Cohen, 1972)
21. When the crow arrives
on the bare, withered branch
true night has come. (Cohen, 1972)
22. Bare branch and a
crow balanced on the twilight:
autumn nocturne. (Cohen, 1972)
23. On a leafless bough
A crow is perched —
The autumn dusk. (Giroux, 1974)
24. On a leafless bough
In the gathering autumn dusk:
A solitary crow! (Britton, 1974)
25. On a withered tree branch
a crow perches —
autumn dusk. (Sawa, 1978)
26. On a withered branch
A crow is perched:
An autumn evening. (Aitken, 1978)
27. On the dead branch
a crow settles —
autumn evening. (Akmakjian, 1979)

Possibilities and Implications

One objective of Translation Studies is no doubt to offer as good and as full as possible an account of what real-life translation *processes* involve. Towards this end, it is not enough to establish lists of factors which may exert influence on a person as s/he is translating. It is no less essential to establish how that person manoeuvres between constraints of various types and sources as s/he goes along, and to assess their interdependencies and relative force, given the circumstances under which the activity takes place. Whereas initial possibilities can often be established by speculative reasoning alone, the study of their workings can hardly be divorced from instances of actual performance. The obvious obstacle here is that translation processes do not lend themselves to direct observation, so that some indirect means of approaching them will always have to be used.

Attempts to rely on *retrospection* while trying to reconstruct internal give-and-take in the course of translational decision-making have often fallen short: to the extent that individual translators are able to recall what passed through their minds in the first place, there are too many factors which may intervene and tamper with the reconstruction even of those parts of which they were conscious, as they were translating. This in itself is enough to cast doubt on the reliability of translators' pronouncements as a source of data, whether these are offered voluntarily or solicited: one can never be sure just what they represent. Indeed, finalized translations can often be shown to be at odds with their translators' claims as made, e.g., in fore- or afterwords to the texts, in independent essays they sometimes write, in interviews they give and the like.¹ State-

1. And see Chapter 2 for a short discussion of the problematics of using normative pronouncements as sources for the establishment of the norms which have actually governed translation (Section 5).

ments elicited by researchers normally do not fare much better, as shown, for instance, in the overview given by Sandrock (1982: Chapter 2).

1. Trying to close in on the 'little black box'

For a long time, the more responsible, even though necessarily rather modest statements on actual decisions made by translators have thus been made on the basis of observable, and hence *external* data. Towards that end, a variety of methods was used, all having in common the fact that the only entities submitted to study were the final output and the text which was taken to have constituted the input of the process. An attempt was invariably made to draw conclusions by recourse to comparisons of target vs. source texts. These studies assumed a multiplicity of shapes, owing to differences in the theoretical frameworks in which they were executed, and the results were therefore far from congruent, even when applied to the very same pairs of texts. Recourse to comparisons as such does not imply repeatability, then, unless care is taken to perform the studies in the same theoretical framework and use exactly the same methods.

Needless to say, there is no way for retrospective studies of this kind to so much as *pretend* to offer a glimpse into the 'black box' itself, where translational considerations take place and decisions are made. All one can hope to arrive at, using any of their varieties, is the formulation of *explanatory hypotheses* capable of accounting for the establishment of (more often than not aspects and parts of) translated texts, preferably in a way which is not at odds with knowledge obtained from other sources, using other methods. Such hypotheses can be more or less plausible, but they are often impossible to validate, except by performing more and more comparisons of the same type in an attempt to come up with recurrent patterns. The underlying assumption here is that regularities of surface realization and/or translation-source relationships and shifts bear immediate witness to regularities of translational behaviour — a very convenient rationale for descriptive studies which, however, is not always that easy to justify, especially as one's corpus transcends the borders of a homogeneous group whose members can be assumed to have come into being under the same set of norms.²

2. And see the brief discussion of the possible psychological validity of the notion of 'bi-text', or the coupled pair of 'replacing + replaced segments', in Chapter 4.

More problematic still, the comparative method entails an inherent weakness precisely as far as translation *processes* are concerned.³ As long as it is only pairs of target vs. source texts that are available for study, there is no way of knowing how many different persons were actually involved in the establishment of a translation, playing how many different roles. Whatever the number, the common practice has been to collapse all of them into one persona and have that conjoined entity regarded as 'the translator'; this would appear to be the only feasible approach, if research applied to pairs of texts is to transcend superficial description. Among other things, this practice enhances the comparability of findings, which is a *sine qua non* for making any kind of justifiable generalization.

Very often, hypotheses which were formulated with respect to the translator and his/her doings are thus applied to a *hypothetical construct*; i.e., a functional entity mediating between two existing texts, rather than a definite person. Even in the case of the most prestigious translators, whose translational products may well have been tampered with least of all, one can never be sure just how many hands were actually involved in the establishment of the translation as we have it, and most cases are not as extreme in any case.

A similar claim could be made in terms of the exact way the target text came into being. After all, the process which led to its establishment may entail different kinds of activity, which may be widely dispersed in terms of both time, space, and agents. Again, for purposes of analysis, a whole range of possible activities, including, e.g., revising, [post-]editing (with or without recourse to the source text) and proofreading, has usually been collapsed into one. This practice is more justified in some cases than in others, but the point is that one can never be sure what situation one is facing when one sets out to compare a finalized translation to its source.

As long as comparisons are only executed for *descriptive* purposes, for instance as a means of uncovering the relations between a target- and a source-text and/or the shifts they involve, these weaknesses are relatively inconsequential: Referring to the TEXTS as such, accounts of this type do not necessitate any regard for the roles played by different persons performing different activities at different points in the gradual establishment of the translation. In fact, the proponents of comparative methods (e.g., van Leuven-Zwart 1989, 1990) often

3. By contrast, studies into the process, especially of the psycholinguistic denomination, often ignore the final version whose establishment forms its very *raison d'être*. This issue will be taken up in Chapter 12.

stop short of making *any* reference to the genesis of the texts they wish to study. This is not to say that studies of this kind too would not benefit a great deal from knowing whether the same attitudes were shared by all those involved in the production of a translation or whether a (direct or indirect) normative negotiation, maybe so much as a struggle took place, and if so – whose norms had the upper hand and on what grounds. Of course they would. The point is only that viable hypotheses can still be formulated even in the absence of such evidence.

By contrast, once *explanations* are sought, especially in terms of decisions and what may have governed them, this kind of information becomes very helpful. And when attention is turned to the PROCESS as an issue in itself, the absence of this information can hardly be justified any longer: If dubious statements on translational procedures or strategies are to be avoided, ways should be found to break the fictitious constructs of both 'translator' and 'translation process' into their components and to start relating them to each other.

In an attempt to study translation processes from closer proximity, recourse to *experimentation* has also begun. Specifically, a series of methods was devised which are normally applied to individuals, most of them as they are performing a more or less uninterrupted act. In the process, the two complementary problems mentioned above are considerably reduced and attempts are also made to keep as many other variables as possible under control. Some experimental methods go so far as to claim success in breaking open the 'black box' itself, but this is obviously exaggerated, to say the least. What they involve is normally little more than an extension of the array of *external(ized)* phenomena by eliciting new kinds of data and making them available for study. Experimentation will be taken up in some detail later on, when we have exhausted the kinds of evidence which come into being in a 'natural' way during translation itself (see Chapter 12). What will now be added to the list is the *interim decisions* made by translators on the way to the final version, as documented in manuscripts, typescripts, corrected galley-proofs and the like.

To the extent that interim versions are available, they can be taken to substantiate the good old notion of 'multiple stage translation' (Voegelin 1954), which has sunk into oblivion. This notion is based on the assumption that, as a rule, translators do not attain a result which they would regard as acceptable (under the norms they have subjected themselves to) in one fell swoop, but rather in a series of moves. This practice enables the translator to practice *self-corrective feedback* involving a constant *self-monitoring process*. From the point of view of the researcher it may be taken as good evidence of a **decision-making**

process (Wilss forthcoming), which is precisely what we are after. To be sure, what such sources offer is still *realized* decisions only, which have not only been formulated in language but also committed to paper, which can never be tantamount to all that goes on in the translator's mind as s/he is translating. On the other hand, it is a lot more than the target text alone. Moreover, and much more importantly, this type of data gets much closer to tapping processes of translation as they actually proceed, even though by no means 'in real time'.

True enough, it is not always easy to lay one's hands on the interim phases of the emergence of a translation, let alone all of them: Even if several versions are available, gaps will very often be found between them, testifying to missing links. Still, its limited availability notwithstanding, this kind of evidence has been used all too little. Thus, over ten years ago, R. R. K. Hartmann argued that

most translation theorists would of course acknowledge such a goal [i.e., getting more information on the successive stages of individual attempts] as desirable, but there are only very few ... who have systematically explored the possibilities of such an approach. (1981: 206)

This fact has not changed much since. In fact, in the one article wholly devoted to "Translation Comparison" and purporting to give a systematic classification of its various forms (Reiß 1981), this kind of study was not even mentioned. Moreover, when it *was* used, e.g., by Hartmann himself (1980: 69-71), the comparative treatment of the different versions left a lot to be desired.

At first sight it would seem that, in the computer age, this kind of study would soon become impossible. However, while it is true that less use is now made of paper, it is also true that there are possibilities of retaining everything that is put into the computer, which may actually offer *more* data than what used to be committed to paper in previous times: It stands to reason that a translator using a computer would put into it more rather than less than a translator using a typewriter, let alone a pen or pencil. The only question is whether modern-time translators would be *willing* to have all the versions they have produced retained, given the possibility that they could then be submitted to study; but then again, most translators in the past did not care to keep their drafts either.⁴

4. In this connection, mention should be made of an idea of the late German translator Elmar Tophoven, the founder of the "Europäische Übersetzer Kollegium" in Straelen; namely, to connect all the terminals in the rooms used by the translators to a main-frame computer where all the versions they produce will be kept for strictly scholarly purposes (see, e.g., Tophoven 1986). Tophoven even carried out some preliminary studies on the basis of the material stored in the computer, mainly of his own work. Other than that, very little seems to have come out of this interesting idea.

2. Tracing the emergence of a translation

Being essentially new, this kind of study seems to require a somewhat lengthy presentation before it can be applied to a textual sample. Although offering many possibilities, it also entails quite a number of methodological problems. Thus, uncovering translational replacements whose adoption was only temporary gives rise to questions which could not have been asked at all under other kinds of observation:

- why particular options, or options of a particular type, were promoted to translation solutions to begin with;
- why they were then rejected;
- why other replacements, or replacements of other types, were opted for instead, and so on and so forth, towards the one which remained fixed in the final version.

(Needless to say, so-called target texts may also entail the re-adoption of previously rejected options, an interesting possibility for anybody thinking in terms of internal negotiation as part of decision-making, which is completely overlooked when any other retrospective method is used.)

Of course, there is no guarantee that satisfactory answers will always be found, but the very possibility of adding new types of *questions* should be considered an important step forward in any field of study.

To the extent that a manuscript, typescript or set of corrected proofs shows traces of revision, they would first have to be broken down into the layers they are found to comprise. When a document is relatively tidy, this procedure is simple enough. On the other hand, the insights it is likely to offer are rather marginal. The procedure gains in complexity as the number and density of revisions grow, but the increased significance of the document as a source of data on translation as a process which involves real *monitoring* is bound to be even greater. At the same time, the very possibility of distinguishing between textual segments, or linguistic phenomena, which have posed serious, and/or constant problems to the translator (represented by a multiplicity of successive revisions) and segments which have posed very few, or even none, will certainly enhance our understanding of the constraints to which translators choose to subject themselves and of the interdependencies and relative force of these constraints.

To be sure, there is absolutely no need for this reconstructed distinction between the more and the less problematic to be identical to the one yielded by an analysis of the source text as such, or even to its translatability vis-à-vis the

target language in question. It is well known that translational conventions often crystallize with respect to particular phenomena, including ones which would have been marked 'problematic' from the initial contrastive point of view. The result may well be a process of [almost] automatic replacement, which would render these same phenomena 'non-problematic' from the point of view of translation and of the strategies it involves. Then again, initially 'non-problematic' phenomena may be found to have posed serious problems; e.g., if and when no conventions have evolved with respect to their translation into the target language in question. (See note 7 to Chapter 3.) Be that as it may, the relationships between the two notions of 'translation problem' would be of the utmost interest for any understanding of the translation process. As would its results, one hastens to add.

To ensure a solid basis for making conjectures about the decisions met in the act, and especially about the considerations which may have motivated them, it is vital that, for each point in the text, the various layers discerned be arranged *in their correct order*, which is not always easy. On the other hand, as long as the documents, or at least the revisions, are in longhand, the intervention of different hands (e.g., the translator's, editor's, proofreader's) is normally not too difficult to discern. This is another advantage of the method, even though, admittedly, the question of whether their respective contributions should be regarded as different activities or just as successive phases of one and the same process will not have been solved yet.

Consider the English translation of the following German sentence of Erich Maria Remarque's novel *Im Westen nichts Neues* as it gradually comes into being in A. Wesley Wheen's manuscript (ms), typescript (ts), proofs (pr) and pre-print (pp):

Gestern wurden wir abgelöst; jetzt haben wir den Magen voll weißer Bohnen und Rindfleisch und sind satt und zufrieden.

(ms) Yesterday we were relieved; now with a belly stomachs full of haricots and beef beans pork and beans bully-beef and beans we are stuffed replete and at peace.

(ts) Yesterday we were relieved; now we have now our stomachs bellies are full of bullybeef and beans haricot hash and we are satisfied and at peace.

(pr) Yesterday we were relieved; and now our bellies are full of beef and haricot hash beans and we are satisfied and at peace.

(pp) Yesterday we were relieved; now, and now our bellies are full of beef and haricot beans and we. We are satisfied and at peace.

To the extent that this presentation, reproduced from Hartmann (1980: 109; 1981: 206), is a faithful reflection of the original documents which the author found at the Australian National Library in Canberra, it can serve as a basis for reconstructing the successive stages of the emergence of the text.

Thus, the emergence of the translation of the highlighted clause can be represented as follows:

- | | |
|---|------|
| 1. now with a belly | (ms) |
| 2. now with stomachs full of haricots and beef | (ms) |
| 3. now with stomachs full of beans | (ms) |
| 4. now with stomachs full of pork and beans | (ms) |
| 5. now with stomachs full of bully-beef and beans | (ms) |
| 6. now we have | (ts) |
| 7. now our stomachs | (ts) |
| 8. now our bellies are full of bullybeef and beans | (ts) |
| 9. now our bellies are full of beef and haricot hash | (ts) |
| 10. and now our bellies are full of beef and haricot hash | (pr) |
| 11. and now our bellies are full of beef and haricot beans | (pr) |
| 12. and now our bellies are full of beef and haricot beans. | (pp) |

In this particular case, the number of successive stages is as high as twelve. However, the number is not the same even for other portions of the same sentence. Thus, the replacement of the segment *und sind satt und zufrieden* can be broken down into four layers only, with a possible missing link between (2) and (3):

- | | |
|---------------------------------------|----------|
| 1. we are stuffed | (ms) |
| 2. we are replete and at peace. | (ms) |
| 3. and we are satisfied and at peace. | (ts, pr) |
| 4. We are satisfied and at peace. | (pp) |

and the translation of *Gestern wurden wir abgelöst*; seems to have been born in its final form:

Yesterday we were relieved;

The expectation that the number of layers will remain constant throughout the text is even lower.

In order to do justice to the difference in the number of discernible layers (= stages), the revisions themselves should be taken to define the coupled pairs which would be submitted to comparative analysis (see Chapter 4). To be sure, not only will the replacements as such be found to have changed, but fluctuations too will often manifest themselves in terms of the *units* with which the translator may be said to have operated and the types of *relationships* which tie

their coupled members together; another obvious approximation of real-life processes of translation as well as a consequential continuation of our basic research method.

What one would have at one's disposal, as a result of breaking each version into layers and ordering them correctly, is a series of related outputs of varying scope and level. The main value of the procedure lies in the possibility it offers of performing *multifarious comparisons*. Thus, each output can be confronted not only with the corresponding *source-language* segment (to the extent that it is the same one), but with *its own alternatives* too – at least those which were verbalized in the versions under study. What should not be overlooked is the fact that the interim outputs **do not constitute a succession of texts**, let alone independent ones. They are just **phases in the emergence of one text**. Even though a revision may well have, and often has implications beyond itself – in extreme cases even global ones, for the text as a whole – the fact is that it is always textual-linguistic *units*, often rather small, and low-level ones, that are revised. As the very number of revisions itself tends to differ from one (reconstructed) unit to another, no justification can normally be given for reducing one multi-layered version to a number of self-contained texts, much less so to a number which would correspond to the number of layers it has been found to comprise: Textual *considerations* may have taken part in (some of) the decisions, but no independent texts came into being as a result.

Let us have another look at the Remarque clause which was broken down into twelve layers, and try to make a few observations with respect to the translator's revisions. To be sure, these revisions offer regularities sufficient to warrant some *generalizations* with respect to at least the **monitoring of the emerging target text** for (partly negative, partly positive) **discourse transfer**, which manifests itself as one of the aspects which When lay special emphasis on – increasingly so with each successive revision.

- a. The translator started by gradually replacing *jetzt haben wir den Magen [voll]* by 'now with a belly' (stage (1)), which would require a complementation by 'full of X' as a replacement of the original *[voll] weißer Bohnen und Rindfleisch*. Within this construction, *Magen* was replaced by 'a belly', both nouns being in the singular. Thus, the grammatical number used in the source text was directly transferred into the target text. This is probably the clearest, even though by no means the only indication of the small textual span which When took into account when making his first decisions; in this particular case, it was probably the one word alone.

- b. After writing down the words 'a belly', the translator halted and crossed them out. Presumably, before he came to actually putting down the expected complementation, a larger co-text came into play which made him realize that he may have created an acceptability problem; in other words, that the transferred feature represented so-called 'negative' transfer, in this particular context: not only is the subject of the *original* sentence in the plural (*wir*), but he himself had started his version that same way, using the pronoun 'we' twice ('We are lying six miles behind the front' and 'Yesterday we were relieved').
- c. To replace the erased words he now writes down 'stomachs', in the plural. The fact that the noun itself was changed too implies that, while realizing the *grammatical* problem, he also took some other considerations into account, involving *semantics* and *stylistics*. To be sure, the decision concerning the noun will be reverted in stage (8). By contrast, the grammatical feature 'plural' will now have been fixed for good.
- d. The time has come for the translator to complement the clause, at this point, and he does so in the expected way: 'full of X'. However, when filling in the slot X, another instance of transfer is introduced into the emerging translation, namely, the order of the members of the compound *weiße Bohnen und Rindfleisch*. This order, which is not really of the 'negative' type, is retained in stage (3) too. It is then reversed (stage (4)), probably to adapt the formulation to English habits, and hence bring the translation still closer to meeting its prospective reader's expectations of a native-like text.
- e. In stage (6), a new transfer phenomenon is introduced, 'now we have' as a replacement of the German onset of the clause, *jetzt haben wir*. The most plausible explanation of this version is that the translator, who had had his manuscript typed, in the meantime, compared the typescript to the source text again and was unhappy with the *structural distance* between 'now with stomachs' and the original formulation. In an attempt to solve what he considered a problem, he reactivated the source-language formulation to produce a closer rendering. However, this interim product was immediately monitored (stage (7)), and no other recourse to the source text occurred in the translation of this particular segment. At the same time, the explicit reintroduction of the feature 'first person plural' was retained (in 'our stomachs/bellies'), which the previous versions (stages (1)–(5)) only implied. Thus, the approximation of the source text is only partial, being mitigated by a distinct wish to reduce interference.
- f. The syntactic status of the English replacement as a clause – which is what the corresponding original segment is – was retained as far as stage (11). It was only made an independent sentence at the very last stage of the genesis of the transla-

tion (12), which is something no student of the final version alone could have come up with. One possible explanation for this last-minute change is that before approving it for publication, somebody (the translator himself? an editor? a proofreader?) read the translation as a *piece of target-language text*, checking it for stylistic acceptability.

Such a study may give rise to the following kind of general observation: to the extent that interim replacements refer to one and the same source segment, there is room for speaking in terms of a 'translation paradigm'. This, however, would not be Levenston's 1965 paradigm, which is basically *prospective*, but a *retrospective* one, encompassing realized decisions only – the same change of meaning that the notion of 'problem' underwent. On occasion, the two notions may of course overlap, in part or even in full. The point is, however, that they needn't. Thus, there is no need for a retrospective paradigm to include all the options that a prospective one would (i.e., that an individual translator would have tried them all out). On the other hand, the retrospective paradigm may also include options which are not part of the prospective one (that is, that the translator, in the act under study, would have tried out but which would not have counted as part of the prospective paradigm), on occasion even as the *final* replacement.

3. Possible implications for Translation Theory

It is clear that this type of study, and the conclusions it allows, can have implications far beyond themselves. In the long run, they may even assist in putting to some kind of test theoretical hypotheses about the process of translation, especially those resulting from sheer speculation.

Consider the following insightful statement offered a few years ago by Vladimir Ivir:

The translator begins his search for translation equivalence from formal correspondence, and it is only when the identical-meaning formal correspondent is either not available or not able to ensure equivalence that he resorts to formal correspondents with not-quite-identical meanings or to structural and semantic shifts which destroy formal correspondence altogether. (Ivir 1981: 58)

In this statement, translation is accounted for in terms of a 'monitor model', which should make it compatible with the nature of our findings with respect to

the Remarque example and accommodate comparisons for the sake of verification/refutation.

To be sure, it is far from clear whether Ivir had in mind real-life processes or ideal(ized) ones and whether his formulation was offered as a *theoretical* pronouncement or a *descriptive* one. (See Part One, Section 4.) In fact, in spite of the lack of modal verbs, the author may also have intended to present it as a recommendation for translators, in which case it would be a *normative* pronouncement [as well]. Be that as it may, tracing the emergence of the Remarque clause in its English translation allows us to say at least this much: that Ivir's characterization of the process of translation is not completely unfounded. After all, a case has been found where the kind of reasoning he used was applied, at least to a considerable extent.

Of course, a seeming verification by just one case has very limited value: Individual cases should not be taken to simply validate a hypothesis, at least so long as their representativeness has not been established. By contrast, any *falsification* of a hypothesis would shed considerable light on its validity. In fact, if a hypothesis which was put in question is to be retained at all, it will have to undergo *modification*, namely, by the specification of circumstances under which this kind of behaviour tends to occur or to give way to other strategies. Incidentally, from the theoretical point of view, such a relativization would count as a huge step forward.

Unfortunately, the investigation of the emergence of individual translations is still on the very margin of DTS. Due to a shortage of findings, there is therefore very little to go by, in terms of such a relativization. The next chapter will thus be devoted to adding to the limited inventory of case studies, which is in such urgent need of enrichment, in sheer numbers as well as in terms of the variety of behaviours. Incidentally, the case which I will be presenting, while still involving a kind of *monitoring*, will be found to be a little less supportive of Ivir's hypothetical characterization than was the Remarque translation.

Chapter 10

A Translation Comes into Being

Hamlet's Monologue in Hebrew

In this chapter, an account of the successive revisions made by one translator while working on one textual segment of considerable independence will be presented in some detail. In the process, an attempt will be made to uncover the *constraints* to which that translator subjected himself as he went along and the way he manoeuvred among them. This will include a glimpse at the *interdependencies* of the various constraints and at the *relative force* of different ones, which is the closest one could get to establishing the norms which governed this particular act of translation. In addition to its constituting a case study in its own right, this chapter is also designed to serve as an extended illustration of the methodological claims concerning the use of interim solutions in the tentative reconstruction of a translation process.

1. The materials under study

The text I have chosen is Avraham Shlonsky's Hebrew translation of Hamlet's monologue "To Be or Not To Be" (*Hamlet* III:1) made in 1946, first and foremost for staging at the Habimah theatre. At that time, Shlonsky held a central position in Hebrew culture at large. In fact, he was considered the epitome of a well-defined set of literary and translational norms known as 'modernist', and all those who abided by those norms, especially in the translational domain, were regarded – not always justifiably – as his disciples. No wonder, then, that it didn't take long for Shlonsky's *Hamlet* to achieve canonization and completely

supplant the previous versions of the play,¹ which had never attained any real canonization anyway, either in the theatrical or even in the literary system. Moreover, Shlonsky's *Hamlet* retained this status for several decades. In fact, in spite of a considerable change of 'mainstream' norms, various sectors of Israeli culture still cling to it, and not just out of nostalgia. Rather, as is so often the case (see Chapter 2), norms which had become obsolete did not disappear completely; though no longer in the cultural epicenter, they even enjoy a certain prominence.

My main source of data will be a *manuscript* which — judging from the quantity and nature of revisions — represents Shlonsky's very first draft. As such, it comes as close as possible to representing a single, though not necessarily an uninterrupted activity. This manuscript (No. 3:41–52) forms part of the Shlonsky Archive at the Katz Institute for the Study of Hebrew Literature, Tel Aviv University.² The pertinent pages bear the numbers 5–7, which makes it clear that the monologue was not the first extract to be translated. (Page 6 is reproduced as Figure 11.) This is not to say that the play was necessarily translated according to its original order: bunches of pages could well have been numbered separately; e.g., when handed over to the typist. Fortunately, this uncertainty does not seem to affect our account of the emergence of the translated monologue as such.

In addition to the manuscript, the Shlonsky Archive also has a *typescript* (No. 3:41–53), whose basic form, however, is not identical to the final layer of the manuscript. Since it is highly improbable that a typist should intervene with a text submitted by a translator of such standing, there is no escaping the conclusion that a link is missing; possibly a clean(er) copy of the manuscript. After being typed, this version was revised by the translator, in his own handwriting. Interestingly enough, these revisions lead in two slightly different directions: one set approximates the text to the version which was then staged,

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1. H.Y. Bornstein's translation, made in Warsaw, was first published (in installments) in the 27th volume of the Zionist periodical *Ha-tsifra* (1900). It did not appear in book form until 1925, still in Warsaw. S.H. Davidovitz's translation was made in Palestine and its publication preceded that of Shlonsky's version by less than five years (1941).
 2. I would like to thank the Katz Institute for the Study of Hebrew Literature, Tel Aviv University, and its former director, Prof. Reuven Tsur, for their permission to use the material and reproduce portions of it. So far, it has proven impossible to locate p. 8 of the bunch in question, which should have included the last five lines of the translated monologue.

whereas the other brings the typescript closer to the first book version.³ To be on the safe side, the two *final versions*, both dated 1946, were also consulted.

Thus, even though we do not have all the interim phases of the monologue, or even all those which were actually committed to paper, we do have quite a few of them, which allows for a thorough survey of the translator's 'laboratory' where solutions were devised, rejected and replaced by others. Moreover, with very few exceptions, it is not too difficult to tease apart the layers comprising each version and put them in their correct order, which is a precondition for any justification of observations in terms of a reconstructed translation process. As to the source text: throughout most of the discussion, it will be found to play a marginal role. Therefore it will be referred to only if and when this is justified by the discussion itself. This practical, or temporary suspension of the source text, discussed rather extensively in the methodological part of the book, will be taken up again in Section 4.

2. Prosodic constraints and the unit of consideration

It is not difficult to show that the text was translated under a rigid **prosodic constraint**, which Shlonsky had already subjected himself to before he ever set out to translate this particular monologue: each one of the lines constitutes one iambic hexameter. In this respect, very few fluctuations or hesitations can be observed, either in the manuscript or at any later stage in the emergence of the translation. Whatever alternatives were considered and whatever changes were made were in line with this constraint, or else were rejected right away. In fact, in terms of variations and deviations, Shlonsky proves to be much more of a *papist* than the author himself. The rigidity with which he strives to adhere to the metrical scheme in question is further attested to by several markings along the manuscript. Evidently, he felt an occasional urge to check the metrical suitability of his verses.

Of course, the Hebrew iambic hexameters are not an immediate reflection of Shakespeare's verses, which are *pentametric* in nature. Rather, they represent

3. The distinction between the translation of a drama as a text (to be read) and as a script (to serve as a basis for theatrical performance) has been gaining ground, most notably in the Ringprojekt "Drama und Theater" of the *Sonderforschungsbereich* 309: "Die literarische Übersetzung", Georg-August-Universität Göttingen. It has already led to some interesting findings. See, e.g., Ranke 1993.

a compromise between the original format and the need to accommodate the typically greater number of syllables of the average Hebrew word. This difference makes it very difficult, often practically impossible, to pack the entire informational contents occupying ten English syllables into a ten-syllable unit in Hebrew. In all likelihood, it was mainly this need that led to the addition of a metrical foot to each line, even though the practice may also have been inspired by a certain tradition of *Russian* poetry. At the same time, no attempt was made to fully 'domesticate' the meter; e.g., by replacing English iambs by Hebrew *anapests*. This last option would certainly have offered a far more convenient way out of the difficulty of reconstructing the necessary semantic content. It would also have been more in line with the nature of the Hebrew language in its 'Sephardic' pronunciation. Since the anapest is so very characteristic of Hebrew poetry, it would also have counted as no less functionally equivalent to the original meter than the iambic hexameter. On the other hand, it would probably have been regarded as too much of a deviance from the *formal* properties of the original and of the tradition it represents.

Nor was this compromise really Shlonsky's. Rather, at that point in time, such a practice was well within the norms governing the translation of English poetry couched in iambic pentameters into Hebrew. All that Shlonsky was required to do was to decide whether to adhere to that sanctioned behaviour – or run the risk of producing a [translated] text which would count as *deviant* (on this particular account). In view of his translational behaviour throughout, it is hardly surprising that he opted for adhering to such a dominant target norm and never really tried to transcend its boundaries.

The replacement of every source-text pentameter by a target-text hexameter did more than pack more material into the translated text without adding extra lines – a kind of solution which had become marginal, almost obsolete, some time before Shlonsky's performance. It also made it possible to treat the verse as the ultimate unit of translational consideration, with all the necessary adjustments being made within the confines of a single poetic line. This then is the second global constraint to which Shlonsky subjected himself, and in this respect there seem to have been no hesitations or fluctuations either.

By contrast, Shlonsky's hexameters do not always observe in full the rules of Hebrew normative grammar. In fact, in terms of linguistic preferences, his position is an interim one between succumbing to grammatical constraints which apply to the *written* varieties of the language and attempting to establish a text which would be *speakable* under very specific conditions. Thus, the translation in question was commissioned by a *theatre*, and was therefore

intended first and foremost to be performed. Notwithstanding, what the norm which governed Hebrew theatre of the mid-forties required was a far cry from a simulation of any kind of language actually spoken in the country. Rather, it was a theatrical convention of *declamation* appropriated from Russian theatre of a previous period and only slightly adapted to either the possibilities or even the requirements of spoken Hebrew.

Before taking up the revisions themselves as a source of data for the reconstruction of some other constraints, let me exemplify what has been found relevant so far on the basis of the first line of the translated monologue.

This line is the only one in the text which was written down in its final form right away. Only one tiny thing was left undecided, namely, whether the line would read

lihyot o lo lihyot? hine ha-še'ela

or

lihyot o lo lihyot? hine hi haš'ela

(The copula *hi* was added in parentheses above the line, between the words *hine* and *haš'ela*.)

In view of the basic prosodic constraint and the adherence to the verse as the ultimate unit of translation consideration, Shlonsky must have had no problem leaving this question unresolved, in the interim: both alternatives occupy exactly six iambs while constituting a full replacement of the source-text's first line; no missing parts, no spill-over to the next line. The two contending options differ in their *grammaticality*, on the one hand, and *declaimability*, on the other, lending each of them both advantages and disadvantages, within the overall set of norms which the translator subjected himself to.

Grammatically speaking, *hine ha-še'ela*, where the initial consonant of the word *še'ela* [question] is followed by a mobile sheva, would have had the upper hand, because such a sheva (says normative grammar) should always be realized as a short vowel. Within the iambic scheme, the semi-syllable *še* thus created is metrically stressed, which would have counted as a flaw in terms of any authentic pronunciation of spoken Hebrew. On the other hand, when related to the norms of poetic diction, this flaw loses much of its force, and virtually disappears in theatrical declamation of the period.

By contrast, within the dominant metrical scheme, the sheva following the *š* in *hine hi haš'ela* would be quiescent, i.e., a zero vowel, which matches the stressed part of the iamb with the (phonetic) syllable *haš*. This pronunciation, which involves the accentuation of a definite article, is abnormal in spoken Hebrew, whereas the occurrence of a quiescent sheva in this position is a serious flaw from the point of view of normative grammar. Prosodically, this lack of an unstressed syllable is offset by the introduction of

the copula *hi*, which, in turn, poses problems of *stylistic acceptability* in sentences of this type. It stands to reason that Shlonsky himself would have been concerned about precisely these considerations.

Except for this hesitation, which – in spite of the many revisions made throughout the manuscript – was left unresolved, the first line was born ready-made. The reason seems to be the success with which the Hebrew version concurs at once with the needs of establishing a six-iamb unit as well as having all the decisions made within a single verse. At the same time, this line represents a very close reflection of the original verse too, which may have been taken as another point of strength:⁴

To | be | or | not | to | be, – | that is | the | question
li | hyot | o | lo | li | hyot? || hine (hi) | ha- | š(e)ʿela

Finally, the choice of the pragmatic connector *hine*, which, from the stylistic point of view, is slightly different from the other lexical choices in the verse, lends the latter an extra tinge of *poeticness*, in keeping with contemporary convention.

Of course, at one point or another, Shlonsky would have to choose between the two alternatives, or come up with a third. However, he went on struggling with the pros and cons of various options in subsequent versions of his translation. Thus, whereas the typescript reflects a previous decision in favour of the first option, Shlonsky then crossed out *hine* and wrote the less ‘poetic’ word *ve-zo* [and that (is)] instead. A seemingly small change, it is in fact highly significant; the fact that simple *zo*, the most normal way of expressing (in writing) ‘that is’ in the feminine (necessitated by the gender of the Hebrew noun *š(e)ʿela*), was never resorted to, no doubt because it lacks a syllable for the all-important completion of an iamb, is highly indicative of the relative force of the different constraints: Shlonsky was sometimes ready to pay a certain price in terms of *poeticness*, at least temporarily, but was never willing to consider a similar price in the *prosodic* domain.

It was the version with *ve-zo* that went into the first book edition. By contrast, in the version intended for the theatre, this word was again crossed out and the more ‘poetic’ *hine* was reintroduced. The same holds true for later editions of the book as well as for the version, performed in 1966 by the Chamber Theatre (the Shlonsky Archive, No. 3:41–51, p. 66). Thus, Shlonsky’s deviation from the kind of language he regarded as preferable was only temporary. Indeed, as attested by many places throughout the

4. Davidovitz’s translation of the monologue (see note 1), which had an almost identical rendering of the first line, must have been known to Shlonsky. However, judging from the rest of his translation, he hardly made use of it except, maybe, in a *polemical* way. (A polemical translation is “an intentional translation in which the translator’s operations are directed against another translator’s operations that are representative of a different or antagonistic conception” [E. Balcerzan, quoted in Popovič [1976]: 21].)

monologue, recourse to poetic language (according to the prevailing norms, and independent of the features which characterize Shakespeare's own use) is another constraint of the translation, and a rather strong one too: Shlonsky will often be found to manoeuvre within the confines of this norm, but he hardly ever transcends them. To put it differently, whenever a target-language item is substituted for another, both the replacing and the replaced items tend to belong to approximately the same type of language.

3. Using revisions to uncover constraints

Even though the rest of the monologue was not born as easily, Shlonsky's submission to the constraints already uncovered, and the way he manoeuvred among them, are very clear throughout. In fact, they are much easier to detect elsewhere, precisely because of the greater number of revisions and their high regularity. Thus, one can easily show how, from one stage to the next, the translation gets closer to satisfying an array of target-dominated constraints in terms of the translator's own concept – the one which dominated the culture within which and for which he was acting – as to what would constitute a proper *theatrical* text.

To be sure, as one observes the extent of manoeuvring in the rest of the monologue, one can piece together more and more of the constraints which must have governed its translation. For instance, beyond the privileged status of the poetic line as an ultimate unit of translational consideration, there also appears to be a clear tendency for this unit to be **cut in two**, preferably **symmetrical parts**. Such a tendency is often a concomitant of the very decision to use hexameter, and a larger corpus may even substantiate the hypothesis that the convenience of working on half-verses further contributed to the adoption of this basic prosodic norm, to begin with. An almost inevitable outcome of the natural symmetrical division of a hexameter, in turn, is a considerable rate of *monotony*, which is not necessarily marked negative, when it comes to declaimability.

To be sure, in some cases, the original verse falls into two equivalent parts as well; still, Shlonsky's application of the same principle to verses where the original entails no division, or a division of a different kind, gives extra weight to the conclusion that this practice involved first and foremost a strategic decision of the translator himself, possibly in accord with the poetics he epitomized. Consequently, the few cases where this strategy is in keeping with

the original may be considered a happy coincidence, the more so as Shlonsky did not really use the English text as his immediate source. By contrast, the practices of whatever translation(s) he may have relied on, primarily translation(s) into Russian, may well have played a role in forming this strategy.

To take an example: Shlonsky initially opened his translation of line 3 with the clause *et kol xitsav* [all the arrows (of)] – close enough to the source text, except for the added quantifier *kol* [all] which completes the first iamb. (The nature of the declined noun *xitsav* as one full iamb cannot be manipulated.) Within the confines of Hebrew grammar, this opening requires the subsequent occurrence of the particle *šel* [of] plus a second noun phrase, e.g., *goral axzar* (to replace ‘outrageous fortune’ of the original or any close translation thereof).

At this stage he must have become aware of the fact that he had used up two whole iambs but achieved very little, in terms of reconstructing the semantic contents and verbal formulation of the line. A version such as

**ēt kōl xitsāv šel gōrāl āxzār*

would have been much too long (9 syllables out of the 12–13 at his disposal for the whole verse) and problematic from the prosodic point of view (a superfluous unstressed vowel introduced with the word *šel*). Complementing the verse to something like

**ēt kōl xitsāv ū-qlā’āv šel gōrāl āxzār*

would have involved him in a still greater number of deviations from the all-important prosodic requirement. Neither could he adopt a solution such as the following:

**ēt kōl xitsē vē-qlā’ē gōrāl āxzār*

This time, (minor) deviation from the prosodic scheme would not have been the only reason for rejection. Rather, the main reason for the impossibility of accepting such a formulation centers on the use of two conjoint dependent nouns in the construct state. While such a construction could have been used by many a writer, it was regarded as a breach of the norms of acceptable style, sometimes as grave as breaking the very rules of normative grammar. Shlonsky thus gave up the whole opening, returned to the beginning of the verse (but no further back!), and adopted a completely different strategy: he divided the line in two and started manoeuvring within each half separately.

The second version Shlonsky actually wrote down reads as follows:

xitsē gōrāl āxzār || vē-xōl āvnē qlā’āv

The result comprises two completely symmetrical halves, 3 + 3 iambs, and a fully grammatical sentence, which only entails one problem: it still has the prosodically motivated word *kol* (here pronounced *xol*), this time to complement the fourth iamb.

In an attempt to get rid of this word, which is superfluous in terms of the semantic reconstruction of the source text, Shlonsky then established a third version, with no retraction from any of the major decisions he had already made:

xītsē gōrāl āxzār || āvnē blīstrā'ōtāv

This formulation has an additional advantage over the previous one, namely, from the stylistic point of view: *blīstrā'ot* is more poetically marked than its near-synonym *qla'im* which was used in the previous version. (The question as to which one of the two is a closer semantic equivalent of the original 'slings' was definitely secondary, in the hierarchical order of Shlonsky's constraints, even though questions of semantic equivalence were by no means ignored.) The only problem with *blīstrā'otav* seems to be that it is a monster of a word in terms of theatrical performance: not many actors would be able to pronounce it properly, not even while declaiming. Consequently, a fourth version comes into being:

xītsē gōrāl āxzār || āvnē mārgēmōtāv

Not only is *avne margemotav* easier to pronounce, but it would also have counted as more poetic still than *avne blīstrā'otav*; not necessarily on account of the stylistic markedness of the words as such (where there is practically no difference between the two), but certainly in terms of their position and functions in the verse. Such a formulation makes it possible to establish a *local sound-pattern* of the *neutral* type.⁵ This pattern, which is based on the recurrence of the sounds E-G-O-R-A-(M), finds its fullest embodiment precisely in the newly introduced word *margemotav*:

xītsE GORAI AxzAR || AvnE MARGEMOTAV

In fact, very few of the sounds of the verse do *not* figure in the pattern, whose main, if not only value is ornamental. To be sure, such a sound-pattern is bound to be highly noticeable precisely when the monologue is being *declaimed*.

As it turns out, we have hit upon another consideration which directed Shlonsky in his revisions, within the general attempt to be 'poetic'; namely, **rich orchestration** — not necessarily on the basis of Shakespeare's practice, to be sure, but most definitely in terms of the governing poetics of the time, in original writing as well as in translation. Of course, phenomena catering to this

5. A neutral sound-pattern is one where no relation between any inherent qualities of sounds and particular meanings of words can be detected (Hrushovski 1968). Such patterns reinforce the density of the texture of a text and thus may be said to activate or enhance the so-called 'poetic function' (Jakobson 1960).

need can be observed in the final version too, but only an ordered comparison of the successive phases of the emergence of the translated monologue can reveal the amount of trouble Shlonsky went to, in his attempt to establish this orchestration.

A few additional examples of this phenomenon will make clear its growing centrality in the translator's considerations as he moved towards the final version of the monologue:

line 16:

first half: *et ol he-aritsim* → *ulam šel aritsim* → *xamas he-aritsim*

second half: *buzam šel geyonim* → *buzam šel ha-ge'im* → *buzam šel šaxtsanim* [in the typescript]

final version: *XAMAS he-AriTSiM, || buZAM šel ŠAXTSAniM*

line 17:

first half: *tša'ar ha-(a)hava ha-nixzava* → *tša'ar ah(a)va nixzevet* → *ke'ev ah(a)va nixzevet* → *xavle ah(a)va nixzevet*, and, at the end, *AtSEVET Ah(a)VA nixZEVET* (a rich sound-pattern coupled with an internal rhyme, the price for which is one of the rare deviations from the basic metrical scheme)

line 18:

et rahav ha-mošlim || ve-xol ha-bizyonot → *AZuT hA-ŠAlITiM || ve-xOl ha-biZyOnOT*

4. Conclusions and implications

All in all, we have been able to uncover six major constraints to which Shlonsky subjected himself in his translation and which account not only for the final version of the monologue, but also for his interim decisions and his successive revisions:

- (1) To operate within one verse unit
- (2) which would eventually constitute a Hebrew iambic hexameter
- (3) tending to fall into two preferably symmetrical parts.
- (4) To establish a script suitable for declamation in a theatrical performance rather than a text for silent reading.
- (5) To demonstrate richness and stylistic elevation in his linguistic choices
- (6) as well as in their organization into higher-level segments, especially in terms of sound orchestration.

There can hardly be a doubt that what all these constraints have in common is the fact that they reflect first and foremost the interests and needs of the *recipient* culture at that particular time.

As we have already implied, this should not be taken to imply that Shlonsky – or the culture in which and for which he was translating – attached no importance at all to reconstructing the source text's features, especially the semantic ones. On the contrary. A certain aspiration for adequacy should therefore be added to our list of constraints. This aspiration, however, would rank hierarchically *lower* than most of the constraints listed above. This means that out of a list of options which are reasonably adequate as translational replacements of a certain source-text segment, the one which tended to be preferred was the one which would have satisfied the greatest number of the remaining criteria, all of them ostensibly target-oriented – even if this involved bypassing more adequate alternatives. Be that as it may, the result is a translation whose *general* character (rather than this or that detail in it) differs from the source text's – and in a direction which indeed contributes to its acceptability into the target culture.⁶

In fact, while attempts to cater for requirements originating in the (immediate) source text(s) seem to have played a part mainly in the first tentative solutions, the aspiration to approximate the translation to the original had only marginal bearing on the translator's subsequent revisions. Rather, as reconstructed, his concern was more and more with the position of his own text in a particular niche of the *recipient* culture. So marked was his preference for acceptability that even the question of what text he actually proceeded from manifests itself as marginal: Intriguing as it may be, the conclusive identification of that text, or those texts, would change very little in our understanding of the translation as a text, or even of the process by which it apparently came into being.

Finally, a similar account of the constraints which Shlonsky seems to have adopted in his translation of Hamlet's monologue could have been arrived at on the basis of the *finalized* version alone (as indeed it was, e.g., by Miron 1963). However, tracing the gradual emergence of the text supplies welcome support for such an account, precisely because the revisions are found to reveal so much consistency.

6. And see Even-Zohar's detailed account of the Hebrew translation of Baudelaire's "Spleen" by Lea Goldberg (1975), or Sandbank's descriptive-evaluative treatment of Goldberg's translations of Petrarch's sonnets (1975). To be sure, these translations originated in the same school, and were hence produced within the same set of norms.

Coming back to Ivir's characterization of the process of translation, which was used as an example of a speculative model (Chapter 9, Section 3), it is clear that this time, our reconstruction supplies *partial* verification, at best. The difference between the Hamlet case and the Remarque one may have to do with a multitude of factors: the source- and target-languages and their relationships, in purely linguistic terms as well as in terms of power relations; the difference in text-type (novel vs. drama); the difference in status and internal tradition between translation into Hebrew and translation into English; and many more. If this is the case, then we are on our way to the welcome *relativization* of Ivir's hypothesis, or of any other model of the translation process offered thus far, especially the more speculative among them.⁷ As we have already claimed, it is much too early to draw too general conclusions from a research method which is still in swaddling clothes. Be that as it may, one would certainly do well to bring the results obtained by using this method to bear on the results obtained by using other research methods, e.g., Thinking-Aloud Protocols – and vice versa.

7. See also Lörcher's critical account of five models "which claim to capture the translation process" in terms of their exactness, simplicity and similarity to their object (1989; 1991: 7-27). Not surprisingly, his final conclusion is that "this claim obviously does *not* coincide with reality" (1989: 63).

Translation-Specific Lexical Items and Their Lexicographical Treatment

Even if one is mainly concerned with acts of communication and with the conditions under which they take place, and would rather regard translation as a mode of generating texts induced by other texts (Neubert and Shreve 1992: 43) and/or intended for 'communication in translated utterances' (Toury 1980a: 11-18), there is no denying that any text is made of lower-level elements. To the extent that form-and-function combinations become habitual as lexical items, they are of course prone to lexicological and lexicographical treatment, items occurring in translations included.

Precisely because the relations between Translation Studies and Lexical Studies seem to offer so many possibilities of cooperation, it is quite discouraging to find out that they have normally been approached from one perspective only, that of the benefit future translators would allegedly derive if dictionaries were based on more 'adequate' treatment of the lexical material of languages or language pairs.¹ There is thus at least one issue of paramount theoretical, methodological and practical implications which tends to be overlooked, namely, the treatment lexical items occurring in translated texts deserve to get; especially those among them which are more or less *translation-specific*.

Lying as it does at the interface of Lexicology and Translation Studies,

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1. See, e.g., the Proceedings of a recent International Conference wholly devoted to "Translation and Lexicography" and which brought together many of the 'big names' in both fields (Snell-Hornby and Pöhl 1989). A preliminary version of this chapter was in fact presented as a paper at that same Conference and published in its Proceedings (pp. 45-53). However, it was markedly out of line with the remaining contributions. A different version of this paper was included in Tomaszczyk and Lewandowska-Tomaszczyk 1990: 287-300.

this issue should be given serious, unbiased consideration. This is precisely what we shall now try to do. The main questions which will be addressed and exemplified in the present chapter are what translation-specific lexemes can imply, how the 'meaning' of such items is to be determined, and what implications all this can have for the possible representation of translation-specific items of this kind in a monolingual dictionary. (How the compiler of a *bilingual* dictionary would go about it is a completely different question which should be addressed separately.)

1. Translation specificity

To begin with, even the practice itself of regarding translations as reservoirs of lexical data liable to lexicographical processing is not as established as one would have imagined, in spite of the ease with which it could be justified. After all, the product of any act of translation is an *intended utterance in the target language*. To judge from the lists of sources which the larger dictionaries often bring, this practice has enjoyed some currency mainly in situations where translations have indeed carried particular cultural weight, which is quite understandable, given that dictionaries are only *selective* documentations of lexicons.

Thus, it should come as no surprise that of the sources cited in Even-Shoshan's *New Hebrew Dictionary* (1966–1970), almost ten percent(!) are translations, and from a variety of languages too. By contrast, the share of translations (mostly of English and French literature) among the sources of dictionaries of contemporary German such as the new *Duden* (Drosdowski et al. 1976–1981), or the revised edition of Paul's *Deutsches Wörterbuch* (1992), is not only very small, but is also going down, and in many dictionaries of English not a single translation is mentioned. Nor is it surprising that in *historical* dictionaries of the very same languages, translations are given more weight. After all, most cultures have had periods when translations fulfilled important functions, among them the introduction of new lexical items, or new uses of old ones. One need only consult the "List of Books Quoted" appended to the *OED*, or the 33rd Volume, which was added to Grimm's *Deutsches Wörterbuch* in 1971, to become aware of this.

Of course, the fact that translations are intended utterances in the target language should not be taken to imply that each one of them represents a straightforward instance of performance within the boundaries of the institutionalized language, let alone fits in with any of its established varieties. On the contrary, it is a well-documented fact that *in translations, linguistic forms and*

structures often occur which are rarely, or perhaps even never encountered in utterances originally composed in the target language. The occurrence of such 'alien' phenomena owes much to the fact that the verbal formulation of a translation is partly governed by a felt need to retain aspects of the corresponding source text invariant, which is a strong target-external constraint on its establishment. This constraint is of course stronger the more the source text is tackled as an organization of lower-level constituents rather than as a holistic whole, and source-text interference is indeed an important source of forms which clearly deviate from general target-language patterns, even though by no means an exclusive one.

Whether deliberate or accidental, a so-called 'translationese' initially comprises ad hoc phenomena. However, it is quite possible for it to undergo a certain institutionalization. Thus, a group of translators may behave in much the same way, and hence produce translational replacements of a similar kind. In the long run, a habitualized translationese may even acquire some distinct markers, which would set it apart from any other mode of language use within the same culture, translational or non-translational. In fact, the more noticed (and accepted) such a differentiation is, the more justified one would be in regarding such a translationese as a *distinct variety* of the target language. As Pedersen (1983: 7) so nicely put it, "translation itself helps to create its medium".² There may of course emerge several varieties of this kind within a language, a major distinguishing factor probably being a regular association of each with a different source language. Finally, whereas it is reasonable to assume that such a distinct status would be most conspicuous in 'minor' or otherwise 'weak' cultures and languages, its occurrence in 'major' or 'strong' ones cannot be ruled out either.

2. Translation-specific lexical items

Since lexical items are relatively easy to discern even for the most naive 'person-in-the-culture', very often it is precisely in the *lexicon* that the distinctiveness of

2. Pedersen himself shows how translation-specific phenomena, including lexical items, were utilized by native speakers of Danish when producing translations "of fair quality" in their mother tongue within the EEC. Only then, and over a certain stretch of time, were those phenomena absorbed into the language at large, causing changes in the system itself.

a language variety used for translation purposes is most conspicuous. The obvious implication is that not only habitual target-language lexemes have been used, but also items which do not pertain to the generally acknowledged repertoire – mostly new combinations of previously existing lexemes, but, on occasion, newly coined words as well. Most of the latter will probably be a realization of *possible* target-language words, in terms of their phonology, morphology and grammar, but even this is not an absolute must. Greater deviations from predominant patterns have been known to occur.

Extreme examples of translation-specific lexical items would be complete innovations such as Hebrew *elef* (for German *Elf*) or *tarit* (for German *Torte*), which were not even derived from existing Hebrew roots.³ A possibly similar example from the remote past concerns the replacement of the biblical Hebrew word *šafan* (presumably referring to the animal *Procavia capensis*) in the Septuagint with *κοιρογρύλλιος*: There is no way of knowing what the translators had in mind, because this Greek word (if indeed it was a habitual Greek word) occurs nowhere else. (See *Encyclopaedia Biblica* VIII [1982]: 251.)

A more common practice would be represented, e.g., by the Hebrew phrase *šexar zangvil* (literally: ‘an intoxicating drink | flavoured with ginger’), which – in the sixties and seventies – was very rarely encountered in translations from languages other than English, let alone non-translated texts. This cannot be attributed solely to the fact that the referent itself is rarely mentioned in the latter; the practice did not undergo appreciable change even when the drink became part of Israeli material culture, and as such was known to contain no alcohol (i.e., not to comprise *šexar*), and when – in other modes of language use – it was already being referred to almost exclusively as *šingjer eyl*.

Then again, lexical items occurring in translations, which do belong in the general lexicon, may often be shown – even by simply referring them to their immediate co-text or to a possible external context – not to have been used in their exact habitual functions, semantic, stylistic and/or pragmatic.

For instance, in translated utterances, especially from English, the Hebrew word *na’ara* occurs in a much wider range of functions than it does within ‘authentic’ Hebrew, where it is mainly used to refer to a teenager. The word has simply taken up some of the

3. And see my discussion of “Phonetic Transposition” (Toury 1990). It is interesting to note, in this connection, that there was a period in the history of modern Hebrew when books, especially translations, were supplied with “lists of obsolete or renewed words”, along with their ‘equivalents’ in other languages, mainly German and/or Russian. In fact, *elef* was taken from such a list.

functions of the English 'girl', especially through the use of a variety of fixed expressions such as 'college girl', 'cover girl' (where the girls featured are not necessarily *ne'arot*) or 'working girl'. (To wit, the movie bearing this title – which, predictably, was rendered in Hebrew as *ne'arot ovdot* – opens with the title character celebrating her *thirtieth* birthday.)

Translational items may also fulfill similar functions, even resembling their non-translational counterparts in terms of 'meaning', but following different distributional patterns.

Thus, Shamaa (1978: esp. 168–171) reports that even common English words such as *say* and *day* occur in translations from Arabic at a significantly higher frequency, up to over twice as high as their 'natural' distribution in original English texts (inverted commas in the original). At the same time, the occurrence of these words in the translations is still considerably *lower* than of the equivalent items *qāla* and *yawm* in the Arabic source texts, which testifies to a non-automatic application of the replacement strategy and makes it a more interesting case in point.⁴

By the same token, I have been able to establish (Toury 1977: 79–81) that not only the distribution of Hebrew Void Pragmatic Connectors such as *ho* or *u-vexen* is much higher in translations than in original texts, but also that these items are particularly common in translations from *English*, that they often occur in positions which, from the Hebrew point of view, are irregular, or in a function which they do not have in that language, and that they are used as habitual replacements of the English 'o(h)' and 'well', respectively (so that the invented combination *ho u-v(e)xen*, rare as it is, epitomizes the whole approach). In fact, the frequency of *ho* and *u-vexen* in Hebrew translations from English had been growing incessantly until, by the sixties and seventies, it was almost identical to the distribution of 'o(h)' and 'well' in the corresponding originals.

Which brings us to another possibility; namely, that items figuring in both translated and non-translated texts may nevertheless occur in them at different points in time; whether the translational use comes first (in which case it would count as an *innovatory* force) or whether it lags behind (in which case translation may in fact amount to a *conservatory* force).⁵

4. I would like to thank Ms. Mona Baker for bringing this dissertation to my attention.

5. For the circumstances under which translations as well as translating as a mode of text generation tend to play either role in the evolution of a culture cf. again Even-Zohar 1990: 45–51 (1978).

This would apply, e.g., to the initial component of the phrase *šexar zangvil* mentioned above: for many generations, when the prevailing norm preferred lexical items of Hebrew origin, the word *šexar* was in general use, at least in written texts, to denote beer. However, in the last few decades it has become strongly marked as translational, whereas in non-translational uses, *bira* is the normal word. More specifically, *šexar* is now known to Hebrew readers mainly as a translational replacement of English 'beer', first and foremost in literary texts. Young Israelis who have had no acquaintance with these texts may thus be completely unaware of the very existence of the word, let alone its denotation; and — since it is a seemingly transparent word — they are likely to mistake it for a generic term referring to alcoholic beverages in general.

3. Translation-specific lexemes as candidates for the dictionary

It stands to reason that a lexical item which is translation-specific in the strong sense first emerges as an ad hoc 'solution' of an individual translator to a particular translation 'problem' posed by a single text, maybe even a single item occurring in it. Being a purely performance phenomenon, such an item would hardly warrant lexicographic treatment, even if it came into being within the shared norms (which is often the case). The same holds true for any unique change of function or use of an existing item. However, in the course of time, new items and changes of function are often picked up by a growing number of translators, either independently, responding in a similar way to a recurring source-language stimulus, or even following in each other's footsteps. In the process, these items gain in habitualness, if only as translational replacements, and themselves become part of the norm. To be more precise, as long as its association with a fixed lexical item in another language has not been resolved, such a lexeme would still pertain to *translation from one particular language* rather than representing 'communication in translated texts' as a whole.

Rather than its (non-existent) general validity, it is the fact that it has ceased to be a bona fide instance of performance that earns it lexicological attention and subsequent representation in the dictionary. It would simply not do to leave habitualized translation-specific items out of a target-language dictionary which aspires to any amount of fullness and discriminative capacity, or precision. On the other hand, to require that a lexeme represent that language 'as a whole' is highly dubious as a condition for granting it lexicographic attention, which only modes of language use which are looked down on are subjected to anyway. Any

rejection of a translation-specific item on account of its restricted distribution is likely to lead to total disqualification of translations as sources of lexical, or any other linguistic data even in cases where they did introduce novelties into the target language. This practice would run counter to simple logic: an item of translationese can simply be part of the language (and only that language) to which the utterances using it claim to belong.

At the same time, if and when these items are entered into a dictionary, the fact that they *are* restricted should not be ignored, or even obscured. It would simply not do to regard those translations which make it into a lexicographer's data base as if they represented the language in any straightforward way. Such an attitude would render the inclusion of a translation in the corpus *nominal* only, and hence as insignificant from the point of view of Translation Studies as it is misleading from the lexicological point of view. The restrictions of its use constitute a substantial part of "all the information that we need to know" about a translation-specific lexical item (Lyons 1977: 514), which is why they should be built into the lexicographic account itself. The real question is: What kinds of information would be deemed necessary and establishable, and in what way would this information best be arrived at.

4. The 'meaning' of translation-specific items

One convenient way of approaching items revealing any kind of specificity is to refer their specificity to the notion of *markedness*, which has proven fruitful in so many domains.

As is well known, in a pair of terms which are not in free variation, one of the terms tends to be marked and the other unmarked in relation to a certain contrast: the unmarked member A is neutral with respect to it whereas its marked counterpart B is not. From the point of view of the addresser in an act of communication it is therefore A which would be selected whenever there are no specific reasons for it not to be. From the complementary viewpoint of the addressee, this implies that whenever B is used, its markedness – and hence the reasons for choosing it – are *part of the overall information conveyed by it*. Any wrong assumption, e.g., that a marked term is in fact unmarked, is thus bound

to yield wrong conclusions, hence giving rise to insufficient, at times even outright incorrect interpretations.⁶

According to John Lyons (1977: Section 9.7), who tried to introduce some order into the use of the terms 'markedness' and 'unmarkedness' insofar as it applies to the analysis of the *lexical* structure of language, there are three interconnected though theoretically "disparate and independent" senses in which a lexeme may be described as 'marked' or 'unmarked', hence three different types of markedness: formal, distributional and semantic. Although he never refers to it in any explicit way, Lyons would presumably have subsumed translation-specificity under *distributional* markedness, the necessary and sufficient condition for which is that the marked member of a pair occur in a narrower range of contexts than its unmarked counterpart. Thus, items which are not translation-specific can be, and often are used in translations too, to no specific avail (which is one major reason why the initial opposition between 'translated' and 'non-translated' texts was hardly ever brought to bear on the compilation of dictionaries, even in cases where there was little reason to assume that it had been completely neutralized). By contrast, items which *are* marked as translational cannot be used in non-translational contexts without this markedness bearing on that use. In other words, as long as an item is marked for translation, this fact is bound to be *functional*, irrespective of the actual genesis of the utterance in which it is embedded. Put as a kind of paradox: using a marked item in a context which it was designed to serve tends to *blur* its markedness, and hence renders the use itself 'normal'. By contrast, using that same item in a context which is 'abnormal' for it would make its markedness *stand out* considerably.

No wonder, then, that *pseudotranslations*, especially those which have been successful in deceiving the public and which often pass for genuine translations, often abound in translation-specific items (see Excursus A), precisely because of the capacity of these items to serve as (in this particular case: deliberate) *signals of 'translation-ness'*. Similar items have also been used by writers of original texts to create a variety of effects, all stemming not only from the nominal fact of their anomaly, but also from a need to mitigate the tension thus created by associating the anomalous items with other languages — preferably with

6. Weizman and Blum-Kulka (1987) indeed posited the existence of an 'adjustment mechanism', which is triggered as soon as shifts from target norms are perceived, to ensure a 'correct' reading, and even exemplified the validity of their assertion by an experiment. See Chapter 12, Section 2.2.

particular items within them, either directly (a rather demanding activity, which necessitates some measure of bilingualism on the readers' part) or indirectly, mainly via the readers' acquaintance with translational habits in their own cultural tradition.⁷

Being markedness for *usage*, translation-specificity has practically nothing to do with Lyons' formal, or even semantic markedness. However, there are good reasons to regard this kind of specificity, like any other distributional restriction, as *part of the overall meaning* of an item; if not in strict terms of a theoretical semantics then at least from the practical point of view of dictionary making and usage. To cite one prominent scholar of lexicological studies:

Provisionally it might be said that any attempt at establishing the 'meaning' of a lexical item must be based on an analysis of usage: the assumption is that a study of the way an item has been used by native speakers of the language will give an indication as to its potential value, the way it will be used, at least in the nearest future. (Tomaszczyk 1976: 77)

It is in this connection that the quite regular tendency of translation-specific lexical items to replace particular items of a particular language acquires its special significance. My strongest claim so far would therefore be that

a translation-specific lexical item has no meaning which is completely detachable from its use as a translational solution; that is, a target-language-intended replacement of a certain item of a certain language other than the target language.

It is fairly obvious that, in this extreme formulation, such an assertion would hold for very few, and very marginal cases only – those which are not only *fully* translation-specific but also *semantically opaque* and *devoid of sufficient disambiguating contexts*. However, in a very real sense, even lexemes which are translation-specific in a much weaker sense still differ from 'normal' target-language items in that they violate the maxim of the complete *intra-systemic* nature of 'meaning'. At least they have the *potential* of doing that, which actually means that there would always be, at the very least, a measure of doubt. Naive

7. A good example would be Hemingway's Spanish-like use of English lexemes such as 'much' in *For Whom the Bell Tolls* in order to evoke an underlying 'reality' in another language (e.g., Fenimore 1943). For an overall presentation of the issue of "Polylingualism as Reality and Translation as Mimesis" see Sternberg 1981.

users of the target language may well fail to realize such a violation, but exposing it certainly lies within the responsibility of the documenting *lexicographer*. At least the ultimate assurance of the 'meaning' of a translation-specific lexeme, which is a *sine qua non* in dictionary production, therefore necessitates the reestablishment of the (more or less habitual) pairs where the item in question serves as a replacing segment, whichever relationships may obtain between the members of such pairs.

5. Submitting translations to lexical study

Once a decision has been made not to lead the users of a dictionary to mistake translation-specific items for regular target-language lexemes, the pairing of these items and their correspondents in actual texts in other languages emerges as *mandatory* for the establishment of the information needed for their lexicographical treatment. Consequently, the methodology yielded by our Rationale presents itself as a tool of particular power for the lexical study of translations. Let us dwell briefly on the adoption of our method for that purpose.

Even though lexical items occurring in translations do belong to the target language and to none other than the target language, it should have become clear that the properties of those which *are* translation-specific can never be fully accounted for within their immediate contexts and co-texts alone: not only would *deviations* from general, or other target-language patterns resist explanation, at times even detection, but one would never be quite sure of the real status of seeming *similarities* either. Moreover, if such an item is to be properly accounted for, its markedness for translation first has to be *established*, a task which Lexicology as such is not designed to undertake. Nor would Contrastive Lexicology do the trick, concocted as it is to apply to parallel lexical items in different languages which have first been established, and accounted for, independently.⁸ The only feasible way to go about it seems to remain within

8. In fact, in Contrastive Lexicology there has been at least one scholar, Christoph Schwarze, who put his finger on this last problem. Consequently, he supplemented the category of 'equivalence', which has always been regarded a self-evident (though by no means easy to handle) basis for the compilation of bilingual dictionaries (e.g., Tomaszczyk 1976; Hartmann 1985), with another category which he characterized as "eine übliche, aber nicht semantisch äquivalente Übersetzung" (Schwarze 1981: 165). He failed however to supply a rationale for the actual utilization of that category in

DTS and to comprise an adaptation of our method, at least to some extent, in keeping with the special requirements of lexical studies.

It will be recalled that the distinction between translated and non-translated utterances, hence between translational and non-translational modes of text production and language use, has been conceived of as a basic *functional* opposition, regardless of whether the surface realizations indeed reflect it, and of whether the differences have been neutralized. This opposition was then adopted as an underlying assumption for *any* study applied to translations, being its marked member. It has of course particular, and most immediate implications in cases like the present one, where it is precisely the *differences* between translational and non-translational language use that one is after. What is of major consequence for lexical studies of translations is the assertion that in a study carried out within DTS, a target-language 'solution' does not merely imply the existence of a corresponding source-language 'problem', but the two determine each other.

Proceeding from the *target* pole, the analyst, hitting upon a target-language segment which recurs in translated texts, thus marks it as a tentative *lexical item*, considering it against the background of the target language 'in general' and/or the patterns that are bound to emerge from the mapping of the translations on their assumed originals. What s/he then looks for is the source-language segments which pair with that tentative lexeme, whether similar to it in rank and scope or not, always keeping in mind the 'no leftover' condition. Thus, the object is not necessarily to try and establish one single coupled pair, which would be an extreme, and therefore probably exceptional case. Very often it is a series of pairs which would be established, all of them having in common the occurrence of one and the same target-language segment as a replacing member.

Contrastive Lexicology and Bilingual Lexicography. Indeed, judging from his attitude towards the empirical problems of Contrastive Linguistics (Schwarze 1975), it is even doubtful whether he would regard it a worthy basis for those purposes in the first place. After all, he does suggest that lack of pragmatic-semantic equivalence between the members of what he terms an *Übersetzungspaar* (according to his conception: "Satzpaare, die in praktischer Übersetzungsrelation stehen") should lead to the *exclusion* of the pair from the data reservoir of the contrastive linguist (and cf. especially the two flow-charts in Schwarze 1975: 22–23). More important still, it doesn't seem to have occurred to him that the existence of this type of relationship may have far-reaching implications not only for Contrastive Lexicology and Bilingual Lexicography, but also for *Monolingual* Lexicology and Lexicography, which is our topic here.

It is the *recurrence of pairs* in larger corpuses that is designed to supply the lexicographer with the needed information about a translation-specific lexeme, should s/he choose to account for it in the dictionary, including the probabilities of its occurrence as a replacement of particular source-language items.

What this means, in practical terms, is that whenever inserted in a dictionary, such an item would be supplied with whatever part of the following information is found establishable, relevant, and potentially helpful to the user, whether a translator or not:

- (i) used also/mainly/only || in translations || from/until such-and-such point in time (etc.)
- (ii) from language X (or languages X, Y, ...)
- (iii) as an occasional/regular/automatic (etc.) replacement of the lexical item(s) a, b, ... in language X (or languages X, Y, ...)
- (iv) which usually mean(s)/is (are) used as ...

Obviously, this would result in much more than a mere 'distribution label' of the kind that users of dictionaries have come to expect in addition to syntactic properties and semantic specifications (in the narrow sense). For one thing, the situational restriction in question is definitely not *stylistic* in nature, as are most of the other restrictions treated in a dictionary; for another, it bears on the very *meaning* of (or at least *information* conveyed by) the item.

The adoption of such a policy has much to offer in terms of increasing the fullness as well as discriminative capacity of a dictionary, and is therefore bound to put Lexicography on much firmer theoretical and methodological grounds, for it enhances not only the *descriptive* function, which is a blessing in itself, but also the *remedial* functions which dictionaries are often called upon to fulfil as well (e.g., Hartmann 1979: 1-2). As far as *historical dictionaries* go, this practice seems a *sine qua non*, to the extent that a historical dictionary is expected to reflect the forces which were at play – and in conflict – during the evolution of the lexicon.

6. Towards exemplary dictionary entries

In order to make the claims made throughout this chapter somewhat more concrete and to validate their implications for the representation of translation-specific items in a monolingual dictionary, let us dwell at some length on two examples:

- (1) the Hebrew noun-compound *tazvig mikrim*, which occurs solely as a translational replacement of the English noun 'coincidence', and never (as far as I have been able to ascertain) of other words, in English or any other language, let alone in Hebrew original writings; and
- (2) the German near-idiomatic conjoint phrase *braun und blau* in one of its uses at the end of the 18th century, namely, as a translational replacement of the French color term *violet*.

These two expressions differ considerably in terms of the relationships that tie them to their habitual counterparts in the two respective source languages. Thus, *braun und blau* hardly reveals any sign of formal correspondence with, or negative transfer from *violet*. Rather, an *existing* German phrase, meaning '[mit] schwarzblauen Flecken', 'finster', 'dunkel' (e.g., Grimm II (1860): 324–325), was adopted as a carrier for a new semantic function, imported from another language, this effecting a considerable change in its semantic coverage. By contrast, *tazvig mikrim* is a clear case of loan translation. In this case, a semantic function was transferred over the linguistic border along with several traits of its source-language carrier, and the latter were replaced by some corresponding target-language substance.⁹

Despite these differences, and despite the fact that they are well-formed in terms of their respective languages, both expressions are elements of *translational varieties* of the two languages. They are also relatively *transparent*, the more so as they tend to occur in more or less *disambiguating contexts*. This, however, by no means ensures that a reader of modern Hebrew will reconstruct and activate the full range of the semantic features of the English word 'coincidence' in the course of his or her decoding a text into which *tazvig mikrim* has been incorporated (which would certainly be the case for the unmarked counterpart *tseruf mikrim*), unless (a) s/he knows some English, and (b) s/he has reason to assume that activating a particular English word would be in any way beneficial.

The same used to hold for the readers of German at the end of the 18th

9. To be sure, the first constituent element of the phrase, the word *tazvig* itself, is a complete neologism. It was derived from the Hebrew root *zvg*, selected as a replacement of the original prefix *co-* because of the semantic feature of 'duality', and combined with the productive nominal pattern *tafil*. However, as far as I was able to ascertain, this new carrier has never been used out of the noun-compound *tazvig mikrim*, and I am therefore not sure whether it would warrant an entry of its own.

century as regards *braun und blau*. Surely it is no mere chance that Joachim Heinrich Campe, the editor of C.F. Cramer's German translation of Jean Jacques Rousseau's *Émile*, felt it necessary to accompany at least one of the instances where that expression was used as a replacement of French *violet* ("ich sah es braun und blau werden") with the note that the child observed became "ganz braun", thus making use of the *unmarked* German term for that colour at that time. As Suzanne Öhman put it in her study on *Wortinhalt und Weltbild*,

während Cramer also, vielleicht durch die französische Unterscheidung von *braun* und *violet* dazu veranlasst, *violet* mit *braun und blau* überträgt, begnügt sich Campe selbst noch mit dem einfachen *braun* als Bezeichnung für dieselbe Farbe. (Öhman 1951: 138)

All in all, both *tazvig mikrim* and *braun und blau* are obvious candidates for inclusion in a monolingual dictionary that does not shun fixed expressions: they are fairly *common* in their respective varieties, and their use and functions in those varieties reveal distinct *regularities*. At the same time, it is clear that *tazvig mikrim* cannot be taken to 'mean', or merely 'be a semantic equivalent of' 'coincidence', much the same as it would be erroneous to claim that, at the end of the 18th century, *braun und blau* 'used to mean' 'violet'. What reducing the representation of the two expressions to such a simplistic formula would actually do is involve the lexicographer in *creating* rather than merely *documenting* linguistic realities.

Any attempt to remain within the documentary function would lead to entries of the following type:

- **tazvig mikrim*: one of the HABITUAL Hebrew TRANSLATION SUBSTITUTES of the ENGLISH noun 'COINCIDENCE' in the second half of the 20th century;
- **braun und blau* (as an irreversible binomial of considerable idiomaticity): a TRANSLATIONAL REPLACEMENT of the FRENCH color term VIOLET in the late 18th century.¹⁰

10. By the same token, dictionary users should expect the entries for 'once upon a time' (in an English dictionary), *Es war einmal* (in a German dictionary), *hayo haya* (in a Hebrew dictionary) or *kāna [yā] mā kāna* (in an Arabic dictionary) to offer the information that it was originally restricted to the OPENING of a STORY which has already COME TO ITS END, and a HAPPY one at that (which many of the dictionaries already do). This *factual* [partial] interchangeability would also be the basis for assigning equivalence to items of this kind in a *bilingual* dictionary.

Have our requirements not been carried a little too far? Not at all, so long as the problem of representing translation-specific lexical items in the dictionary is still approached 'in principle'. As often noted,

there is no exaggeration involved in the answer [to the question as to how much information about contexts in which particular lexical items appear should be included in a dictionary] that *as much information about both linguistic and extralinguistic settings as is possible to contain in a dictionary should be contained*. This is to say that, other things being equal, the quality of a dictionary is in proportion to its size seen as the volume of the material to the right of each lexical item. (Krzyszowski 1981: 146; emphases added)

The only problems in realizing such an ambitious program seem to lie in the *practical* domain, and in the electronic age even these seem less valid by the day.

Experimentation in Translation Studies

Achievements, Prospects and Some Pitfalls

In spite of the rapid growth of Translation Studies towards semi-independence, most of the researchers in the field have had their basic training elsewhere. In fact, more often than not, Holmes' 1972 claim still holds:

As the interest [in translation] has solidified and expanded, more and more scholars have moved into the field, particularly from the adjacent fields of linguistics, linguistic philosophy, and literary studies, but also from such seemingly more remote disciplines as information theory, logic, and mathematics, each of them carrying with him paradigms, quasi-paradigms, models and methodologies that he felt could be brought to bear on this new problem. (Holmes 1988: 67-68 [1972])

The only real change seems to be that paradigms and models from ever new disciplines are being applied to the study of translation.

The introduction of experimental methods is no exception: it can hardly qualify as an *internal* evolution of any previous paradigm of Translation Studies itself. Not even the impetus for applying them to translations and translating originated within its boundaries. More often than not, experimental methods were *imported* by scholars who were looking for new, intriguing areas of application for tools they already had at their disposal – areas which would be close enough to their mother disciplines as well as sufficiently underexplored to warrant study with the help of these tools. No wonder, then, that many outsiders chose to apply them to translation, which indeed satisfies both needs.

All this notwithstanding, the introduction of empirical methods proved a true landmark in the evolution of the discipline, and it certainly looks like they are here to stay. In fact, to my mind, the greatest contribution of experimenta-

tion lies precisely in its potential for shedding new light on the interdependencies of all factors which [may] act as constraints on translation and on the effects of these interdependencies on the process, its products, and the functions which any of them may serve in the recipient culture, and in increasing their predictive capacity. This potential, as I see it, derives from two of the inherent traits of experimentation: relative *controllability of variables* — and high rate of *replicability*. At the same time, most of these new methods have hardly been confronted with other achievements of modern Translation Studies, with a view to increasing their relevancy to our understanding just what translation involves. Which is basically what this chapter sets out to do.

1. Empirical sciences and empirical methods

Obviously, empirical *sciences*, Translation Studies included, can proceed perfectly well with no recourse to empirical *methods*, at least for a considerable period of time. However, the very possibility of their introduction (which is only feasible in sciences of the empirical type, in the first place) is bound to encourage the adoption of such methods, especially in a period like ours, when the scientific scene is practically haunted by experimentation. This makes it perfectly understandable why empirical methods have started being applied to translation in spite of the fact that, as a non-prescriptive discipline, Translation Studies is very young indeed. In fact, I am inclined to take the very spread of experimentation among translation scholars as a promise of accelerated movement towards growing non-prescriptivism. After all, nobody would claim in any serious way that it is what translation 'should be' that is, or even can be studied empirically.

Whereas an empirical *study*, like any other descriptively oriented studies in translation, can be product-, process- or function-oriented, empirical *methods* can only be applied to strictly observational data. "A critical point in the investigation of mental processes is" precisely "that the processes themselves can never be observed directly", such as "their investigation is always based on the *products* of such processes" (Hoffstaedter 1987: 76). This however should not be taken to imply that, in empirical studies, the same products are merely taken up which have already been used in descriptive studies of a *non-empirical* nature. Both modes of inquiry indeed proceed by directing questions "to some external source of information"; namely, one that is not the underlying theory itself (Hilpinen 1988: 20). However, whereas in observational studies questions are addressed directly to the world of our experience, empirical studies first have to

secure the data to which questions will then be addressed. *Elicitation techniques* thus form an integral part of any experiment; and since these techniques, and the types of data they yield, are intimately connected with the objectives of the study, they too have to be justifiable in terms of Translation Studies.

Needless to say, within Empirical Translation Studies, product-, process- and function-oriented approaches differ not only in their focus, but also in terms of the data they elicit and process. In fact, the crucial problem lies precisely in choosing the right kinds of product – those “for which we can safely assume that they tell us something”, and something “relevant” at that – about whatever we are using them to study (Hoffstaedter 1987: 76). One important fact here is that even when translations are elicited in the course of an experiment, it is not really they which are then used as data. Thus, in product- and function-oriented studies, analysis is mainly applied to *reactions* to translations, whereas process-oriented empirical studies normally make use of elicited manifestations of the *gradual emergence* of a translated utterance, often to the complete neglect of its final version.

Having said this, we will move on to examine some of the empirical methods which have been adopted for the study of translation. Following the proper historical order, a number of product-oriented methods will be reviewed first. Only then will we move on to the process-oriented ones, which involve the greatest element of novelty.

2. Product-oriented empirical studies

2.1. Cloze tests

One of the first empirically oriented suggestions ever made vis-à-vis translation was to submit translated texts to CLOZE TESTS, the good old technique used for testing two complementary issues: the *reading comprehension of subjects* and the *readability of texts* (e.g., Nida 1964: 140; Nida and Taber 1969: 169-170). To be sure, it is not at all clear what exactly is measured by this method. Even less clear is its explanatory power within Translation Studies. After all, there can be no guarantee that the application of a cloze test to a text which just ‘happens’ to be translated will have anything particularly ‘translational’ about it!

One way out of this deadlock may be to devise comparative studies into the readability of *translations* into a language vs. *original texts* in that same language. Studies of this kind are likely to yield interesting insights into similarities and dissimilarities in the processing of texts of both kinds; and if the fact that a text

is translated does affect the way it is processed, this may be taken as the missing justification for applying the cloze technique to it. (The crucial issue of the possible specificity of translations among target-language texts will be taken up again in Section 2.2.)

Combined with other research methods, the cloze may yield even greater insights into the ways addressees process and translators construct translations. In this last respect, the following multi-stage experiment, which has mini-cloze as one of its components, seems highly indicative, in spite of the loose way it was conducted (by Mary Snell-Hornby 1983):

First, a short passage was excerpted from an article in the *Neue Zürcher Zeitung* (7.4.1978) describing visual impressions of Belfast from the viewpoint of a Swiss journalist, and a visual simile which it contained was replaced by a blank space:

Die Hauptstrassen, die [] vom Zentrum ausgehen, teilen die Stadt in Kreissektoren auf, die in abwechselnder Folge von Katholiken und Protestanten bewohnt sind, so dass jede Gruppe sich immer von beiden Seiten her bedroht fühlt.

A diagram was then prepared, reproducing the visual information encoded in the sentence, particularly as expressed by the obliterated simile (see Figure 12).

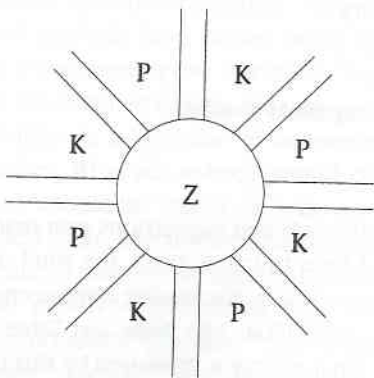


Figure 12. *The visual information encoded by the obliterated simile*

The sentence (minus simile) could be rendered in English as follows (the letters in brackets refer to the diagram):

The main streets, which lead from the centre (Z) [simile], divide the city into districts alternately inhabited by Catholics (K) and Protestants (P), so that each group always feels threatened from both sides.

The tampered-with text was then given to a group of native speakers of German, and the diagram — to a group of native speakers of English. The German speakers were asked to supply the missing word, whereas the English speakers were asked what they thought the diagram represented (hence what they thought the simile could be). Both groups answered without hesitation, and almost unanimously — but their responses differed considerably: whereas the German speakers identified the missing word correctly as *strahlenförmig* (literally: ‘having the shape of rays’), the English speakers saw in the lines of the diagram the spokes of a wheel.

On the basis of the spontaneous reactions of native speakers it would seem that, if (as many a wishful thinker would like us to believe) translation indeed consisted in supplying replacements to source-text segments on the highest possible level of the textual hierarchy, then the expression ‘the spokes of a wheel’ could be taken as a convenient basis for the establishment of an English translational replacement (not to be confounded with a lexical, or dictionary equivalent!) of *strahlenförmig*, in this particular context, especially as the constituent *Strahl(en)* does not partake of the text’s web of relationships and hence has no special function in it. What we have here is merely a habitual(ized) way of expressing the *idea* contained in the text.

However, the experiment also had a third part: over 100 students of translation were asked to actually translate the (full) German text into English. The result was that an overwhelming majority rendered the word *strahlenförmig* either with ‘like the rays of the sun’, or with ‘like rays of a star’. Not even one of the student-translators transformed the rays into spokes. Most of them made the implied source of light explicit, into ‘sun’ or ‘star’, but none into ‘a wheel’.

It would seem, then, that even people who are well aware of native situational equivalents, and use them in comparable native-like situations, tend to ignore these equivalents as *translational* replacements, even if they are trained to try and establish translation relationships on the highest possible level (as the subjects of this experiment, being students of translation in a modern institute, probably were). To me this is highly indicative of the fact that the requirement to “communicate in translated utterances” (Toury 1980a: 11–18) may impose behavioural patterns of its own, a statement which certainly deserves some more consideration — and specification — using experimental methods too.

Another type of comparative study which could be carried out with the help of the cloze procedure concerns *relative readability*, especially the relative readability of *different translations of a text into one and the same language*.

A study of this kind was recently carried out by Tiina Puurtinen (1989a; 1989b) as a corrobory of a linguistic analysis where one syntactic feature was isolated whose

presence presumably reduces a text's readability. The assumption was indeed verified by a cloze test: the translation which had that feature was deemed significantly less readable than another translation, done in the same year, which did not have it; and since this was "the only significant difference" between the two texts, it was concluded that "this difference must have a noticeable effect on readability" (Puurtinen 1989b: 210).

The cloze technique was applied again in 1991 by one of my students, Ami Singer, in an attempt to check the comprehension of old vs. new [translated] texts by readers of different age-groups as well as their ability to identify the texts as translations. The assumption was that the more old-fashioned a text (with respect to the norms prevailing at the time when it is read), the less comprehensible it will be and the greater the tendency to mark it as a translation based on the very 'oddities' which served as obstacles for fuller comprehension. It was also assumed that older subjects would fare better than younger ones, i.e., give more 'correct' answers. The texts used in the study were parallel excerpts of two translations of one short story, one done in 1959 (but according to much older norms), the other in 1985, plus a third version which was prepared by the examiner himself. Here pains were taken to approximate the formulation to the one used in modern written texts in order to lead the subjects astray. All in all, the assumptions were verified, even though the main profit was no doubt methodological, in terms of correctives for future use of the technique itself.

Finally, the *relative readability of translations and their source texts* can also be tackled. To be sure, this issue was one of the first to be raised in the literature, and yet, to the best of my knowledge, the cloze method was never applied to its solution, at least not in any controlled way. Which is not a great pity either, given that the whole orientation of the first suggestions was critical-normative rather than descriptive-explanatory. After all, can there be a way of testing the assumption that the readability of a translation 'should be' equal to that of the original text, which was the starting point of the proponents of this type of experiment (e.g., Van Hauwermeieren 1972)? Nor would simply rephrasing it as a proposition of the type "the readability of a translation 'is' equal to that of the original" do the trick. This would constitute, at best, a *descriptive statement*, applicable to one or another individual case, but hardly a law of general validity. To be theoretically valid, the *conditions* must also be specified under which the proposition would tend to hold. (See Part Four.)

Another proposition of this status seems to be the well-known 'explicitation hypothesis', which – in one of its most general formulations – claims that

it might be the case that [... a rise in the target text's level of explicitness] is a universal strategy inherent in the process of language mediation, as practiced by language learners, non-professional translators and professional translators alike. (Blum-Kulka 1986: 21)

Moreover, due to the obvious, even though by no means simple correlations between explicitness and readability, the cloze method may well serve as one of the testing techniques of the explicitation hypothesis. Again, the point would not be to simply validate or invalidate the hypothesis 'in general', but rather to *modify* it by exploring the circumstances under which it tends to have the upper hand, vis-à-vis other possible factors. For instance, are there any differences in the application of explicitation strategies by language learners and translators? By non-professional and professional translators? In oral vs. written translation? In translation carried out from L_1 into L_2 and vice versa? Within cultures that assign centrality to translating and translations vs. cultures where they are marginalized? etc., etc. And if the *similarities* are indeed attributable to the fact that these are all "processes of language mediation", what would the *differences* be attributed to? — Needless to say, one would immediately find oneself on one's way to formulating yet another conditioned law.

To be sure, the use of the cloze to study the relative readability of translations and their source texts involves some inherent difficulties too. As is well known, the exact application of the technique is, at least in part, language-dependent; and unless compatible applications for texts in different languages are established (which, as such, is not among the tasks of a translation student), the data collected would hardly be comparable.¹ Even more difficult would it be to justify investigation into the relative readability of the *translations of one text into various languages*, which should, in principle, offer a possibility of discerning between what is universal and what is specific, or norm-governed in translational behaviour. In spite of these difficulties, were I to foresee future evolution, I would not be surprised to find the cloze technique tried out in each of these types of study.²

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1. For the same reason, the application of *Readability Formulas* to translations vs. source texts (as suggested — and tried out in a rather embarrassing way — in Dye 1971) is highly problematic too.
 2. Shlomo Haramati of the Hebrew University (Jerusalem) has recently claimed that Hebrew translations of children's books tend to exhibit much lower readability than texts originally written for children in that language. This claim, which seems to make a lot of sense, was indeed substantiated by cloze tests administered by several of

2.2. The use of questionnaires

By and large, the most common way of studying aspects of translated texts (or, rather, addressees' responses to them) in an empirical way consists in devising QUESTIONNAIRES, and having groups of subjects – hopefully big enough³ as well as controlled for their background variables – react to the texts by answering the questions. From among the many experiments where questionnaires were used I have chosen to dwell at some length on one, and to bring a few elements of another to bear on it. The locus of my discussion will be the study carried out by Sonja Tirkkonen-Condit (1986), in an attempt to test the psychological validity of a set of textlinguistic features (hence the justification of using them as criteria for the assessment of translations).

The experiment consisted in rating five Finnish translation variants⁴ of one English text by 30 native speakers of Finnish on an intuitive basis. The rank order of the texts was to be established with the paired comparison method, according to which every two texts are compared to each other separately: A – B, A – C, A – D, A – E, B – C, B – D, B – E, C – D, C – E, D – E. The results of all paired comparisons (ten, in this case) were computed into a group-to-text ratio, and its numerical value was then compared with the results of a so-called professional assessment, one which was based not on people's reactions, but on the occurrence (or non-occurrence) of certain textlinguistic features which had been defined in the course of a previous study (Tirkkonen-Condit 1982). The hypothesis was that 'impressionistic' assessment – the one supposedly carried out by the subjects – is involuntarily governed by rules which concur with those textlinguistic features that a 'professional' type of assessment would rely upon in a more conscious way. It is systematic agreements between the results of two different modes of rating, then, which were taken as validating the hypothesis, apparently on the assumption that similar surface patterns attest to similar underlying factors.

Haramati's students. It was tentatively explained on the basis of the relative *conservatism* of translators into Hebrew, which is particularly noticeable in the marginal system of children's literature. (Personal communication.) Concerning this kind of conservatism see also Ben-Ari 1992 and B. Even-Zohar 1992.

3. Insufficient samples size seems to have been a common weakness of all experiments carried out so far – a shortcoming which has not gone unnoticed by the researchers themselves.
4. Even though some of the translations were prepared especially for the experiment, it is fully justified to regard them as fair representatives of the category of 'translation'; not just on the superficial grounds of their *factual* existence, but first and foremost because of the initial *possibility* of their coming into being.

To be sure, my sole interest here lies in the *experiment's design*, not in the results obtained by the whole procedure. Therefore I do not wish to say anything about the text-linguistic features themselves nor about their status as 'criteria' for assessments of any kind. These factors may have a lot to do with the tested *hypothesis*, but very little with the experiment as such. After all, a claim could easily be made that 'professional' assessment could be carried out again at any time, within a different conceptual framework, and its results compared anew with the very same experimental data. Even if the conclusions thus obtained were different, this would not necessarily invalidate the *experiment*.

It should be noted that here, again, the subjects were presented with translation variants only. Thus, even though it was not stated in so many words, what they were required to assess was not really 'the translations', but just one aspect thereof; namely, their *acceptability* in the target system.⁵ Of course, there is nothing basically wrong with rating translations in terms of their acceptability. The point is only that the kind of acceptability thus assessed is not altogether clear, much less so controlled: Is it the translations' acceptability as *texts* in the target language — or as *translations* in(to) it?

To be sure, the subjects were all told that what they had before them was a set of parallel translations. But how did this background information affect the rating? In other words, would the results have been any different, had the subjects *not* been told, or even led to assume that the texts were in fact translations? Not only was the question never raised, but the very possibility was not provided for. And this possibility is a very real one. After all, there have indeed been cultures where *different* criteria were in force for the acceptability of texts as target-language utterances and as translations into that language.⁶

Incidentally, as has been shown by many descriptive studies into translation, this special status of 'acceptability as a translation' holds true especially for

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5. A further fact, namely that they were rating acceptability in an *indirect* way, through comparison, was dealt with by the way the results were computed.
 6. One famous example would be the King James Version of the Bible, which would hardly have passed a general test of 'acceptability as an *English* text' of the 17th century. If more modern, and less literary examples are preferred, I would mention the most recent translation of Freud's writings into Hebrew: it is definitely considered an impossible *text* and no Israeli student of psychology would think of ever using it. On the other hand, it did bring its translator the most prestigious translation prize in the country by virtue of his compliance with the requirements that a *translation* into Hebrew was expected to satisfy.

cultures where translations play a role in shaping the very center of the system, and – as far as I am able to tell – the Finnish culture may well be one of these.

On top of all this, there has been at least one questionnaire-like pilot study, which seems to indicate that normally, the tentative identification of a text as a translation is not merely possible, but may indeed have crucial implications for the way the text would be interpreted; and if interpretation so, why not assessment?

The experiment I have in mind was carried out by a student of Shoshana Blum-Kulka's at the Hebrew University (Jerusalem)⁷ and duplicated by me. In this experiment, subjects were not presented with translations only, but with a mixture of both original and translated passages in one language, Hebrew. Thus far they all knew, but none of them was told *what* passages were translated. Rather, it was precisely their ability to *identify* target-language utterances – in this case, short pieces of dialogue – as either original (authentic or literary) or translated that was tested, along with the reasons they gave for their identifications.

Weizman and Blum-Kulka (1987), who processed the results of the experiment, noted that texts tend to be identified – correctly or incorrectly – as translations when shifts from target culture norms are perceived – not only linguistic, stylistic or textual, but pragmatic ones as well. The perception of these shifts triggers in the addressee a kind of *adjustment mechanism* which ensures the 'correct' interpretation of the passages. I, in turn, would add another claim: it may well be the case that it is precisely deviations which have become habitually associated with translation on the basis of previous encounters with translated texts that trigger the adjustment mechanism – the more so when readers are not led to believe that they will have to deal with translations, in the first place (which is something the two experiments referred to share, albeit in different ways and to a different extent). At any rate, the fact is that my own replication of the experiment yielded markedly different responses from subjects who had been made aware of certain characteristic features of translations into Hebrew (namely, students who had the greater part of an introductory course in Translation Studies behind them) and subjects who had nothing of the sort.

7. Shoshana Fishler. "Register and Translation Perception of Native Speakers". A seminary paper (in Hebrew), 1986. I would like to thank Shoshana Blum-Kulka for supplying me with a copy of Fishler's paper and the original questionnaire.

To come back to Tirkkonen-Condit's experiment, the situation was further complicated by the fact that the text she used dealt with environment conservation. This selection could hardly have constituted an *indifferent* factor in the assessment. Thus, it is perfectly possible that, in addition to being read as either utterances in the target language or translations into it, the translation variants could also be identified as 'biological', and hence read differently by different subjects on the basis of their previous acquaintance with texts of this type. And would it not be the case that those acquainted with 'biological' texts in a country like Finland will have had most of their previous encounters with either texts in a *foreign* language, mainly English, or outright *translations* from it? Is it at all clear, then, that all subjects responded to 'the same' texts?

This last difficulty reflects a general problem, which is particularly pertinent in any experiment in the so-called sciences of man, namely, the difficulty, if not impossibility, of taking into account, let alone fully controlling every single variable which may bear on its results. Under these circumstances, *awareness* of the problem is a minimum requirement: It is the only guarantee for partially justified conclusions, at least.

Although Tirkkonen-Condit showed admirable prudence in this respect, this still proved insufficient. What she did was have her subjects evenly divided into two groups, on the basis of two parameters: *conversance with the subject-matter*, on the one hand, and (to use her own formulation) *linguistic sophistication*, on the other. Thus, the members of the one group were all biology students who had taken part in specific environment seminars, whereas the members of the other group were all teachers of Finnish, the language of the texts they were required to compare and assess.

I do not wish to go into tricky questions such as what being a language teacher guarantees, in terms of 'linguistic sophistication' (whatever that may mean), or whether being a biology student excludes such sophistication. Suffice it to say that these two variables can hardly be expected to yield two mutually exclusive groups whose responses will therefore be comparable along a single dimension. The advantage of the division should be seen in the fact that the population experimented with was not an amorphous lot, and that *any* kind of division offers at least *some* basis for comparison, if need arise.

And need did indeed arise. As it turned out, the impressionistic assessments of the two groups did *not* concur. In fact, the results exhibited by the language teachers seemed to go so far as to *invalidate* the very hypothesis that was being put to test; namely, that impressionistic assessment of texts presented as translations is involuntarily governed by a definable set of textlinguistic features. Solution for this difficulty was sought in the assumption that the rate of reliability of the subjects may not have been the same across the two groups — an assumption which could be put to the test only because of the initial division. Thus, Tirkkonen-Condit applied Cronbach's alfa coefficient to measure the agreement among the subjects of each group, hence the probability that they may have had homogeneous criteria of judgment. And, indeed, it emerged that the evaluation criteria used by the mother-tongue teachers had been *heterogeneous*, whereas

for the biology students the high 'judge reliability' was taken as indicative of *homogeneous* criteria. At least an admission of different *criteria*, then, if not yet of different *objects of assessment*!

The obvious next step would have been to break the problematic group of mother-tongue teachers down according to some other parameters, and compare the assessments of the resulting sub-groups. This measure was not so much as suggested, however, probably not only because of the small number of subjects, but mainly because no one as yet has any clear idea as to which variables could be of [higher and lower] relevancy for such a subdivision, in terms of Translation Studies: Age? Teaching experience? Belonging to one or another subculture? Previous encounters with translations? Practical experience in translating? Or?.. Be that as it may, I would argue at least this: on the basis of other parameters, *other* groups could very well have been established, each consisting of members of *both* original groups, which would have yielded more instructive conclusions. For I would certainly challenge Tirkkonen-Condit's conviction that biology students are simply "better representatives of the *ordinary* reader" (1986: 60; italics added). When it comes to texts in their own field of expertise?! Especially when, for them, texts in this field may have become almost tantamount to translations?

In spite of all these pitfalls, I would by no means disclaim the value of Tirkkonen-Condit's experiment. Not only is it already possible to take its results, with the necessary measure of caution, as highly indicative, but the experiment itself can easily be *replicated* in a more elaborate way, taking all reservations into account. Moreover, similar experiments can be administered in other cultures too, thus shedding even more light on what is universal and what is culture-specific in so-called 'intuitive assessments' of translations.⁸

3. Process-oriented empirical studies

Of much greater significance still is the application of empirical methods to the study of the *process* of translation; after all, experiments here were not designed just to supplement other research methods, or add to the precision and/or reliability of their results. In an important sense, they entered into a real void, were applied to new types of data and opened completely new vistas.

8. In this connection, the reader may also wish to study the elaborate, though by no means non-problematic way in which questionnaires were recently used by Stegeman 1991.

Of course, it is not as if there have never been speculations concerning the mental processes involved in translating. We have had, for example, Jiří Levý's 1967 model of 'translation as a decision process', James S Holmes' 'two-plane text-rank translation model' of 1976 (1988: 82-86), or Vladimir Ivir's model of the ordered, rule-governed oscillation between 'equivalence' and 'formal correspondence' during the act (1981: 58). It is only that none of the speculative models was ever put to any real test. (And cf., in this connection, Lörscher 1989; 1991: 7-27.) On occasion, we also had process-oriented hypotheses to account for the findings of contrastive analyses of the output and input of individual acts of translation, but these were, and could be only tentative. Even if they did have a certain amount of explanatory power, they could hardly make a claim to *psychological* validity, that is, to account for the process 'as it really was'; not even at the level of one translator in one text, let alone in any general way.

Somewhat greater approximation to the 'little black box' was achieved through the analysis of those stages in the emergence of individual translations which were realized and fixed in writing (see Chapters 9-10 above). What we have here is a series of *interim* outputs which, if ordered correctly, could be compared not only to the one input of the process, but to each other as well, thus yielding insights into certain aspects of the translation process, e.g., the way translators replace one tentative solution by another, keep changing the units they are working with, etc. Even though processes other than pure translating may be involved in the gradual emergence of the end product, this type of study (which, to be sure, is descriptive proper, not empirical) is bound to result in many interesting findings. At the same time, its main contribution to date seems to have been casting doubt on the universal validity of any model of the process which had been formulated on speculative grounds only. What it was able to demonstrate, in other words, was the urgent need to *modify* those models, first and foremost in the *probabilistic* direction which we so strongly advocate.

One major limitation of observational methods in this problem area stems from the fact that they are all applicable *in retrospect* only. Once the wish arose to close in on the mental process as it evolves – in real time, as it were – there was no escape from turning to experimental methods, such as have already proved their usefulness in adjacent fields. Thus, for instance, certain conclusions as to the parsing of the source text during a translation session were tentatively formulated on the basis of an experiment where the EYE MOVEMENTS of translators were measured (McDonald and Carpenter 1981), and correlations between PUPIL DILATION and the MENTAL LOAD experienced by simultaneous interpreters are now being investigated (see Tommola and Hyönä 1990). However, the method

which seems to have gained widest popularity, even though it has only been some ten years since it was first applied to subjects as they were translating, is the technique of 'Thinking-Aloud Protocols', which was already mentioned in Chapter 4, in the context of establishing coupled pairs of 'replacing + replaced' segments.

3.1. *Thinking-Aloud Protocols*

To be sure, Thinking-Aloud Protocols, TAPs for short, have been used for the collection of introspective data on translating in several, slightly different ways. However, this is basically what the technique boils down to:

Subjects who are faced with the task of producing a translation are asked to say aloud whatever comes into their minds while they are working on it. The verbalizations are recorded — ideally, video-recorded, in order to catch non-verbal features as well, the recorded protocols transcribed, and the running transcripts submitted to meticulous analysis.⁹ The claim is that this method

allows particular analyses of the levels, steps, units of processing, the role and the interaction of the [source] and [target] language,¹⁰ the amount of proceduralization, the origin and course of search processes, and the times used for these processes. (Dechert and Sandrock 1986: 115)

9. The following is a slightly modified version of the notation system used for that purpose in one of the first studies where TAPs were applied to translation, a 1986 article by Hans Wilhelm Dechert and Ursula Sandrock which draws on Sandrock's *Diplomarbeit* of 1982. It is offered by way of an example only:

Reading of source text

Reading of target text

Utterances concerning punctuation

General commenting utterances

Utterances concerning [mental] lexical search

Utterances concerning dictionary search

Explicit utterances concerning translating

Concurrent verbalizations of the writing process

Three different temporal variables are noted too:

Pauses (periods of silence between utterances)

Speaking time

Writing time

10. Originally, the labels 'first' and 'second' language were used, given that the main focus of psycholinguistic research, at that time, was on translation as carried out by language learners.

As already noted, it would be wrong to maintain that thinking-aloud provides any *direct* access to the mental process. Rather, the analysis is applied to *products* again – protocols, this time – which the investigators assume tell them “something relevant about the underlying processes” (Hoffstaedter 1987: 76). The argument is, then, that even though TAPS “should not be taken as direct reflections of thought processes”, they can be regarded “as data which are *correlated* with underlying thought processes” (Olson et al. 1984: 254; italics added), and hence taken as strongly *indicative* of them.

The validity of introspective data for the study of cognitive processes has often been questioned. However, most of the objections seem to have been disproved in an admirable way. In fact, it has been so much as claimed (e.g., in Krings 1987: 166) that, of all mental processes, translating is particularly suitable for verbal reporting. Be that as it may, our concern here is not with psycholinguistic validity at all (which was touched upon in Chapter 4), but with the relevancy of the technique as such from the viewpoint of Translation Studies. And here I can see at least one problem, namely, the prospect that, during the verbalization, activities of various types interfere with each other. To be sure, I am not referring again to the claim that the need to verbalize will in itself interfere with the translation task – a claim which has been taken care of by the researchers themselves. My concern is rather with the *possible interference of two modes of translation*.

Thus, what the experiment claims to involve is basically the gradual production of a *written* translation of a *written* text. However, the need to verbalize aloud forces the subjects to produce not just *mental*, but *spoken* translation before the required written one; and there is a real possibility that spoken and written translation do not involve the exact same strategies. For instance, researchers into interpreting – spoken translation *par excellence* – have noted that it is a gross “misconception that you can put written products of any kind into the system and get a[n oral] product at the other end that possesses all the characteristics of the original” (Jumpelt 1985: 83), or, for that matter, all the characteristics a *written* translation of those “written products” may possess. This is so even if the interpreter has before him/her a printed version of the ‘same’ text (which is never really the same anyway), which would make his/her task more compatible with the normal application of TAPS. Needless to say, the differences observed are not necessarily due to the *medium* alone that the translated utterance is transmitted in (i.e., spoken vs. written); they may well involve features of the *utterances* as well. After all, in any living language different variants are used for speaking vs. writing.

Nor would a remedy be found in simply claiming that the results should be taken to refer to just that process; namely, a kind of 'spoken-written' translation. To the extent that this hybrid has any existence in the world of our experience, it is at best marginal, notwithstanding the observation (Krings 1987: 166) that some translators sometimes do betray certain external symptoms of 'inner speech'. Its relevancy for the establishment of a general 'psycholinguistic model of translation' (which is often presented as the ultimate goal of the very use of TAPS [e.g., Krings 1987: 166]) therefore seems problematic, at best.

It stands to reason that many of the differences between the spoken-written and the purely written modes of translating would be traceable in retrospect too; namely, in the end products of the two. This kind of evidence is all too often neglected by researchers of the psycholinguistic denomination, in spite of its high significance for their purposes. After all, the establishment of a product not only marks the end and forms the very *raison d'être* of the problem-solving process which translating is; it also lends it its *directionality*. Let us have a brief retrospective look, then, and see what we can get out of it, with respect to the understanding of a Thinking-Aloud Protocol.

<i>Trotz</i>	<i>der</i>	<i>Tatsache,</i>	<i>daß</i>	<i>es bestimmte Unterschiede gibt</i>
In spite of	the	fact	that	there are certain differences
Trotz	der	Tatsache,	daß	es zwischen britischem und amerikanischem

<i>zwischen</i>	<i>britischer</i>	<i>und</i>	<i>amerikanischer</i>	<i>Aussprache</i>	<i>und</i>	<i>Wortschatz,</i>
between	British	and	American	spelling	and	vocabulary,
Englisch bestimmte Unterschiede in Hinblick auf Schreibung und Wortschatz						

<i>Standard</i>	<i>Englisch,</i>	<i>das</i>	<i>geschriebene</i>	<i>Englisch</i>	<i>in</i>	<i>Büchern</i>	<i>und</i>
Standard	English —	the	written	English	of	books	and
gibt, ist Standard-Englisch — das	Schriftenglisch	in	Büchern	und			

<i>Zeitung</i>	<i>ist</i>	<i>ziemlich</i>	<i>das</i>	<i>gleiche</i>	<i>in der ganzen</i>	<i>Englisch-sprechenden</i>	<i>Welt.</i>
newspapers —	is	much	the	same	throughout the	English-speaking	world.
Zeitschriften —	ist	überall	in der	englisch-sprechenden	Welt weitgehend gleich.		

Figure 13. An English text accompanied by two German translations

In Figure 13, the first sentence of an English text is given, accompanied by two German translations. The top translation, printed in *italics*, is the written output of a translation act which was verbalized and protocolized (Sandrock 1982); the bottom one, printed in **boldface**, represents the end product of a process which was not. The two versions obviously differ greatly; not so much in their lexical choices, but certainly in terms of the segments which can be tentatively coupled with corresponding segments of the source

text they have in common. Thus, the top translation manifests a much larger number of segments, coupled with an average small *scale* and *rank* of individual segments. In terms of the relationships which hold together the members of each pair thus established, it is clear that the top translation reveals a greater tendency towards *formal correspondence*. In other words, most of the target-language replacements show traces of source-language *transfer*, some of them so much as of the 'negative' type.

Now, the top version was produced by an advanced German learner of English, whereas the bottom one is the model translation offered by the teacher's manual of a textbook which used the text as a translation exercise.¹¹ Whatever the latter's status as a 'model' translation, it was evidently produced by a more experienced translator than Sandrock, an "English and French major in her ninth semester" who "had had 9 years of English in secondary school and the opportunity for intensive training by English and American native speakers" at the University (Dechert and Sandrock 1986: 115). Had the two versions been produced by one person, we might have concluded that the differences observed reflect the 'spoken-written' opposition alone. As it is, however, they can be related to the 'novice-experienced' opposition too.¹² In my fervent search for interdependencies I would even venture to hypothesize that the two variables may be correlated such as it is the need to perform orally which brings experienced translators who have had no experience in oral translation closer to inexperienced ones; for instance, by pushing their utterances from the 'literate' towards the 'oral' pole of the famous continuum.¹³

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11. *English G*, vol. 6A, Lehrerhandbuch. Berlin: Cornelsen-Vehlhagen and Klasing, 1983: 207.
 12. Another possibility is that, unlike Sandrock's translation, the 'model' translation underwent some *post-editing* after it had been put down on paper. While translation editing — even when performed by the very same person — may well be a different kind of activity from translation proper, it is normally inseparable from it in retrospective studies (which is an inherent weakness of this kind of research). These reservations, however, should not prevent us from reconstructing two different concepts of translation, each underlying one of the *texts* as entities in themselves. Thus, it is clear that Sandrock's translation is more source-language-oriented, while the 'model' translation is more target-text-oriented: whereas the latter is primarily geared to acceptability in a certain sector of the recipient culture, the former makes do with solving a series of discrete cross-linguistic problems. Two contending concepts of one type of activity, to be sure, rather than two different activities, a translational and a non-translational one!
 13. For the interpretation of translational shifts in terms of the oral-literate continuum see, e.g., Zeller Mayer 1987; M. Shlesinger 1989a.

To be sure, these are all untested hypotheses which still need exploring. However, I do believe it is about time that the application of TAPS stop being oriented towards the establishment of that general 'psycholinguistic model' alone and also be used to test smaller-scale, hence more specific hypotheses. Needless to say, any in-depth study of this kind is bound to have positive implications for attaining the ultimate goal as well, which is all the more reason to turn to it, for theoretical as well as applied purposes.

Thus, some differences of performance along the 'inexperienced-experienced' axis have now been substantiated on the basis of TAPS too (e.g., Krings 1988; Tirkkonen-Condit 1989; Jääskeläinen 1989; Lörcher 1991). At the same time, this substantiation was not as decisive as could have been expected. In fact, in several studies (e.g., Gerloff 1988; Jääskeläinen 1990), 'professional' translators seemed to engage in considerably larger amounts of processing activities than novices. As a result, the distinction between *routine* and *non-routine* tasks in professional translation started being taken into account as an additional variable. While routine task performances can rely heavily on automatized processing, non-routine tasks probably demand conscious processing as well, so that competent translation may well depend precisely on the ability to shift flexibly between automatized and controlled processing. Furthermore, it seems that while some processes become automatized and are dealt with unconsciously, other kinds of processing may take up the released processing capacity in working memory (see Jääskeläinen and Tirkkonen-Condit 1991: 105).

Finally, it is not enough to simply take heed of differences of these kinds, not even by establishing distinct variants of the overall psycholinguistic model. It is also vital to give some thought to the process whereby one type of 'translation competence' evolves into, or is perhaps replaced by the other. What we need to know, in other words, is not only what it takes *to perform translation*, but also what it takes *to become a translator*: is it reducible to the mere technicality of accumulating experience (which may indeed call for an explanatory model which is purely psycholinguistic), or does it involve the internalization of external feedback as well (which may entail switching the focus to socio-cultural factors, and hence make the psycholinguistic model of translation far more complex)? It would be easy to guess that this writer's tendency is towards the second possibility, which will soon be elaborated on (see Excursus C). Needless to say, finding the appropriate answers lies beyond the capacity of TAPS alone. At the same time, it won't be a bad idea to supplement this experimental technique with other research methods, experimental and observational alike, for the benefit of the discipline as a whole.

4. Concluding remarks

The overview offered in this chapter proceeded from the conception of the experiment as "an empirical procedure performed with a view to *testing* something, be it a tentative conjecture, a rather elaborated hypothesis, or perhaps a whole theory" (Agazzi 1988: 3). It now seems clear that the emphasis should have been put elsewhere; namely, on the direct object "something". The point is that many of the experiments so far applied to translation have been characterized precisely by a certain uncertainty as to what they are designed to do. The questions underlying them were often very general, even vague, and the investigators were all too ready to settle for general 'insights' rather than insist on answers which would bear directly on either theory or even ordered application.

The philosophy of science has taught us that an inquiry is a process whereby a set of hypotheses "which is in some respect unsatisfactory is modified and transformed into a new system. If the inquiry is successful, the resulting system is less unsatisfactory than the original one", in terms of both "informational completeness" and "error avoidance" (Hilpinen 1988: 16). In this vein, the first phase of the application of experimental methods to translation has had, at best, marginal success. At any rate, only at a pinch can one say that, in their wake, translation *theory* has become any "less unsatisfactory".

I certainly regard the transfusion of fresh blood as highly beneficial for a discipline like ours, which has long suffered from stagnation. However, it may well be the case that the introduction of experimentation to Translation Studies was a bit premature. Not necessarily because of any deficiencies in the methods per se but most certainly because, at that time, there were hardly any 'translational' hypotheses which could be put (or, for that matter, which were worth putting) to the test with their help. There can be no wonder that many researchers were carried away by the sheer joy of the experiment, or by the wish to try out a certain method as an end in itself. Also, it is no wonder that more than one study was contaminated by the notorious 'experimenter's effect': As is well known, experimenters can easily be biased towards facts that strengthen their hypotheses and views, while playing down those which are indifferent to, let alone weaken them.

In the meantime, things seem to have changed. Translation scholars are now formulating more and more small-scale, and hence more testable hypotheses within the discipline itself (rather than merely importing them from without

and projecting them onto translational phenomena).¹⁴ It is to be hoped that the mark of empirical studies will be more noticeable too, towards the establishment of a full-fledged, multi-facet theory of translation of a high explanatory (and some predictive) power.

14. In this connection, see also the important theoretical and methodological discussion offered by Gile 1991.

A Tentative Development Model

As already indicated, a prerequisite for becoming a translator within a cultural environment is gaining *recognition* in this capacity. Translatorship is not merely taken, then; it is granted. And since it should be *earned* first, it stands to reason that the process involves the acquisition of those norms which are favoured by the group that would grant the recognition. The crucial question is, of course, how all this comes about; in other words, how a bilingual would emerge as a translator, especially outside of the schooling system.

1. Nature vs. nurture in the making of translators

It was in 1973 that Brian Harris first argued for the importance of **natural translation**, "the translating done in everyday circumstances by people who have had no special training for it", as a primary source of data for "the scientific study of translating" as a human skill (Harris 1973). Unaware of this proposal, I myself put forward, a few years later, a seemingly similar notion, that of **native translator** (Toury 1980b), namely, within the applied framework of translation teaching.

The logic underlying my proposal was simple enough. As I was to learn, it was also very much akin to the justification which Harris had given for his own notion. For one thing, translation was seen as having obvious precedence over any formal teaching (and learning) of it, both chronologically and logically, in phylogenesis as well as in ontogenesis. For another, it seemed reasonable to assume that the graduates of a translation program would be called upon to produce utterances which would not differ in essence from those that have come

to be associated with translation in the community which those graduates would ultimately be performing in and for. It thus seemed desirable that translation didactics aiming at the attainment of this object would base both its goals and its methods on translation *as it is*, and not on any prefabricated notion as to what it should allegedly be like, which – no matter how it is formulated – is bound to be reductionist in its very nature and to fail to pay due heed to essential components of any native-like performance. At that initial stage, a 'native' translator was tentatively conceived of as one who has gradually grown into that role, with no formal training for it, i.e., hardly more than a crude counterpart of the common linguistic notion of 'native speaker'.

Painfully aware of the vagueness of the concept, I picked it up again a few years later (Toury 1984b) in an attempt to elaborate on the characteristic features of a native translator. I also ventured to present a tentative outline of the course of his/her development in a 'normal' setting, which – far from being a vacuum – was conceived of as an existing *societal environment* which entertains certain concepts as to what translation is all about. Meanwhile, Harris has not dropped the matter either: he too has devoted considerable effort (e.g., 1977a and 1978) to the notion of natural translation and to a (rather futile) attempt to gain wider recognition for it.¹ More important still, he made a few initial attempts at looking more closely at certain points along the acquisition of the skill among a particular group of subjects, namely young bilingual children. (See especially his richly documented account of "How a Three-Year-Old Translates" [Harris 1980].)

The notions of natural translation and native translator, which were not only launched in complete independence but also designed to serve different purposes, are symptomatic of the welcome change of orientation that Translation Studies has been undergoing on its way to becoming an autonomous discipline of an *empirical* nature. As the evolution of any science clearly shows, a change of paradigm always involves the raising of new questions, which were unthinkable within the replaced paradigm. An issue of precisely this kind is that of nature vs. nurture in the making of a translator, which is inherent in both notions.²

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1. Harris 1988b is an annotated bibliography on natural translation and related notions until 1987. See also the ongoing debate between Harris and Hans P. Krings on how relevant natural translation actually is for Translation Studies (Krings 1986: 19–22; Harris 1992; Krings 1992).
 2. It is interesting to cite, in this connection, a pioneering formulation by the Bulgarian scholar Alexander Ljudskanov: "Kraft einer bestimmten Intuition und einer Gewohnheit

In an attempt to advance the treatment of this issue, the relationships between the two seemingly similar notions will now be examined as will their respective explanatory power in accounting for both the emergence and the development of translating skills. For lack of any data that have not been used before, we will address ourselves to the notions as they have been put forward thus far and to the developmental models they entail, incomplete though they may be. We will then ask how such a model can be validated, using descriptive methods, and what implications its acknowledgement may have for deliberate attempts to train translators. The actual testing of the model will have to remain a desideratum.

In 1978, Harris took a decisive step in trying to couple the notion of natural translation with the developmental aspect: together with Bianca Sherwood he published a most intriguing article entitled "Translation as an Innate Skill" (Harris and Sherwood 1978), in which a number of case histories of young people, virtually from birth to 18, who had never received any instruction in translating and therefore could justifiably be said to have practiced natural translation, were presented and analyzed. One object of the analysis was "to trace the stages that the young natural translator goes through" (p. 155), and the article indeed presents a tentative model of how translational behaviour evolves. This model consists of three main types of behaviour, which – the claim goes – are acquired one after the other, in a strict order:

- (1) **Pretranslation** – "transposition from some language medium to another via an act which [is] linked by association with words in both" (p. 167).³ Here, a non-verbal entity serves as a bridge between linguistic items in two different languages, and it is the *inevitability* of such a bridge which is perceived as characteristic of the initial phase in the unfolding of translating skills, as are the *unconsciousness* of the whole process and the *small scale* and *low rank* – usually single words – of the items between which it proceeds.

übersetzt jedes zweisprachige Subjekt auf irgendeine Weise. Folglich stand bei der Wissenschaft von der HÜ [Humanübersetzung] im Grunde gar nicht die Frage, wie der Mensch überhaupt das Übersetzen lernt, sondern wie er vorgehen muß, um ein solches Resultat zu erreichen, das den apriorisch normativ festgelegten Kriterien entspricht" (Ljudskanov 1972: 223, note 42).

3. Leopold's entry which Harris and Sherwood lean on reads as follows: "One evening (B 1;4) she [Hildegard] said often and with enjoyment ['ap], when she wanted to get up on the davenport; ... On the same evening, when I told her 'aufstehen', *she got up and said* ['ap]" (1939–1949, 1: 35–36; italics added).

- (2) **Autotranslation** — translating what one has just said in one language into another; (i) to oneself (intrapersonal autotranslation) and (or: and then) (ii) to others (interpersonal autotranslation).
- (3) **Transduction**, where “the translator acts as an intermediary between two other people” (p. 165), (i) within the family (intrafamily transduction) and later on (ii) out of it as well (extrafamily transduction).

To be sure, the Harris-Sherwood model was not devised as an overall account of the emergence and development of translating as a human skill. Rather, it was confined, right from the start, to very young natural translators. Even within those limits, however, the adequacy of the model seems questionable. Above all, it is striking how very little concern was shown for the circumstances under which the transition from one phase to another tends to occur, which should form an inseparable part of any model that is inherently developmental. The relations between these circumstances and the characteristic features of the individual types of behaviour presented as phases are lacking too. What the authors focus most of their attention on is the *succession of phases* itself, along with its alleged *universality*.

Admittedly, in most of the case histories analyzed in the article the circumstances are simply not included in the account, and those that are referred to, in one or another individual case, are often incompatible with each other. The point, however, is that the authors have neglected so much as to raise the issue, as if it were of no consequence. What they did, in fact, was to collapse all the factors which may figure in the development of natural translation into one single variable, that of *age*.

Moreover, for them, age seems to be strictly *biological* rather than *bilingual*, as might have been expected, in view of the underlying postulate that “translating is coextensive with bilingualism” (p. 155): What is measured is always time since birth, and not since the commencement of bilingualism. Identifying the two is of course relatively easy to justify as long as the onset of bilingualism coincides with that of language acquisition in general, especially when a person is brought up bilingually right from the start. However, it becomes more and more dubious as the gap between the two onsets grows. Many of the case histories dealt with here indeed represent such an incongruity, which serves to undermine the identification of biological and bilingual age and casts serious doubt on the very selection of the former as one all-embracing factor.

In view of all this, I dare say that the establishment of a well-rounded developmental model, even for very young translators, does not emerge as a major object of the article. Thus, while Harris and Sherwood do maintain that

"one does not expect a natural skill to develop fully overnight" (p. 155), their article itself consists mainly in devising a defensible method for dealing with the precursory hypothesis that "the basic ability to translate is an innate verbal skill" (ibid.) — a hypothesis whose verification hardly requires the support of any evidence of a strictly *developmental* nature.

In order to achieve their aim, realizing that to confirm innateness it is "the youngest examples [which] are the most pertinent" (p. 156), the authors set forth to trace the different manifestations of natural translation *in reverse age order*, that is, from oldest to youngest. Developmentally speaking, what they are mainly able to show in this mode of presentation is that translation in older age is, as a rule, *non-voluntary* (whether actually elicited, or only stimulated by others) and *socially functional*, whereas the more one goes back in age, the more one is likely to encounter cases which are *spontaneous* and/or *functionally redundant*. This finding allows them to dispose of social motivation and functions as intervening factors and to regard them as almost alien to natural translation in its purest form, which thus seems to occur very early; not only developmentally (and in terms of bilingual age), but also in terms of sheer biological age. Having gone all this way, Harris and Sherwood are finally in a position to maintain that translation can indeed be regarded "humanly innate", if only in the 'weak' sense of innateness, namely as a mere "specialized predisposition in children" (p. 168).

2. An innateness hypothesis is not enough

There can be very little quarrel indeed with the argument that a *predisposition* for translating, which may indeed be coextensive with bilingualism, is "part and parcel of mankind's basic linguistic equipment" (Wilss 1982: 39). In fact, without an assumption of its innateness, there would hardly be a logical basis for the explanation of any of its overt manifestations, especially those which are indeed spontaneous and/or functionally redundant (to which cf. also Albert and Obler 1978: 218–219 and the studies referred to there). The qualitative leap would simply be unaccountable. On the other hand, the identification of translating as a *skill* with mere bilingualism seems an unwarranted oversimplification.⁴ After all, there is no answer in it to the crucial question of what it is

4. There is some evidence that translating can indeed be dissociated from other linguistic skills that go with bilingualism; and if it really *is* independent of them, it can hardly be

that brings forth the *unfolding* of the skill (that is, its realization in actual behaviour) and the way (or ways?) it then *develops*.

The supreme test of a skill (as distinct from the mere predisposition towards it) is no doubt in the performance, and it would seem rather far-fetched to assume that all bilingual speakers (merely by virtue of their bilingualism, that is), or even all infant bilinguals (by virtue of the added factor of their young age), will indeed *externalize* their innate competence, irrespective of anything else, and perform so much (or so little) as pretranslation and/or intrapersonal autotranslation. (In this connection, it makes absolutely no difference whether this performance of theirs would or would not concur with any particular conception of what translation 'is'.)⁵ It would seem much more convincing to argue that some additional factors are needed in order to trigger off the "specialized predisposition" for translating and set in motion — most probably, a certain combination of personality and environmental circumstances. Such an argument is nowhere to be found in Harris and Sherwood's account, even though they have purported to trace natural translation to the point where it just began unfolding. Therefore it is my contention that, in the course of the presentation, a rift has revealed itself between the innateness hypothesis and the need to account for the **emergence** of translating as a realized skill.

An even greater rift seems to have occurred between that hypothesis and the authors' proclaimed wish to account for the **development** of the skill. Thus, notwithstanding the lack of sufficient data (or maybe precisely because of it), I

conceived of as an immediate function of bilingualism as such, requiring no additional factors. One of the more convincing kinds of evidence to this effect comes from polyglot aphasia, where a bilingual is sometimes able to speak the two languages, but is no longer able to translate between them (Albert and Obler 1978: 218, referring to the case reported in Goldstein 1948: 141f.) as well as the other way around (Paradis 1980).

5. I am well aware of the argument that bilinguals, even 'natural' ones, are often "not in a position to translate or interpret *with facility* between [their] two languages" (Beardsmore 1982: 8; italics added), or that "many bilinguals who can function extremely well in two languages in clearly demarcated situational contexts often find it difficult to spontaneously translate between their languages *without heavy interference*" (Beardsmore 1982: 88; italics added). However, even if arguments of this type are correct, they do not constitute counter-evidence for the innateness hypothesis as such (as suggested, e.g., by Newmark 1981: 97), or for the existence of a "rudimentary ability to mediate" (Lörscher 1991: 41–45). As will become clear later, the ability to translate in accordance **with particular normative requirements** ("with facility", "without heavy interference", or whatever), which is at stake here, is precisely a matter of the *development* of the skill, not of the *predisposition* for it.

would hesitate to subscribe to a second claim of theirs, namely that "it may be that (i) through (iii) [i.e., pretranslation, intrapersonal autotranslation and interpersonal autotranslation] are *necessary* predecessors to socially functional transduction, at least in infants" (p. 167). What is actually maintained here is that all bilinguals who perform natural transduction and/or interpersonal autotranslation will have behind them, if they started their translating career early enough, a *personal history* of spontaneous, functionally redundant translation. Intuitively, this deduction would seem rather dubious, even though it cannot be refuted (nor, indeed, defended) for the time being, given the marked scantiness of data. Even more dubious would be the possible implication that, given sufficient time, every bilingual infant who has practiced the non-voluntary varieties of translation will necessarily move beyond intrapersonal autotranslation and ultimately reach the stage of socially relevant transduction.

The rift between the mere presence of a mental predisposition and its realization in actual performance, on the one hand, and the development of the skill over time, on the other, seems to reflect a deplorable deterioration of the promising notion of natural translation itself, which served as a starting point for the collection of data and a framework for its classification and analysis. After all, there can be no doubt that socially motivated and functional instances of interpersonal autotranslation and transduction all lie *within* Harris' initial concept of natural translation, namely, to the extent that they are performed "in everyday circumstances" and as long as their performers "have had no special training" for translating. Indeed, this variety (or phase) should be regarded as no less natural than the spontaneous, functionally redundant ones; it was never motivation and function but only circumstances and (lack of) special training that formed part of the concept.

In fact, once one agrees to follow Harris and Sherwood in gradually reducing natural translation in order to finally use its manifestations as evidence for an *innateness* hypothesis, and once that reduction is pushed to its logical extreme, one is liable to end up with the conclusion that, as far as translating goes, 'naturalness' culminates in a complete non-occurrence of overt performance, a paradox which would deal a deadly blow to any discussion of the *developmental* aspects of the skill. As a result, the notion of natural translation would have entirely lost its usefulness in accounting for both the emergence and the development of a bilingual speaker as a translator. The very notion may even appear virtually redundant, a mere synonym for bilingualism (at least in infants), especially if one also joins Harris and Sherwood in postulating that translating *itself* "is coextensive with bilingualism" (p. 155).

This is where the notion of **native translator** seems to come in much

handler, precisely because it proceeds from the observation that the acquisition of translating as a skill does *not* amount to the mere unfolding of an innate predisposition; in some internally organized succession of phases, or otherwise. Rather than being regarded as alien factors to be suppressed and disposed of, at least in the practice of argumentation, the importance of the social motivation for, and of the social functions of translating and/or its end products, should be acknowledged, and assigned their proper place within the concept and the developmental model that goes with it.

Thus, it is my contention that, whereas the predisposition itself for translating is indeed "coextensive with bilingualism", its emergence as a skill should be taken as coextensive with the ability to establish similarities and differences across languages, which may be termed 'interlingualism'. The unfolding of this skill, in turn, hinges upon the presence of a kind of transfer mechanism, which makes it possible to actually *activate* one's interlingual capacity and apply it to utterances in one or another of one's languages. It stands to reason that these added capacities are inherently different in different people, part of different mental structures, one possible factor still being the pertinent type of bilingualism and/or bilingual age. At the same time, these facilities seem to be *trainable* too, at least up to a point, a training which involves actual practice in translating in context, along with the reactions one may receive to one's behaviour.⁶ What the developmental model we endorse thus involves is a consequential extension of the notion of norm from translational performance of an individual to the very acquisition of the skill.

3. The making of a 'native' translator

Translating proper, even if done by young children, is basically a mode of *communicative* text production. As such, it is *interactional* in its very nature, involving — as any kind of interaction does — *environmental feedback*. This

6. To be sure, there are now neurophysiologists who maintain that the mind is not only trainable in principle, but is actually being trained all the time. Moreover, they agree that one of the factors which contribute to the training of the mind is *environmental feedback* to actual performance which is added to the information flowing from the cortex to that place in the brain where the process actually occurs. (Personal communication from the Finnish neurophysiologist Matti Bergström during the International Symposium on "Stand und Standorte der Translationstheorie", Tampere, 1988.)

feedback, which is bound to act at least as a kind of post factum product monitoring device, comes not only from the recipients of the translated utterance, but also from those who have commissioned the act of translating, and sometimes from the originator of the utterance to be translated as well. When realized by actual persons, these roles (in the sociological sense) — all parts of the interactional makeup of a translating *event* — may, of course, partially overlap. At the same time, they never fully coincide, or else the act loses its motivation and becomes communicatively redundant.⁷

The feedback that a translator receives is *normative* in essence: it concerns the well-formedness of a translation not just as an utterance in the receptor language and culture, but first and foremost as a realization of the specific mode of text production that translating is taken to be. At least by implication, the norms embodied in that feedback also apply to the relationships between translated utterances and their sources, especially in terms of whatever remains invariant under transformation. As such, they also determine the appropriateness/inappropriateness of the procedures utilized for the derivation of a translational output from a given input utterance.

It should be recalled that the notion of norm involves that of *sanctions*. It is the (real or assumed) presence of the latter which lends those norms that apply to translating their influence on a translator's behaviour: Under normal conditions, one would wish to avoid negative sanctions on 'improper' behaviour as much as obtain the rewards which go with a 'proper' one. This attitude holds especially for the novice, who — due to lack of sufficient experience — is likely to feel insecure as to what translating is all about, according to the conception of the societal group in and for which s/he starts operating. It is this conception which gradually crystallizes for him/her in a process of *initiation*.

In the initial stages of a translator's development, the feedback directed at him/her is exclusively *external*: a novice simply has no means of assessing the appropriateness of various possible products and/or of the alternative strategies that would yield them. After all, there is nothing which can be stipulated as a 'universal' criterion of appropriateness, irrespective of anything else: since complete interlingual equivalence — at all levels at once — is ruled out a priori,

7. Even according to Harris and Sherwood's account, pretranslation and intrapersonal autotranslation occupy a very small portion of the personal record, even of an *infant* translator. On the other hand, there is an interesting sense in which intrapersonal translation can also be considered communicational (e.g., Jakobson 1971b: 697f.), which would leave only pretranslation out of the strictly communicative domain.

there will always be more than one option. The only way is to have the cultural-ly acknowledged criteria supplied from without, namely, by those who already have, or believe they have them, and who are powerful enough to impose their will.

At the beginning, the main environmental feedback one receives is thus *overt* responses to one's verbal products, final or interim. In the latter case, the responses may act as a kind of an external monitoring device, to the extent that the translator immediately responds and revises his/her production accordingly. S/he may even so much as rephrase what s/he has already formulated, or, indeed, change his/her entire translational strategy. Little by little, however, translators may start taking *potential* responses into account too, i.e., those generated during the production itself. They thus develop an internal kind of monitoring mechanism, which can operate on the (interim) product as well as on the act of translation as such.⁸

Socio-culturally speaking, what emerging translators thus undergo is **socialization as concerns translating**. During this process, parts of the normatively motivated feedback they receive are assimilated by them, modifying their basic competence and gradually becoming part of it. At every phase of its development, a native translator's 'competence' therefore represents a characteristic blend of nature and nurture, of the humanly innate, the individually assimilated and the socially determined. It may also be hypothesized that to the extent that a norm has indeed been internalized and made part of a modified competence, it will also be applied to the production of more spontaneous translated utterances, in situations where no sanctions are likely to be imposed. When analyzed, the behavioural varieties involved in changes of translational competence may therefore prove a useful tool for checking not only the prevailing norms as such, but also their assimilation by individuals and, in the long run, the universals of the process of assimilation itself.

During their socialization as translators, bilinguals who indulge in the act develop routines which enable them to cope in an effective way not with some abstract notion of 'translation', but with a concrete task, that of translating in compliance with what conception of that mode of text production is pertinent to a particular societal group. This conception may well differ from one group

8. For the notions of 'process' and 'product' monitoring see especially Færch and Kasper 1983: Section 3; on the monitoring effect of the presence of expert interpreters on the performance of an acting one see Anderson 1978: 220f. and 222.

to the next, in place, time, social class and various other respects.⁹ Consequently, the process of initiation may have to be repeated when a translator moves into a different subculture, let alone a completely new culture, or else when one tries one's hand at new, and hence non-routinized translation tasks. At the same time, the repeated process will probably not have to start from scratch, due to those features which are universal to mediation as such and to many other components which seem to be relatively easy to transfer or modify.¹⁰

The nature of translation competence as it is acquired in real practice may also have much to do with the exact types of translational tasks one is called upon to perform. Thus, translators undergoing socialization also develop strategies for coping with specific types of problems that are likely to recur during actual translation. In extreme cases, they may in fact develop automatized ways of handling specific problems, even a series of fixed solutions which are mobilized whenever a certain problem occurs. To be sure, this is often an efficient way of evading a problem rather than plunging headlong into it, thus

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9. To take an analogy from a completely different area: a recent book on *Medicine and Culture* (Payer 1988) presents a comprehensive survey of the ways patients are treated in four different countries: the United States, Great Britain, France and former West Germany. The citizens of these countries are regarded as equally healthy and can expect to live to the same respectable old ages. Yet their physicians treat them in vastly different ways. Thus (as a *Newsweek* reporter put it), "It isn't how sick you are but where you are when you get sick that determines how you're treated by a doctor" (Clark 1988). For example, thousands of Americans undergo coronary bypass surgery every year for narrowing in the arteries of the heart, whereas an Englishman suffering chest pain would get many more anti-angina drugs. The reason for this difference seems to be that — owing to the aggressive, "can do" spirit of the frontier — American doctors want to do as much as possible, whereas British medicine, like British understatement, is low key. Payer's overall conclusion, supported by many similar examples, is univocal: "the way doctors deal with patients and their ailments is largely determined by attitudes acquired from their national heritage" (ibid.). Would anybody seriously claim that translation is any less culture-bound than medical treatment?!
10. This is one important reason why it is often so difficult to shed the translational norms which were internalized at school, e.g., while studying a foreign language, and to adopt those guiding principles which are pertinent to translational behaviour in other sectors of society, including institutes for translator training. Unfortunately, it is often the case that, when going out into the 'real world', graduates of translation programs too are required to get rid of mannerisms they were taught; specifically, whenever the notion of 'native translator' has *not* been adopted as a didactic model. (See Section 5.)

leaving more time and reserving more energy for less proceduralized parts of the task.¹¹ Such shortcuts seem to form an important part of a translator's acquired ability to cope with problems in real-life situations, involving, e.g., time pressure, growing fatigue, incomplete knowledge, and much more. From a certain point on, it may even compensate for incomplete and/or unbalanced bilingualism, which is one possible verification of the claim that an increase in the rate of bilingualism and an increase in translation proficiency do not necessarily run completely parallel.

It is on the basis of these considerations that I ventured to explain why, along with the enormous decrease that gradually occurs in the rate of negative transfer in the production of a translator who evolves in a culture which disfavours its manifestations, positive transfer is also considerably reduced. In a nutshell, my claim was that, during the initiation process, translators who internalize the feedback directed towards them train themselves to monitor so-called 'discourse transfer' *as such*.

To be sure, discourse transfer seems to be a universal concomitant of translating, a production strategy where

the second language which may be said to be activated during the attempted production of a translated utterance in a certain target language ... is not, as a rule, retrieved from the speaker's 'knowledge' but is directly available to him in the source utterance itself (or from where this utterance — or some part of it — is stored in the translator's brain). (Toury 1986b: 82)

To the extent that transfer is indeed disfavoured, the ensuing norm concerns first and foremost its *negative* manifestations. After all, even though they do reflect features of another text, in another language, the results of *positive* transfer are hardly discernible from normal target-language productions. In fact, the interference inherent in them becomes evident only when a translation is confronted with its source. However, when the anti-transfer norm has been assimilated by the translator and has become an *internal* monitoring device, it may well be activated not only *post factum*, on (interim or final) products, but also during the act itself; and since, psycholinguistically speaking, there is really only *one* type of procedure which yields both 'negative' and 'positive' results, in terms of their acceptability in the target language, a process of *generalization* may be said to ensue whereby the internalized monitoring device is gradually applied to transfer *as a strategy*. (The fact that such monitoring is indeed operative has been demonstrated, for

11. "It is interesting to notice ... how the translators' conscious decision-making changes with the growth of professionalism. While some decisions become non-conscious, or 'automatic', the translator becomes sensitised to new aspects of the task which require conscious decision-making" (Jääskeläinen and Tirkkonen-Condit 1991: 106).

instance, by tracing the genesis of single translations done by more or less accomplished translators. And see the analysis of the emergence of the English translation of a German phrase of Remarque's *Im Westen nichts Neues* in Chapter 9.)

Unfortunately, I was, and still am unable to put this tentative account of the process of initiation with respect to discourse transfer (see Toury 1986b, especially 86–91) to the test by using any developmental data of individual translators. These were simply unavailable. Instead, I resorted to the comparative use of translational performance of different persons of one cultural background at several points along the hypothesized continuum. I wouldn't claim that this surrogate did the trick, because it certainly didn't, but it did at least allow the two *extremes* to be demonstrated with relative ease. Not surprisingly, they seem to be interconnected with the number of segments into which a source-language utterance is decomposed for the purpose of its translation (which decreases with monitored experience) and with the average scale and rank of the segments themselves (both of which increase considerably).¹² Transfer – negative and positive alike – may thus be said to have been relegated to higher textual-linguistic levels rather than eliminated completely.

Another feasible developmental hypothesis seems to be the following: the greater the variety of situations one is put into – and hence the greater the variety of different, even though certainly partly overlapping subsets of translational norms one is exposed to – the greater the range and flexibility of one's ultimate ability to perform in a socially adequate manner. What would thus be acquired is *adaptability*, i.e., an ability to adjust to changing situations requiring different attitudes. By contrast, *specialization*, if and when it occurs, would probably be found to counteract one's adaptability and hence reduce one's overall translation competence. Different blends of specialization and adaptability are no doubt the rule here, and their implications make an issue for empirical studies.

All in all, the more advanced the developmental stage, the greater the weight of the acquired parts in the sum-total, at least up to the point where one's social standing is high enough to start *resisting* normative pressure with no real risk of negative sanctions. From that point on, a translator may not only act contrary to prevailing norms him-/herself, but may ultimately effect *changes* in them for the culture s/he is working in. It is the prestige that goes with socially acknowledged professionalism that is no doubt at the bottom of such a license. At the same time, there is no contradiction in terms between becoming a

12. See also the passage analysed in Chapter 12, Section 3.1.

professional and remaining a native translator. Rather, growing professionalism is one possible outcome of consequential nativeness, which therefore lends it the power to change the very notion of translation for the society in question.

Be that as it may, in contradistinction to Harris' natural translation, where a distressing gap was revealed between the innateness hypothesis and the need to account for the emergence and development of translating as a skill, the notion of native translator applies to overt behaviour only and is intrinsically developmental. It is therefore not only more flexible and convenient to work with; it also seems more in keeping with the actual process of initiation one would undergo on one's way to becoming a culturally acknowledged translator.

4. How would a developmental model be validated?

The centrality of norms for accomplished translators has already been amply demonstrated. The problem which faces us now is how to go about validating a developmental model which is constructed exclusively around the evasive notion of socialization.

The data available in the literature, including the invaluable additions in Harris and Sherwood 1978, are of very little help here, for at least three reasons:

- (1) they were collected for diverse purposes and in a variety of theoretical frameworks and methods (including simple 'recall', after twenty odd years; see Harris and Sherwood 1978: 156), most of which had very little to do with Translation Studies of any kind;
- (2) most of them cover very short periods of time for each subject, and only a very small part — and at a pinch (e.g., Ronjat 1913; Leopold 1939-1949; Raffler-Engel 1970; Sherwood in Harris and Sherwood 1978) — can be characterized as longitudinal;
- (3) almost all available data are fragmentary and therefore offer very little basis for direct comparison. More often than not, they are also available in a processed version only, rather than as 'raw material'. The processing itself was inevitably done according to the assumptions and requirements of a particular study too, which makes it difficult to fully reconstruct the raw data.

From the non-existent, the required can easily be inferred: what we need is sets of *comparable* data susceptible to a *developmental* interpretation, and especially *longitudinal* studies in the true sense of the word, in which data would

be collected according to the requirements of a study of *translation as a distinct skill*. Since what is at stake is not one or another variable, but the totality of factors which may have a role to play in translation as a communicative interaction, along with their relationships and the way they all develop, the most appropriate type of data to be collected and submitted to analysis would no doubt be the **interactional event as a structured whole**. Contextualization thus appears no less central in the study of the evolution of translation capacities than it has been found to be for single instances of performance.

To take a final example: As I have claimed elsewhere (Toury 1982a: 74ff.; 1982b: 72ff.), there is a real possibility that — in non-voluntary translation — different stimuli (which I referred to there as 'translate!' instructions) lead to the utilization of different procedures and strategies, hence yielding different products, each taking different (clusters of) features of the source utterance as their invariant. This hypothesis may now be tentatively supplemented by the developmental claim that the feedback a translator receives from his/her immediate environment during and after the act of translating is somehow interconnected with that instruction and with the mode of performance it enhances. Moreover, this association is one of the main ways in which the resulting mode of performance finally becomes habitually connected with the type of instruction in question.

Hypotheses of this type could hardly have been expected to be put forward within previous paradigms of Translation Studies. There is no possibility of putting them to the test either, unless the unit of analysis is indeed the translation event — and the studies themselves longitudinal.

5. Possible implications for translator training

The fact that teachers have been endowed with the power to enforce norms on their students often results in an irresistible temptation to do so. To wit, many students actually prefer to have an authority dictate narrowly defined modes of behaviour and train them in the application of rigid principles. Insecure as they understandably are, they simply like to 'play it safe', first and foremost in class, where they are constantly under observation, not to mention in exam situations, where the teacher's power can be exerted to capacity. What they do not know is that the unfortunate graduates of translation programs often have to undergo a painful process of forgetting much of what they have been taught, and adjusting, at least in part, to prevalent norms of socio-cultural appropriateness which their teacher tried to change rather than endorse.

Granted, the process of becoming an accomplished translator does involve the acquisition of more, and more elaborate routines. Acting automatically, however, according to some mode of normative conditioning at too early a stage, may well be detrimental to the evolution of a translator-in-the-making — which should ideally be gradual. What trainees really need is the opportunity to abstract *their own guiding principles and routines* from actual instances of behaviour, with the help of responses to their performance which are as variegated as possible. Thus, during the training period, the pedagogically most appropriate key concepts are those associated with experiencing, exploration and discovery, involving as they do a considerable element of trial and error. What this amounts to, in fact, is a plea for partial implementation in the teaching context of the principles underlying socialization with respect to translating.

To be sure, what I have in mind is not a close imitation, in its entirety, of the process whereby a bilingual speaker becomes a 'native' translator — which would be counter-productive. After all, today's schooling system is anything but a 'natural' environment. At the same time, the curriculum makes it possible to construct ordered syllabi in pursuit of consciously and explicitly formulated goals, thus avoiding futile efforts and saving the community time and money, due to the accelerated pace so typical of training programs. Then again, a syllabus is simply much more focussed, much less diffuse than any assortment of real-life situations one may randomly come across. The graded progression also makes it possible to bypass certain types of error, which is good — as long as room is left for at least *some* mistakes.

A major flaw in the curriculum has to do with the fact that the environmental feedback which a trainee *qua* translator normally finds him-/herself exposed to tends to be rather limited; not only, nor even mainly, in strict quantitative terms, but first and foremost in terms of its relative *one-sidedness*. After all, most of the responding agents, and certainly all the ones that count, are teachers, who, moreover, tend to adhere to a very similar set of norms. To ensure proper preparation for real-life situations, this one-sidedness deserves to be mitigated, namely, by exposing the student to additional kinds of feedback. True enough, the additional responses may well be much more intuitive, coming (as some of them no doubt would) from less professional parties. However, as such, they are also likely to be more representative of society at large, and hence of the norms which actually govern translational behaviour in it.

To cater for a greater variety of attitudes within the schooling system itself, tutors could be hired who are pronounced *non-conformists*. Moreover, care should be taken not to unduly pressurize them to adjust, lest the very reason for

hiring them be lost. In addition, students themselves should be encouraged to respond to their peers' performance, even to assess it outright. Students' assessments should be taken very seriously too, even serving as a basis for the final grade of at least some of their peers' assignments; and it should be made quite clear to the students just how important their evaluation would be, so that they treat their task as assessors in all seriousness.

Above all, training time is the time to introduce students to the give-and-take so characteristic of real-life acts of translation. They should be made aware of the fact that there are many factors which may act as constraints on the process, its products and the ways in which these products will be received at, and function at the target end – and that their job is really to manoeuvre among the various constraints, so that for every gain something will necessarily be given up. In other words, the 'We Know Better' stance, underlying the implementation of normative conditioning in translation teaching, should be dropped, and an alternative slogan adopted instead; namely: Everything Has Its Price. Students would thus be trained to consider for themselves what stands to be gained by making a certain decision, what would have to be sacrificed, whether the gain is worth the loss – and whether there are any more appropriate modes of behaviour in terms of the balance between gain and loss within the constellation of the recipient culture.

Of course, solutions arrived at in this way will often resemble those advocated by an external authority. After all, many teachers are acknowledged translators in their cultures who resort to this kind of inner negotiation when they themselves engage in translating. However, even in these cases, being the result of genuine decision-making, the status of the solutions reached would be very different, offering as they would a strong element of *achievement*, which is an important pedagogical merit in itself. Decisions reached in this conscious way would also be explainable and/or defensible in more rational terms; arguments in terms of 'gain' vs. 'loss' are simply more cogent than any doctrinaire haggling as to whether something is being done the way it allegedly 'should be'. It would in fact be appropriate to introduce these negotiations directly into the classroom setting as a matter of course, not only among the students themselves, but also vis-à-vis their teacher, with the teacher's willingness to capitulate implied.

One enlightening way of coming to grips with the idea that, in translation, there are always several ways to go and that there is never just one legitimate option – which hence 'should be' adopted – is to have the students (and teachers!) analyze many existing translations, past and present. This analysis should be carried out with an eye to determining how their make-up and/or the

relationships obtaining between them and their respective sources are interconnected with their (intended or realized) positions in the target culture, and with no immediate evaluative purpose in mind.

This exercise in 'Descriptive Translation Studies', which will inevitably involve the notion of norm, along with its all-important features of culture-specificity and changeability, could easily be supplemented by an attempt to actively apply the governing principles as they emerge. Thus, students could try their hand at producing a translation the way translator X might have done, or the way it would normally have been done in period Y, or for audience Z, and the like. This would often result in translations which run counter to current norms, and yet, given the nature of the exercise, there would no longer be any fear of negative sanctions. This, in fact, is how my longstanding suggestion to train translators so as to violate translational norms (Toury 1980b) should be understood: not as an end in itself but rather as a means of opening the students' eyes to the multiplicity of modes of translation, all of which may be legitimate, according to one set of norms or another, and helping them wend their own way through the dark forest.

Beyond Descriptive Studies Towards Laws of Translational Behaviour

Sciences *qua* sciences are characterized by an incessant quest for laws, i.e., theoretical formulations purporting to state the relations between all variables which have been found relevant to a particular domain. In fact, "while the nature of these 'laws', their status and the norms for their formulation, accessibility and acceptability may well have changed throughout history, getting them in the first place has remained an unchanged goal. No scientific activity, indeed no 'theory', is conceivable without them" (Even-Zohar 1986: 75).

Granted, in Translation Studies, very little conscious effort has so far been invested in attempts to establish such laws. However, as soon as the applicability of science to the complex of problems clustered around translation has been accepted as such, there is no reason why the formulation of laws should not mark the horizon here too. Moreover, any quest for laws would seem desirable not only in terms of the discipline itself, but from the point of view of its applied extensions as well, to the extent that the underlying rationale favours basing them on *reality* rather than on some kind of wishful thinking. Let us therefore conclude by devoting some energy to an exemplified discussion of the possible nature of laws of translational behaviour, premature as such a discussion may still seem.

1. Non-lawlike generalizations

As is often the case, the notion of law can be brought into relief by first considering a few types of statements which have been put forward as laws but which are not in a position to perform anything of what a truly *theoretical* law is called upon to perform.

1. *Lists of possibilities do not constitute laws.*

First of all, the mere enumeration of everything that is initially possible in acts, texts or any other phenomena, which may be presented and/or regarded as translational, would yield no satisfactory laws.

To take a simple example, which will run throughout this discussion: Encountering a living metaphor in a text and approaching it as a single unit, on whatever grounds, one always has a *closed set* at one's disposal rather than an infinite number of translational modes of behaviour. Moreover, the process of scanning the list and selecting from among the options which comprise it can easily be given a *logical structure*, as a "game with complete information", i.e., "a game in which every succeeding move is influenced by the knowledge of previous decisions and by the situation which resulted from them" (Levy 1967: 1172). For instance:

I. *replace vs. not-replace (i.e., omit)*

1. if replace, then by a *metaphor vs. non-metaphor*

A. if replace by a metaphor, then by a *living vs. dead* metaphor

i. if replace by a living metaphor, then by *the same vs. a different* metaphor

a. if replace by the same metaphor, then by ... (etc., etc.)

b. if replace by a different metaphor, then by ... (etc., etc.)

ii. if replace by a dead metaphor, then by ... (etc., etc.)

B. if replace by a non-metaphor, then by ... (etc., etc.)

Such a structure can even be presented as a flow chart, as it was by Levy himself (1967: 1173), or by Leppihalme (in press) with respect to the allusion as a translation problem.

Disregarding the complex issue of their psychological validity, which should certainly be taken into account in all seriousness, flow-chartable accounts of this kind are no doubt *theoretical*: they present both options and logic of selection in a highly systematized form. The main reason why they do not, as yet, constitute proper laws of translational behaviour is that bifurcation in their various nodes appears to offer free either-or choices, unmotivated by anything except maybe the nature of the translation 'problem' itself, as it is determined from the vantage point of the source text.

Lists of initial possibilities thus appear to be *neutral* with respect to any factor which may affect decision-making in real-life situations, where the game is far from being played "with complete information": the relations between the two languages involved in the act, genetic and/or historical; the texts in which the problem-items are incorporated, and the solution-items will be; the models

underlying those texts and the tradition to which each model pertains; features inherent to the 'bilingual brain', and to acts of linguistic mediation; and the general concept of translation underlying a particular act, which is always norm-governed and hence culture-specific by its very nature.

All in all, lists of this kind are equipped to deal with *options* rather than actual *choices* involving *decisions* – which makes them necessary, but all too elementary, in terms of a theory of translation.

2. *Directives are not laws either.*

On the other hand, directives cannot serve as laws either. True enough, unlike mere lists of possibilities, normative formulations are often supplied with modifying factors of one kind or another. However, this in itself does not grant them any theoretical status, let alone the status of a *law*.

To return to our living metaphors approached as translation units, what would fall under 'directives' is not only flat prescriptions like the following dogma concerning the low-level I.1.A.i. choice:

the image of an original metaphor, unlike that of a stock metaphor, should normally be transferred [in translation]. (Newmark 1981: 93)

It also covers more sophisticated formulations such as the following view of the highest-level choice (1):

whether a metaphor ... should be translated at all ... must depend on the structure and function of the particular metaphor within the text concerned. (Snell-Hornby 1988: 59)

To be sure, formulations using modals – mainly verbs such as 'should' and 'must', or their antonyms – abound in our field. Not only are they much more common than neutral lists of initial possibilities, but they seem to constitute the rule in many paradigms of Translation Studies. More often than not they are even masked as downright theoretical formulations, i.e., as inherently binding rather than as a reflection of a particular socio-cultural attitude. Still, however urgent its presentation, the truth of the matter is that a directive represents little more than a recommendation: while entailing a clear wish to promote one or another mode of behaviour, the kind of behaviour it tries to promote can always be either *accepted* or *rejected* – either followed or ignored – in actual practice.

Thus, while stating what a translator 'had better do, or else' (in the opinion of those responsible for its formulation), there is absolutely no certainty that a

normative pronouncement would draw on, or even reflect, any kind of behaviour which is truly regular within the culture it purports to represent. Moreover, in spite of the authoritative tone in which it is often presented, ignoring it would not necessarily call for any sanctions. In other words, directives which do not reflect any existing behaviour would not unconditionally create new behavioural patterns either.¹

The schism between actual and recommended types of behaviour manifests itself in the clearest way whenever a directive is reformulated in the affirmative – and the truth value of the resulting wording is submitted to examination. It is here that serious doubts immediately occur.²

Newmark's dogma: "the image of an original metaphor ... IS NORMALLY transferred [in translation]"; or is it? And, since it sometimes is, what does the modifier "normally" stand for? Does it really imply "the majority of cases" (has anybody bothered to count?), or maybe just cases which would be *positively* marked by Newmark and those who accept his norms?

Snell-Hornby's formulation: "WHETHER a metaphor ... IS translated at all ... DEPENDS ON the structure and function of the particular metaphor within the text concerned" – how is one to tell, without going back to the behaviour of real-life translators? And what is one to do, should that behaviour be found to *refute* the sweeping totality of this maxim?

The absence of a recommended mode of behaviour on the list of initial possibilities would of course call for immediate action of a *theoretical* nature, namely, having the list itself submitted to revision; unless it could be shown that the option in question is initially impossible, that is, despite its having been presented as a directive. Such a falsification would involve theoretical reasoning again, albeit of a different kind. However, recourse to theoretical considerations

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1. One notorious exception is the classroom, where the teacher's (and examiner's) authority is what counts. However, in most societies, a translation class is not only a *marginal* case, but a rather *anomalous* one too, in terms of socio-cultural relevance. As already indicated, it is quite often the case that the graduates of a translation program have to forget some of what they have been taught, if they wish to fulfill their task in a socially relevant manner.
 2. This applies not only to (lower-rank) behaviour *within* translation, but also to translation itself as a kind of behaviour. For instance, "translation is always of the speaker's intended meanings" (Triki and Atari 1993: 241), which is what many (but not all!) would like it to be and what it very often isn't, at least not in full, or not exclusively.

as such is no reason to grant a recommended type of behaviour any special theoretical status. After all, the absence from the list of an *existing* behaviour of any kind would require the same kind of revision, and more justifiably so. It is simply that – unlike recommendation vis-à-vis impossibility – existence and impossibility constitute a flat contradiction. This holds true even if the legitimacy of a particular mode of behaviour is questioned within a given normative framework: Existence should be justification enough for inclusion among the initially possible options, and in a privileged position too, due precisely to its having been favoured by some ‘persons-in-the-culture’.

The series of existing and recommended options thus emerge as subsets of the overall list of possibilities, yet each of them embodies a different constitutive principle, empirical vs. postulated (and see end of Part One again, including Table 1). The position of either subset with respect to the entire range of possibilities is thus straightforward. By contrast, the relations between the two subsets themselves cannot be regarded as given in any way; establishing them requires conscious research efforts, so that they are always pertinent to particular cases only. Any attempt to draw an immediate generalization, with no prior account of cases of other kinds, would entail a methodological fallacy.

Of course, the position of an option in the repertoire of real-life translators could be taken as a crucial factor in selecting it as the recommended one, but this does not seem to be the rule in Translation Studies. On the contrary: Even when studied and established (which is still a rarity in itself), regularities of behaviour – past or present – have hardly ever been taken very seriously as a basis for normative pronouncements with respect to what the future holds in store; unless, of course, the findings happened to concur with the attitude the researcher came equipped with to begin with.³ In fact, a major motive for issuing directives has often been overall dissatisfaction with what was going on in the world of our experience, coupled with a wish – an almost uncontrollable urge, at times – to effect changes in it. Very often, the recommended changes are not even drawn from the behaviour of a definable group of translators, not even from the subset comprising those regarded as ‘competent’, or ‘qualified

3. A seeming exception is a recent corpus-based study by Hans Lindquist (1989) of English adverbials and their Swedish renderings, aimed both at explicating (past) translational behaviour and at providing guidelines for (future) translation choices (pp. 27–28). The author was careful to state that “no hard and fast rules [= directives] should be formulated on the basis of the material” (p. 28) and made it a point of suppressing any evaluation. However, his preferences often show nevertheless.

professionals' within a culture, as recently suggested by Chesterman (1993). Even less so can these changes be said to represent an ordered – and reasoned – transition from 'is' to 'ought', as Chesterman further suggests (following Searle 1964).⁴

Thus, even in cases where a corpus-based study has been carried out (e.g., – with respect to our metaphor example – Dagut 1976; 1978: 91–120, or Kjær 1988), general formulations are seldom an immediate outcome of the regularities observed, much less so justified on the basis of these regularities. More often than not, these generalizations, and especially the guidelines for future behaviour allegedly derived from them, are filtered through a particular concept of what would constitute a 'better' (or 'worse') translational strategy or solution, which enables the researcher to look down on the translators whose behaviour is supposedly under observation and claim better knowledge (see Chapter 3, Section 5). This patronizing attitude also accounts for the fact that recommendations are very often put forward as being far more valid than warranted by the underlying study and its corpus. More often than not, no attempt is made to even justify such an extension of application, from, e.g., translation into language/culture A to translation at large, irrespective of the target language/culture. (And see my critical review of Kjær's study [Toury 1989], which is still one of the best.) To be sure, it is not easy to see how it could be justified either, without further studies in the same theoretical framework, using the same methods, into different corpuses, and a comparison of their results in an attempt to go beyond the norms which pertain to the cultural (sub)system directly represented by the initial corpus or any other.

2. The probabilistic nature of translational laws

Obviously, descriptive-explanatory investigations can be rewarding not only in the attempt to draw the applied extensions of Translation Studies closer to real-life behaviour, thus mitigating whatever pretentiousness they are liable to display. They also form a vital link in the elaboration of translation theory itself,

4. In addition to all the difficulties inherent in his suggestion itself to base the derivation of what he termed 'normative laws' on the behaviour of 'competent professional translators', Chesterman neglected to tackle the crucial question of what would constitute 'competent professionals' for that purpose, especially if they are not to be defined in a circular way; i.e., as those translators who 'do' perform translation in the way it 'ought' to be performed, according to anybody's view.

in the helical way outlined in Part One: from the most elementary kind of theoretical framework, equipped only to deal with what translation *can*, in principle, *involve*, through that which translation *does involve*, under varying circumstances, to the statement of what *it is likely to involve*, under one set of conditions or another. The latter represents a more elaborate form of a theory, and one which is bound to become more and more intricate with each subsequent study.

It is this kind of gradually unfolding theory which is most suitable to host the kind of laws we seek, since it is the *relations between variables* that they are after, more than each variable in itself. Establishing individual variables is, of course, a necessary condition for being able to state their relations, but it is far from sufficient. In fact, as I see it, translation theory will not be released from the corner it has been driven into until it loses its neutrality with respect to the manifold factors which may affect translation, on the one hand, while resisting all temptation to replace this neutrality by mere normative, often arbitrarily constituted formulations, on the other.

Each relational law, when uncovered and properly formulated, will have an unmistakably **conditional** form of the following basic type:

if X, then the greater/the lesser the likelihood that Y

(where Y is an observed behaviour, or a certain part/aspect thereof, and/or their observable result, and X is the conditioning factor.) The desired theory — a set of laws of this type striving at growing coherence — would thus acquire a **probabilistic** form, which would put it in line with recent developments in other sciences of man (see especially Halliday 1991).

Obviously, no conditioned law is conceivable unless the conditions are *specifiable*, to begin with. On the other hand, it would be impossible to formulate so much as one single law without first *specifying* the conditions pertinent to it. Thus, unlike the directives which now dominate the field, the quest for laws would have to take into full consideration regularities of actual behaviour obtained by an evergrowing (and ever more variegated) series of studies into well-defined corpuses (see also Baker 1993). This, in fact, is the main justification for my insistence on the vital role of descriptive-explanatory inquiries into translation in the evolution of the discipline, and on the pivotal position of DTS as a distinct branch. As I have tried to demonstrate throughout, what we need is not only *observational*, but *experimental* studies as well, where some of the

variables are far easier to control and the studies themselves have a much higher level of replicability, at least in principle.

This is not to say that every descriptive study is, or even should be carried out with a view to revising a theory. In fact, as I see it, one of the weaknesses of Translation Studies in the present phase of its evolution lies precisely in the fact that descriptivism as such is often looked down upon, driving almost every scholar to theorize, very often in a highly speculative manner. It will be recalled that a main point in regarding the discipline as a divisible territory was to cater for a better *division of labour* between practitioners locating themselves at different points on the 'map'. It is to be hoped that acknowledgement of the centrality of descriptive-explanatory work of all kinds will indeed bring about a healthier distinction between the tasks of researchers and theoreticians.

At the same time, the findings of a well-executed study will always bear on the theory in whose framework it has been performed, thus contributing to the verification/refutation/modification of this theory, whether theory-relevant implications are drawn by the researchers themselves or by empirically minded theoreticians. A theory thus refined will, in turn, make possible the execution of yet more elaborate studies, which will then reflect on the theory and render it even more intricate; and so on and so forth, towards an increasingly better understanding of the ways translation and translators, as individuals and members of societal groups alike, manoeuvre within the manifold constraints imposed on them, and produce texts which look and function the way they do.

In this process of refinement it will become necessary to do more than just accumulate isolated variables and state the relations between *pairs* of them, which would have led to formulations of the type

if X_1 , and/or X_2 , and/or ... X_n , then the greater the likelihood that Y, whereas if Z_1 , Z_2 , and/or ... Z_n , then the lesser the likelihood that Y,

which are basically *linear*. It will also be necessary to weigh the individual factors and their bearing on translation against each other, as well as establishing their interconnections. The ultimate objective is thus to give every law a *multi*-conditional format, i.e.,

if X_1 and Z_1 , then the likelihood that Y is greater than if X_1 and Z_2 , and even greater than if X_1 and Z_3 .

Proceeding this way, translation theory will ultimately become a series of truly interconnected hypotheses, which is the only kind of theory which would offer a possibility of supplementing exhaustive **descriptions** and viable **explanations** with justifiable **predictions**. Incidentally, it would also make it possible to explain the occasional *failure* of a prediction, by entailing the option of searching for variables which have presumably remained unknown, or changing the respective positions and relationships of [some of] those which have been known all along; in other words, by continuing to perform the helical movement.

A progression of this kind is of course infinite: On the one hand, there will always be something further to account for, so that future studies will become more and more focussed, more and more targeted; on the other hand, the theory of translation can always be further refined. And most fortunately so. After all, one would hate to be accused of foreseeing the end of one's discipline at the very outset... Looking at it the other way around, Translation Studies can be viewed as an inherently *optimistic* kind of discipline, ever seeking to refine the descriptions and explanations performed within its framework while improving the probability of its predictions, as boldly suggested by Chesterman (1993).

At this point, the presentation of a couple of exemplary laws of translational behaviour is called for. Unfortunately, the little deliberate effort invested so far in the establishment of such laws will not enable me to do much more than present two of them in their raw form and trace parts of their conditionalization: the law of growing standardization and the law of interference, which will, furthermore, be found to be largely interconnected.

3. Two exemplary laws

I will start with a gradually unfolding and partly exemplified exposition of one basic law which decades of text-based research into translational products, in many different cultures, have been able to come up with; namely, the law of growing standardization, which can also be presented as the law of the conversion of textemes to repertoremes.

3.1. *The law of growing standardization*

In its most general form, where no conditions have been specified, the tentative law of growing standardization may read as follows:

in translation, source-text textemes tend to be converted into target-language (or target-culture) repertoireemes.

The rationale underlying this formulation is simple enough:

In every community, phenomena of various types, linguistic and non-linguistic alike, which have semiotic value for its members, undergo *codification*. Sets of codified items form *repertoires*, i.e., aggregates governed by systemic relations, which determine the relative availability of items pertaining to such an aggregate for any particular use within the community's culture. In other words, a repertoire amounts to the range of choices which makes cultural functions realizable through real products and practices (see Even-Zohar 1990: 40–43). Any sign, irrespective of rank and scope, which forms part of such an institutionalized repertoire would be defined as a **repertoireme**.

When a repertoireme is retrieved from the repertoire it is part of and is put to actual use (i.e., inserted in a particular *text*, in the most general sense of the word), it enters into a unique network of internal relations, peculiar to that act/text. These relations lend the retrieved item *ad hoc textual functions*, by virtue of which it is rendered a **texteme**. One and the same item – or, more precisely, different aspects of it – can, of course, partake of several textual structures. As a result, its textemic status is enhanced through *syncretism*, i.e., the simultaneous coexistence of a number of functions on one single carrier.

On the basis of this brief account, our law can be reformulated to read as follows:

in translation, textual relations obtaining in the original are often modified, sometimes to the point of being totally ignored, in favour of [more] habitual options offered by a target repertoire.

Implied is the claim that the dissolution of textual relations, inevitable in translation, is by no means temporary, and is far from characterizing the initial phase of the process only. Rather, the disintegration of the patterns exhibited by a source text is hardly ever reparable in full: its traces can still be observed after the final phase of recomposition has been completed, especially under a comparative observation of translated vs. source texts. This is one of many reasons why translations so often manifest greater standardization than their sources: in practice, decomposed textual patterns are normally reconstructed to a lesser extent than is initially possible.

Regard the English phrase 'drifting up', whose initial collocability with either 'lawn' or 'vines' is rather low. In the following text, this phrase was made part of a figurative network implying a race over hurdles (see the italicized portions), thus being elevated to the textemic status of a (relatively) living metaphor:

The lawn *started* at the beach and *ran* toward the front door for a quarter of a mile, *jumping over* sundials and brick walks and burning gardens — finally when it reached the house *drifting up* the side in bright vines as though from the momentum of its run. (Fitzgerald 1972 [1926]: 12–13; all emphases added)

And this is how 'drifting up' in this ad hoc use was translated into three different languages; independently, to be sure:

- TT₁: (German): **emporrante**
TT₂: (French): **comme emportée**
TT₃: (Hebrew): (kedei) **letapes** [= (in order) to climb]

All three translations thus used items of their respective linguistic repertoires whose collocability with the replacements of 'lawn' and/or 'vines' (which occur in all three of them) is **much more habitual**, rendering the overall textual structure of each of them considerably looser than that of the original.

The following is another alternative formulation of our law:

in translation, items tend to be selected on a level which is lower than the one where textual relations have been established in the source text.

Regard, for instance, the "Rocky" passage used by Anderson et al. (1977) to illustrate the different possible interpretations of one piece of text in terms of knowledge structures or 'schemata':

Rocky slowly *got up* from the mat, *planning* his escape. He hesitated a moment and thought. Things were not going well. What bothered him most was *being held*, especially since the *charge against him* had been weak. He considered his present situation. The *lock that held him* was strong, but he thought he could *break* it.

Each of the following German translations, supplied by advanced translation students at Heidelberg (Germany) and presented by Kurt Kohn in a recent Conference (see Neubert et al. in press), is no doubt much simpler than the source text, which — devoid of background information — allows for both readings — in full accordance with the law:

TT₁: "Ringkampf"

Rocky gelang es, langsam von der *Matte* wieder *hochzukommen*. Er *versuchte*, sich zu *befreien*. Einen Augenblick lang zögerte er und überlegte. Es stand nicht gerade gut. Am meisten ärgerte ihn, daß er sich in einem *festen Griff* befand; dabei war der *Angriff* eher schwach gewesen. Rocky konzentrierte sich auf seine Situation. Der *Griff*, mit dem er gehalten wurde, war zwar stark, aber er war überzeugt, daß er ihn würde *brechen* können.

TT₂: "Gefängnis"

Langsam *stand* Rocky von seiner *Pritsche* (Matraze) *auf*. Er plante seinen *Ausbruch*. Für einen Moment zögerte er noch und dachte nach. Die Dinge hatten sich nicht gerade gut entwickelt. Das schlimmste war, daß er jetzt *im Gefängnis* saß; dabei war die *Anklage* eher schwach gewesen. Er konzentrierte sich auf seine augenblickliche Lage. Das *Schloß* war zwar stark, aber er war überzeugt, daß er es würde *knacken* können.

Under certain circumstances, the ambiguity inherent in the source text may itself count as functional; that is, it may form part of the overall semantic structure of the text. Under such circumstances, any disambiguation (a feature the two German translations share) would result in greater simplification still, as it would involve an irreparable dissolution of more, and more intricate textual relations.

Having Kohn's experiment duplicated in a controlled way – that is, having subjects of *different* backgrounds translate the "Rocky" passage – may well shed more light on the issue at hand. Thus, for reasons beyond the scope of this discussion, the "Gefängnis" option seems to be the *unmarked* among the two modes of disambiguation. However, within a milieu where the name Rocky has come to be habitually associated with boxing, this last fact may well tilt the balance in favour of the "Ringkampf" option. Then again, it would not be unreasonable to assume that there would also be translations – e.g., those produced by inexperienced translators, who often resort to rather small, and low-rank translation units – which would be impure, in terms of that disambiguation; these would present an admixture of elements belonging to *both* organizing frameworks.

All this, and much more, would of course add weight to the claim that in real-life translation situations, decisions tend to be made on a level which is lower than that of (the network of relationships which constitutes) the text. What is more, other variables (such as biological and bilingual age, or previous experience in translation of different kinds and for different purposes) may all turn out to be in a position to influence translational behaviour, and may hence

be rendered as 'conditions' in a more elaborate, multiconditional formulation of the law of the conversion of textemes into repertoemes.

This fact notwithstanding, the law can also be conditionalized beyond the level of previous knowledge, or experience, of translators as individuals; for instance, by recourse to one of Even-Zohar's cultural-semiotic hypotheses, first put forward in 1978. Thus, there seems to be a discernible correlation between the degree of flexibility (or rigidity) with which the law is adhered to in a particular (sub)culture and the position assigned in it to translation, both as a type of activity and as an aggregate of texts, such that

the more peripheral this status, the more translation will accommodate itself to established models and repertoires.

That is to say, only when *centrality* is assigned to translating and/or translations will the law show signs of cracking – within one culture (e.g., with respect to texts of different types), or cross-culturally (e.g., literary translation in England vs. Israel), including different periods in the history of one culture (e.g., translation into Hebrew in Germany during the Enlightenment period and in today's Israel).⁵

If this condition holds true, then the operation of our law, or its failure to operate, may serve, conversely, as an indication of the position assumed by translation, or by a certain section thereof, in the target system – which is an important part of any *individual* study. Since descriptive research has amply demonstrated that this law is only seldom broken, and even then only to a rather limited extent, this can be taken as a verification of another of Even-Zohar's 1978 hypotheses; namely, that

5. It seems that the most common *historical* move has been towards greater reliance on the verbal formulation of the source text, even at the price of a lesser extent of acceptability (which was actually connected with the establishment of different concepts of acceptability as a text in the target language/culture and a translation into it). This is one way of connecting the present law with the one which will be discussed later on, the law of interference. Needless to say, this move has not occurred at the same time, or evolutionary phase, in all cultures either, nor even with respect to all types of translation and/or translation of all text-types within one and the same culture. And see, e.g., the claim made by Robyns (1990) that in the 1950s–1960s, Anglo-American detective novels were translated into French according to the tradition of translations known as *belles infidèles*, which was said to have disappeared in the 19th century; for canonized literature, to be sure.

translation tends to assume a *peripheral* position in the target system, generally employing secondary models and serving as a major factor of conservatism.

More, and better focussed studies are obviously needed to determine what other factors there are which may reinforce (or attenuate) the operation of our law as well as establish the relative force of these factors themselves and their interdependencies. Thus, there are probably basic cognitive factors which enhance the validity of the conversion law; for instance, limitations of memory and/or other difficulties in drawing abstractions over longer stretches of text, a *sine qua non* for being able to construct 'mental maps' of either the source or the target text (as posited, for instance, by Holmes 1988: 81-91 [1978]) and to compare them to each other. Consequently, it would seem that special socio-cultural circumstances are not enough for breaking loose from the bonds of the conversion law. Special cognitive efforts are also probably required, if one is to approximate the reconstruction in target-language material of the network of relationships constituting a source-language text. Moreover, even though basic cognitive capacities are probably universal, it may well be that the way they manifest themselves in individual instances is socio-culturally constrained too, or else they would immediately be marked as deviating from dominant patterns of behaviour, with all that this implies.

To be sure, it is clear that in translation, **repertoremes are converted into textemes** too, inasmuch as the end product is identifiable as a *text* to begin with (which implies the existence of an internal network of relations). Textemes occurring in a translated text can of course result from either *reconstruction* (of source-text relations and their resulting functions) or *construction* (of new webs).

For instance, a thorough comparative analysis by Even-Zohar (1975) has demonstrated in great detail how a semantic pattern running through Baudelaire's "Spleen" ("Quand le ciel bas et lourd"), where the sky, likened to a cover of a vessel, pours a black day on the world, has disappeared from the Hebrew translation and a developed image of a grave appeared instead. Both translation and source text are thus highly intricate and rich in elements of textemic status, yet their constitutive principles are very different.

To be sure, care should be taken not to couple the obliteration of a source textual pattern and the introduction into the translation of a different set of textual relations in such a way as to regard them automatically — that is, on the basis of absence vs. presence alone — as a pair, allegedly testifying to what is

sometimes, often rather naively, referred to as *compensation*. The two practices may well be *independent* and reflect two completely different, and totally unconnected series of translation decisions.⁶ This seems to have been the case in the Baudelaire poem: there is absolutely no reason to assume that the substitution as such of the 'grave' pattern for the 'vessel' one was *intentional*. It was more likely a result of a universal of the kind expressed by our law of conversion reinforced by certain needs of the *recipient* culture of the time.

The relations between obliterating source textual patterns and introducing new ones into its translation are not a given quantity then. Rather, they should always be submitted to investigation; and not from the point of view of the *end product* alone (as a possible explanatory hypothesis of the shifts which may manifest themselves), but also from the point of view of the *process* which has yielded it (i.e., as a possible translation strategy). Also, interestingly, descriptive studies have been able to show that even if new structures have been introduced into a translation, and even if there is reason to regard them (or some of them) as compensating for dissolved source-text structures, translations still tend to reveal reduced rates of structuration (that is, simplification, or flattening) vis-à-vis their sources; which is justification enough for our selection of the law of converting *textemes* into *repertoremes* as a privileged law of translation behaviour.

The gradually unfolding theory of translation emerges as optimistic in yet another respect, due to the possibility of taking the laws formulated within it as a basis for conscious *manipulation*, e.g., in translator training. Thus, while a theory should certainly not be concerned with bringing about changes in the world of our experience, it is precisely one of the advantages of laws of the kind envisaged here that they *can* be projected onto the applied extensions of Translation Studies too. After all, once a law has been uncovered and formulated, it can be passed on as a piece of knowledge, conditioning factors and all. From this point on, one can be taught how to behave; not only *in accordance with the law* (which is what one tends to do anyway, otherwise it would hardly have emerged as a law in the first place), but also *contrary to it*, if this is deemed appropriate, be it for the sake of sheer exercise or for any other reason — fully aware, however, of the deviation from prevalent patterns of behaviour, and hence ready to take the consequences.

A decision to consciously adopt 'compensation' as a strategy (e.g., Hervey

6. And compare Jones' brief discussion of a target-language replacement "which adds an extra level of structuration" which did not exist in the source text (1989: 192-193).

and Higgins 1992: 34–40) would thus count as a manipulation of the tension between textemic and repertoireemic status, and the principle itself could be imparted to translation trainees with relative ease.

Coming back one last time to our metaphor example, one could thus be trained to give up any attempt to reconstruct each and every living metaphor on the spot, just because that is where it is, and resort to establishing other target-language metaphors, located elsewhere, instead. In this way, the very *occurrence* of living metaphors, even metaphoric *density*, would be retained as a **textual feature**, which would amount to giving priority to 'global' over 'local' considerations while paying an obvious price in terms of small-scale matching. (And see my detailed treatment of metaphor as a translation problem in Chapter 3, Section 5.)

A much more general manipulative step in the same vein might be to promote 'textemic translation', in spite of the fact that it has proved so marginal – the exception rather than the rule – in actual practice. In such a case, the recommendation would read:

replace source-text textemes with ad hoc combinations of textual relations equivalent to those found in that text and target-language items capable of fulfilling these functions,

even if, as a result, certain deviations from target repertoires would ensue, with possible bearing on the acceptability of the target text; either as a target-language text or even as a translation into it. Or else it would be realized that the law of converting textemes into repertoireemes has proven more powerful again, despite the conscious attempt to prevent it from becoming operative. The latter situation would incidentally contribute to greater awareness of the basic nature of the law and to acknowledgement of the crucial fact that every translational decision has its price elsewhere, which – to me – constitutes the most appropriate conceptual framework for translator training anyway (see Excursus C, Section 5).

3.2. *First steps towards a law of interference*

Let me conclude with a gradual unveiling of a second law which I have been contemplating for years, a so-called law of interference. Nor is this law totally unconnected to the law of conversion, which should be taken as a first welcome step towards the establishment of translation theory as an intricate system of laws.

In its most general form, the law of interference would read:

in translation, phenomena pertaining to the make-up of the source text tend to be transferred to the target text,

whether they manifest themselves in the form of *negative transfer* (i.e., deviations from normal, codified practices of the target system), or in the form of *positive transfer* (i.e., greater likelihood of selecting features which do exist and are used in any case). This general formulation implies that interference is a kind of *default*, so that the establishment of an interference-free output (or even of an output where interference has been relegated to less disturbing domains) necessitates special conditions and/or special efforts on the translator's part.

Basically, what I have called 'discourse transfer' (see Excursus C) has to do with the basic mental processes involved in translation, especially the series of rapid switchings between source and target codes, alternating in both directions. It is thus our mental apparatus which is probably at the root of the universality of transfer in translation. (See also Danchev 1982; James 1988.) In this sense, discourse transfer is the external manifestation of a **general cognitive law** (see, e.g., Albert and Obler 1978: 209-212). However, were it a matter of pure cognition, all translators, in all cultures, would be expected to behave in just one way while translating anything and everything. Similarly, all cultures, in whatever phase of development, would have to accept interference, at least in the same way/to the same extent, and there would be little point in waging war against it. The fact that none of these consequences hold should lead to the conclusion that tolerance of interference — and hence the realization of interference itself — have to do with the **socio-cultural conditions** in which translation is performed and consumed as much as they have to do with our cognitive machinery. There are therefore good reasons to count socio-cultural factors again among the important conditions of the law. This will be done soon enough. First let us stay a little longer within the cognitive paradigm and carry out a tentative process of conditionalization, starting from the following, rather trivial observation concerning discourse transfer.

The extent to which interference actually shows in a translation has to do with whether the source text was approached and processed as *one entity*, a holistic message in an act of communication, or as an *organization of lower-level linguistic entities*; for every text is obviously both. The real question is, of course, one of balance. After all, performing translation (and establishing a translational product) with absolutely no recourse to, and no concern for the source text as a

higher-level organization of linguistic items – which might have yielded no interference, not even of the ‘positive’ type – is hardly conceivable; not even in the case of very short, very simple texts which could presumably be retained in short-term memory. On the other hand, attempts to produce translations which would represent their sources ‘as closely as possible’ are in constant tension with the attempts to establish in a target culture and language native-like texts, especially if these are designed to perform for the target audience a job which is more or less similar to the one performed by the source texts for their own addressees (which has been a recurrent requirement in many cultural domains).

From this it should be clear that

the more the make-up of a text is taken as a factor in the formulation of its translation, the more the target text can be expected to show traces of interference,⁷

no matter (for the time being) what it was that brought about the dependence on the make-up of the source-text. Suffice it to say that, popular beliefs notwithstanding, the distance between languages, textual traditions and/or entire cultures seems to have no *automatic* bearing on the extent of interference. Similarity may well have its implications for the proportion of ‘negative’ vs. ‘positive’ transfer, but as, psycholinguistically speaking, there is only *one* procedure which yields both, interference as such will always be present. It may just be more or less readily seen.

As we have seen in the case of the train notice example (Chapter 4), even with regard to short, relatively simple texts, which are, moreover, codified entities in their respective cultures, translational behaviour cannot be expected to be automatic; and most texts in our world of experience – both originals and translations – are not of that kind anyway. Rather, they are more or less *unique* acts of linguistic behaviour, which would add grounds for processing them as organizations of entities pertaining to lower levels.⁸

7. Of course, from the *researcher's* point of view, what is actually at work is the reciprocal theorem:

The more a translation shows traces of interference, the more closely the make-up of the source text can be hypothesized to have been leaned upon in the translation process.

8. And see again the experiment reported in Chapter 12, Section 2.1, where advanced students of translation were found to replace the German simile *strahlenförmig* by the

This fact notwithstanding (and here we are moving towards a possible conditioning factor), tackling larger and/or higher-level constituents of a text has normally been presented as a function of *professionality*, so that

even when taking the source text as a crucial factor in the formulation of its translation, accomplished translators would be less affected by its actual make-up.

Such a correlation is of course far from wrong. However, there is much more to it, and therefore it seems insufficient even as an explanation of the difference of approach between more and less experienced translators, or even between experienced translators and novices.

Even if it is true that "Experten und Novizen unterscheiden sich nicht in der kognitiven Hardware ihres Gedächtnisses, sondern eben in der kognitiven Software und der Organisation der Datenbasis" (Esser 1990: 85), the transition from text-, let alone language-orientedness to communication-orientedness, or, alternatively, from source-bound to target-bound decisions, cannot count as self-evident in any sense. This is amply demonstrated by huge portions of the history of translation, even that part of it which was carried out by persons considered 'accomplished translators' in their own time and place: they were no more ignorant of what could be achieved than are today's translators; they simply operated within different socio-cultural settings and hence had different norms as guidelines for their translational behaviour.

The alleged undesirability of interference is thus not 'natural' in any sense. Rather, if and when it is rejected, its undesirability is always a function of a host of **socio-cultural factors**, which may therefore be said to condition our law. Here it would be quite safe to start by arguing, very generally, that

communities differ in terms of their resistance to interference, especially of the 'negative' type.

Strong resistance to interference may indeed lead to a considerable reduction of its manifestations, especially in the translational output of professionals, shaped

English expressions 'like the rays of the sun' or 'like rays of a star'. This tendency reveals a high rate of interference, especially in view of the practices of speakers of English under non-translational conditions, where 'the spokes of a wheel' was normally used to express the same visual contents.

as it is by the environmental feedback (see Excursus C). Thus, resistance quite readily leads to the activation of *purification*, or other *ensorial mechanisms*, whose influence, however, can hardly ever be absolute, due to cognitive as well as behavioural factors. These mechanisms are often resorted to post factum, after the act of translation has been terminated, by way of [post]-editing, whether by the translator him-/herself or by some other agent, who may have had a different kind of training and was charged with other responsibilities. Often, such a revisor is not even required to know the source language, and even if s/he does, it is not necessarily the case that s/he also falls back on it. Censorship can also be activated during the act of translation itself though, inasmuch as the translator has *internalized* the norms pertinent to the culture, and uses them as a constant monitoring device.

However, even the correlation of interference, and tolerance of it, with the overall normative structure of a particular target culture should be submitted to further modification. For it is not as if, within one culture, interference of all 'foreign' languages, all textual traditions, all cultures is always equally tolerated (or rejected). What should be brought in as another conditioning factor is thus the *relative prestige* of cultures and languages (as seen from the vantage point of the prospective target system) and their *power relations* with the latter. The rule here seems to be that

tolerance of interference — and hence the endurance of its manifestations — tend to increase when translation is carried out from a 'major' or highly prestigious language/culture, especially if the target language/culture is 'minor', or 'weak' in any other sense,

'majority' and 'minority', 'strength' and 'weakness' being relative rather than fixed, let alone inherent features of languages and cultures.

The sweeping generality of the correlation just established between socio-cultural circumstances and discourse interference in translation may well have to undergo further modifications still, along various additional axes; for instance, with respect to *text-type*. Thus, towards the end of the 20th century, one would hardly be surprised to find — within one target culture, and with respect to the same source culture — differences in the tolerance of, leading to differences in recourse to interference between, say, instructions for the use of an electric appliance and a lyrical poem; not only, not even mainly on the (lower) linguistic levels, but also on the (highest) level of the textual model.

For instance, the claim may well be that instructions for the use of a VCR translated from Japanese into German are likely to be recast into a textual model favoured by the German culture (which may well entail a movement away from the organizational principles of the source text) – whereas a translated haiku would often be acceptable, maybe even *more* acceptable, when the Japanese model is reconstructed to capacity. To be sure, this has not always been the case, which means that more work is warranted on the correlations between socio-cultural factors and text-type. Thus, technical translation has not always involved an overall change of model, whereas the dominant tendency in the translation of haiku poems into western languages has long been to remove elements of the original model and adopt conspicuous elements of *domestic* textual models instead in order to make the translations look more like European poems (see Excursus B).

There are no doubt many more conditioning factors which need – and deserve – to be established, along with their implications for our law. Thus, the following theorem, which stems from the fact that interference on one level, or in one domain, does not necessarily entail interference on all other levels, or in all other domains, may well represent the beginning of yet another process of conditionalization:

even for one and the same text, neither interference nor tolerance of it are necessarily the same with respect to all linguistic and textual levels.

This fact also makes it possible to *manipulate* interference on the basis of discriminate treatment; e.g., work towards its reduction where its manifestations are considered most annoying while ignoring it in domains where it is considered less problematic. On occasion, this would even make it possible to deliberately adopt interference as a *strategy*; e.g., in an attempt to enrich the target culture/language, in domains regarded as needing such enrichment, in an act of cultural planning. (See Toury 1985b.)

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A replacement of the author's well-known book on Translation Theory, *In Search of a Theory of Translation* (1980), this book makes a case for Descriptive Translation Studies as a scholarly activity as well as a branch of the discipline, having immediate consequences for issues of both a theoretical and applied nature. Methodological discussions are complemented by an assortment of case studies of various scopes and levels, with emphasis on the need to contextualize whatever one sets out to focus on.

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"Toury has written a highly thought-provoking book. It opens up new horizons not only to descriptive but also to applied translation studies. I hope the discussion between them will go on to their mutual benefit."

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ISBN 978 90 272 1606 9 (EUR) / 978 1 55619 687 4 (US)

John Benjamins Publishing Company